The Next Task of Astronomy.¹

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A VERY large amount of work is in progress at present upon astronomical photometry. The older branches of astronomy, astrodynamics and measures of position, are far from being exhausted, and it is a matter for great regret that the former, especially, with its splendid traditions of great workers, of discovery and invention, should have fallen into relative neglect. The middle branch, astrophysics, has so far been based, almost exclusively, upon measures of position, namely, precise determination of spectral lines. Beyond that it has been little more than a descriptive branch—an avenue to science rather than a part of it. But lately from every direction it is being forced to address itself in earnest to the problems of photometry—that is, to face directly and with as much science as it can command, the question of measuring quantity of light-emission.

One is apt to forget that the estimation of stellar magnitudes is coeval with our earliest measures of position. It is found in Ptolemy's "Almagest," accompanied even by a few notes on colour. The six magnitudes into which we divide the naked-eye stars are a legacy from his sexagesimal arithmetic. The subsequent development of the two is in curious contrast. The edifice of positional astronomy is the most extensive and the best understood in all science, while light measurement is only beginning to emerge from a collection of meaningless schedules.

Measures of positional astronomy were originally nearly as meaningless as magnitudes. Successively the position of the observer, the phase of the earth's rotation, precession, the atmosphere, stellar aberration, nutation, the solar motion, stellar parallax, and finally relativity-shifts, required to be discovered, measured and eliminated, before we got down to clean results that referred to the stars and nothing else. To these may be added personality and the theory of instruments, or the allowances that are necessary before measures made by different persons in different circumstances can be combined. All this has been done with a success that is very nearly complete and permits one to remark, first on the extreme complexity and variety of unknown troubles by which simple geometric measures proved to be actually embarrassed, and secondly, on the enormous collateral advantages that accrued to knowledge in general by their solution, including unforeseen conquests of theory, necessity for invention of new methods, new instruments and new outlooks.

Now the place of a star is certainly the datum that matters least about it—except perhaps in statistical grouping. Its mass, its size, its surface temperature, the state of its atmosphere, its radiative emission—if we can get them—are all intrinsically a great deal more significant. All these are absolutely basic data or *quæsitæ* of physics. They relate to circumstances of Nature which we cannot imitate in any experiment of our own, owing to the quantities and the temperatures involved, and the isolation. These are Nature's experiments, in her own laboratory, and their variety is for our purposes endless and may be called complete.

Our only access to them is by parcels of starlight, and our only treatment for these parcels, after a certain amount of general description, is to measure them, and to cast out from our measures what is irrelevant, personal to ourselves, our situation or our instruments. Difficult and laborious as the prospect may seem, it will falsify all experience if our labours do not yield us unexpected stores of gain, new and unforeseen relations, besides what we set out to master.

I would repeat that the measurement of radiation is a physical question of absolutely basic importance. Every step matters on the road to its elucidation. This is the question I refer to as "The Next Task of Astronomy," because a large part of it, and necessarily a critical part of it, falls in the astronomer's field. In spite of all the work that has been done upon it up to now, it is still, in the main, in a crude stage, much behind where we might, with proper effort, place it, and out of all comparison behind the position of measures of place. Therefore it offers a field which both astronomer and physicist can be sure they will not cultivate in vain.

Very few years ago there was but a single star for which a photometry that was physically significant was attempted. This was the sun. Here all the necessary allowances that were known were made, the first being a very elaborate and difficult determination of the effect of the earth's atmosphere, and the elimination of it. Then there was the measure of the sun's own radiation. This might be done in bulk, or separately for each wave-length. In either case the true measure is taken as a heat measure, an energy measure. As to that, there is more to say than used to appear. Questions of fundamental interest suggest themselves, and will certainly call for investigation hereafter. For the sun, the standard detecting instrument at present is the bolometer, which in principle is a Wheatstone's bridge, balanced upon a pair of exceedingly fine short strips of platinum, one of which receives the selected radiation and responds to it by an increase of resistance. Increase of telescopic power has made it possible to extend this method to certain stars, employing as detector either Nichol's radiometer or Coblentz's vacuum thermocouple. C. G. Abbot, with the 100-inch telescope at Mount Wilson, has been able to record the actual distribution of heat-radiation of nine stars, over the whole visible spectrum and far into the infra-red. Coblentz has obtained results nearly equal to these. Deflexions corresponding to integrated radiation for the whole spectrum are readily obtained by the same means for far fainter stars.

Interesting though this is, personally I regard it as a *tour de force*; like the interferometer measures of stellar diameters, it does not hold out any promise of general use, and gives measures which serve only to confirm results that are more conveniently and as certainly obtained otherwise.

