

A reproduction of the latter, which is almost equal to the original, is given in the catalogue.

Among several interesting radiographs, Dr. C. Thurstan Holland exhibits one of an "Adult male chest, through all the clothing, taken with a Snook Transformer. Instantaneous exposure." Dr. H. Ronen, Dr. W. J. S. Lockyer, and Stonyhurst College contribute astronomical and spectroscopical photographs. There are two or three excellent telephotographs, and a single exhibit of photo-mechanical work.

THE BRITISH ASSOCIATION AT SHEFFIELD.

WRITING on the eve of the meeting, there is every prospect that the Sheffield meeting of the association will be a successful one, and a certainty that, with an improvement in the weather conditions, it will be an enjoyable one. A considerable amount of private hospitality is being dispensed, but, even so, late arrivals will have some difficulty in finding accommodation. Local interest is considerable, and all classes are combining to give a welcome worthy of the city. One special feature will be the opportunity afforded to members to inspect the operations connected with armour and armament, all the firms engaged in naval shipbuilding opening their works to large parties. Members should be, however, careful to apply for tickets immediately on arrival, as some are open only on the Thursday. The technical instruction and investigations in iron and steel metallurgy carried on in the University have been of very material assistance in enabling Sheffield to maintain its leading position as the centre of high-class steel production, and the University is arranging to run its furnaces and special plants on afternoons at the close of the sectional meetings. Amongst the most interesting may be mentioned the crucible house, the Kjellin electric melting furnace, the electric hardening furnace, and the new form of the Siemens' acid open-hearth furnace. Interest will also be taken in the exhibition of Dr. Sorby's original micro-sections.

The Sheffield Musical Union is giving a concert to the association on the Saturday evening, under the conductorship of Dr. Coward. This is an innovation which will enable members to hear the famed Sheffield Choir, the programme including, besides part-songs, choral selections from the "Messiah" and the "Golden Legend."

In consonance with the growing fancy in England for ceremonial, the local committee have arranged for a procession on Sunday of representatives of the association, the Town Council, and the University from the Town Hall to the Parish Church, where the Archbishop of York is to preach the official sermon.

On Tuesday the University will hold a congregation to confer honorary degrees on leading representatives of the association. The recipients are to be:—The President (Dr. Bonney), the Lord Mayor (Earl Fitzwilliam, chairman of the local committee), Mr. W. H. Bateson, Sir W. Crookes, Mr. Francis Darwin, Sir A. Geikie, Prof. Hobson, Sir J. Jonas, Sir Norman Lockyer, Sir O. Lodge, Principal Miers, Sir W. Ramsay, Prof. Rhys Davies, Prof. Sherrington, Mr. J. E. Stead, Sir J. J. Thomson, and Sir W. White. Later the same evening the Chancellor of the University (the Duke of Norfolk, E.M., K.G.) will hold a reception. In connection with this an attempt is being made to arrange a scientific exhibition, more especially in relation to papers read before the various sections. It is hoped thereby to encourage this method of illustrating papers and at the same time to render them available to a wider circle of members. Simultaneously with the reception at the University, the Lord Mayor is receiving local guests at the Art Gallery; and the Weston Park, between it and the University, is to be the scene of an

evening garden-party, with a military tattoo and torch-light procession.

INAUGURAL ADDRESS BY THE REV. PROF. T. G. BONNEY, Sc.D., LL.D., F.R.S., PRESIDENT OF THE ASSOCIATION.

THIRTY-ONE years have passed since the British Association met in Sheffield, and the interval has been marked by exceptional progress. A town has become a city, the head of its municipality a Lord Mayor; its area has been enlarged by more than one-fifth; its population has increased from about 280,000 to 479,000. Communication has been facilitated by the construction of nearly thirty-eight miles of electric tramways for home service and of new railways, including alternative routes to Manchester and London. The supplies of electricity, gas, and water have more than kept pace with the wants of the city. The first was just being attempted in 1879; the second has now twenty-three times as many consumers as in those days; the story¹ of the third has been told by one who knows it well, so that it is enough for me to say your water supply cannot be surpassed for quantity and quality by any in the kingdom. Nor has Sheffield fallen behind other cities in its public buildings. In 1897 your handsome Town Hall was opened by the late Queen Victoria; the new Post Office, appropriately built and adorned with material from almost local sources, was inaugurated less than two months ago. The Mappin Art Gallery commemorates the munificence of those whose name it bears, and fosters that love of the beautiful which Ruskin sought to awaken by his generous gifts. Last, but not least, Sheffield has shown that it could not rest satisfied until its citizens could ascend from their own doors to the highest rung of the educational ladder. Firth College, named after its generous founder, was born in the year of our last visit; in 1897 it received a charter as the University College of Sheffield, and in the spring of 1905 was created a University, shortly after which its fine new buildings were opened by the late King; and last year its library, the generous gift of Dr. Edgar Allen, was inaugurated by his successor, when Prince of Wales. I must not now dwell on the great work which awaits this and other new universities. It is for them to prove that, so far from abstract thought being antagonistic to practical work, or scientific research to the labour of the factory or foundry, the one and the other can harmoniously cooperate in the advance of knowledge and the progress of civilisation.

You often permit your President on these occasions to speak of a subject in which he takes a special interest, and I prefer thus trespassing on your kindness to attempting a general review of recent progress in science. I do not, however, propose, as you might naturally expect, to discuss some branch of petrology; though for this no place could be more appropriate than Sheffield, since it was the birthplace and the lifelong home of Henry Clifton Sorby, who may truly be called the father of that science. This title he won when, a little more than sixty years ago, he began to study the structure and mineral composition of rocks by examining thin sections of them under the microscope.² A rare combination of a singularly versatile and active intellect with accurate thought and sound judgment, shrewd in nature, as became a Yorkshireman, yet gentle, kindly, and unselfish, he was one whom his friends loved and of whom this city may well be proud. Sorby's name will be kept alive among you by the Professorship of Geology which he has endowed in your University; but, as the funds will not be available for some time, and as that science is so intimately connected with metallurgy, coal-mining, and engineering, I venture to express a hope that some of your wealthier citizens will provide for the temporary deficiency, and thus worthily commemorate one so distinguished.

But to return. I have not selected petrology as my subject, partly because I think that the great attention which its more minute details have of late received has tended to limit rather than to broaden our views, while

¹ "History and Description of Sheffield Water Works." W. Terrey, 1908.

² His subsequent investigations into the microscopic structure of steel and other alloys of iron, in the manufacture of which your city holds a foremost place, have been extended by Mr. J. E. Stead and others, and they, besides being of great value to industrial progress, have thrown important sidelights on more than one dark place in petrology.

for a survey of our present position it is enough to refer to the suggestive and comprehensive volume published last year by Mr. A. Harker;¹ partly, also, because the discussion of any branch of petrology would involve so many technicalities that I fear it would be found tedious by a large majority of my audience. So I have preferred to discuss some questions relating to the effects of ice which had engaged my attention a dozen years before I attempted the study of rock slices. As much of my petrological work has been connected with mountain districts, it has been possible for me to carry on the latter without neglecting the former, and my study of ice-work gradually led me from the highlands into the lowlands.² I purpose, then, to ask your attention this evening to some aspects of the glacial history of Western Europe.

At no very distant geological epoch the climate in the northern part of the earth was much colder than it is at present. So it was also in the southern; but whether the two were contemporaneous is less certain. Still more doubtful are the extent and the work of the ice which was a consequence, and the origin of certain deposits on some northern lowlands, including those of our own islands, namely, whether they are the directavings of glaciers or were laid down beneath the sea by floating shore-ice and bergs. Much light will be thrown on this complex problem by endeavouring to ascertain what snow and ice have done in some region which, during the Glacial Epoch, was never submerged, and none better can be found for this purpose than the European Alps.

At the present day one school of geologists, which of late years has rapidly increased in number, claims for glaciers a very large share in the sculpture of that chain, asserting that they have not only scooped out the marginal lakes, as Sir A. Ramsay maintained full half a century ago, but have also quarried lofty cliffs, excavated great cirques, and deepened parts of the larger Alpine valleys by something like two thousand feet. The other school, while admitting that a glacier, in special circumstances, may hollow out a tarn or small lake and modify the features of rock scenery, declares that its action is abrasive rather than erosive, and that the sculpture of ridges, crags, and valleys was mainly accomplished in pre-Glacial times by running water and the ordinary atmospheric agencies.

In all controversies, as time goes on, hypotheses are apt to masquerade as facts, so that I shall endeavour this evening to disentangle the two, and direct attention to those which may be safely used in drawing a conclusion.

In certain mountain regions, especially those where strong limestones, granites, and other massive rocks are dominant, the valleys are often trench-like, with precipitous sides, having cirques or corries at their heads, and with rather wide and gently sloping floors, which occasionally descend in steps, the distance between these increasing with that from the watershed. Glaciers have unquestionably occupied many of these valleys, but of late years they have been supposed to have taken a large share in excavating them. In order to appreciate their action, we must imagine the glens to be filled up and the district restored to its former condition of a more or less undulating upland. As the mean temperature³ declined, snow would begin to accumulate in inequalities on the upper slopes. This, by melting and freezing, would soften and corrode the underlying material, which would then be removed by rain and wind, gravitation and avalanche. In course of time the hollow thus formed would assume more and more the outlines of a corrie or a cirque by eating into the hillside. With an increasing diameter it would be occupied, as the temperature fell, first by a permanent snowfield, then by the *névé* of a glacier. Another process now becomes important, that called "sapping." While ordinary glacier-scour tends, as we are told, to produce "sweeping curves and eventually a graded slope," "sapping" produces "benches and cliffs, its action being horizontal and backwards," and often dominant over scour. The author of this hypothesis⁴ convinced himself of its truth in the Sierra Nevada by descending a *bergschrund* 150 feet in

depth, which opened out, as is so common, beneath the walls of a cirque. Beginning in the *névé*, it ultimately reached the cliff, so that for the last 30 feet the bold investigator found rock on the one hand and ice on the other. The former was traversed by fracture planes, and was in all stages of displacement and dislodgment; some blocks having fallen to the bottom, others bridging the narrow chasm, and others frozen into the *névé*. Clear ice had formed in the fissures of the cliff; it hung down in great stalactites; it had accumulated in stalagmitic masses on the floor. Beneath the *névé* the temperature would be uniform, so its action would be protective, except where it set up another kind of erosion, presently to be noticed; but in the chasm, we are informed, there would be, at any rate for a considerable part of the year, a daily alternation of freezing and thawing. Thus the cliff would be rapidly undermined and be carried back into the mountain slope, so that before long the glacier would nestle in a shelter of its own making. Farther down the valley the moving ice would become more effective than subglacial streams in deepening its bed; but since the *névé*-flow is almost imperceptible near the head, another agency must be invoked, that of "plucking." The ice grips, like a forceps, any loose or projecting fragment in its rocky bed, wrenches that from its place, and carries it away. The extraction of one tooth weakens the hold of its neighbours, and thus the glen is deepened by "plucking," while it is carried back by "sapping." Streams from melting snows on the slopes above the amphitheatre might have been expected to cooperate vigorously in making it, but of them little account seems to be taken, and we are even told that in some cases the winds probably prevented snow from resting on the rounded surface between two cirque-heads.¹ As these receded, only a narrow neck would be left between them, which would be ultimately cut down into a gap or "col." Thus a region of deep valleys with precipitous sides and heads, of sharp ridges, and of more or less isolated peaks, is substituted for a rather monotonous, if lofty, highland.

The hypothesis is ingenious, but some students of Alpine scenery think more proof desirable before they can accept it as an axiom. For instance, continuous observations are necessary to justify the assumption of diurnal variations of temperature sufficient to produce any sensible effect on rock at the bottom of a narrow chasm nearly fifty yards deep and almost enclosed by ice. Here the conditions would more probably resemble those in a *glacière*, or natural ice cave. In one of these, during the summer, curtains and festoons of ice depend from the walls; from them and from the roof water drips slowly, to be frozen into stalagmitic mounds on the floor, which is itself sometimes a thick bed of ice. On this the quantity of fallen rock débris is not greater than is usual in a cave, nor are the walls notably shattered, even though a gap some four yards deep may separate them from the ice. The floors of cirques, from which the *névé* has vanished, cannot as a rule be examined, because they are masked by débris which is brought down by the numerous cascades, little and big, which seam their walls; but glimpses of them may sometimes be obtained in the smaller corries (which would be cirques if they could), and these show no signs of either "sapping" or "plucking," but some little of abrasion by moving ice. Cirques and corries also not infrequently occur on the sides as well as at the heads of valleys, such, for instance, as the two in the *massif* of the Uri Rothstock on the way to the Surenen Pass and the Fer a Cheval above Sixt. The Lago di Ritom lies between the mouth of a hanging valley and a well-defined step, and just above that is the Lago di Cadagno in a large, steep-walled corrie, which opens laterally into the Val Piora, as that of the Lago di Tremorgio does into the southern side of the Val Bedretto. Cirques may also be found where glaciers have had a comparatively brief existence, as the Creux des Vents on the Jura; or have never been formed, as on the slopes of Salina, one of the Lipari Islands, or in the limestone desert of Lower Egypt.² I have seen a miniature stepped valley carved by a rain-storm on a slope of Hampstead Heath; a cirque, about a yard in height and breadth, similarly excavated in the

¹ "The Natural History of Igneous Rocks," 1900.

² May I add that hereafter a statement of facts without mention of an authority means that I am speaking from personal knowledge.

³ In the remainder of this address "temperature" is to be understood as mean temperature. The Fahrenheit scale is used.

⁴ W. D. Johnson, *Science*, N.S., ix. (1899), pp. 106, 112.

¹ This does not appear to have occurred in the Alps.

² A. J.ukes-Browne, *Geol. Mag.*, 1877, p. 477.

vertical wall of a gravel pit; and a corrie, measured by feet instead of furlongs, at the foot of one of the Binns near Burntisland, or, on a much reduced scale, in a bank of earth. On all these the same agent, plunging water, has left its marks—runlets of rain for the smaller, streams for the larger; convergent at first, perhaps, by accident, afterwards inevitably combined as the hollow widened and deepened. Each of the great cirques is still a “land of streams,” and they are kept permanent for the greater part of the year by beds of snow on the ledges above its walls.

The “sapping and plucking” process presents another difficulty—the steps already mentioned in the floors of valleys. These are supposed to indicate stages at which the excavating glacier transferred its operations to a higher level. But, if so, the outermost one must be the oldest, or the glacier must have been first formed in the lowest part of the incipient valley. Yet, with a falling temperature, the reverse would happen, for otherwise the snow must act as a protective mantle to the mature pre-glacial surface almost down to its base. However much age might have smoothed away youthful angularities, it would be strange if no receptacles had been left higher up to initiate the process; and even if sapping had only modified the form of an older valley, it could not have cut the steps unless it had begun its work on the lowest one. Thus, in the case of the Creux de Champ, if we hesitate to assume that the sapping process began at the mouth of the valley of the Grande Eau above Aigle, we must suppose it to have started somewhere near Ormont Dessus and to have excavated that gigantic hollow, the floor of which lies full 6000 feet below the culminating crags of the Diablerets.

