lakes, except when the movements were unusually rapid or extensive, the argument from geographical distribution fails; for we have no evidence to show whether rock basins are more or less abundant in regions that have been glaciated, than in those that have not; and seeing, further, that differential movements are known to take place, while it has never been proved that a glacier is physically capable of excavating a rock basin, the *onus probandi* rests with the advocates of the glacial theory; and until they have shown that rock basins are less common in regions that have been glaciated than in those that have not, this argument is not logically admissible. Observations on this point are very desirable, but it must be remembered that filled up lake basins are not the only thing to be looked for; what is desired is evidence of the production of rock basins, or of such differential movements as would have led to their formation, had the erosion of the barrier been less rapid. In the Himalayas such rock basins appear to have been formed in quite as great abundance as in the mountains of Europe, and to correspond with them in position and form; but the elevation of the mountains has been so recent, and the rainfall is so great, that the processes of nature are more rapid than in Europe, and the rock basins have consequently not only been filled up, but the barrier has afterwards in many cases been destroyed, and the deposits largely removed by erosion, so that the fact of their having originally been accumulated in a rock basin is not always easily recognisable.

The one new argument of Dr. Wallace's is that derived from the supposed difference between the outlines of existing lakes and those that would result from the submergence of river valleys. In the selected instances, however, he has compared mountain lakes with submerged lowland valleys instead of with mountain valleys. In the latter, long stretches are frequently found where the slopes of the beds of the side streams are much steeper than that of the main valley; these valleys if submerged would give rise to lakes of great length in comparison with their breadth, and without the numerous deep embayments of the shore line which would be usually found in a submerged lowland valley. As a single easily verified instance to show that a submerged mountain valley need not have numerous deep bays, I may instance the Pangong lake in the Himalayas, which will be found on any good map of India, and is nothing more than a submerged subaërially formed river valley; on a smaller scale the Malwa Tal near Naini Tal and the Pil lake in the hills east of Quetta, both of which are river valleys dammed by landslips, have simple outlines without any embayments. The instances I have chosen are from regions where there has not been a great extension of the glaciers, and where the form of the valley before its submergence was entirely produced by subaërial denudation.

R. D. OLDHAM.

On the Change of Superficial Tension of Solid-Liquid Surfaces with Temperature.

IN a recent very interesting communication to the Birmingham Phil. Soc. (*Bir. Phil. Soc. Proc.*, vol. ix. part 1, 1893), upon the effect of a solid in concentrating a substance out of a solution into the superficial film in accordance with Prof. J. J. Thomson's investigation ("Applications of Dynamics to Physics," p. 191), Dr. Gore has quoted an observation of Pouillet's (*Annales de Chemie*, 1822, vol. xx. pp. 141-162), that when inert powders like silica are mixed with liquids that do not act on them heat is evolved. On the other hand, when the superficial area of contact between a liquid and its gas is increased heat is absorbed. This latter is known to be the case because the superficial tension diminishes with rise of temperature. In the case of the solid-liquid surface being produced, it would appear at first sight to follow that the superficial tension should increase with increased temperature. The matter is, however, somewhat more complicated. When a dry solid is mixed with a liquid we are substituting a solid-liquid surface for a solid-air surface, and from the fact that most liquids soak *up* into a mass of dry powder, we may conclude that the superficial potential energy of the solid-liquid is less than that of the solid-air surface, *i.e.* that more work must be done to separate the liquid from the solid than is developed by the air getting at the solid. *If these actions are reversible*, we may apply the laws of thermodynamics, and conclude that as heat is evolved when the system does work, *i.e.* when the solid-liquid surface is increased, that it must require more work to separate the solid from the liquid at high temperatures than at low ones, and in the case of silica and water, for instance, that is very much what one would expect from the action of water at very high temperatures on silica. If we could assume that the superficial tension of air-solid were zero, it would follow from this that the superficial tension of a solid-liquid surface is negative, *i.e.* that there is a superficial pressure, and that the liquid has more attraction for the solid than for its own particles, and that this difference increases with increased temperature, *i.e.* the superficial pressure increases.

The whole subject deserves careful investigation and quantitative treatment, but the difficulty of measuring the superficial tensions of solid-liquid surfaces seems almost insurmountable, so that it would be very difficult to verify the theory. Perhaps something might be done with finely divided liquids that did not mix, and whose superficial tensions might be measured.