Besides the interest attaching to a new feat, however, such measures are true physical photometry, as absolute as we can make them. The same cannot be said of our chief standard, the north polar sequence. Perhaps nothing speaks more clearly of the detached,

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inconclusive and essentially unscientific position of stellar photometry than the labour spent upon establishing and verifying this work, without reference to its subsequent physical interpretation. If my words are depreciatory, this does not mean that I undervalue those who worked at it, or what they have given us; it is to remind workers of to-day that we have got into a different era, in which astronomy has definitely and finally joined hands with physics, each serving and responsible to the other, for mutual profit.

The north polar sequence was devised by E. C. Pickering at Harvard, as his ultimate standard of photometric reference. It consists of a carefully graded series of stars within 4° of the north pole.

The accepted scale for magnitude is Pogson's, which he derived from J. Herschel, defining increment of magnitude as proportional to increment of log (intensity), so that an increment of magnitude, dm , varies as $-dI/I$, where I is the intensity. This is in accordance with Fechner's law, by which a physical intensity is translated into a sense-measure; its significance is the escape from introducing an arbitrary external unit to measure dI —the instantaneous value I serving for that purpose. To establish a standard sequence in accordance with this definition, Pickering deliberately employed a variety of instruments and methods, relying upon their mean to cancel the comparatively large discrepancies. Such a plan may have been the best to use at the time it was devised, but it is obvious that it may leave a residue of systematic error. It was ascertained to do so by F. H. Seares of Mount Wilson, and this was confirmed by H. Spencer Jones at Greenwich. But these are no more than the incidents in fixing a critical standard, and one can scarcely suppose equal results could have been better or more readily obtained.

What I criticise in the investigation of 'magnitudes,' of which the standard sequence is merely the best, and one of the least extensive, examples, is that the measure is a measure of a mixed phenomenon, which has never been analysed to its physical basis, and which it is usually impossible to complete afterwards. There are always and obviously three elements that contribute to the measure—the actual radiative emission of the source, losses in transit by dispersion, absorption and reflection in the instrument and the atmosphere, and finally the peculiar sensitivity characteristic of the receiving apparatus. One form of the latter is the eye, supposed reduced to a standard, and equivalent to a photovisually corrected lens used in conjunction with an isochromatic photographic plate and a rather ill-defined yellow filter. The result can be made sensibly the same and sensibly uniform.

The position is decidedly more unsatisfactory when we deal in photographic magnitudes. Apart from such investigations as I advocate below, which have not yet received a recognised place in astronomy, the basis of the adopted treatment is frankly empiric, and empiric in a way that holds out no promise of a better foundation. After numerous experiments a formula has been settled on, $m = a + b\sqrt{d}$, where d is the measured diameter of the photographed image, and a, b turn out to be sensibly constant over considerable ranges. It is a very convenient rule, and it has turned

out a great deal of very useful work, but in a physical sense it is hopeless.

Now if we accept the heat radiation as standard, it is of course familiar that the estimate of intensity made by the eye, whether analysed or summed, is distorted. Abbot, for example, extended his bolometric measures beyond wave-length 2μ , to which the eye is perfectly blind. We require an equation of conversion and a different equation according to the partition of emissive energy along the spectrum. We can imagine then a reduced 'bolometric' magnitude, as introduced by Eddington, which corresponds to the true heat emission of the star, the correction that produces it being a function of the spectral class of the star. This correction is large, exceeding a magnitude at both ends of the scale. Equally a photographic magnitude, derived from an ordinary photographic plate, would require a different, similar correction, but it is more usual first to reduce the photographic magnitude to the photovisual, by estimating the difference between the two known as the 'colour correction,' and itself an index of the spectral class.

Thus, apart from instrumental and atmospheric difficulties, photometry is helpless without at least two measures, to give, between them, the total heat emission and the spectral class, or in other words the size and the shape of the Planck radiation curve. This statement assumes that the star radiates as a black body, which is sufficiently though not closely or invariably verified. So many ways are known for finding spectral class that it is sufficient merely to mention them. They are designed to apply to different cases, according as we want detail or collective summary, and can dispose of much light or little. The slit spectroscope, the prismatic camera, effective wave-lengths determined by the position of the secondary images formed by a very coarse grating, photographs with different colour screens interposed, and even eye estimates of colour.

To speak first of one of the desiderata, it has been verified beyond any doubt that the temperature indicated by the shape of the Planck emission curve of the star, which is taken as the 'effective temperature,' or the surface temperature of the star, implies quite strictly the spectral class, with its intricate detail of absorption lines. It remains only to confirm the fact by defining the relation more closely and better cleared from impurities, to examine how nearly the black body radiation actually applies to a star, and finally to search out exceptional cases.