But even if “sapping and plucking” were assigned a comparatively unimportant position in the cutting out of cirques and corries, it might still be maintained that the glaciers of the Ice Age had greatly deepened the valleys of mountain regions. That view is adopted by Profs. Penck and Brückner in their work on the glaciation of the Alps,¹ the value of which even those who cannot accept some of their conclusions will thankfully admit. On one point all parties agree—that a valley cut by a fairly rapid stream in a durable rock is *V*-like in section. With an increase of speed the walls become more vertical; with a diminution the valley widens and has a flatter bed, over which the river, as the base-line is approached, may at last meander. Lateral streams will plough into the slopes, and may be numerous enough to convert them into alternating ridges and furrows. If a valley has been excavated in thick horizontal beds of rock varying in hardness, such as limestones and shales, its sides exhibit a succession of terrace walls and shelving banks, while a marked dip and other dominant structures produce their own modifications. It is also agreed that a valley excavated or greatly enlarged by a glacier should be *U*-like in section. But an Alpine valley, especially as we approach its head, very commonly takes the following form. For some hundreds of feet up from the torrent it is a distinct *V*; above this the slopes become less rapid, changing, say, from 45° to not more than 30°, and that rather suddenly. Still higher comes a region of stone-strewn upland valleys and rugged crags, terminating in ridges and peaks of splintered rock, projecting from a mantle of ice and snow. The *V*-like part is often from 800 to 1000 feet in depth, and the above-named authors maintain that this, with perhaps as much of the more open trough above, was excavated during the Glacial Epoch. Thus the floor of any one of these valleys prior to the Ice Age must often have been at least 1800 feet above its present level.² As a rough estimate, we may fix the deepening of one of the larger Pennine valleys, tributary to the Rhone, to have been, during the Ice Age, at least 1600 feet in their lower parts. Most of them are now hanging valleys, the stream issuing, on the level of the main river, from a deep gorge. Their tributaries are rather variable in form, the larger, as a rule, being more or less *V*-shaped; the shorter, and especially the smaller, corresponding more with the upper

part of the larger valleys, but their lips generally are less deeply notched. Whatever may have been the cause, this rapid change in slope must indicate a corresponding change of action in the erosive agent. Here and there the apex of the *V* may be slightly flattened, but any approach to a real *U* is extremely rare. The retention of the more open form in many small, elevated recesses, from which at the present day but little water descends, suggests that where one of them soon became buried under snow,¹ but was insignificant as a feeder of a glacier, erosion has been for ages almost at a standstill.

The *V*-like lower portion in the section of one of the principal valleys, which is all that some other observers have claimed for the work of a glacier, cannot be ascribed to subsequent modification by water, because ice-worn rock can be seen in many places, not only high up its sides, but also down to within a yard or two of the present torrent.

Thus valley after valley in the Alps seems to leave no escape from the following dilemma: Either a valley cut by a glacier does not differ in form from one made by running water, or one which has been excavated by the latter, if subsequently occupied, is but superficially modified by ice. This, as we can repeatedly see in the higher Alpine valleys, has not succeeded in obliterating the physical features due to the ordinary processes of erosion. Even where its effects are most striking, as in the Spitalamm below the Grimsel Hospice, it has not wholly effaced those features; and wherever a glacier in a recent retreat has exposed a rock surface, that demonstrates its inefficiency as a plough. The evidence of such cases has been pronounced inadmissible, on the ground that the glaciers of the Alps have now degenerated into senile impotence; but in valley beds over which they passed when in the full tide of their strength, the flanks show remnants of rocky ridges only partly smoothed away, and rough rock exists on the “lee-sides” of ice-worn mounds which no imaginary plucking can explain. The ice seems to have flowed over rather than to have plunged into the obstacles in its path, and even the huge steps of limestone exposed by the last retreat of the Unter Grindelwald Glacier have suffered little more than a rounding off of their angles, though that glacier must have passed over them when in fullest development, for it seems impossible to explain these by any process of sapping.

The comparatively level trough, which so often forms the uppermost part of one of the great passes across the watershed of the Alps, can hardly be explained without admitting that in each case the original watershed has been destroyed by the more rapid recession of the head of the southern valley, and this work bears every sign of having been accomplished in pre-Glacial times. Sapping and plucking must have operated on a gigantic scale to separate the Viso from the Cottian watershed, to isolate the huge pyramid of the Matterhorn, with its western spur, or to make, by the recession of the Val Macugnaga, that great gap between the Strahlhorn and Monte Rosa. Some sceptics even go so far as to doubt whether the dominant form of a non-glaciated region differ very materially from those of one which has been half buried in snowfields and glaciers. To my eyes, the general outlines of the mountains about the Lake of Gennesaret and the northern part of the Dead Sea recalled those around the Lake of Annecy and on the south-eastern shore of Leman. The sandstone crags, which rise here and there like ruined castles from the lower plateau of the Saxon Switzerland, resembled in outlines, though on a smaller scale, some of the Dolomites in the Southern Tyrol. The Lofoten Islands illustrate a half-drowned mountain range from which the glaciers have disappeared. Those were born among splintered peaks and ridges, which, though less lofty, rival in form the Aiguilles of Chamoni, and the valleys become more and more ice-worn as they descend, until the coast is fringed with skerries every one of which is a *roche moutonnée*. The *névé* in each of these valleys has been comparatively ineffective; the ice has gathered strength with the growth of the glacier. As can be seen from photographs, the scenery of the heart of the

¹ My own studies of mountain districts have led me to infer that on slopes of low grade the action of snow is preservative rather than destructive. That conclusion was confirmed by Prof. Garwood in a communication to the Royal Geographical Society on June 20 of the present year.

² The amount varies in different valleys; for instance, it was fully 2880 feet at Amsteg on the Reuss, just over 2000 feet at Brieg, in the Rhone Valley, about 1000 feet at Guttanen in the Aare Valley, about 1550 feet above Zermatt, and 1100 feet above Saas Grund.

Caucasus or of the Himalayas differs in scale rather than in kind from that of the Alps. Thus the amount of abrasion varies, other things being equal, with the latitude. The grinding away of ridges and spurs, the smoothing of the walls of troughs,¹ is greater in Norway than in the Alps; it is still greater in Greenland than in Norway, and it is greatest of all in the Antarctic, according to the reports of the expeditions led by Scott and Shackleton. But even in Polar regions, under the most favourable conditions, the dominant outlines of the mountains, as shown in the numerous photographs taken by both parties, and in Dr. Wilson's admirable drawings, differ in degree rather than in kind from those of mid-European ranges. It has been asserted that the parallel sides of the larger Alpine valleys—such as the Rhone above Martigny, the Lüttschne near Lauterbrunnen, and the Val Bedretto below Airolo—prove that they have been made by the ice-plough rather than by running water; but in the first I am unable to discern more than the normal effects of a rather rapid river which has followed a trough of comparatively soft rocks; in the second, only the cliffs marking the channel cut by a similar stream through massive limestones—cliffs like those which elsewhere rise up the mountain flanks far above the levels reached by glaciers; while in the third I have failed to discover, after repeated examination, anything abnormal.

Many lake basins have been ascribed to the erosive action of glaciers. Since the late Sir A. Ramsay advanced this hypothesis, numbers of lakes in various countries have been carefully investigated and the results published, the most recent of which is the splendid work on the Scottish lochs by Sir J. Murray and Mr. L. Pullar.² A contribution to science of the highest value, it has also a deeply pathetic interest, for it is a father's memorial to a much-loved son, F. P. Pullar, who, after taking a most active part in beginning the investigation, lost his life while saving others from drowning. As the time at my command is limited, and many are acquainted with the literature of the subject, I may be excused from saying more than that even these latest researches have not driven me from the position which I have maintained from the first, namely, that while many tarns in corries and lakelets in other favourable situations are probably due to excavation by ice, as in the mountainous districts of Britain, in Scandinavia, or in the higher parts of the Alps, the difficulty of invoking this agency increases with the size of the basin—as, for example, in the case of Loch Maree or the Lake of Annecy—until it becomes insuperable. Even if Glas Llyn and Llyn Llydaw were the work of a glacier, the rock basins of Gennesaret and the Dead Sea, still more those of the great lakes in North America and in Central Africa, must be assigned to other causes.

I pass on, therefore, to mention another difficulty in this hypothesis—that the Alpine valleys were greatly deepened during the Glacial Epoch—which has not yet, I think, received sufficient attention. From three to four hundred thousand years have elapsed, according to Penck and Brückner, since the first great advance of the Alpine ice. One of the latest estimates of the thickness of the several geological formations assigns 4000 feet³ to the Pleistocene and Recent, 13,000 to the Pliocene, and 14,000 to the Miocene. If we assume the times of deposit to be proportional to the thickness, and adopt the larger figure for the first-named period, the duration of the Pliocene would be 1,300,000 years, and of the Miocene 1,400,000 years. To estimate the total vertical thickness of rock which has been removed from the Alps by denudation is far from easy, but I think 14,000 feet would be a liberal allowance, of which about one-seventh is assigned to the Ice Age. But during that age, according to a curve given by Penck and Brückner, the temperature was below its present amount for rather less than half (0.47) the time. Hence it follows that, since the sculpture of the Alps must have begun at least as far back as the Miocene period, one-seventh of the work has been done by ice in not quite one-fifteenth of the time, or its action must be very potent. Such data as are at our command make it probable that

¹ If one may judge from photographs, the smoothing of the flanks of a valley is unusually conspicuous in Milton Sound, New Zealand.

² "Bathymetrical Survey of the Scottish Freshwater Lochs." Sir J. Murray and Mr. L. Pullar, 1910.

³ I have doubts whether this is not too great.

a Norway glacier at the present day lowers its basin by only about 80 millimetres in 1000 years; a Greenland glacier may remove some 421 millimetres in the same time, while the Vatnajökul in Iceland attains to 647 millimetres. If Alpine glaciers had been as effective as the last-named, they would not have removed, during their 188,000 years of occupation of the Alpine valleys, more than 121.6 metres, or just over 397 feet; and as this is not half the amount demanded by the more moderate advocates of erosion, we must either ascribe an abnormal activity to the vanished Alpine glaciers, or admit that water was much more effective as an excavator.

We must not forget that glaciers cannot have been important agents in the sculpture of the Alps during more than part of Pleistocene times. That sculpture probably began in the Oligocene period; for rather early in the next one the great masses of conglomerate, called *Nagelfluh*, show that powerful rivers had already carved for themselves valleys corresponding generally with, and nearly as deep as, those still in existence. Temperature during much of the Miocene period was not less than 12° F. above its present average. This would place the snow-line at about 12,000 feet.¹ In that case, if we assume the altitudes unchanged, not a snowfield would be left between the Simplon and the Maloja, the glaciers of the Pennines would shrivel into insignificance, Monte Rosa would exchange its drapery of ice for little more than a tippet of frozen snow. As the temperature fell, the white robes would steal down the mountain-sides, the glaciers grow, the torrents be swollen during all the warmer months, and the work of sculpture increase in activity. Yet with a temperature even 6° higher than it now is, as it might well be at the beginning of the Pliocene period, the snow-line would be at 10,000 feet; numbers of glaciers would have disappeared, and those around the Jungfrau and the Finster Aarhorn would be hardly more important than they now are in the Western Oberland.

But denudation would begin so soon as the ground rose above the sea. Water, which cannot run off the sand exposed by the retreating tide without carving a miniature system of valleys, would never leave the nascent range intact. The Miocene Alps, even before a patch of snow could remain through the summer months, would be carved into glens and valleys. Towards the end of that period the Alps were affected by a new set of movements, which produced their most marked effects in the northern zone from the Inn to the Durance. The Oberland rose to greater importance; Mont Blanc attained its primacy; the *massif* of Dauphiné was probably developed. That, and still more the falling temperature, would increase the snow-fields, glaciers, and torrents. The first would be, in the main, protective; the second, locally abrasive; the third, for the greater part of their course, erosive. No sooner had the drainage system been developed on both sides of the Alps than the valleys on the Italian side (unless we assume a very different distribution of rainfall) would work backwards more rapidly than those on the northern. Cases of trespass, such as that recorded by the long level trough on the north side of the Maloja Kulm and the precipitous descent on the southern, would become frequent. In the interglacial episodes—three in number, according to Penck and Brückner, and occupying rather more than half the epoch—the snow and ice would dwindle to something like its present amount, so that the water would resume its work. Thus I think it far more probable that the V-like portions of the Alpine valleys were in the main excavated during Pliocene ages, their upper and more open parts being largely the results of Miocene and yet earlier sculpture.

During the great advances of the ice, four in number, according to Penck and Brückner,² when the Rhone glacier covered the lowlands of Vaud and Geneva, welling on one occasion over the gaps in the Jura, and leaving its erratics in the neighbourhood of Lyons, it ought to have given

¹ I take the fall of temperature for a rise in altitude as 1° F. for 300 feet or, when the differences in the latter are large, 3° per 1000 feet. These estimates will, I think, be sufficiently accurate. The figures given by Hann (see for a discussion of the question, Report of Brit. Assoc., 1909, p. 93) work out to 1° F. for each 318 feet of ascent (up to about 10,000 feet).

² On the exact number I have not had the opportunity of forming an opinion.

signs of its erosive no less than of its transporting power. But what are the facts? In these lowlands we can see where the ice has passed over the Molasse (a Miocene sandstone); but here, instead of having crushed, torn, and uprooted the comparatively soft rock, it has produced hardly any effect. The huge glacier from the Linth Valley crept for not a few miles over a floor of stratified gravels, on which, some eight miles below Zurich, one of its moraines, formed during the last retreat, can be seen resting, without having produced more than a slight superficial disturbance. We are asked to credit glaciers with the erosion of deep valleys and the excavation of great lakes, and yet, wherever we pass from hypotheses to facts, we find them to have been singularly inefficient workmen!

I have dwelt at considerable, some may think undue, length on the Alps, because we are sure that this region from before the close of the Miocene period has been above sea-level. It accordingly demonstrates what effects ice can produce when working on land.

In America also, to which I must now make only a passing reference, great ice-sheets formerly existed: one occupying the district west of the Rocky Mountains, another spreading from that on the north-west of Hudson's Bay, and a third from the Laurentian hill-country. These two became confluent, and their united ice-flow covered the region of the Great Lakes, halting near the eastern coast a little south of New York, but in Ohio, Indiana, and Illinois occasionally leaving moraines only a little north of the 39th parallel of latitude.¹ Of these relics my first-hand knowledge is very small, but the admirably illustrated reports and other writings of American geologists² indicate that, if we make due allowance for the differences in environment, the tills and associated deposits on their continent are similar in character to those of the Alps.³

In our own country and in corresponding parts of Northern Europe we must take into account the possible cooperation of the sea. In these, however, geologists agree that, for at least a portion of the Ice Age, glaciers occupied the mountain districts. Here ice-worn rocks, moraines and perched blocks, tarns in corries, and perhaps lakelets in valleys, demonstrate the former presence of a mantle of snow and ice. Glaciers radiated outwards from more than one focus in Ireland, Scotland, the English Lake District, and Wales, and trespassed, at the time of their greatest development, upon the adjacent lowlands. They are generally believed to have advanced and retreated more than once, and their movements have been correlated by Prof. J. Geikie with those already mentioned in the Alps. Into that very difficult question I must not enter; for my present purpose it is enough to say that in early Pleistocene times glaciers undoubtedly existed in the mountain districts of Britain, and even formed piedmont ice-sheets on the lowlands. On the west side of England, smoothed and striated rocks have been observed near Liverpool, which can hardly be due to the movements of shore-ice, and at Little Crosby a considerable surface has been cleared from the overlying Boulder Clay by the exertions of the late Mr. T. M. Reade and his son, Mr. A. Lyell Reade. But, so far as I am aware, rocks thus affected have not yet been discovered in the Wirral peninsula. On the eastern side of England similar markings have been found down to the coast of Durham, but a more southern extension of land ice cannot be taken for granted. In this direction, however, so far as the tidal valley of the Thames, and in corresponding parts of the central and western lowlands, certain deposits occur which, though to a great extent of glacial origin, are in many respects different from those left by land ice in the Alpine regions and in Northern America.

They present us with problems the nature of which may be inferred from a brief statement of the facts. On the Norfolk coast we find the glacial drifts resting, sometimes

¹ Some of the glacial drifts on the eastern side of the continent, as we shall find, may have been deposited in the sea.