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A Lecture Experiment.

WHEN charcoal, which has been allowed to absorb as much sulphuretted hydrogen as it can take up, is introduced into oxygen gas, the charcoal will burst into flame owing to the energy of the action of the oxygen upon the sulphuretted hydrogen.

This fact is stated in most text-books on chemistry, but no description that I have ever seen of this experiment is calculated to bring about the effect with certainty. The following is a simple method for illustrating this reaction upon the lecture table, which I have never found to fail:—

A few grammes (from five to ten) of powdered charcoal are introduced into a bulb which is blown in the middle of a piece of combustion tube about twenty-five centimetres long. A gentle stream of coal gas is then passed over the charcoal, which is heated by means of a bunsen lamp until it is perfectly dry. This point may be ascertained by allowing the issuing gas to impinge upon a small piece of mirror, and when no further deposition of moisture takes place the charcoal may be considered to be dry, and the heating may be stopped. The charcoal is then allowed to cool in the stream of coal gas until its temperature is so far reduced that the bulb can just be grasped by the hand, when the coal gas is replaced by a stream of sulphuretted hydrogen. The sulphuretted hydrogen should be passed over the charcoal for not less than fifteen minutes, by which time the bulb and its contents will be perfectly cold, and the charcoal will have saturated itself with the gas. (In practice it will be found convenient to prepare the experiment to this stage, and allow a very slow stream of sulphuretted hydrogen to continue passing through the apparatus until the experiment is to be performed.) The supply of sulphuretted hydrogen is then cut off, and a stream of oxygen passed through the tube. Almost immediately the charcoal will become hot, and moisture will be deposited upon the glass. The supply of oxygen should be sufficiently brisk to carry the moisture forward from the charcoal, but not so rapid as to prevent it from condensing on the glass tube beyond the bulb. In a few moments the temperature of the charcoal will rise to the ignition point, when it will inflame and continue to burn in the supply of oxygen.

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PIERRE JOSEPH VAN BENEDEN.

THIS distinguished Belgian zoologist was born on December 19, 1809, at Malines, in the province of Antwerp, a town once well known for its extensive manufacture of lace. He received an excellent education, and early showed a decided taste for natural history; his native town being built on the borders of a tidal river, his attention was soon called to the examination of the littoral fauna of Belgium, though it will be remembered that Belgium only evolved itself into a kingdom in the year 1830, when Beneden came of age. His first promotion was to the keepership of the Natural History Collections at Louvain, and in 1835 he was made an assistant professor in the University of Gand, a post which he appears to have held for only one Term, as we find him in the same year professor of the Catholic University at Louvain, which professorship he continued to fill for more than half a century. Van Beneden belonged to a generation of zoologists that connected Cuvier with the present age, and followed so far in this great master's steps, that they worked at almost all the branches of the animal kingdom. If we were to give a summary of the very extensive writings of van Beneden we should begin with his memoirs on apes, seals, whales, and so through the various classes, with perhaps the exception of the birds and reptiles, to the gregarines. Circumstances made him devote a great deal of attention to the groups of parasitic worms and Annelides. Most of his papers on these forms were communicated to the Brussels Academy of Sciences or to the Paris Academy; the latter we find reported on by Quatrefages. He took a leading part in the, at one time, exciting controversy about the "alternation of generations," with the elder Sars, D'Udekem, and others.

Among the more important works of Beneden may be mentioned "The Natural History of the Fresh-water Polyzoa," in collaboration with Du Mortier, published in 1850, which obtained the Grand Prize of the Paris Academy; the "Zoologie Médicale," in 1859, of which Paul Gervais was joint author; the "Recherches sur la Faune Littorale de Belgique" (Polypes), in 1866. In connection with this work it may be mentioned that Beneden's artistic powers were quite remarkable, and that many of his memoirs owe a great deal to his excellent illustrations. A good correspondent, he kept himself acquainted with the work of most of his contemporaries, and he was the writer of many of the short biographical sketches referring to zoologists that appeared from year to year in the Reports of the Brussels Academy. Some of our readers may remember what an active part he took in the Liverpool (1870) meeting of the British Association; Rolleston was president of the biological section, and gave a morning to the discussion of the subject of "commensalism," which at that time Beneden's mind was occupied with, and about which he afterwards (1875) published a volume in the "Bibliothèque Scientifique Internationale," that has been translated into German and English. Peradventure some too may remember how delighted Beneden, with Stricker, Dohrn, and some of the other "foreigners" present at that meeting were, to find that a little nucleus of the great body combined to make the "Association Sunday" as little sad as possible by the practice of a proper commensalism. Full of honour after a long life well and usefully spent, Beneden had the additional reward of seeing his son Edward take a high rank in the modern biological school, in this resembling his great contemporary Henri Milne Edwards. Beneden was a member of very many of the Academies and Societies of Europe, and was an honorary LL.D. of the University of Edinburgh. He died at Louvain on January 8, 1894.

