In Definition I, density is taken as the primary property of matter, although left undefined by Newton; while *Quantitas Materiae*, our W or P, is the product of density and volume.

The *Materiae Vis Insita* of Definition III is described as the same as *Inertia Massae*. This, however, is not the definition of *Massa*, but *Inertia*, although the two are treated as the same thing in modern interpretation.

Newton is not consistent with himself, as asserted, in always using *Pondus* as meaning the attraction of the earth on a body on the surface. As often as not he uses *Pondus* in the popular acceptation, as in the Act of Parliament, and a search in the "Principia" will reveal numerous instances.

This distinction, insisted on so carefully in modern instruction, was ignored in language and thought till about fifty or sixty years ago, when Absolute Measure was first introduced into dynamical teaching.

The artless definition of Mass, as the quantity of matter in the body, is near enough to serve in a dictionary, as a synonym in one line. It is merely the selection of a new name as a label in the long list already in Def. I. But a real definition will give at the same time the best way to measure the quantity. In a recent Royal Society memoir, on "mass determination" as the author is careful to call it, the question of its measurement turned on a "Study of the Balance, in its greatest precision, in a projective series of weighings of small masses," the most accurate of all physical operations we know.

Libra, sign in the Zodiac of the Balance, is an appropriate emblem of justice holding the scales.

It is contrary to the strict legal language of the Act of Parliament on weights and measures to start off with another artless definition—the weight of a body is the force with which it is attracted by the earth. At that rate, what is the weight of the moon, the sun? The definition is not supposed to apply to a body, so long as it is not terrestrial.

The attraction of the earth on the pound weight as the unit of force (gravitation) will never be abandoned by the engineer, as it is susceptible to the same degree of accuracy of measurement as the operation of weighing.

But when Tait took in hand the reform of dynamical teaching, he altered the equations in our form of (1), (2), (3), (4) in a new way, with the view of exterminating g. He discarded the old *sui generis* mass, with unit of g lb., and taking mass in its new meaning of the invariable quantity of matter in the body, he measured it in terms of the Act of Parliament unit of weight, the pound weight. This involved him in a change in the unit of force, to what was called a poundal, such that the engineer's gravitation unit of force, the pound (force), was equivalent to g poundals.

Tait's change merely amounted to labelling M the quantity formerly labelled W. But he insisted on retaining $W=Mg$, and so measuring what he called weight in poundals, contrary to the strict law of the Act, and rendering himself liable to a fine for every offence. Better if Tait had retained the letter W for lb., writing the equation $P=Wf$, and rejecting the useless $W=Mg$, as perpetuating the old *sui generis*, and breaking the law in the Act of Parliament.

This trouble of mere terminology would be exorcised if the habit was inculcated of always stating the unit of a dynamical quantity, as, for example, of a mass M, g, a weight W, lb. The engineer refuses to accept the poundal or to give a weight W in poundals. Scrap the name as useless, except for passing certain examinations.

To the masses in general the word mass implies a combination of bulk and density as in Definition I, as when we speak of mass of stuff, the mass of the earth—"Die Erde und ihre eigene ungeheure Last" (Mach). In ordinary language the mass will mean the multitude, or majority, as in the statement attributed to Herbert Spencer, "The mass of woman is insensible to gravity," which might mean a reminiscence of the ballroom floor; but this was before the women began to take themselves so seriously; and when we read the critic's snarl of the "Vast Mass of his writings consigned to Oblivion," Vast Mass here is forcible-feeble for *Major Pars*.

The word is spelt *Maas* in German; "Mass für Mass" is the title of the German version of Shakespeare's play "Measure for Measure."

Wegener's Hypothesis of Continental Drift.[1]

By PHILIP LAKE.

WEGENER'S hypothesis is based on the idea that the continental masses are patches of lighter rock floating and moving in a layer of denser rock, and this denser rock forms the floor of the oceans. Following, with a slight alteration, the terminology of Suess he calls the lighter material the Sial and the denser layer the Sima. Suess uses the words Sal and Sima, and thinks that the Sal covers the globe completely.

I shall not here discuss the possibility of Wegener's conception. He does not profess to explain completely why the continents should move, but he claims to have proved conclusively that such movement has taken place. It is the evidence on which he relies, and more particularly the geological evidence, that I propose to examine.

[1] Abridged from an address to the Royal Geographical Society on January 22.

One of the arguments on which he lays great stress is derived from the relative frequency of different heights and depths upon the earth. His diagram of frequencies shows two well-marked maxima, one at about 100 metres above sea-level and the other about 4700 metres below it. Wegener concludes that two distinct surfaces standing at these two altitudes must have been involved in the subsequent movements. He assumes that these surfaces were originally level—or, more strictly, equipotential—and that they were the surfaces of the Sal and the Sima respectively. He holds that if originally there were only one such level, the deformation of that level could not produce two maxima and "the frequency must be regulated according to Gauss's law of errors."