² See the "Reports of the United States Geological Survey" (from vol. iii. onwards), *Journal of Geology*, *American Journal of Science*, and local publications too numerous to mention. Among these the studies in Greenland by Prof. Chamberlin are especially valuable for the light they throw on the movement of large glaciers and the transport of debris in the lower part of the ice.

³ Here, however, we cannot always be so sure of the absence of the sea.

on the chalk, sometimes on strata of very late Pliocene or early Pleistocene age. The latter show that in their time the strand-line must have oscillated slightly on either side of its present level. The earliest of the glacial deposits, called the Cromer Till and Contorted Drift, presents its most remarkable development in the cliffs on either side of that town. Here it consists of Boulder Clays and alternating beds of sand and clay; the first-named, two or three in number, somewhat limited in extent, and rather lenticular in form, are slightly sandy clays, full of pieces of chalk, flint, and other kinds of rock, some of the last having travelled from long distances. Yet more remarkable are the huge erratics of chalk, in the neighbourhood of which the sands and clays exhibit extraordinary contortions. Like the beds of till, they have not been found very far inland, for there the group appears as a whole to be represented by a stony loam, resembling a mixture of the sandy and clayey material, and this is restricted to a zone some twenty miles wide, bordering the coast of Norfolk and Suffolk, not extending south of the latter county, but being probably represented to the north of the Humber. Above these is a group of false-bedded sands and gravels, variable in thickness and character—the Mid-glacial Sands of Searles V. Wood and F. W. Harmer. They extend over a wider area, and may be traced, according to some geologists, nearly to the western side of England, rising in that direction to a greater height above sea-level. But as it is impossible to prove that all isolated patches of these materials are identical in age, we can only be certain that some of them are older than the next deposit, a Boulder Clay, which extends over a large part of the lowlands in the Eastern Counties. This has a general resemblance to the Cromer Till, but its matrix is rather more clayey and is variable in colour. In and north of Yorkshire, as well as on the seaward side of the Lincolnshire wolds, it is generally brownish or purplish, but on their western side, and as far as the clay goes to the south, it is some shade of grey. Near to these wolds, in mid-Norfolk, and on the northern margin of Suffolk, it has a whitish tint, owing to the abundance of comminuted chalk. To the south and west of this area it is dark, from the similar presence of Kimeridge Clay. Yet further west it assumes an intermediate colour by having drawn upon the Oxford Clay. This Boulder Clay, whether the chalky or the purple, in which partings of sand sometimes occur, must once have covered, according to Mr. F. W. Harmer, an area about ten thousand square miles in extent. It spreads like a coverlet over the pre-glacial irregularities of the surface. It caps the hills, attaining sometimes an elevation of fully 500 feet above sea-level;¹ it fills up valleys,² sometimes partly, sometimes wholly, the original floors of which occasionally lie more than 100 feet below the same level. This Boulder Clay, often with an underlying sand or gravel, extends to the south as far as the neighbourhood of Muswell Hill and Finchley; hence its margin runs westward through Buckinghamshire, and then, bending northwards, passes to the west of Coventry. On this side of the Pennine Chain the matrix of the Boulder Clay is again reddish, being mainly derived from the sands and marls of the Trias; pieces of chalk and flint are rare (no doubt coming from Antrim), though other rocks are often plentiful enough. Some authorities are of opinion that the drift in most parts of Lancashire and Cheshire is separable, as on the eastern coasts, into a lower and an upper Boulder Clay, with intervening gravelly sands, but others think that the association of the first and third is lenticular rather than successive. Here also the lower clay cannot be traced very far inland, eastward or southward; the others have a wider extension, but they reach a greater elevation above sea-level than on the eastern side

¹ Not far from Royston it is found at a height of 525 feet above O.D. See F. W. Harmer, "Pleistocene Period in the Eastern Counties," p. 115.

² At Old North Road Station, on a tributary of the Cam, the Boulder Clay was pierced to a depth of 180 feet, and at Impington it goes to 60 feet below sea level. Near Hitchin, a hidden valley, traced for seven or eight miles, was proved to a depth of 68 feet below O.D., and one near Newport in Essex, to 140 feet. Depths were also found of 120 feet at West Horseheath in Suffolk, of 120 feet on low ground two miles S.W. of Sandy in Bedfordshire, of from 100 to 160 feet below the sea at Fossdyke, Long Sutton, and Boston, and at Glemsford in the valley of the Stou. A 77 feet of drift was passed through before reaching the chalk. See F. W. Harmer, *Quart. Journ. Geol. Soc.*, lxxiii. (1907), p. 494.

of England. The sand is inconstant in thickness, being sometimes hardly represented, sometimes as much as 200 feet. The upper clay runs on its more eastern side up to the chalky Boulder Clay, and extends on the south at least into Worcestershire. On the western side it merges with the upper member of the drifts radiating from the mountains of North Wales, which often exhibit a similar tripartite division, while (as we learn from the officers of the Geological Survey) Boulder Clays and gravelly sands, which it must suffice to mention, extend from the highlands of South Wales for a considerable distance to the south-east and south. Boulder Clay has not been recognised in Devon or Cornwall, though occasional erratics are found which seem to demand some form of ice-transport. A limited deposit, however, of that clay, containing boulders now and then more than a yard in diameter, occurs near Selsey Bill on the Sussex coast, which most geologists consider to have been formed by floating rather than by land ice.

Marine shells are not very infrequent in the lower clays of East Anglia and Yorkshire, but are commonly broken. The well-known Bridlington Crag is the most conspicuous instance, but this is explained by many geologists as an erratic—a piece of an ancient North Sea bed caught up and transported, like the other molluscs, by an advancing ice-sheet. They also claim a derivative origin for the organic contents of the overlying sands and gravels, but some authorities consider the majority to be contemporaneous. Near the western coast of England, shells in much the same state of preservation as those on the present shore are far from rare in the lower clay, where they are associated with numerous striated stones, often closely resembling those which have travelled beneath a glacier, both from the Lake District and the less distant Trias. Shells are also found in the overlying sands up the valleys of the Dee and Severn, at occasional localities, even as far inland as Bridgnorth, the heights of the deposits varying from about 120 feet to more than 500 feet above the sea-level. If we also take account of the upper Boulder Clay, where it can be distinguished, the list of marine molluscs, ostracods, and foraminifers from these western drifts is a rather long one.¹

Marine shells, however, on the western side of England are not restricted to the lowlands. Three instances, all occurring more than 1000 feet above sea-level, claim more than a passing mention. At Macclesfield, almost thirty miles in a straight line from the head of the estuary of the Mersey, Boulder Clays associated with stratified gravels and sands have been described by several observers.² The clay stops at about 1000 feet, but the sands and gravels go on to nearly 1300 feet, while isolated erratics are found up to about 100 feet higher. Sea shells, some of which are in good condition, have been obtained at various elevations, the highest being about 1200 feet above sea-level. About forty-eight species of molluscs have been recognised, and the fauna, with a few exceptions, more Arctic in character and now found at a greater depth, is one which at the present day lives in a temperate climate at a depth of a few fathoms.

The shell-bearing gravels at Gloppe, near Oswestry, which are about thirty miles from the head of the Dee estuary, were carefully described in 1892 by Mr. A. C. Nicholson. He has enumerated fully sixty species, of which, however, many are rare. As his collection³ shows, the bivalves are generally broken, but a fair number of the univalves are tolerably perfect. The deposit itself consists of alternating seams of sand and gravel, the one generally about an inch in thickness, the other varying from a few inches to a foot. The difference in the amount of rounding shown by the stones is a noteworthy feature. They are not seldom striated; some have come from Scotland, others from the Lake District, but the majority from Wales, the last being the more angular. Here and there a block, sometimes exceeding a foot in diameter, and usually from the last-named country, has

¹ W. Shone, *Quart. Journ. Geol. Soc.*, xxxiv. (1878), p. 383.

² "Memoirs of the Geological Survey: Country around Macclesfield." T. I. Pocock (1906), p. 85. For some notes on Moel Tryfaen and the altitudes of other localities at which marine organisms have been found, see J. Gwyn Jeffreys, *Quart. Journ. Geol. Soc.*, xxxvi. (1880), p. 351. For the occurrence of such remains in the Vale of Clwyd see a paper by T. McK. Hughes in *Proc. Chester Soc. Nat. Hist.*, 1884.

³ Now deposited in the Oswestry Museum.

been dropped among the smaller material, most of which ranges in diameter from half an inch to an inch and a half. The beds in one or two places show contortions; but, as a rule, though slightly wavy and with a gentle dip rather to the west of south, they are uniformly deposited. In this respect, and in the unequal wearing of the materials, the Gloppe deposit differs from most gravels that I have seen. Its situation also is peculiar. It is on the flattened top of a rocky spur from higher hills, which falls rather steeply to the Shropshire lowland on the eastern side, and on the more western is defined by a small valley, which enlarges gradually as it descends towards the Severn. If the country were gradually depressed for nearly 1200 feet, this upland would become, first a promontory, then an island, and finally a shoal.

The third instance, on Moel Tryfaen in Carnarvonshire, was carefully investigated and described by a Committee of this Association⁴ about ten years ago. The shells occur in an irregularly stratified sand and gravel, resting on slate, and overlain by a Boulder Clay, no great distance from, and a few dozen feet below, the rocky summit of the hill, being about 1300 feet above the level of the sea and at least five miles from its margin. About fifty-five species of molluscs and twenty-three of foraminifers have been identified. According to the late Dr. J. Gwyn Jeffreys,² the majority of the molluscs are littoral in habit, the rest such as live in from ten to twenty fathoms of water. Most of the erratics have been derived from the Welsh mountains, but some rocks from Anglesey have also been obtained, and a few pebbles of Lake District and Scotch rocks. If the sea were about 1300 feet above its present level, Moel Tryfaen would become a small rocky island, open to the storms from the west and north, and nearly a mile and a half away from the nearest land.

I must pass more rapidly over Ireland. The signs of vanished glaciers—ice-worn rocks and characteristic Boulder Clays—are numerous, and may be traced in places down to the sea-level, but the principal outflow of the ice, according to some competent observers, was from a comparatively low district, extending diagonally across the island from the south of Lough Neagh to north of Galway Bay. Glaciers, however, must have first begun to form in the mountains on the northern and southern side of this zone, and we should have expected that, whatever might happen on the lowlands, they would continue to assert themselves. In no other part of the British Islands are eskers, which some geologists think were formed when a glacier reached the sea, so strikingly developed. Here also an upper and a lower Boulder Clay, the former being the more sparsely distributed, are often divided by a widespread group of sands and gravels, which locally, as in Great Britain, contains, sometimes abundantly, shells and other marine organisms; more than twenty species of molluscs, with foraminifers, a barnacle, and perforations of annelids, having been described. These are found in Counties Dublin and Wicklow, at various altitudes,³ from a little above sea-level to a height of 1300 feet.

Not the least perplexing of the glacial phenomena in the British Isles is the distribution of erratics, which has been already mentioned in passing. On the Norfolk coast, masses of chalk, often thousands of cubic feet in volume, occur in the lowest member of the glacial series, with occasional great blocks of sand and gravel, which must have once been frozen. But these, or at any rate the larger of them, have no doubt been derived from the immediate neighbourhood. Huge erratics also occasionally occur in the upper Boulder Clay—sometimes of chalk, as at Roslyn Hill near Ely, and at Ridlington in Rutland, of Jurassic limestone, near Great Ponton, to the south of Grantham, and of Lower Kimeridge Clay near Biggleswade.⁴ These also probably have not travelled more than a few miles. But others of smaller size have often made much longer journeys. The Boulder Clays of eastern England are full of pieces of rock, commonly ranging from about half an inch to a foot in diameter. Among these are samples of the Carboniferous, Jurassic, and Cretaceous rocks of Yorkshire and the adjacent counties,

¹ *Brit. Assoc. Report*, 1899 (1900), pp. 414-423.

² *Quart. Journ. Geol. Soc.*, xxxvi. (1880), p. 355.

³ See T. M. Reade, *Proc. Liverpool Geol. Soc.* 1893-4, p. 183, for some weighty arguments in favour of a marine origin for these deposits.

⁴ H. Home, *Quart. Journ. Geol. Soc.*, lix. (1903), p. 375.

the Red Chalk from either Hunstanton, Speeton, or some part of the Lincolnshire wolds being found as far south as the northern heights of London. Even the chalk and flint, the former of which, especially in the Upper Boulder Clay, commonly occurs in well-worn pebbles, are frequently not the local, but the northern varieties. And with these are mingled specimens from yet more distant sources—Cheviot porphyrites, South Scotch basalts, even some of the crystalline rocks of the Highlands. Whatever was the transporting agent, its general direction was southerly, with a slight deflection towards the east in the last-named cases.

But the path of these erratics has been crossed by two streams, one coming from the west, the other from the east. On the western side of the Pennine watershed the Shap granite rises at Wasdale Crag to a height of about 1600 feet above sea-level. Boulders from it have descended the Eden valley to beyond Penrith; they have travelled in the opposite direction almost to Lancaster,¹ and a large number of them have actually made their way near the line of the Lake District watershed, across the upper valley of the Eden, and over the high pass of Stainmoor Forest,² whence they descended into Upper Teesdale. Subsequently the stream seems to have bifurcated, one part passing straight out to the present sea-bed, by way of the lower course of the Tees, to be afterwards driven back on to the Yorkshire coast. The other part crossed the low watershed between the Tees and the Ouse, descended the Vale of York, and spread widely over the plain.³ Shap boulders by some means penetrated into the valleys tributary to the Ouse on its west bank, and they have been observed as far to the south-east as Royston, near Barnsley. It is noteworthy that Lake District rocks have been occasionally recorded from Airedale and even the neighbourhood of Colne, though the granite from Shap has not been found there. The other stream started from Scandinavia. Erratics, some of which must have come from the north-western side of the Christiania Fjord, occur on or near the coast from Essex to Yorkshire, and occasionally even as far north as Aberdeen, while they have been traced from the East Anglian coast to near Ware, Hitchin, and Bedford.⁴ It may be important to notice that these Scandinavian erratics are often water-worn, like those dispersed over Denmark and parts of northern Germany.

On the western side of England the course of erratics is not less remarkable. Boulders from south-western Scotland, especially from the Kirkcudbright district, both water-worn and angular, are scattered over the lowlands as far south as Wolverhampton, Bridgnorth, and Church Stretton. They may be traced along the border of North Wales, occurring, as has been said, though generally small, up to about 1300 feet on Moel Tryfaen, 1100 feet at Gloppe, and more than that height on the hills east of Macclesfield. Boulders from the Lake District are scattered over much the same area and attain the same elevation, but extend, as might be expected, rather farther to the east in Lancashire. They also have been found on the eastern side of the Pennine watershed, perhaps the most remarkable instances being in the dales of the Derbyshire Derwent and on the adjacent hills as much as 1400 feet above the sea-level.⁵ A third remarkable stream of erratics from the neighbourhood of the Arenig mountains extends from near the estuary of the Dee right across the paths of the two streams from the north, its eastern border passing near Rugeley, Birmingham, and Bromsgrove. They also range high, occurring almost 900 feet above sea-level on Romsley Hill, north of the Clents, and being common at Gloppe. Boulders also from the basalt mass of Rowley Regis have travelled in some cases between four and five miles, and in directions ranging from rather west of south to north-east; and, though the mass hardly rises above the 700-foot contour line, one lies with an Arenig boulder on Romsley Hill. From Charnwood

¹ A pebble of it is said to have been identified at Moel Tryfaen.

² The lowest part of the gap is about 1400 feet. A little to the south is another gap about 200 feet lower, but none of the boulders seem to have taken that route.

³ A boulder was even found above Grosmont in the Eske valley, 345 feet above sea-level.

⁴ R. H. Rastall and J. Romanes, *Quart. Journ. Geol. Soc.*, lxx. (1909), p. 246.

