compound like NaCl, which is decomposed into two ions Na^+ and Cl^-, will thus produce double the normal effect on the osmotic pressure and its consequences the diminution of vapour pressure and the lowering of the freezing point. In the same way, a molecule like H_2SO_4, which gives three ions, will produce three times the effect which would be obtained if it were undissociated.

Thus Prof. Poynting's conditions would be satisfied, and at the same time the advantages of the dissociation theory would be retained. W. C. D. WHETHAM.

Trinity College, Cambridge, October 12.

Responsibility in Science.

As one who supposes himself a physicist, I wish to protest against some of Prof. Poulton's remarks in his recent British Association address, as given in NATURE, September 24.

From the statements on p. 502, one would suppose that physicists as a body had long been tyrannising over geologists and zoologists, and that this reign of terror had remained unbroken until recently, save for some slight diversions afforded by mathematicians.

When it comes to details, the physicists seem to resolve themselves into two individuals, Lord Kelvin and Prof. Tait, and perhaps a third, von Helmholtz; all of whom, by the way, have an equally good claim to the title mathematician. Prof. Poulton apparently regards all physicists as committed to every theory propounded by every individual physicist. This would certainly be unlimited liability with a vengeance.

Personally I do not hold myself committed to the truth of any theory, past, present, or future, until such time as I have explicitly signified my assent to it. If one were explicitly to signify one's dissent from every physical theory, or every statement of physical facts, which one is not prepared to accept, there might be little time left to do anything else. Perhaps I can bring this home most clearly to Prof. Poulton by expressing my views as to one or two of his own statements.

On page 502 he says, "the earth, even when solid, will alter its form when exposed *for a long time* to the action of *great* forces" (italics mine). Here, and in the rest of the passage, is a strong flavour of the erroneous view that a solid is *rigid* in the mathematical sense, except when viscous under great and prolonged stress. It is surely time that scientific men in all departments grasped the conception of elastic strain and displacement.

On the same page are other imperfections in the statement of the arguments against deducing the time of consolidation of the earth from its present form. Prof. Poulton apparently considers it proved that the earth's angular velocity of rotation is diminishing, and that the only agent to be considered is the action of the tides. If, however, the earth's temperature is diminishing, and the material contracts in cooling—conclusions most generally accepted—the consequent diminution in the moment of inertia tends to *shorten* the period of rotation. Such shortening was in fact made the basis of his speculations by the eminent French mathematician Prof. E. Roche (Académie . . . de Montpellier, *Mémoires de la Section des Sciences*, vol. x., 1880–84, p. 232).

On page 503, we are told "there is some evidence which indicates that the interior of the earth in all probability conducts better than the surface. Its far higher density is consistent with the belief that it is rich in metals, free or combined. Prof. Schuster concludes that the internal electric conductivity must be considerably greater than the external."

When one considers the enormous pressures which existing theories point to in the earth's interior, and remembers how conspicuously less the accepted mean density is than that of the lightest of the heavy metals under atmospheric pressure, one can only recognise the inconclusiveness of the evidence from this source.

The reference to Prof. Schuster's conclusion is ambiguous. Does Prof. Poulton believe electrical and thermal conductivity necessarily to vary together? If so, then the fact that electrical conductivity diminishes in metals and ordinary alloys as the temperature rises, is one he ought to consider. In any case he might be well advised to allow for the possibility that Lord Kelvin's speculations do not possess a monopoly of physical uncertainties.

The direct experiments by Lord Kelvin (NATURE, June 1895, p. 182) on the influence of temperature on thermal conductivity are very probably, in Prof. Poulton's opinion, not sufficiently varied, as regards either material or range of temperature, to form a substantial basis for wide conclusions. Still I should have expected him to refer to them, if only to mark his recognition of an attempt on Lord Kelvin's part to meet his critics with something better than surmises.

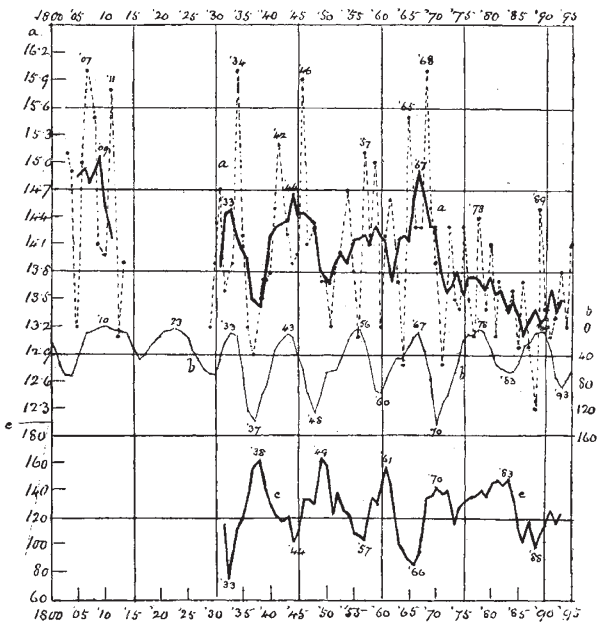
Our uncertainty as to the true value of the mean temperature gradient near the earth's surface might fairly, I think, have been adduced by Prof. Poulton. Observations have, in fact, been limited to comparatively small areas of the surface, and the results obtained have varied much. There are also sources of error whose elimination is difficult. Irregularities in the surface, the presence of the recording apparatus, and the disturbance caused by previous excavations, tend to alter the temperature it is intended to measure; while the conditions may prejudice the correct working of the apparatus. An instructive example of this last defect came under my notice recently. Very fairly accordant readings with two maximum thermometers in a deep boring full of water indicated an excess of some 30° F. in the bottom over the surface temperature. Direct experiment in a hydraulic press proved, however, that fully half the rise was fictitious, being simply due to the contraction of the bulbs under the pressure to which they were exposed. C. CHREE.

September 28.

The Climate of Bremen in Relation to Sun-spots.

MAY I invite attention to the variation of mean temperature of the summer half (April to September) at Bremen, which seems to me to suggest sun-spot influence? The observations used are those given by Dr. Bergholz in his "Ergebnisse." They extend (with a break from 1814 to 1823) from the beginning of the century.

The dotted curve in the diagram (*a*) shows this variation. After smoothing with averages of 5, we have the continuous



a. Dotted curve, mean temperature April to September, Bremen. Continuous curve, smoothed with averages of 5. *b*. Sun-spot curve (inverted). *c*. Smoothed curve of rainfall, August and September, Bremen.

curve traversing the other. Below, (*b*), is the sun-spot curve, *inverted*, and a general agreement will be made out with the aid of the figures given; the wave-crests of the smoothed curve corresponding, more or less, with the minima of sun-spots. This result is in harmony with those of Köppen and others.

We may note the salient years (half-years) 1811, 1834, 1842 (1846), 1857, 1868, 1878, 1889; all near minima.

Representing the half-years as + or −, according as they are above or below the average, and selecting the sun-spot maximum and minimum years, I find five out of seven of those seasons in the former case (maximum) to be *below* average, and four out