least disturbing it. From what is already known, we can predicate that a ball of iron entering the atmosphere with a velocity of six or seven miles a second would instantly be melted, burnt, and converted into a red powder, and that before reaching the earth it would probably be scattered by the aerial currents into comparatively so vast an area as never to be afterwards noticed. If we suppose the mechanical force produced by the condensation of the nebulous mass from which a planet is forming to be slower than the equivalent of radiation from the same, it would seem as if there could be no great internal heat; but it is to be remembered that the vertical law of conduction requires an increase of temperature downwards, so that if a planetary mass were exposed perfectly cold to the sun's rays, it must continue to absorb heat until that vertical equilibrium of temperature had been attained:—the centripetal energy enabling it to imbibe a quantity of heat vastly greater than the surface temperature would seem to indicate. In respect to extra-terrestial bodies such subterranean heat is latent. With regard to the sun, on the other hand, the mechanical force generated centripetally must originally have far exceeded the equivalent of radiation. If its present condition is stationary in respect to temperature, its mass must be increasing. If its mass is not increasing, its temperature must be diminishing, the annual loss being represented by about 1·54 millionth of its mass lowered 1,800 million of degrees, per annum, supposing it to have the specified heat of iron: supposing, also that it does not contract or become further condensed, because this would of itself engender *vis viva*. It may be shown that so small an increase of density as would diminish the sun's diameter 860 feet represents the equivalent of the annual radiation. In the bodies that surround us, we remark that cooling and contraction are generally simultaneous. If such is the case in the sun, 33 degrees must be too high an estimate of the yearly loss of temperature. The ratio between the diminution of bulk and of temperature, were it known in the case of the sun, would enable us to compare their mechanical equivalents. The *vis viva* produced by the diminution of bulk would be classed with the phenomena of what is called latent heat in liquids, solids and gases. It would seem from these computations, which rest upon M. Pouillet's data, that the probable annual loss of temperature in the sun is by no means inconsiderable in absolute amount, but its relative value in respect to the temperature of the sun may be, and probably is quite insignificant. Is there any way of arriving at an estimate of the temperature of the sun's radiating surface? Let us consider what meaning is to be given to the expression "temperature of space," occasionally to be met with in the writings of physicists. If heat is the motion of the elementary parts of bodies, and not a subtle species of matter, as certain phenomena of latent heat seem to have suggested the idea, it is hardly correct to speak of vacant space as having a temperature, although the heat force may in various directions and with various intensities be radiating through it. In the same way, space is not considered as luminous, although traversed by most intense light. A thermometer placed in a perfect vacuum although it shows the same temperatnre as the substance that incloses the vacuum, actually exhibits the effect of the intensity of the heat radiations that are passing through it. If we suppose a thermometer situated at the opposite point of the earth's orbit, and subject to the influence of the sun's rays only, it would no doubt rise until the radiation from its surface amounted to what was radiated into its surface; but the temperature indicated by it cannot be accepted either as constant, for it depends on the specific radiating and absorbing qualities of the thermometer; or as affording the means of deducing the sun's temperature, for we are ignorant of the relation between temperature and the rate of emission, also of the absolute value of any given temperature unless we deduce it from the dynamic theory of gases which represents the zero of gaseous tension (—461° Fahr.) as the absolute zero of heat. If the thermometer thus isolated, is supposed to be surrounded, on all sides but the one exposed to the sun, by matter that is kept artificially heated up, to within a few degrees of the temperature shown by the thermometer, it is impossible that it could receive an accession of heat from any other source but the sun; and it seems obvious that when at last it became stationary, the temperature is one that must be independent of any specific quality of the thermometer or its artificially heated envelope, but dependent entirely on the distance and temperature of the sun. Some years ago I made an attempt to imitate the conditions of this hypothetical experiment by inclosing a thermometer within three concentric boxes well protected from external influences, and capable of being equally heated all round to any temperature below 400° Fahr. by means of flues ascending from an Argand lamp. The rays of the sun when near the meridian, (within the Tropics) were admitted to fall when required on the bulb of a thermometer through a triple glass partition. Before applying the lamp, the temperature of the interior of the box being t, a rise of about 50° took place by exposing the bulb to the sun; when the thermometer had become stationary at $t + 50°$ the sun's rays were excluded and the lamp applied to heat the box to $t +$ 50°. When the temperature was again stationary at this point, the sun was re-admitted upon the thermometer, which again rose 50° or until the temperature was $t + 100°$. The same operations were repeated up to 250°, but without any diminution of the step 50° which seemed to be made with the same alacrity at the higher as at the lower temperature. I had hoped to have detected some very obvious difference, and from its amount to infer the value of the limiting temperature that expressed the sun's power at the earth's distance. I should then have added 46° to this temperature to obtain its absolute value, then increase this in the inverse ratio of the square of the distance from the sun's centre, obtain an approximate value of the sun's temperature. It seemed to me at the time that this experiment, though not made with sufficient means, or perhaps, care to insure much accuracy, proved that the intrinsic force of the sun's rays of heat was much greater than might be inferred from the temperature of the atmosphere. I purpose at a future opportunity to consider the Dynamical Sequence of Latent Heat and Molecular Force.