⁵ Communication from Dr. H. Arnold-Bemrose.

Forest, the crags of which range up to about 850 feet above sea-level, boulders have started which have been traced over an area to the south and west to a distance of more than twenty miles.

Such, then, are the facts which call for an interpretation. More than one has been proposed; but it will be well, before discussing them, to arrive at some idea of the climate of these islands during the colder part of the Glacial Epoch. Unless that were associated with very great changes in the distribution of sea and land in northern and north-western Europe, we may assume that neither the relative position of the isotherms nor the distribution of precipitation would be very materially altered. A general fall of temperature in the northern hemisphere might so weaken the warmer ocean current from the south-west that our coasts might be approached by a cold one from the opposite direction.¹ But though these changes might diminish the difference between the temperatures of London and Leipzig, they would not make the former colder than the latter. At the present day the snow-line in the Alps on either side of the Upper Rhone Valley is not far from 8000 feet above sea-level, and this corresponds with a temperature of about 30°. Glaciers, however, are not generally formed until about 1000 feet higher, where the temperature is approximately 27°. Penck and Brückner place this line during the coldest part of the Ice Age at about 4000 feet.² In that case the temperature of the Swiss lowland would be some 15° lower than now, or near the freezing point.³ If this fall were general, it would bring back the small glaciers on the Gran Sasso d'Italia and Monte Rotondo in Corsica; perhaps also among the higher parts of the Vosges and Schwarzwald.⁴ In our own country it would give a temperature of about 35° at Carnarvon and 23° on the top of Snowdon, of 32° at Fort William and 17.5° on the top of Ben Nevis. If, in addition to this, the land were 600 feet higher than now (as it probably was, at any rate in the beginning of the Glacial Epoch), there would be a further drop of 2°, so that glaciers would form in the corries of Snowdon, and the region round Ben Nevis might resemble the Oetzthal Alps at the present day. This change of itself would be insufficient, and any larger drop in the ocean-level would have to be continental in its effects, since we cannot assume a local upheaval of much more than the above amount without seriously interfering with the river system of North Central Europe. But these changes, especially the former, might indirectly diminish the abnormal warmth of winter on our north-western coasts.⁵ It is difficult to estimate the effect of this. If it did no more than place Carnarvon on the isotherm of Berlin (now lower by 2°), that would hardly bring a glacier from the Snowdonian region down to the sea. At the present time London is about 18° warmer than a place in the same latitude near the Labrador coast or the mouth of the Amur River, but the removal of that difference would involve greater changes in the distribution of sea and land than seems possible at an epoch, comparatively speaking, so recent. I am doubtful whether we can attribute to changed currents a reduction in British temperatures of so much as 11°; but, if we did, this would amount to 28° from all causes, and give a temperature of 20° to 22° at sea-level in England during the coldest part of the Glacial Epoch.⁶ That is now

¹ Facts relating to this subject will be found in "Climate and Time," by J. Croll, ch. ii. and iii. (1875). Of course the air currents would also be affected, and perhaps diminish precipitation as the latitude increased.

² *Loc. cit.*, p. 586, *et seq.* They say the snow-line, which would mean that the temperature was only 12° lower than now; but as possibly this line might then more nearly correspond with that of glacier formation, I will provisionally accept the higher figures, especially since Corsica, the Apennines, and some other localities in Europe, seem to require a reduction of rather more than 12°.

³ It would be 32.5° at Zurich, 31.6° at Bern, 34.2° at Geneva, about 39° on the plain of Piedmont, and 35° at Lyons.

⁴ See for particulars the author's "Ice Work" (International Scientific Series), p. 237.

⁵ For much valuable information on these questions see a paper on Climate of the Pleistocene Epoch (F. W. Harmer, *Quart. Journ. Geol. Soc.*, lvii. 1901, p. 405).

⁶ The present temperature in Ireland over the zone (from S. of Belfast to N. of Galway Bay) which is supposed to have formed the divide of the central snowfield may be given as from 49° to 50°, nearly the same as at the sea-level in Carnarvonshire. Thus, though the district is less mountainous than Wales, it would not need a greater reduction, for the snowfall would probably be rather larger. But this reduction could hardly be less than 20°, for the glaciers would have to form nearly at the present sea-level.

found, roughly speaking, in Spitsbergen, which, since its mountains rise to much the same height, should give us a general idea of the condition of Britain in the olden time.

What would then be the state of Scandinavia? Its present temperature ranges on the west coast from about 45° in the south to 35° in the north.¹ But this region must now be very much, possibly 1800 feet, lower than it was in pre-Glacial, perhaps also in part of Glacial, times.² If we added 5° for this to the original 15° , and allowed so much as 18° for the diversion of the warm current, the temperature of Scandinavia would range from 7° to -3° , approximately that of Greenland northwards from Upernivik. But since the difference at the present day between Cape Farewell and Christiania (the one in an abnormally cold region, the other in one correspondingly warm) is only 7° , that allowance seems much too large, while without it Scandinavia would correspond in temperature with some part of that country from south of Upernivik to north of Frederikshaab.³ But if Christiania were not colder than Jakobshavn is now, or Britain than Spitsbergen, we are precluded from comparisons with the coasts of Baffin Bay or Victoria Land.

Thus the ice-sheet from Scandinavia would probably be much greater than those generated in Britain. It would, however, find an obstacle to progress westwards which cannot be ignored. If the bed of the North Sea became dry land, owing to a general rise of 600 feet, that would still be separated from Norway by a deep channel extending from the Christiania Fjord round the coast northward. Even then this would be everywhere more than another 600 feet deep, and almost as wide as the Strait of Dover.⁴ The ice must cross this and afterwards be forced for more than 300 miles up a slope, which, though gentle, would be in vertical height at least 600 feet. The task, if accomplished by thrust from behind, would be a heavy one, and, so far as I know, without a parallel at the present day; if the viscosity of the ice enabled it to flow, as has lately been urged,⁵ we must be cautious in appealing to the great Antarctic barrier, because we now learn that more than half of it is only consolidated snow.⁶ Moreover, if the ice floated across that channel, the thickness of the boulder-bearing layers would be diminished by melting (as in Ross's Barrier), and the more viscous the material the greater the tendency for these to be left behind by the overflow of the cleaner upper layers. If, however, the whole region became dry land, the Scandinavian glaciers would descend into a broad valley, considerably more than 1200 feet deep, which would afford them an easy path to the Arctic Ocean, so that only a lateral overflow, inconsiderable in volume, could spread itself over the western plateau.⁷ An attempt to escape this difficulty has been made by assuming the existence of an independent centre of distribution for ice and boulders near the middle of the North Sea bed⁸ (which would demand rather exceptional conditions of temperature and precipitation); but in such case either the Scandinavian ice would be fended off from England, or the boulders, prior to its advance, must have been dropped by floating ice on the neighbouring sea-floor.

If, then, our own country were but little better than Spitsbergen as a producer of ice, and Scandinavia only surpassed southern Greenland in having a rather heavier snowfall, what interpretation may we give to the glacial phenomena of Britain? Three have been proposed. One asserts that throughout the Glacial Epoch the British Isles generally stood at a higher level, so that the ice which almost buried them flowed out on to the beds of the

North and Irish Seas. The Boulder Clays represent its moraines. The stratified sands and gravels were deposited in lakes formed by the rivers, which were dammed up by ice-sheets.¹ A second interpretation recognises the presence of glaciers in the mountain regions, but maintains that the land, at the outset rather above its present level, gradually sank beneath the sea, until the depth of water over the eastern coast of England was fully 500 feet, and over the western nearly 1400 feet, from which depression it slowly recovered. By any such submergence Great Britain and Ireland would be broken up into a cluster of hilly islands, between which the tide from an extended Atlantic would sweep eastwards twice a day, its currents running strong through the narrower sounds, while movements in the reverse direction at the ebb would be much less vigorous. The third interpretation, in some respects intermediate, was first advanced by the late Prof. Carvill Lewis, who held that the peculiar Boulder Clays and associated sands (such as those of East Anglia), which, as was then thought, were not found more than about 450 feet above the present sea-level, had been deposited in a great fresh-water lake, held up by the ice-sheets already mentioned and by an isthmus, which at that time occupied the place of the Strait of Dover. Thus these deposits, though indirectly due to land-ice, were actually fluvial or lacustrine. But this interpretation need not detain us, though the former existence of such lakes is still maintained, on a small scale in Britain, on a much larger one in North America, because, as was pointed out when it was first advanced, it fails to explain the numerous erratic blocks and shell-bearing sands which occur far above the margin of the hypothetical lake.

Each of the other two hypotheses involves grave difficulties. That of great confluent ice-sheets creeping over the British lowlands demands, as has been intimated, climatal conditions which are scarcely possible, and makes it hard to explain the sands and gravels, sometimes with regular alternate bedding, but more generally indicative of strong current action, which occur at various elevations to more than 1300 feet above sea-level, and seem too widespread to have been formed either beneath an ice-sheet or in lakes held up by one, for the latter, if of any size, would speedily check the velocity of influent streams. Also the mixture and crossing of boulders, which we have described, are inexplicable without the most extraordinary oscillations in the size of the contributing glaciers. To suppose that the Scandinavian ice reached to Bedfordshire and Herts, and then retired in favour of north British glaciers, or *vice versa*, assumes an amount of variation which, so far as I am aware, is without a parallel elsewhere. So also the mixture of boulders from South Scotland, the Lake District, and North Wales, which lie, especially in parts of Staffordshire and Shropshire, as if dropped upon the surface, far exceeds what may reasonably be attributed to variations amplified by lateral spreading of mountain glaciers on reaching a lowland, while the frequent presence of shells in the drifts, dozens of miles away from the present coast, implies a rather improbable scooping up of the sea-bed without much injury to such fragile objects. The ice also must have been curiously inconstant in its operations. It is supposed in one place to have glided gently over its bed, in another to have gripped and torn out huge masses of rock.² Both actions may be possible in a mountain region, but it is very difficult to understand how they could occur in a lowland or plain. Besides this, we can only account for some singular aberrations of boulders, such as Shap granite well above Grosfont in Eskdale, or the Scandinavian rhomb-porphyr in Lockwood,³ near Huddersfield, by assuming a flexibility in the lobes of an ice-sheet which it is hard to match at

¹ It is $44^{\circ}42'$ at Bergen, $38^{\circ}48'$ at Bodo, $35^{\circ}42'$ at Hammerfest, $41^{\circ}36'$ at Christiania and Stockholm.

² For particulars see *Geol. Mag.*, 1899, p. 97 (W. H. Hudleston) and p. 282 (I. G. Bonney).

³ Christiania and Cape Farewell (Greenland) are nearly on the same latitude.

⁴ For details see *Geol. Mag.*, 1899, pp. 97 and 282.

⁵ H. M. Deeley, *Geol. Mag.*, 1909, p. 230.

⁶ E. Shackleton, "The Heart of the Antarctic," ii., p. 277.

⁷ It has indeed been affirmed (Brøgger, *Om de sen-glaciale og post-glaciale nivaforandringer i Kristianiafeltet*, p. 682) that at the time of the great ice-sheet of Europe the sea-bottom must have been uplifted at least 8500 feet higher than at present. This may be a ready explanation of the occurrence of certain dead shells in deep water, but, unless extremely local, it would revolutionise the drainage system of Central Europe.

⁸ *Geol. Mag.*, 1901, pp. 142, 187, 284, 332.

¹ See Warren Upham, Monogr. U.S. Geol. Survey, xxv. (1896). This explanation commends itself to the majority of British geologists as an explanation of the not-parallel roads of Glenroy, but it is premature to speak of it as "conclusively shown" (Quart. Journ. Geol. Soc., lviii. (1902) p. 473) until a fundamental difficulty which it presents has been discussed and removed.

² That this has occurred at Cromer is a very dubious hypothesis (see *Geol. Mag.*, 1905, pp. 397, 524). The curious relations of the drift and chalk in the islands of Møen and Rügen are sometimes supposed to prove the same action. Knowing both well, I have no hesitation in saying that the chalk there is, as a rule, as much *in situ* as it is in the Isle of Wight.

³ About half-way across England and 870 feet above sea-level. P. F. Kendall, Quart. Journ. Geol. Soc., lviii. (1902), p. 498.

the present time. Again, the Boulder Clay of the eastern counties is crowded, as we have described, with pebbles of chalk, which generally are not of local origin, but have come from north of the Wash. Whether from the bed of a river or from a sea-beach, they are certainly water-worn. But if pre-Glacial, the supply would be quickly exhausted, so that they would usually be confined to the lower part of the clay. As it is, though perhaps they run larger here, they abound throughout. The so-called moraines near York (supposed to have been left by a glacier retreating up that vale), those in the neighbourhood of Flamborough Head and of Sheringham (regarded as relics of the North Sea ice-sheet), do not, in my opinion, show any important difference in outline from ordinary hills of sands and gravels, and their materials are wholly unlike those of any indubitable moraines that I have either seen or studied in photographs. It may be said that the British glaciers passed over very different rocks from the Alpine; but the Swiss molasse ought to have supplied abundant sand, and the older interglacial gravels quantities of pebbles; yet the differences between the morainic materials on the flank of the Jura or near the town of Geneva and those close to the foot of the Alps are varietal rather than specific.

Some authorities, however, attribute such magnitude to the ice-sheets radiating from Scandinavia that they depict them, at the time of maximum extension, as not only traversing the North Sea bed and trespassing upon the coast of England, but also radiating southward to overwhelm Denmark and Holland, to invade northern Germany and Poland, to obliterate Hanover, Berlin, and Warsaw, and to stop but little short of Dresden and Cracow, while burying Russia on the east to within no great distance of the Volga and on the south to the neighbourhood of Kiev. Their presence, however, so far as I can ascertain, is inferred from evidence¹ very similar to that which we have discussed in the British lowlands. That Scandinavia was at one time almost wholly buried beneath snow and ice is indubitable; it is equally so that at the outset the land stood above its present level, and that during the later stages of the Glacial Epoch parts, at any rate of southern Norway, had sunk down to a maximum depth of 800 feet. In Germany, however, erratics are scattered over its plain and stranded on the slopes of the Harz and Riesengebirge up to about 1400 feet above sea-level. The glacial drifts of the lowlands sometimes contain dislodged masses of neighbouring rocks like those at Cromer, and we read of other indications of ice action. I must, however, observe that since the glacial deposits of Möen, Warnemünde, and Rügen often present not only close resemblances to those of our eastern counties, but also very similar difficulties, it is not permissible to quote the one in support of the other, seeing that the origin of each is equally dubious. Given a sufficient "head" of ice in northern regions, it might be possible to transfer the remains of organisms from the bed of the Irish Sea to Moel Tryfaen, Macclesfield, and Gloppe; but at the last-named, if not at the others, we must assume the existence of steadily alternating currents in the lakes in order to explain the corresponding bedding of the deposit. This, however, is not the only difficulty. The "Irish Sea glacier" is supposed to have been composed of streams from Ireland, south-west Scotland, and the Lake District, of which the second furnished the dominant contingent, the first-named not producing any direct effect on the western coast of Great Britain, and the third being made to feel its inferiority and "shouldered in upon the mainland." But even if this ever happened, ought not the Welsh ice to have joined issue with the invaders a good many miles to the north of its own coast?² Welsh boulders, at any rate, are

¹ A valuable summary of it is given in "The Great Ice Age," J. Geikie, ch. xxix., xxx. (1894).