In reality, if it is only a single level that has been deformed, it is improbable that the resulting altitudes will conform with the normal law of errors. The crust of the earth is not so constituted that each point can move independently of the rest, and the movements therefore are not analogous to the errors in a series of independent observations. According to the geological evidence the greater movements, which have most influence on the frequencies, are of a widespread character, and their general effect is to throw the surface into broad undulations. Upon these broader movements are superimposed the more intense but more local mountain-building movements.

Mr. G. V. Douglas points out in a paper to appear shortly in the *Geological Magazine* that if we start with a level, or equipotential, surface, and suppose it affected by movements of the types referred to, the resulting altitudes will necessarily give a frequency curve showing two maxima. The actual frequency curve is, in fact, perfectly consistent with ordinary geological conceptions and does not require the original existence of the two distinct surfaces postulated by Wegener.

Wegener imagines that at the close of the Carboniferous period the Sal formed one continuous patch covering about half the globe, and the Sima covered the rest. He professes that he has taken the forms of the existing land-masses, including their continental shelves: he has modified the present forms by unfolding the mountain ranges which have been raised since the Carboniferous period; and he finds that the different patches can then be fitted together into one continuous whole, like the pieces of a puzzle. It is evident, however, that Wegener has given free play to his imagination. In following the edge of the continental shelf he has allowed himself a very considerable amount of latitude, and he has not hesitated to distort the shapes of the masses. Few geologists who are familiar with mountain structures will attach much value to Wegener's estimates of the effect of Post-Carboniferous folding.

It is easy to fit the pieces of a puzzle together if you alter their shapes, but your success is no proof that you have placed them in their original positions. It is not even a proof that the pieces belong to the same puzzle. If Wegener's hypothesis rested solely on the evidence of fitting that he brings forward it might well be ignored. But there is more to be said for it than this.

In the Indian Peninsula the oldest fossiliferous deposits are of terrestrial origin and contain remains of plants and of reptiles. The flora is commonly called the Glossopteris flora and is very distinct from the contemporaneous flora of north-western Europe. There is a similar series of terrestrial deposits in South Africa and another in Brazil, both of which contain the Glossopteris flora and remains of reptiles. The Glossopteris flora occurs, moreover, in Australia, the Falkland Islands, the Antarctic continent, and in other parts of South America besides Brazil. In Wegener's reconstruction all these areas are brought together, and it is easy to understand why they should have a common flora and why that flora should be different from the flora of the distant Europe.

But the Glossopteris flora is found also in Kashmir, north-western Afghanistan and north-eastern Persia, Tonquin, northern Russia and Siberia. In Wegener's reconstruction all these areas lie far from the masses that he has grouped together in the south.

The Russian deposits are especially interesting. Not only do they contain representatives of the Glossopteris flora, but they also include reptiles of the same type as those which are found in South Africa, and several species of freshwater shells which are identical with those in the South African beds. Wegener's explanation has not by any means simplified the problem of the distribution of the Glossopteris flora and of the fauna associated with it.

In India, South Africa, South America, and Australia the system containing the Glossopteris flora begins with a boulder bed, which is universally admitted to be of glacial origin. These glacial deposits are now scattered over a wide extent of the earth's surface. Even if we admit movement of the pole, on the most favourable supposition the ice must have spread much farther towards the equator than the ice-sheets of the Pleistocene Glacial period ever did. Nor is it possible to invoke the aid of icebergs, for the associated deposits, except in the case of Australia, are all of terrestrial origin. With Wegener's reconstruction these difficulties disappear. The areas are grouped together and the pole may be placed conveniently in the middle of the mass.

But the boulder beds of this period are not limited to these areas. There is a boulder bed in the Salt Range which appears to be of the same age as the Talchir boulder bed of the Indian Peninsula. In north-western Afghanistan Griesbach found a boulder bed similar to the Talchir boulder bed, and in the beds overlying it he found several of the characteristic plants of the Glossopteris flora. According to Wegener's maps this boulder bed must have been deposited within 30 degrees of the equator of the period; and it cannot have been laid down at a great elevation, for the beds that conformably follow it include both marine and terrestrial deposits. Wegener's ideas have not very greatly reduced the area that must have been affected by the ice of the Permo-Carboniferous Glacial period.

There is another line of evidence that Wegener puts forward. There are five geological features, according to him, which occur on the two sides of the Atlantic and are re-united when the patches of Sal are fitted together.

The strike of the ancient gneiss of the Hebrides and northern Scotland becomes, he says, continuous with that of the gneiss of Labrador. The former, according to him, now runs from north-east to south-west, the latter from east to west. But according to the Geological Survey of Scotland the prevalent direction in Scotland is W.N.W.–E.S.E. or east to west. If Wegener's direction fits the other side the real direction does not.

The Caledonian folds of Scotland and Ireland, he says, become continuous with those of Newfoundland. But the Newfoundland folds are of considerably later date. If there was actual contact the earlier Scottish folding, in spite of its great intensity, must have ended abruptly at the line where separation was to take place ages afterwards, and on the other side of the line the commencement of the later Newfoundland folds must have been equally abrupt.

Farther south the Armorican folds of Europe, in Wegener's reconstruction, are continued by the Appalachian folds of North America, and no objection can be raised on the score of age. But a single coincidence of this sort has no value, for Wegener has adopted the simple plan of bending North America so that the ends of the two systems meet and the folds fall into line.