Mr. Hopkins addressed the Section, pointing out the important hints and valuable lines of inquiry which the paper suggested; but also showing with what caution it was to be received in many parts as statements of determined scientific truth.

Section B.—CHEMICAL SCIENCE.

'*On the Chemical Action of the Solar Radiations*,' *by* Mr. R. Hunt.—This was a report to the section of the continuation of an examination of the chemical action of the prismatic spectrum, after it had been subjected to the absorptive influences of different coloured media. The mode of examination has been to obtain well defined spectra of a beam of light passing through a fine vertical slit in a steel plate by prisms of flint and crown glass and of quartz. The spectrum, being concentrated by a lens, was received upon a white tablet and submitted to careful admeasurement; the coloured screen (sometimes coloured glass and sometimes coloured fluid) was then interposed, and the alterations in the chromatic image were carefully noted; the chemical preparation was then placed upon the tablet, and the chemical impression obtained. The relation which this image bore to the luminous image was a true representation of the connexion between the colour of a ray, and its power to produce chemical change. In the report made to the Belfast meeting of the British Association, the results of experiments made upon glass plates prepared by the so-called collodion process were alone given. In the present report the examination has been extended to the photographic preparation known as the calotype, and to iodide and bromide of silver in their pure state and when excited by gallic acid. M. Edmond Becquerel, in a paper communicated to the Academy of Sciences, of which an abstract appears in the *Comptes Rendus*, tom xvii. p. 883, states "that when any part of the luminous spectrum is absorbed or destroyed by any substance whatever, the part of the chemical rays of the same refrangibility is also destroyed." The author's experiments, as recorded in the former report and those now detailed, prove that his conclusion has been formed too hastily. Although there are many absorptive media which, at the same time as they obliterate a particular coloured ray, destroy the chemical action of that portion of the spectrum, yet there are still more extensive series which prevent the passage of a ray of given refrangibility, and do not, at the same time, obstruct those rays which are chemically active of the same degree of refrangibility. This is particularly exemplified in the case of glass turned yellow by different preparations. With some of these the blue rays are obliterated, the chemical action of this part of the spectrum not being interrupted, whereas in some other examples, those rays permeate the glass, but are almost entirely deprived of chemical power. A still more curious fact is noticed in this report, for the first time, of some media which have the power, as it were, of developing chemical action in a particular part of the spectrum where the rays did not appear previously to possess this power. Several glasses exhibited this phenomenon to a certain extent, particularly such as were stained yellow by the oxide of silver; but one glass showed this in a remarkable manner. This glass was yellow when viewed by transmitted light, but it reflected pale blue light from one of its surfaces; it obliterated the more refrangible rays down to the green, and rendered the yellow rays far less luminous than usual. In nearly every case the yellow rays are found to be not merely inactive, chemically, but to actively prevent chemical action. After the spectrum has been submitted to the action of this glass, *all chemical power is confined to this yellow ray*. The author has hitherto supported the view that photographic phenomena and the illuminating power of the sunbeam were distinct principles, united only in their modes of motion. He was led to this from observing that where there was the most light there was the least power of producing chemical change; and that as illuminating power diminished, the chemical phenomena