² From Moel Tryfaen to the nearest point of Scotland is well over a hundred miles, and it is a few less than this distance from Gloppe to the Lake District. In order to allow the Irish Sea ice-sheet to reach the top of Moel Tryfaen the glacier productive power of Snowdonia has been minimised (Wright, "Man and the Glacial Epoch," pp. 171, 172). But the difference between that and the Arenig region is not great enough to make the one incompetent to protect its own borderland while the other could send an ice-sheet which could almost cover the Clent Hills and reach the neighbourhood of Birmingham. Anglesey also if we suppose a slight elevation and a temperature of 29° at the sea-level would become a centre of ice-distribution and an advance guard to North Wales.

common near the summit of Moel Tryfaen, and I have no hesitation in saying that the pebbles of riebeckite-rock, far from rare in its drifts, come from Mynydd Mawr, hardly half a league to the E.S.E., and not from Ailsa Craig.¹

As such frequent appeal is made to the superior volume of the ice-sheet which poured from the northern hills over the bed of the Irish Sea, I will compare in more detail the ice-producing capacities of the several districts. The present temperature of West-Central Scotland may be taken as 47°, its surface as averaging about 2500 feet, rising occasionally to nearly 4000 feet above sea-level. In the western part of the Southern Uplands the temperature is a degree higher, and the average for altitude at most not above 1500 feet. In the Lake District and the Northern Pennines the temperature is increased by another degree, and the heights are, for the one 1800 feet with a maximum of 3162 feet, for the other 1200 feet and 2892 feet. In North Wales the temperature is 50°, the average height perhaps 2000 feet, and the culminating point 3571 feet. For the purpose of comparing the ice-producing powers of these districts, we may bring them to one temperature by adding 300 feet to the height for each degree below that of the Welsh region. This would raise the average elevation of Central and Southern Scotland to 3400 feet and 2100 feet respectively, for the Lake District and Northern Pennines to 2100 feet and 1500 feet. We may picture to ourselves what this would mean, if the snow-line were at the sea-level in North Wales, by imagining 8000 feet added to its height and comparing it with the Alps. North Wales would then resemble a part of that chain which had an average height of about 10,000 feet above sea-level, and culminated in a peak of 11,571 feet; the Lake District would hardly differ from it; the Northern Pennines would be like a range of about 9000 feet, its highest peak being 11,192 feet. Southern Scotland would be much the same in average height as the first and second, and would rise, though rarely, to above 11,000 feet; the average in Central Scotland would be about 11,400 feet, and the maximum about 13,000 feet. Thus North Wales, the Lake District, and the Southern Uplands would differ little in ice-productive power, while Central Scotland would distinctly exceed them, but not more than the group around the Finsteraarhorn does that giving birth to the Rhone glacier. In one respect, however, all these districts would differ from the Alps—that, at 8000 feet, the surface, instead of being furrowed with valleys, small and great, would be a gently shelving plateau, which would favour the formation of piedmont glaciers. Still, unless we assume the present distribution of rainfall to be completely altered (for which I do not know any reason), the relative magnitudes of the ice coming from these centres (whether separate glaciers or confluent sheets) could differ but little. Scotch ice would not appreciably "shoulder inland" that from the Lake District, nor would the Welsh ice be imprisoned within its own valleys.

During the last few years, however, the lake-hypothesis of Carvill Lewis has been revived under a rather different form by some English advocates of land-ice. For instance, the former presence of ice-dammed lakes is supposed to be indicated in the upper parts of the Cleveland Hills by certain overflow channels. I may be allowed to observe that, though this view is the outcome of much acute observation and reasoning,² it is wholly dependent upon the ice-barriers already mentioned, and that if they dissolve before the dry light of sceptical criticism, the lakes will "leave not a rack behind." I must also confess that, to my eyes, the so-called "overflow channels" much more closely resemble the remnants of ancient valley-systems, formed by only moderately rapid rivers, which have been isolated by the trespass of younger and more energetic streams, and they suggest that the main features of this picturesque upland were developed before rather than after the beginning of the Glacial Epoch. I think that even "Lake Pickering," though it has become an accepted fact with several geologists of high repute, can be more simply explained as a two-branched "valley of

¹ The boulders of picrite near Porth Nobla, from Llanerchymedd, though they have travelled southward, have moved much to the west.

² P. F. Kendall, Quart. Journ. Geol. Soc., lviii. (1902), 471.

strike," formed on the Kimeridge Clay, the eastern arm of which was beheaded, even in pre-Glacial times, by the sea.¹ As to Lake Oxford,² I must confess myself still more sceptical. Some changes, no doubt, have occurred in later Glacial and post-Glacial times; valleys have been here raised by deposit, there deepened sometimes by as much as 100 feet; the courses of lowland rivers may occasionally have been altered; but I doubt whether, since those times began, either ice-sheet or lake has ever concealed the site of that university city.

The submergence hypothesis assumes that, at the beginning of the Glacial Epoch, our islands stood rather above their present level, and during it gradually subsided, on the west to a greater extent than on the east, until at last the movement was reversed, and they returned nearly to their former position. During most of this time glaciers came down to the sea from the more mountainous islands, and in winter an ice-foot formed upon the shore. This, on becoming detached, carried away boulders, beach pebbles, and finer detritus. Great quantities of the last also were swept by swollen streams into the estuaries and spread over the sea-bed by coast currents, settling down especially in the quiet depths of submerged valleys. Shore-ice in Arctic regions, as Colonel H. W. Feilden³ has described, can striate stones and even the rock beneath it, and is able, on a subsiding area, gradually to push boulders up to a higher level. In fact, the state of the British region in those ages would not have been unlike that still existing near the coasts of the Barents and Kara Seas. Over the submerged region southward, and in some cases more or less eastward, currents would be prevalent, though changes of wind⁴ would often affect the drift of the floating ice-rafts. But though the submergence hypothesis is obviously free from the serious difficulties which have been indicated in discussing the other one, gives a simple explanation of the presence of marine organisms, and accords with what can be proved to have occurred in Norway, Waigatz Island, Novaia Zemlya, on the Lower St. Lawrence, in Grinnell Land, and elsewhere,⁵ it undoubtedly involves others. One of them—the absence of shore terraces, caves, or other sea marks—is perhaps hardly so grave as it is often thought to be. It may be met by the remark that unless the Glacial Age lasted for a very long time and the movements were interrupted by well-marked pauses, we could not expect to find any such record. In regard also to another objection, the rather rare and sporadic occurrence of marine shells, the answer would be that, on the Norway coast, where the ice-worn rock has certainly been submerged, sea-shells are far from common, and occur sporadically in the raised deltaic deposits of the fjords. An advocate of this view might also complain, not without justice, that, if he cited an inland terrace, it was promptly dismissed as the product of an ice-dammed lake, and his frequent instances of marine shells in stratified drifts were declared to have been transported from the sea by the lobe of an ice-sheet; even if they have been carried across the path of the Arenig ice, more than forty miles as the crow flies, from the Irish Sea up the Valley of the Severn, or forced some 1300 feet up Moel Tryfaen.⁷ The difficulty in the latter case, he would observe, is not met by saying the ice-sheet would be able to climb that hill "given there were a

¹ See for instance the courses of the Medway and the Beult over the Weald clay (C. Le Neve Foster and W. Topley, *Quart. Journ. Geol. Soc.*, xxi. (1865), p. 443).

² F. W. Harmer, *Quart. Journ. Geol. Soc.*, lxxiii. (1907), p. 470.

³ *Quart. Journ. Geol. Soc.*, xxxiv. (1878), p. 556.

⁴ See p. 23, and for the currents now dominant consult Dr. H. Basset in Prof. Herdman's Report on the Lancashire Sea Fisheries, *Trans. Biol. Soc. Liverpool*, xxiv. (1910), p. 123.

⁵ See "Ice Work," p. 221, and *Geol. Mag.*, 1900, p. 289.

⁶ If, as seems probable, the temperature was changing rather rapidly the old fauna would be pauperised and the new one make its way but slowly into the British fjords.

⁷ Critics of the submergence hypothesis seem to find a difficulty in admitting downward and upward movements, amounting sometimes to nearly 1,400 feet during Pleistocene Ages; but in the northern part of America the upheaval, at any rate, has amounted to about 1,000 feet, while on the western coast, beneath the lofty summit of Mount St. Elias, marine shells of existing species have been obtained some 5,000 feet above sea-level. It is also admitted that in several places the pre-glacial surface of the land was much above its present level. On the Red River, whatever be the explanation, foraminifers, radiolarians, and sponge spicules have been found at 700 feet above sea-level, and near Victoria, on the Saskatchewan, even up to about 1,900 feet.

sufficient head behind it."¹ That ice can be driven uphill has long been known, but the existence of the "sufficient head" must be demonstrated, not assumed. There may be "no logical halting-place between an uplift of ten or twenty feet to surmount a *roche moutonnée* and an equally gradual elevation to the height of Moel Tryfaen," yet there is a common-sense limitation even to a destructive *sortes*. The argument, in fact, is more specious than valid, until we are told approximately how thick the northern ice must be to produce the requisite pressure, and whether such an accumulation would be possible. The advocates of land ice admit that, before it had covered more than a few leagues on its southward journey, its thickness was less than 2000 feet, and we are not entitled, as I have endeavoured to show, to pile up ice indefinitely on either our British highlands or the adjacent sea-bed. The same reason also forbids us largely to augment the thickness of the latter by the snowfall on its surface, as happens to Antarctic barrier ice. Even if the thickness of the ice-cap over the Dumfries and Kirkcudbright hills had been about 2500 feet, that, with every allowance for viscosity, would hardly give us a head sufficient to force a layer of ice from the level of the sea-bed to a height of nearly 1400 feet above it, and at a distance of more than 100 miles.

Neither can we obtain much support from the instance in Spitsbergen, described by Profs. Garwood and Gregory, where the Ivory Glacier, after crossing the bed of a valley, had transported marine shells and drift from the floor (little above sea-level) to a height of about 400 feet on the opposite slope. Here the valley was narrow, and the glacier had descended from an inland ice-reservoir, much of which was at least 2800 feet above the sea, and rose occasionally more than a thousand feet higher.²

But other difficulties are far more grave. The thickness of the chalky Boulder Clay alone, as has been stated, not unfrequently exceeds 100 feet, and, though often much less, may have been reduced by denudation. This is an enormous amount to have been transported and distributed by floating ice. The materials, also, are not much more easily accounted for by this than by the other hypothesis. A continuous supply of well-worn chalk pebbles might indeed be kept up from a gradually rising or sinking beach, but it is difficult to see how, until the land had subsided for at least 200 feet, the chalky Boulder Clay could be deposited in some of the East Anglian valleys or on the Leicestershire hills. That depression, however, would seriously diminish the area of exposed chalk in Lincolnshire and Yorkshire, and the double of it would almost drown that rock. Again, the East Anglian Boulder Clay, as we have said, frequently abounds in fragments and finer detritus from the Kimeridge and Oxford Clays. But a large part of their outcrop would disappear before the former submergence was completed. Yet the materials of the Boulder Clay, though changing as it is traced across the country, more especially from east to west, seem to vary little in a vertical direction. The instances, also, of the transportation of boulders and smaller stones to higher levels, sometimes large in amount, as in the transference of "brockram" from outcrops near the bed of the Eden valley to the level of Stainmoor Gap, seem to be too numerous to be readily explained by the uplifting action of shore-ice in a subsiding area. Such a process is possible, but we should anticipate it would be rather exceptional.

Submergence also readily accounts for the above-named sands and gravels, but not quite so easily for their occurrence at such very different levels. On the eastern side of England gravelly sands may be found beneath the chalky Boulder Clay well below sea-level to three or four hundred feet above it. Again, since, on the submergence hypothesis, the Lower Boulder Clay about the estuaries of the Dee and the Mersey must represent a deposit from piedmont ice in a shallow sea, the mid-glacial sand (sometimes not very clearly marked in this part) ought not to be more than forty or fifty feet above the present Ordnance datum. But at Manchester it reaches more than 200 feet, while near Heywood it is at least 425 feet. In other

¹ P. F. Kendall in Wright's "Man and the Glacial Period," p. 171.

² *Quart. Jour. Geol. Soc.*, liv. (1898), p. 205. Earlier observations of some upthrust of materials by a glacier are noted on p. 219.

words, the sands and gravels, presumably (often certainly) mid-glacial, mantle, like the Upper Boulder Clay, over great irregularities of the surface, and are sometimes found, as already stated, up to more than 1200 feet. Either of these deposits may have followed the sea-line upwards or downwards, but that explanation would almost compel us to suppose that the sand was deposited during the submergence and the upper clay during the emergence, so that, with the former material, the higher in position is the newer in time, and with the latter the reverse. We must not, however, forget that in the island of Rügen we find more than one example of a stratified gravelly sand between two beds of Boulder Clay (containing Scandinavian erratics) which present some resemblance to the Boulder Clays of eastern England, while certain glacial deposits at Warnemünde, on the Baltic coast, sometimes remind us of the Contorted Drift of Norfolk.

Towards the close of the Glacial Epoch, the deposition of the Boulder Clay ceased¹ and its denudation began. On the low plateaux of the Eastern Counties it is often succeeded by coarse gravels, largely composed of flint, more or less water-worn. These occasionally include small intercalations of Boulder Clay, have evidently been derived from it, and indicate movement by fairly strong currents. Similar gravels are found overlying the Boulder Clay in other parts of England, sometimes at greater heights above sea-level. Occasionally the two are intimately related. For instance, a pit on the broad, almost level, top of the Gogmagog Hills, about 200 feet above sea-level and four miles south of Cambridge, shows a current-bedded sand and gravel, overlain by a Boulder Clay, obviously rearranged, while other pits in the immediate neighbourhood expose varieties and mixtures of one or the other material. But, as true Boulder Clay occurs in the valley below, these gravels must have been deposited, and that by rather strong currents, on a hill-top—a thing which seems impossible under anything like the existing conditions; and, even if the lowland were buried beneath ice full 200 feet in thickness, which made the hill-top into the bed of a lake, it is difficult to understand how the waters of that could be in rapid motion. Rearranged Boulder Clays also occur on the slopes of valleys² which may be explained, with perhaps some of the curious sections near Sudbury, by the slipping of materials from a higher position. But at Old Oswestry gravels with indications of ice action are found at the foot of the hills almost 700 feet below those of Gloppa.

Often the plateau gravels are followed at a lower level by terrace gravels,³ which descend towards the existing rivers, and suggest that valleys have been sometimes deepened, sometimes only re-excavated. The latter gravels are obviously deposited by rivers larger and stronger than those which now wind their way seawards, but it is difficult to explain the former gravels by any fluvial action, whether the water from a melting ice-sheet ran over the land or into a lake, held up by some temporary barrier. But the sorting action of currents in a slowly shallowing sea would be quite competent to account for them, so they afford an indirect support to the hypothesis of submergence. It is, however, generally admitted that there have been oscillations both of level and of climate since any Boulder Clay was deposited in the districts south of the Humber and the Ribble. The passing of the Great Ice Age was not sudden, and glaciers may have lingered in our mountain regions when Palæolithic man hunted the mammoth in the valley of the Thames or frequented the caves of Devon and Mendip. But of these times of transition, before written history became possible, and of sundry interesting topics connected with the Ice Age itself—of its cause, date, and duration, whether it was persistent or interrupted by warmer episodes, and, if so, by what number, of how often it had already recurred in the history of the earth—I must, for obvious reasons, refrain from speaking, and content myself with having endeavoured to place before you the facts of which, in my opinion, we must take account in reconstructing the

¹ Probably deposits of a distinctly glacial origin (such as those near Hessele in Yorkshire) continued in the northern districts, but on these we need not linger.

² For instance, at Stanningfield in the valley of the Lark.

³ These contain the instruments worked by palæolithic (Acheulean) man who, in this country at any rate, is later than the chalky boulder clay.

physical geography of Western Europe, and especially of our own country, during the Age of Ice.