In Africa, according to Wegener, the ancient gneiss foundation shows a sudden change of strike at the head of the Gulf of Guinea, and in South America there is a similar sudden change at Cape St. Roque. When the two continents are brought together the two different strikes and the line of separation between them become continuous. But in bringing about this coincidence he gives to the gneiss north of the Gulf of Guinea a north-east to south-west strike, and this is very far from the truth. Over a large part of the area the actual observations indicate that the prevalent direction is from north to south.

In South Africa a folded mountain range runs from east to west. In Buenos Ayres a folded range belonging to the same period has been described. According to Wegener one was the direct continuation of the other. But before they reach the western coast the South African folds, and the range that they have formed, turn to the north and run roughly parallel to the western coast. Wegener's explanation of this deviation is far from convincing.

It will thus be clear that the geological features of the two sides of the Atlantic do not unite in the way that Wegener imagines, and if the continental masses ever were continuous they were not fitted as Wegener has fitted them.

Obituary.

PROF. GEORGE LUNGE.

ON January 3 Prof. Lunge died in his eighty-fourth year. For more than thirty years, from 1876 to 1907, he held the professorship of applied chemistry in the Polytechnic Institute of Zürich, directing the destinies of this department with characteristic energy, and with a success that attracted students from far and near, who sought to equip themselves for a career in industrial chemistry by a training under one who was recognised as the authority, especially in the branch of the manufacture of "heavy chemicals."

Dr. Lunge by his literary activity, as in other ways, contributed greatly to the advancement of chemical technology. His treatise on "Sulphuric Acid and Alkali," which has passed through several editions, is not only indispensable to the technologist, but is also replete with knowledge. As Mr. T. W. Stuart, himself a leader in the alkali industry in this country, and one of the few early contemporaries of Dr. Lunge, recently stated, "When you refer to these books on any obscure subject in the Alkali industry, you never go empty away, but always find in them a wealth of information."[1] A similar statement might justly be made in respect to Lunge's "Coal Tar and Ammonia," his "Technical Chemists' Handbook," and his "Handbook of Methods of Technical Gas Analysis," etc., each and all of which are essential to the equipment of the chemical technologist.

George Lunge was born at Breslau on September 15, 1839; from 1856 to 1859 he studied at the universities of Breslau and Heidelberg, graduating as Ph.D. In 1864 he came to England, with the object of obtaining technical experience. For a part of the twelve years spent in this country he was employed in the tar distillery of Messrs. Major and Co. at Wolverhampton, and in 1868 he was appointed chemist and manager to the Tyneside Alkali Company at South Shields. Dr. Lunge's efforts to obtain a footing in one or other of the twenty-six chemical works on the Tyne were at first far from encouraging, for, as Mr. Stuart tells us, a partner in one of the largest of these works offered Dr. Lunge the post of chemist at 1*l*. per week, which even at that time was but 2*s*. above the wage of a labourer! In the small works at South Shields Dr. Lunge continued until 1876, when he received the call to the chair of applied chemistry at Zürich. It is not without interest to note that his chief publications and researches deal with those phases of chemical industry, with the actual practice of which his sojourn in England had made him familiar.

[1] *Chemical Trade Journal and Chemical Engineer*, January 19.

At the time of his residence on Tyneside the Newcastle Chemical Society was founded, with Mr. Isaac Lowthian Bell (later Sir Lowthian Bell, Bart.) as its first president. Dr. Lunge became a member of this society, taking an active part in its proceedings and was elected president in 1872. In 1883 this society became merged into the Society of Chemical Industry and was formed into a local section of that society. However, Dr. Lunge, until the time of his death, retained his membership of the local section, using its Proceedings as the medium of publication from time to time of important scientific communications, and in many other ways evincing his sustained interest in its welfare.

The first Hurter Memorial Lecture was delivered in 1899 by Dr. Lunge before the Liverpool section of the Society of Chemical Industry, who selected for the subject of the lecture—"Impending changes in the general development of industry, and particularly the Alkali industry."

Drs. Hurter and Lunge, like many German chemists, *e.g.* Caro, Pauly, Otto Witt and others, came to England in the sixties of last century to gain a practical knowledge of British chemical industries. Dr. Hurter remained in this country and became identified with the Lancashire alkali industry, while Dr. Lunge returned to the continent, and based his teachings and writings on experience gained in the rival industry of the Tyne. Dr. Lunge had a complete command of the English language, writing and speaking it with ease and fluency. He married Miss Bowron, the daughter of a member of the firm of the owners of the Tyneside Alkali works at South Shields. P. P. B.

PROF. JAMES RITCHIE.

WE much regret to record the death of Prof. James Ritchie, Irvine professor of bacteriology in the University of Edinburgh. Up to the end of the summer term of 1922 Prof. Ritchie carried on his work with his customary energy and zest. In the holiday which he took during August in Perthshire, however, the early symptoms of his last illness began to give anxiety, and he died on January 28.

The record of Ritchie's life shows that since he