THE GREAT GALE OF NOVEMBER 16–20.

THE past autumn and early winter were especially characterised by a mild and humid atmosphere, due to the very marked prevalence of south-westerly winds which have blown with great persistence from the Atlantic. These conditions are without doubt intimately associated with the frequency with which gales have occurred.

The violent storm which was experienced over the entire area of the United Kingdom, as well as over the sea and the parts of the continent adjacent to our islands, from November 16 to 20, was more severe than the other gales which have recently occurred, and it is necessary to refer back many years before a storm so violent and so destructive can be found to have traversed the country.

Prior to the advent of the storm an anticyclonic area, with fairly high barometer readings, was situated over our islands, and north-easterly winds were prevalent. On November 14 and 15 a small cyclonic disturbance travelled over the south-western portion of the kingdom, and caused a general giving way in the area of high barometer readings, while the large anticyclone over Europe also materially decreased in its energy. At this time a large cyclonic disturbance was out in the Atlantic, and was rapidly approaching our western coasts; the first intimation of a renewal of bad weather was shown by a fresh fall of the barometer which set in at Valencia at 4 p.m. 15th, and an hour or so later the wind was freshening from the south-east.

On November 16 the conditions had so far changed that at eight o'clock in the morning the weather chart prepared by the Meteorological Office gave unmistakable indications of an important disturbance at no great distance from the Irish coast, and the Official Weather Report has the following remark:—"A large depression is approaching our western coasts from the south-westward, and is likely to cause rough wet weather over the kingdom generally, especially in the west and north." At this time a strong south-easterly wind was blowing in the south-west, but the force of the wind had not attained to that of a fresh gale (force eight of Beaufort notation) in any part of the United Kingdom, although the wind, which on the previous day had been north-easterly, was now everywhere southerly. The self-recording barograph at Valencia shows that the lowest barometer occurred at 7 p.m. 16th, and between eight and nine in the evening the wind shifted from east by south to west-south-west. The central area of the storm was not far distant from Valencia at this time, and during the succeeding night it traversed Ireland in a direction from south-west to north-east, the whole storm-system progressing at the rate of of about twenty-five geographical miles an hour. By the morning of November 17 the heart of the storm had reached the west of Scotland, the lowest barometer reading reported to the Meteorological Office being 28·53 ins. at Ardrossan. Strong gales had blown during the preceding night in the north and west, and the force of a gale was still reported at many places on our coasts, while the wind had shifted to the north-westward over Ireland. The weather information for the evening of the 17th shows that the storm had continued its course to the north-eastward, and at six o'clock the centre of the disturbance was not far from Wick, where the barometer was 28·57 ins. The north-westerly gale was still blowing over the western portion of the kingdom, but there was a decided lull in the strength of the wind in the east and south-east of England. It was shortly after this time that the greatest violence of the storm burst suddenly over the northern part of the country, and at Deerness, in the Orkneys, the wind at 6 p.m. shifted suddenly from east by north to north by east.

The subsequent track of the storm had a most important influence on the increased violence of the wind, and there seems no reason to suppose that if the disturbance had continued its north-easterly track the gales experienced would have been at all unusual. A very important change in the distribution of atmospheric pressure was in progress over Western Europe, and the change of track and subsequent violence is clearly to be traced to these barometer changes. The anticyclone over Central Russia, which had given way for the small disturbance which first traversed the southern portion of England on the 14th and 15th, was now reasserting itself, and this formed a most effectual barrier to the further north-easterly progress of the storm. In addition to