Not unnaturally you will expect a decision in favour of one or the other litigant after this long summing up. But I can only say that, in regard to the British Isles, the difficulties in either hypothesis appear so great that, while I consider those in the "land-ice" hypothesis to be the more serious, I cannot as yet declare the other one to be satisfactorily established, and think we shall be wiser in working on in the hope of clearing up some of the perplexities. I may add that, for these purposes, regions like the northern coasts of Russia and Siberia appear to me more promising than those in closer proximity to the North or South Magnetic Poles. This may seem a "lame and impotent conclusion" to so long a disquisition, but there are stages in the development of a scientific idea when the best service we can do it is by attempting to separate facts from fancies, by demanding that difficulties should be frankly faced instead of being severely ignored, by insisting that the giving of a name cannot convert the imaginary into the real, and by remembering that if hypotheses yet on their trial are treated as axioms, the result will often bring disaster, like building a tower on a foundation of sand. To scrutinise, rather than to advocate any hypothesis, has been my aim throughout this address, and, if my efforts have been to some extent successful, I trust to be forgiven, though I may have trespassed on your patience and disappointed a legitimate expectation.

SECTION A.

MATHEMATICAL AND PHYSICAL SCIENCE.

OPENING ADDRESS BY PROF. E. W. HOBSON, SC.D., F.R.S., PRESIDENT OF THE SECTION.

SINCE the last meeting of our Association one of the most illustrious of the British workers in science during the nineteenth century has been removed from us by the death of Sir William Huggins. In the middle of the last century Sir William Huggins commenced that pioneer work of examination of the spectra of the stars which has ensured for him enduring fame in connection with the foundation of the science of Astrophysics. The exigencies of his work of analysis of the stellar spectra led him to undertake a minute examination of the spectra of the elements with a view to the determination of as many lines as possible. To the spectroscope he later added the photographic film as an instrument of research in his studies of the heavenly bodies. In 1864 Sir William Huggins made the important observation that many of the nebulae have spectra which consist of bright lines, and two years later he observed, in the case of a new star, both bright and dark lines in the same spectrum. In 1868 his penetrating and alert mind made him the first to perceive that the Doppler principle could be applied to the determination of the velocities of stars in the line of sight, and he at once set about the application of the method. His life-work, in a domain of absorbing interest, was rewarded by a rich harvest of discovery, obtained as the result of most patient and minute investigations. The "Atlas of Representative Stellar Spectra," published in the names of himself and Lady Huggins, remains as a monumental record of their joint labours.

The names of the great departments of science, Mathematics, Physics, Astronomy, Meteorology, which are associated with Section A, are a sufficient indication of the vast range of investigation which comes under the purview of our Section. An opinion has been strongly expressed in some quarters that the time has come for the erection of a separate Section for Astronomy and Meteorology, in order that fuller opportunities may be afforded than hitherto for the discussion of matters of special interest to those devoted to these departments of Science. I do not share this view. I believe that, whilst the customary division into sub-sections gives reasonable facilities for the treatment of questions interesting solely to specialists in the various branches with which our Section is concerned, a policy of disruption would be injurious to the wider interests of science. The close association of the older Astronomy with Mathematics, and of the newer Astronomy with Physics, form strong pre-

sumptions against the change that has been suggested. Meteorology, so far as it goes beyond the purely empirical region, is, and must always remain, a branch of Physics. No doubt the more technical problems which arise in connection with these subjects, though of great importance to specialists, are often of little or no interest to workers in cognate departments. It appears to me, however, that it is unwise, in view of the general objects of the British Association, to give too much prominence in the meetings to the more technical aspects of the various departments of science. Ample opportunities for the full discussion of all the detailed problems, the solution of which forms a great and necessary part of the work of those who are advancing science in its various branches, are afforded by the special Societies which make those branches their exclusive concern. The British Association will, in my view, be performing its functions most efficiently if it gives much prominence to those aspects of each branch of science which are of interest to a public at least in some degree larger than the circle of specialists concerned with the particular branch. To afford an opportunity to workers in any one department of obtaining some knowledge of what is going on in other departments, to stimulate by means of personal intercourse with workers on other lines the sense of solidarity of men of science, to do something to counteract that tendency to narrowness of view which is a danger arising from increasing specialisation, are functions the due performance of which may do much to further that supreme object, the advancement of science, for which the British Association exists.

I propose to address to you a few remarks, necessarily fragmentary and incomplete, upon the scope and tendencies of modern Mathematics. Not to transgress against the canon I have laid down, I shall endeavour to make my treatment of the subject as little technical as possible.

Probably no other department of knowledge plays a larger part outside its own narrower domain than Mathematics. Some of its more elementary conceptions and methods have become part of the common heritage of our civilisation, interwoven in the everyday life of the people. Perhaps the greatest labour-saving invention that the world has seen belongs to the formal side of Mathematics; I allude to our system of numerical notation. This system, which, when scrutinised, affords the simplest illustration of the importance of Mathematical form, has become so much an indispensable part of our mental furniture that some effort is required to realise that an apparently so obvious idea embodies a great invention, one to which the Greeks, with their unsurpassed capacity for abstract thinking, never attained. An attempt to do a multiplication sum in Roman numerals is perhaps the readiest road to an appreciation of the advantages of this great invention. In a large group of sciences, the formal element, the common language, so to speak, is supplied by Mathematics; the range of the application of mathematical methods and symbolism is ever increasing. Without taking too literally the celebrated dictum of the great philosopher Kant, that the amount of real science to be found in any special subject is the amount of Mathematics contained therein, it must be admitted that each branch of science which is concerned with natural phenomena, when it has reached a certain stage of development, becomes accessible to, and has need of, mathematical methods and language; this stage has, for example, been reached in our time by parts of the science of Chemistry. Even Biology and Economics have begun to require mathematical methods, at least on their statistical side. As a science emerges from the stages in which it consists solely of more or less systematised descriptions of the phenomena with which it is concerned in their more superficial aspect; when the intensive magnitudes discerned in the phenomena become representable as extensive magnitudes, then is the beginning of the application of mathematical modes of thought; at a still later stage, when the phenomena become accessible to dynamical treatment, Mathematics is applicable to the subject to a still greater extent.

Mathematics shares with the closely allied subject of Astronomy the honour of being the oldest of the sciences. When we consider that it embodies, in an abstract form, some of the more obvious, and yet fundamental, aspects of our experience of the external world, this is not

altogether surprising. The comparatively high degree of development which, as recent historical discoveries have disclosed, it had attained amongst the Babylonians more than five thousand years B.C., may well astonish us. These times must have been preceded by still earlier ages, in which the mental evolution of man led him to the use of the tally, and of simple modes of measurement, long before the notions of number and of magnitude appeared in an explicit form.

I have said that Mathematics is the oldest of the sciences; a glance at its more recent history will show that it has the energy of perpetual youth. The output of contributions to the advance of the science during the last century and more has been so enormous that it is difficult to say whether pride in the greatness of achievement in his subject, or despair at his inability to cope with the multiplicity of its detailed developments, should be the dominant feeling of the mathematician. Few people outside the small circle of mathematical specialists have any idea of the vast growth of mathematical literature. The Royal Society Catalogue contains a list of nearly thirty-nine thousand papers on subjects of Pure Mathematics alone, which have appeared in seven hundred serials during the nineteenth century. This represents only a portion of the total output, the very large number of treatises, dissertations, and monographs published during the century being omitted. During the first decade of the twentieth century this activity has proceeded at an accelerated rate. Mathematical contributions to Mechanics, Physics, and Astronomy would greatly swell the total. A notion of the range of the literature relating, not only to Pure Mathematics, but also to all branches of science to which mathematical methods have been applied, will be best obtained by an examination of that monumental work, the "Encyclopädie der mathematischen Wissenschaften"—when it is completed.

The concepts of the pure mathematician, no less than those of the physicist, had their origin in physical experience analysed and clarified by the reflective activities of the human mind; but the two sets of concepts stand on different planes in regard to the degree of abstraction which is necessary in their formation. Those of the mathematician are more remote from actual unanalysed precepts than are those of the physicist, having undergone in their formation a more complete idealisation and removal of elements inessential in regard to the purposes for which they are constructed. This difference in the planes of thought frequently gives rise to a certain misunderstanding between the mathematician and the physicist, due in the case of either to an inadequate appreciation of the point of view of the other. On the one hand it is frequently and truly said of particular mathematicians that they are lacking in the physical instinct, and on the other hand a certain lack of sympathy is frequently manifested on the part of physicists for the aims and ideals of the mathematician. The habits of mind and the ideals of the mathematician and of the physicist cannot be of an identical character. The concepts of the mathematician necessarily lack, in their pure form, just that element of concreteness which is an essential condition of the success of the physicist, but which to the mathematician would often only obscure those aspects of things which it is his province to study. The abstract mathematical standard of exactitude is one of which the physicist can make no direct use. The calculations in Mathematics are directed towards ideal precision; those in Physics consist of approximations within assigned limits of error. The physicist can, for example, make no direct use of such an object as an irrational number; in any given case a properly chosen rational number approximating to the irrational one is sufficient for his purpose. Such a notion as continuity, as it occurs in Mathematics, is, in its purity, unknown to the physicist, who can make use only of sensible continuity. The physical counterpart of mathematical discontinuity is very rapid change through a thin layer of transition, or during a very short time. Much of the skill of the true mathematical physicist and of the mathematical astronomer consists in the power of adapting methods and results carried out on an exact mathematical basis to obtain approximations sufficient for the purposes of physical

measurement. It might perhaps be thought that a scheme of Mathematics on a frankly approximative basis would be sufficient for all the practical purposes of application in Physics, Engineering Science, and Astronomy, and no doubt it would be possible to develop, to some extent at least, a species of Mathematics on these lines. Such a system would, however, involve an intolerable awkwardness and prolixity in the statement of results, especially in view of the fact that the degrees of approximation necessary for various purposes are very different, and thus that unassigned grades of approximation would have to be provided for. Moreover, the mathematician working on these lines would be cut off from his chief sources of inspiration, the ideals of exactitude and logical rigour, as well as from one of his most indispensable guides to discovery, symmetry, and permanence of mathematical form. The history of the actual movements of mathematical thought through the centuries shows that these ideals are the very life-blood of the science, and warrants the conclusion that a constant striving towards their attainment is an absolutely essential condition of vigorous growth. These ideals have their roots in irresistible impulses and deep-seated needs of the human mind, manifested in its efforts to introduce intelligibility into certain great domains of the world of thought.

There exists a widespread impression among physicists, engineers, and other men of science that the effect of recent developments of Pure Mathematics, by making it more abstract than formerly, has been to remove it further from the order of ideas of those who are primarily concerned with the physical world. The prejudice that Pure Mathematics has its sole *raison d'être* in its function of providing useful tools for application in the physical sciences, a prejudice which did much to retard the due development of Pure Mathematics in this country during the nineteenth century, is by no means extinct. It is not infrequently said that the present devotion of many mathematicians to the interminable discussion of purely abstract questions relating to modern developments of the notions of number and function, and to theories of algebraic form, serves only the purpose of deflecting them from their proper work into paths which lead nowhere. It is considered that mathematicians are apt to occupy themselves too exclusively with ideas too remote from the physical order in which Mathematics had its origin, and in which it should still find its proper applications. A direct answer to the question *cui bono?* when it is raised in respect of a department of study such as Pure Mathematics, seldom carries conviction, in default of a standard of values common to those who ask and to those who answer the question. To appreciate the importance of a sphere of mental activity different from our own always requires some effort of the sympathetic imagination, some recognition of the fact that the absolute value of interests and ideals of a particular class may be much greater than the value which our own mentality inclines us to attach to them. If a defence is needed of the expenditure of time and energy on the abstract problems of Pure Mathematics, that defence must be of a cumulative character. The fact that abstract mathematical thinking is one of the normal forms of activity of the human mind, a fact which the general history of thought fully establishes, will appeal to some minds as a ground of decisive weight. A great department of thought must have its own inner life, however transcendent may be the importance of its relations to the outside. No department of science, least of all one requiring so high a degree of mental concentration as Mathematics, can be developed entirely, or even mainly, with a view to applications outside its own range. The increased complexity and specialisation of all branches of knowledge makes it true in the present, however it may have been in former times, that important advances in such a department as Mathematics can be expected only from men who are interested in the subject for its own sake, and who, whilst keeping an open mind for suggestions from outside, allow their thought to range freely in those lines of advance which are indicated by the present state of their subject, untrammelled by any preoccupation as to applications to other departments of science. Even with a view to applications, if Mathematics is to be adequately equipped for the purpose of coping with the

intricate problems which will be presented to it in the future by Physics, Chemistry, and other branches of physical science, many of these problems probably of a character which we cannot at present forecast, it is essential that Mathematics should be allowed to develop itself freely on its own lines. Even if much of our present mathematical theorising turns out to be useless for external purposes, it is wiser, for a well-known reason, to allow the wheat and the tares to grow together. It would be easy to establish in detail that many of the applications which have been actually made of Mathematics were wholly unforeseen by those who first developed the methods and ideas on which they rest. Recently, the more refined mathematical methods which have been applied to gravitational Astronomy by Delaunay, G. W. Hill, Poincaré, E. W. Brown, and others, have thrown much light on questions relating to the solar system, and have much increased the accuracy of our knowledge of the motions of the moon and the planets. Who knows what weapons forged by the theories of functions, of differential equations, or of groups, may be required when the time comes for such an empirical law as Mendeléeff's periodic law of the elements to receive its dynamical explanation by means of an analysis of the detailed possibilities of relatively stable types of motion, the general schematic character of which will have been indicated by the physicist? It is undoubtedly true that the cleft between Pure Mathematics and Physical Science is at the present time wider than formerly. That is, however, a result of the natural development, on their own lines, of both subjects. In the classical period of the eighteenth century, the time of Lagrange and Laplace, the nature of the physical investigations, consisting largely of the detailed working out of problems of gravitational Astronomy in accordance with Newton's law, was such that the passage was easy from the concrete problems to the corresponding abstract mathematical ones. Later on, mathematical physicists were much occupied with problems which lent themselves readily to treatment by means of continuous analysis. In our own time the effect of recent developments of Physics has been to present problems of molecular and sub-molecular Mechanics to which continuous analysis is not at least directly applicable, and can only be made applicable by a process of averaging the effects of great swarms of discrete entities. The speculative and incomplete character of our conceptions of the structure of the objects of investigation has made the applications of Dynamics to their detailed elucidation tentative and partial. The generalised dynamical scheme developed by Lagrange and Hamilton, with its power of dealing with systems, the detailed structure of which is partially unknown, has, however, proved a powerful weapon of attack, and affords a striking instance of the deep-rooted significance of mathematical form. The wonderful and perhaps unprecedentedly rapid discoveries in Physics which have been made in the last two decades have given rise to many questions which are as yet hardly sufficiently definite in form to be ripe for mathematical treatment, a necessary condition of which treatment consists in a certain kind of precision in the data of the problems to be solved.

The difficulty of obtaining an adequate notion of the general scope and aims of Mathematics, or even of special branches of it, is perhaps greater than in the case of any other science. Many persons, even such as have made a serious and prolonged study of the subject, feel the difficulty of seeing the wood for trees. The severe demands made upon students by the labour of acquiring a difficult technique largely accounts for this; but teachers might do much to facilitate the attainment of a wider outlook by directing the attention of their students to the more general and less technical aspects of the various parts of the subject, and especially by the introduction into the courses of instruction of more of the historical element than has hitherto been usual.

All attempts to characterise the domain of Mathematics by means of a formal definition which shall not only be complete, but which shall also rigidly mark off that domain from the adjacent provinces of Formal Logic on the one side and of Physical Science on the other side, are almost certain to meet with but doubtful success; such success as they may attain will probably be only

transient, in view of the power which the science has always shown of constantly extending its borders in unforeseen directions. Such definitions, many of which have been advanced, are apt to err by excess or defect, and often contain distinct traces of the personal predilections of those who formulate them. There was a time when it would have been a tolerably sufficient description of Pure Mathematics to say that its subject-matter consisted of magnitude and geometrical form. Such a description of it would be wholly inadequate at the present day. Some of the most important branches of modern Mathematics, such as the theory of groups, and Universal Algebra, are concerned, in their abstract forms, neither with magnitude nor with number, nor with geometrical form. That great modern development, Projective Geometry, has been so formulated as to be independent of all metric considerations. Indeed, the tendency of mathematicians under the influence of the movement known as the Arithmetisation of Analysis, a movement which has become a dominant one in the last few decades, is to banish altogether the notion of measurable quantity as a conception necessary to Pure Mathematics, Number, in the extended meaning it has attained, taking its place. Measurement is regarded as one of the applications, but as no part of the basis, of mathematical analysis. Perhaps the least inadequate description of the general scope of modern Pure Mathematics—I will not call it a definition—would be to say that it deals with *form*, in a very general sense of the term; this would include algebraic form, geometrical form, functional relationship, the relations of order in any ordered set of entities such as numbers, and the analysis of the peculiarities of form of groups of operations. A strong tendency is manifested in many of the recent definitions to break down the line of demarcation which was formerly supposed to separate Mathematics from formal logic; the rise and development of symbolic logic has no doubt emphasised this tendency. Thus Mathematics has been described by the eminent American mathematician and logician B. Pierce as “the Science which draws necessary conclusions,” a pretty complete identification of Mathematics with logical procedure in general. A definition which appears to identify all Mathematics with the Mengenlehre, or Theory of Aggregates, has been given by E. Papperitz: “The subject-matter of Pure Mathematics consists of the relations that can be established between any objects or thought when we regard those objects as contained in an ordered manifold; the law of order of this manifold must be subject to our choice.” The form of definition which illustrates most strikingly the tendencies of the modern school of logicist is one given by Mr. Bertrand Russell. I reproduce it here, in order to show how wide is the chasm between the modes of expression of adherents of this school and those of mathematicians under the influence of the ordinary traditions of the science. Mr. Russell writes: “Pure Mathematics is the class of all propositions of the form ‘ p implies q ,’ where p and q are propositions containing one or more variables, the same in the two propositions, and neither p nor q contains any constants except logical constants. And logical constants are all notions definable in terms of the following: Implication, the relation of a term to a class of which it is a member, the notion of *such that*, the notion of relation, and such further notions as may be involved in the general notion of propositions of the above form. In addition to these, Mathematics uses a notion which is not a constituent of the propositions which it considers—namely, the notion of truth.”

The belief is very general amongst instructed persons that the truths of Mathematics have absolute certainty, or at least that there appertains to them the highest degree of certainty of which the human mind is capable. It is thought that a valid mathematical theorem is necessarily of such a character as to compel belief in any mind capable of following the steps of the demonstration. Any considerations tending to weaken this belief would be disconcerting, and would cause some degree of astonishment. At the risk of this, I must here mention two facts which are of considerable importance as regards an estimation of the precise character of mathematical know-

¹ “Principles of Mathematics,” p. 1.

ledge. In the first place, it is a fact that frequently, and at various times, differences of opinion have existed among mathematicians, giving rise to controversies as to the validity of whole lines of reasoning, and affecting the results of such reasoning; a considerable amount of difference of opinion of this character exists among mathematicians at the present time. In the second place, the accepted standard of rigour, that is, the standard of what is deemed necessary to constitute a valid demonstration, has undergone change in the course of time. Much of the reasoning which was formerly regarded as satisfactory and irrefutable is now regarded as insufficient to establish the results which it was employed to demonstrate. It has even been shown that results which were once supposed to have been fully established by demonstrations are, in point of fact, affected with error. I propose here to explain in general terms how these phenomena are possible.

In every subject of study, if one probes deep enough, there are found to be points in which that subject comes in contact with general philosophy, and where differences of philosophical view will have a greater or less influence on the attitude of the mind towards the principles of the particular subject. This is not surprising when we reflect that there is but one universe of thought, that no department of knowledge can be absolutely isolated, and that metaphysical and psychological implications are a necessary element in all the activities of the mind. A particular department, such as Mathematics, is compelled to set up a more or less artificial frontier, which marks it off from general philosophy. This frontier consists of a set of regulative ideas in the form of indefinables and axioms, partly ontological assumptions, and partly postulations of a logical character. To go behind these, to attempt to analyse their nature and origin, and to justify their validity, is to go outside the special department and to touch on the domains of the metaphysician and the psychologist. Whether they are regarded as possessing apodictic certainty or as purely hypothetical in character, these ideas represent the data or premises of the science, and the whole of its edifice is dependent upon them. They serve as the foundation on which all is built, as well as the frontier on the side of philosophy and psychology. A set of data ideally perfect in respect of precision and permanence is unattainable—or at least has not yet been attained; and the adjustment of frontiers is one of the most frequent causes of strife. As a matter of fact, variations of opinion have at various times arisen within the ranks of the mathematicians as to the nature, scope, and proper formulation of the principles which form the foundations of the science, and the views of mathematicians in this regard have always necessarily been largely affected by the conscious or unconscious attitude of particular minds towards questions of general philosophy. It is in this region, I think, that the source is to be found of those remarkable differences of opinion amongst mathematicians which have come into prominence at various times, and have given rise to much controversy as to fundamentals. Since the time of Newton and Leibnitz there has been almost unceasing discussion as to the proper foundations for the so-called infinitesimal calculus. More recently, questions relating to the foundations of geometry and rational mechanics have much occupied the attention of mathematicians. The very great change which has taken place during the last half-century in the dominant view of the foundations of mathematical analysis—a change which has exercised a great influence extending through the whole detailed treatment of that subject—although critical in its origin, has been constructive in its results. The Mengenlehre, or theory of aggregates, had its origin in the critical study of the foundations of analysis, but has already become a great constructive scheme, is indispensable as a method in the investigations of analysis, provides the language requisite for the statement in precise form of analytical theorems of a general character, and, moreover, has already found important applications in geometry. In connection with the Mengenlehre, there has arisen a controversy amongst mathematicians which is at the present time far from having reached a decisive issue. The exact point at issue is one which may be described as a matter of mathematical ontology; it turns upon the question of

what constitutes a valid definition of a mathematical object. The school known as mathematical "idealists" admit, as valid objects of mathematical discussion, entities which the rival "empiricist" school regard as non-existent for mathematical thought, because insufficiently defined. It is clear that the idealist may build whole superstructures on a foundation which the empiricist regards as made of sand, and this is what has actually happened in some of the recent developments of what has come to be known as Cantorism. The difference of view of these rival schools, depending as it does on deep-seated differences of philosophical outlook, is thought by some to be essentially irreconcilable. This controversy was due to the fact that certain processes of reasoning, of very considerable plausibility, which had been employed by G. Cantor, the founder of the Mengenlehre, had led to results which contained flat contradictions. The efforts made to remove these contradictions, and to trace their source, led to the discussion, disclosing much difference of opinion, of the proper definitions and principles on which the subject should be based.

The proposition $7+5=12$, taken as typical of the propositions expressing the results of the elementary operations of arithmetic, has since the time of Kant given rise to very voluminous discussion amongst philosophers in relation to the precise meaning and implication of the operation and the terms. It will, however, be maintained, probably by the majority of mankind, that the theorem retains its validity as stating a practically certain and useful fact, whatever view philosophers may choose to take of its precise nature—as, for example, whether it represents, in the language of Kant, a synthetic or an analytic judgment. It may, I think, be admitted that there is much cogency in this view; and, were Mathematics concerned with the elementary operations of arithmetic alone, it could fairly be held that the mathematician, like the practical man of the world, might without much risk shut his eyes and ears to the discussions of the philosophers on such points. The exactitude of such a proposition, in a sufficiently definite sense for practical purposes, is empirically verifiable by sensuous intuition, whatever meaning the metaphysician may attach to it. But Mathematics cannot be built up from the operations of elementary arithmetic without the introduction of further conceptual elements. Except in certain very simple cases, no process of measurement, such as the determination of an area or a volume, can be carried out with exactitude by a finite number of applications of the operations of arithmetic. The result to be obtained appears in the form of a limit, corresponding to an interminable sequence of arithmetical operations. The notion of "limit," in the definite form given to it by Cauchy and his followers, together with the closely related theory of the arithmetic continuum, and the notions of continuity and functionality, lie at the very heart of modern analysis. Essentially bound up with this central doctrine of limits is the concept of a non-finite set of entities, a concept which is not directly derivable from sensuous intuition, but which is, nevertheless, a necessary postulation in mathematical analysis. The conception of the infinite, in some form, is thus indispensable in Mathematics; and this conception requires precise characterisation by a scheme of exact definitions, prior to all the processes of deduction required in obtaining the detailed results of analysis. The formulation of this precise scheme gives an opening to differences of philosophical opinion which has led to a variety of views as to the proper character of those definitions which involve the concept of the infinite. Here is the point of divergence of opinion among mathematicians to which I have alluded above. Under what conditions is a non-finite aggregate of entities a properly defined object of mathematical thought, of such a character that no contradictions will arise in the theories based upon it? That is the question to which varying answers have been offered by different mathematical thinkers. No one answer of a completely general character has as yet met with universal acceptance. Physical intuition offers no answer to such a question; it is one which abstract thought alone can settle. It cannot be altogether avoided, because, without the notion of the infinite, at least in connection with the central conception of the "limit,"

mathematical analysis as a coherent body of thought falls to the ground.

Both in geometry and in analysis our standard of what constitutes a rigorous demonstration has in the course of the nineteenth century undergone an almost revolutionary change. That oldest text-book of science in the world, "Euclid's Elements of Geometry," has been popularly held for centuries to be the very model of deductive logical demonstration. Criticism has, however, largely invalidated this view. It appears that, at a large number of points, assumptions not included in the preliminary axioms and postulates are made use of. The fact that these assumptions usually escape notice is due to their nature and origin. Derived as they are from our spatial intuition, their very self-evidence has allowed them to be ignored, although their truth is not more obvious empirically than that of other assumptions derived from the same source which are included in the axioms and postulates explicitly stated as part of the foundation of Euclid's treatment of the subject. The method of superimposition, employed by Euclid with obvious reluctance, but forming an essential part of his treatment of geometry, is, when regarded from his point of view, open to most serious objections as regards its logical coherence. In analysis, as in geometry, the older methods of treatment consisted of processes of deduction eked out by the more or less surreptitious introduction, at numerous points in the subject, of assumptions only justifiable by spatial intuition. The result of this deviation from the purely deductive method was more disastrous in the case of analysis than in geometry, because it led to much actual error in the theory. For example, it was held until comparatively recently that a continuous function necessarily possesses a differential coefficient, on the ground that a curve always has a tangent. This we now know to be quite erroneous, when any reasonable definition of continuity is employed. The first step in the discovery of this error was made when it occurred to Ampère that the existence of the differential coefficient could only be asserted as a theorem requiring proof, and he himself published an attempt at such proof. The erroneous character of the former belief on this matter was most strikingly exhibited when Weierstrass produced a function which is everywhere continuous, but which nowhere possesses a differential coefficient; such functions can now be constructed *ad libitum*. It is not too much to say that no one of the general theorems of analysis is true without the introduction of limitations and conditions which were entirely unknown to the discoverers of those theorems. It has been the task of mathematicians under the lead of such men as Cauchy, Riemann, Weierstrass, and G. Cantor, to carry out the work of reconstruction of mathematical analysis, to render explicit all the limitations of the truth of the general theorems, and to lay down the conditions of validity of the ordinary analytical operations. Physicists and others often maintain that this modern extreme precision amounts to an unnecessary and pedantic purism, because in all practical applications of Mathematics only such functions are of importance as exclude the remoter possibilities contemplated by theorists. Such objections leave the true mathematician unmoved; to him it is an intolerable defect that, in an order of ideas in which absolute exactitude is the guiding ideal, statements should be made and processes employed, both of which are subject to unexpressed qualifications, as conditions of their truth or validity. The pure mathematician has developed a specialised conscience, extremely sensitive as regards sins against logical precision. The physicist, with his conscience hardened in this respect by the rough-and-tumble work of investigating the physical world, is apt to regard the more tender organ of the mathematician with that feeling of impatience, not unmingled with contempt, which the man of the world manifests for what he considers to be over-scrupulosity and unpracticality.

It is true that we cannot conceive how such a science as Mathematics could have come into existence apart from physical experience. But it is also true that physical precepts, as given directly in unanalysed experience, are wholly unfitted to form the basis of an exact science. Moreover, physical intuition fails altogether to afford any trustworthy guidance in connection with the concept of the infinite, which, as we have seen, is in some form

indispensable in the formation of a coherent system of mathematical analysis. The hasty and uncritical extension to the region of the infinite, of results which are true and often obvious in the region of the finite, has been a fruitful source of error in the past, and remains as a pit-fall for the unwary student in the present. The notions derived from physical intuition must be transformed into a scheme of exact definitions and axioms before they are available for the mathematician, the necessary precision being contributed by the mind itself. A very remarkable fact in connection with this process of refinement of the rough data of experience is that it contains an element of arbitrariness, so that the result of the process is not necessarily unique. The most striking example of this want of uniqueness in the conceptual scheme so obtained is the case of geometry, in which it has been shown to be possible to set up various sets of axioms, each set self-consistent, but inconsistent with any other of the sets, and yet such that each set of axioms, at least under suitable limitations, leads to results consistent with our perception of actual space-relations. Allusion is here made, in particular, to the well-known geometries of Lobatchewsky and of Riemann, which differ from the geometry of Euclid in respect of the axiom of parallels, in place of which axioms inconsistent with that of Euclid and with one another are substituted. It is a matter of demonstration that any inconsistency which might be supposed to exist in the scheme known as hyperbolic geometry, or in that known as elliptic geometry, would necessarily entail the existence of a corresponding inconsistency in Euclid's set of axioms. The three geometries therefore, from the logical point of view, are completely on a par with one another. An interesting mathematical result is that all efforts to prove Euclid's axiom of parallels, *i.e.* to deduce it from his other axioms, are doomed to necessary failure; this is of importance in view of the many efforts that have been made to obtain the proof referred to. When the question is raised which of these geometries is the true one, the kind of answer that will be given depends a good deal on the view taken of the relation of conceptual schemes in general to actual experience. It is maintained by M. Poincaré, for example, that the question which is the true scheme has no meaning; that it is, in fact, entirely a matter of convention and convenience which of these geometries is actually employed in connection with spatial measurements. To decide between them by a crucial test is impossible, because our space perceptions are not sufficiently exact in the mathematical sense to enable us to decide between the various axioms of parallels. Whatever views are taken as to the difficult questions that arise in this connection, the contemplation and study of schemes of geometry wider than that of Euclid, and some of them including Euclid's geometry as a special case, is of great interest, not only from the purely mathematical point of view, but also in relation to the general theory of knowledge, in that, owing to the results of this study, some change is necessitated in the views which have been held by philosophers as to what is known as Kant's space-problem.

The school of thought which has most emphasised the purely logical aspect of Mathematics is that which is represented in this country by Mr. Bertrand Russell and Dr. Whitehead, and which has distinguished adherents both in Europe and in America. The ideal of this school is a presentation of the whole of Mathematics as a deductive scheme in which are employed a certain limited number of indefinables and unprovable axioms, by means of a procedure in which all possibility of the illicit intrusion of extraneous elements into the deduction is excluded by the employment of a symbolism in which each symbol expresses a certain logical relation. This school receives its inspiration from a peculiar form of philosophic realism which, in its revolt from idealism, produces in the adherents of the school a strong tendency to ignore altogether the psychological implications in the movements of mathematical thought. This is carried so far that in their writings no explicit recognition is made of any psychological factors in the selection of the indefinables and in the formulation of the axioms upon which the whole structure of Mathematics is to be based. The

actually worked-out part of their scheme has as yet reached only the mere fringe of modern Mathematics as a great detailed body of doctrine; but to any objection to the method on the ground of the prolixity of the treatment which would be necessary to carry it out far enough to enable it to embrace the various branches of Mathematics in all the wealth of their present development, it would probably be replied that the main point of interest is to establish in principle the possibility only of subsuming Pure Mathematics under a scheme of logic. It is quite impossible for me here to attempt to discuss, even in outline, the tenets of this school, or even to deal with the interesting question of the possibility of setting up a final system of definables and axioms which shall suffice for all present and future developments of Mathematics.

I am very far from wishing to minimise the high philosophic interest of the attempt made by the Peano-Russell school to exhibit Mathematics as a scheme of deductive logic. I have myself emphasised above the necessity and importance of fitting the results of mathematical research in their final form into a framework of deduction for the purpose of ensuring the complete precision and the verification of the various mathematical theories. At the same time, it must be recognised that the purely deductive method is wholly inadequate as an instrument of research. Whatever view may be held as regards the place of psychological implications in a completed body of mathematical doctrine, in research the psychological factor is of paramount importance. The slightest acquaintance with the history of Mathematics establishes the fact that discoveries have seldom, or never, been made by purely deductive processes. The results are thrown into a purely deductive form after, and often long after, their discovery. In many cases the purely deductive form, in the full sense, is quite modern. The possession of a body of indefinables, axioms, or postulates, and symbols denoting logical relation, would, taken by itself, be wholly insufficient for the development of a mathematical theory. With these alone the mathematician would be unable to move a step. In face of an unlimited number of possible combinations, a principle of selection of such as are of interest, a purposive element, and a perceptive faculty are essential for the development of anything new. In the process of discovery, the chains in a sequence of logical deduction do not at first arise in their final order in the mind of the mathematical discoverer. He divines the results before they are established; he has an intuitive grasp of the general line of a demonstration long before he has filled in the details. A developed theory, or even a demonstration of a single theorem, is no more identical with a mere complex of syllogisms than a melody is identical with the mere sum of the musical notes employed in its composition. In each case the whole is something more than merely the sum of its parts; it has a unity of its own, and that unity must be, in some measure at least, discerned by its creator before the parts fall completely into their places. Logic is, so to speak, the grammar of Mathematics; but a knowledge of the rules of grammar and the letters of the alphabet would not be sufficient equipment to enable a man to write a book. There is much room for individuality in the modes of mathematical discovery. Some great mathematicians have employed largely images derived from spatial intuition as a guide to their results; others appear wholly to have discarded such aids, and were led by a fine feeling for algebraic and other species of mathematical form. A certain tentative process is common, in which, by the aid of results known or obtained in special cases, generalisations are perceived and afterwards established, which take up into themselves all the special cases so employed. Most mathematicians leave some traces, in the final presentation of their work, of the scaffolding they have employed in building their edifices, some much more than others.

The difference between a mathematical theory in the making and as a finished product is, perhaps, most strikingly illustrated by the case of geometry, as presented in its most approved modern shape. It is not too much to say that geometry, reduced to a purely deductive form—as presented, for example, by Hilbert, or by some of the modern Italian school—has no necessary connection with space. The words "point," "line," "plane" are em-

ployed to denote any entities whatever which satisfy certain prescribed conditions of relationship. Various premises are postulated that would appear to be of a perfectly arbitrary nature, if we did not know how they had been suggested. In that division of the subject known as metric geometry, for example, axioms of congruency are assumed which, by their purely abstract character, avoid the very real difficulties that arise in this regard in reducing perceptual space-relations of measurements to a purely conceptual form. Such schemes, triumphs of constructive thought at its highest and most abstract level as they are, could never have been constructed apart from the space-perceptions that suggested them, although the concepts of spatial origin are transformed almost out of recognition. But what I want to direct attention to here is that, apart from the basis of this geometry, mathematicians would never have been able to find their way through the details of the deductions without having continual recourse to the guidance given them by spatial intuition. If one attempts to follow one of the demonstrations of a particular theorem in the work of writers of this school, one would find it quite impossible to retain the steps of the process long enough to master the whole, without the aid of the very spatial suggestions which have been abstracted. This is perhaps sufficiently warranted by the fact that writers of this school find it necessary to provide their readers with figures, in order to avoid complete bewilderment in following the demonstrations, although the processes, being purely logical deductions from premises of the nature I have described, deal only with entities which have no necessary similarity to anything indicated by the figures.

A most interesting account has been written by one of the greatest mathematicians of our time, M. Henri Poincaré, of the way in which he was led to some of his most important mathematical discoveries.¹ He describes the process of discovery as consisting of three stages: the first of these consists of a long effort of concentrated attention upon the problem in hand in all its bearings; during the second stage he is not consciously occupied with the subject at all, but at some quite unexpected moment the central idea which enables him to surmount the difficulties, the nature of which he had made clear to himself during the first stage, flashes suddenly into his consciousness. The third stage consists of the work of carrying out in detail and reducing to a connected form the results to which he is led by the light of his central idea; this stage, like the first, is one requiring conscious effort. This is, I think, clearly not a description of a purely deductive process; it is assuredly more interesting to the psychologist than to the logician. We have here the account of a complex of mental processes in which it is certain that the reduction to a scheme of precise logical deduction is the latest stage. After all, a mathematician is a human being, not a logic-engine. Who that has studied the works of such men as Euler, Lagrange, Cauchy, Riemann, Sophus Lie, and Weierstrass, can doubt that a great mathematician is a great artist? The faculties possessed by such men, varying greatly in kind and degree with the individual, are analogous to those requisite for constructive art. Not every great mathematician possesses in a specially high degree that critical faculty which finds its employment in the perfection of form, in conformity with the ideal of logical completeness; but every great mathematician possesses the rarer faculty of constructive imagination.

The actual evolution of mathematical theories proceeds by a process of induction strictly analogous to the method of induction employed in building up the physical sciences; observation, comparison, classification, trial, and generalisation are essential in both cases. Not only are special results, obtained independently of one another, frequently seen to be really included in some generalisation, but branches of the subject which have been developed quite independently of one another are sometimes found to have connections which enable them to be synthesised in one single body of doctrine. The essential nature of mathematical thought manifests itself in the discernment of fundamental identity in the mathematical aspects of what are superficially very different domains. A striking example of this species of immanent identity of mathematical form was exhibited by the discovery of that distinguished

mathematician, our General Secretary, Major Macmahon, that all possible Latin squares are capable of enumeration by the consideration of certain differential operators. Here we have a case in which an enumeration, which appears to be not amenable to direct treatment, can actually be carried out in a simple manner when the underlying identity of the operation is recognised with that involved in certain operations due to differential operators, the calculus of which belongs superficially to a wholly different region of thought from that relating to Latin squares. The modern abstract theory of groups affords a very important illustration of this point; all sets of operations, whatever be their concrete character, which have the same group, are from the point of view of the abstract theory identical, and an analysis of the properties of the abstract group gives results which are applicable to all the actual sets of operations, however diverse their character, which are dominated by the one group. The characteristic feature of any special geometrical scheme is known when the group of transformations which leave unaltered certain relations of figures has been assigned. Two schemes in which the space elements may be quite different have this fundamental identity, provided they have the same group; every special theorem is then capable of interpretation as a property of figures either in the one or in the other geometry. The mathematical physicist is familiar with the fact that a single mathematical theory is often capable of interpretation in relation to a variety of physical phenomena. In some instances a mathematical formulation, as in some fashion representing observed facts, has survived the physical theory it was originally devised to represent. In the case of electromagnetic and optical theory, there appears to be reason for trusting the equations, even when the proper physical interpretation of some of the vectors appearing in them is a matter of uncertainty and gives rise to much difference of opinion; another instance of the fundamental nature of mathematical form.

One of the most general mathematical conceptions is that of functional relationship, or "functionality." Starting originally from simple cases such as a function represented by a power of a variable, this conception has, under the pressure of the needs of expanding mathematical theories, gradually attained the completeness of generality which it possesses at the present time. The opinion appears to be gaining ground that this very general conception of functionality, born on mathematical ground, is destined to supersede the narrower notion of causation, traditional in connection with the natural sciences. As an abstract formulation of the idea of determination in its most general sense, the notion of functionality includes and transcends the more special notion of causation as a one-sided determination of future phenomena by means of present conditions; it can be used to express the fact of the subsumption under a general law of past, present, and future alike, in a sequence of phenomena. From this point of view the remark of Huxley that Mathematics "knows nothing of causation" could only be taken to express the whole truth, if by the term "causation" is understood "efficient causation." The latter notion has, however, in recent times been to an increasing extent regarded as just as irrelevant in the natural sciences as it is in Mathematics; the idea of thorough-going determinancy, in accordance with formal law, being thought to be alone significant in either domain.

The observations I have made in the present address have, in the main, had reference to Mathematics as a living and growing science related to and permeating other great departments of knowledge. The small remaining space at my disposal I propose to devote to a few words about some matters connected with the teaching of the more elementary parts of Mathematics. Of late years a new spirit has come over the mathematical teaching in many of our institutions, due in no small measure to the reforming zeal of our General Treasurer, Prof. John Perry. The changes that have been made followed a recognition of the fact that the abstract mode of treatment of the subject that had been traditional was not only wholly unsuitable as a training for physicists and engineers, but was also to a large extent a failure in relation to general education, because it neglected to bring out clearly the bearing of the subject on the concrete side of things. With the general principle that a much less abstract mode of treatment than was

¹ See the "Revue du Mois," 1908.

formerly customary is desirable for a variety of reasons, I am in complete accord. It is a sound educational principle that instruction should begin with the concrete side, and should only gradually introduce the more general and abstract aspects of the subject; an abstract treatment on a purely logical basis being reserved only for that highest and latest stage which will be reached only by a small minority of students. At the same time I think there are some serious dangers connected with the movement towards making the teaching of Mathematics more practical than formerly, and I do not think that, in making the recent changes in the modes of teaching, these dangers have always been successfully avoided.

Geometry and mechanics are both subjects with two sides: on the one side, the observational, they are physical sciences; on the other side, the abstract and deductive, they are branches of Pure Mathematics. The older traditional treatment of these subjects has been of a mixed character, in which deduction and induction occurred side by side throughout, but far too much stress was laid upon the deductive side, especially in the earlier stages of instruction. It is the proportion of the two elements in the mixture that has been altered by the changed methods of instruction of the newer school of teachers. In the earliest teaching of the subjects they should, I believe, be treated wholly as observational studies. At a later stage a mixed treatment must be employed, observation and deduction going hand in hand, more stress being, however, laid on the observational side than was formerly customary. This mixed treatment leaves much opening for variety of method; its character must depend to a large extent on the age and general mental development of the pupils; it should allow free scope for the individual methods of various teachers as suggested to those teachers by experience. Attempts to fix too rigidly any particular order of treatment of these subjects are much to be deprecated, and, unfortunately, such attempts are now being made. To have escaped from the thralldom of Euclid will avail little if the study of geometry in all the schools is to fall under the domination of some other rigidly prescribed scheme.

There are at the present time some signs of reaction against the recent movement of reform in the teaching of geometry. It is found that the lack of a regular order in the sequence of propositions increases the difficulty of the examiner in appraising the performance of the candidates, and in standardising the results of examinations. That this is true may well be believed, and it was indeed foreseen by many of those who took part in bringing about the dethronement of Euclid as a text-book. From the point of view of the examiner it is without doubt an enormous simplification if all the students have learned the subject in the same order, and have studied the same text-book, but, admitting this fact, ought decisive weight to be allowed to it? I am decidedly of opinion that it ought not. I think the convenience of the examiner, and even precision in the results of examinations, ought unhesitatingly to be sacrificed when they are in conflict—as I believe they are in this case—with the vastly more important interests of education. Of the many evils which our examination system has inflicted upon us, the central one has consisted in forcing our school and university teaching into moulds determined not by the true interests of education, but by the mechanical exigencies of the examination syllabus. The examiner has thus exercised a potent influence in discouraging initiative and individuality of method on the part of the teacher; he has robbed the teacher of that freedom which is essential for any high degree of efficiency. An objection of a different character to the newer modes of teaching geometry has been frequently made of late. It is said that the students are induced to accept and reproduce, as proofs of theorems, arguments which are not really proofs, and thus that the logical training which should be imparted by a study of geometry is vitiated. If this objection really implies a demand for a purely deductive treatment of the subject, I think some of those who raise it hardly realise all that would be involved in the complete satisfaction of their requirement. I have already remarked that Euclid's treatment of the subject is not rigorous as regards logic. Owing to the recent exploration of the foundations of geometry we possess at the present time tolerably satisfactory methods of purely deductive treatment of the subject; in regard to mechanics, notwith-

standing the valuable work of Mach, Herz, and others, this is not yet the case. But, in the schemes of purely deductive geometry, the systems of axioms and postulates are far from being of a very simple character; their real nature, and the necessity for many of them, can only be appreciated at a much later stage in mathematical education than the one of which I am speaking. A purely logical treatment is the highest stage in the training of the mathematician, and is wholly unsuitable—and, indeed, quite impossible—in those stages beyond which the great majority of students never pass. It can then, in the case of all students, except a few advanced ones in the universities, only be a question of degree how far the purely logical factor in the proofs of propositions shall be modified by the introduction of elements derived from observation or spatial intuition. If the freedom of teaching which I have advocated be allowed, it will be open to those teachers who find it advisable in the interests of their students to emphasise the logical side of their teaching to do so; and it is certainly of value in all cases to direct the attention of students to those points in a proof where the intuitional element enters. I draw, then, the conclusion that a mixed treatment of geometry, as of mechanics, must prevail in the future, as it has done in the past, but that the proportion of the observational or intuitional factor to the logical one must vary in accordance with the needs and intellectual attainments of the students, and that a large measure of freedom of judgment in this regard should be left to the teacher.

The great and increasing importance of a knowledge of the differential and integral calculus for students of engineering and other branches of physical science has led to the publication during the last few years of a considerable number of text-books on this subject intended for the use of such students. Some of these text-books are excellent, and their authors, by a skilful insistence on the principles of the subject, have done their utmost to guard against the very real dangers which attend attempts to adapt such a subject to the practical needs of engineers and others. It is quite true that a great mass of detail which has gradually come to form part—often much too large a part—of the material of the student of Mathematics, may with great advantage be ignored by those whose main study is to be engineering science or physics. Yet it cannot be too strongly insisted on that a firm grasp of the principles, as distinct from the mere processes of calculation, is essential if Mathematics is to be a tool really useful to the engineer and the physicist. There is a danger, which experience has shown to be only too real, that such students may learn to regard Mathematics as consisting merely of formulæ and of rules which provide the means of performing the numerical computations necessary for solving certain categories of problems which occur in the practical sciences. Apart from the deplorable effect, on the educational side, of degrading Mathematics to this level, the practical effect of reducing it to a number of rule-of-thumb processes can only be to make those who learn it in so unintelligent a manner incapable of applying mathematical methods to any practical problem in which the data differ even slightly from those in the model problems which they have studied. Only a firm grasp of the principles will give the necessary freedom in handling the methods of Mathematics required for the various practical problems in the solution of which they are essential.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

A MERCHANT VENTURERS' research scholarship of the value of 50*l.*, tenable for one year in the faculty of engineering of the University of Bristol, which is provided and maintained in the Merchant Venturers' Technical College, has been awarded to Mr. Harold Heaton Emsley.

The Child, a new monthly journal devoted to child welfare, will appear in the early autumn, under the general editorship of Dr. T. N. Kelynack. The journal will be suited to the requirements of all engaged in child study or working for the betterment of child life. The publishers will be Messrs. John Bale, Sons and Daniels, Ltd., 83-91 Great Titchfield Street, Oxford Street, London, W.