As this branch of the research has been carried out with fair success and with general agreement in result for different methods of treatment, some detailed reference may be made to the questions it has evoked. The investigation proceeds from the evident fact that the balance of intensity of a star's radiation shifts from the blue to the red as we cross the spectral series. Presumably this is a temperature effect which may be interpreted by means of Planck's radiation curves. If we assume that the star's radiation is itself black-body radiation, it would suffice to use Wien's law and determine the summit or maximum of the curve, but it is preferable to make no assumption and attempt to determine the whole curve. In any case we require to calibrate our apparatus of reception by seeing what

it records when exposed to a standard source. It is possible to make such a source terrestrial. Wilsing employed as his standard an electric oven, constructed as a black body, against which he calibrated a glow lamp, which in turn was compared with the stellar spectrum. His work was visual. The method requires the subsequent elimination of instrumental and atmospheric losses. The latter vary rapidly and greatly. Accordingly Wilsing's measures show a rather wide dispersion, and there are some improbable results for individual stars. Besides, the general scale seems low, so that systematic error has not been avoided. But these are minor criticisms by which one tries to learn the lesson of the difficulties. The whole work, including that of his collaborators Scheiner and Münch, is a monument of skill and care.

To vary the method as Wilsing's experience suggests, I have photographed the spectra upon panchromatic plates sensitive up to Fraunhofer's (B), and in place of the terrestrial source as standard I have made comparison with a similar spectrum of Polaris, taken first and last on each plate, to guard so far as possible against atmospheric vagaries. Apart from altitude effect, it is assumed that both stars are equally affected by losses, so that there is no allowance to be made in the relative measures. We have then before us an interesting problem of interpreting the photographic deposit in terms of the active light. Different times of exposure and different wave-lengths enter with the changing sensitivity of the plate. It is in fact the basic problem of sensitometry. One might say that little can be said of it, except that it is enormously complicated. What the star contributes is merely a 'latent image' that cannot be examined, and the outcome depends on the chemicals used, their handling and temperature. The material employed is none too uniform. It is altogether a region that has hitherto been avoided, and that one would rather avoid if the gain were not so manifest of reducing it, even partially, to order. Besides, what is the alternative? There are exactly the same obscurities in using the eye, but it gives no permanent record that one can experiment on and cross-examine. Two other detectors of the same order of sensibility are offered—the selenium detector and the photo-electric cell. Whether photography may be more or less obscure than these, in a metric sense, it is certain we are more familiar with its troubles, and are in any case bound to use it for other purposes. Hence it appears to me, the sooner the discussion of the photographic deposit is reduced to a standard for scientific purposes, the better.

My own effort has been confined to reducing the complexity by defining the circumstances. If the temperature, chemicals and handling are kept the

same, and if the photographic constants p and γ are separately determined in the laboratory for each batch of plates and for each wave-length used, then it would appear that the blackening of the plate can only depend on the intensity of the light and the time of exposure, or alternatively, the intensity is a function of the blackening and the time. The last two are measurable, so that to derive the former requires merely the establishment of a calibration curve in the laboratory as indicated above. Departures must be treated as accidental anomalies. It can be shown that a manageable theory may be built on this basis, applying at any rate to the case when the desideratum is the shape of the radiation curve, leading to an easy equation between the relative temperatures of two sources, as indicated from any wave-lengths at which their spectra are compared.

It will be noted that this method fails to provide an absolute standard temperature unit, as basis. This, however, may be supplied by taking the temperature of a 'Go' star the same as that of the sun; but in fact when a great many stars are used, the indeterminateness is distributed over them all, and it is found that a coherent treatment permits of very little latitude, without thrusting very improbable figures upon one or other of the extremities.

A parallel treatment of the area of the radiation curve, giving the total bolometric emission of the star, should not present any insuperable difficulty. We should then have by direct determination the two fundamental elements of the star's radiation, temperature and quantity.

The measurement of the intensity of individual lines in the spectrum promises at least as much interest as a treatment of the whole. But in writing this summary directed to a special object, I have omitted many points which mean a great deal to those conversant with astronomical literature. If we want to pass from the 'magnitude' of a star to its radiative emission, we require to know the magnitude of the sun and the distance of the star. A few decades ago the number of stellar distances known was so insignificant that no general treatment was possible. It was the want of this knowledge that really accounts for some of the deficiencies of which I have spoken above. But that is now past. Some thousands are now determined, and their number is still rapidly increasing. Of stellar masses we still have only a very few determined directly; but a well-credited theory permits us to calculate these too, even for individual stars. These are necessary adjuncts if astronomy is to unite itself with physics, and the astronomer may be relied upon not to fail in supplying them.

The Active Principles of the Pituitary Gland.

SINCE the discovery by Oliver and Schafer of the blood-pressure raising or pressor effect of extracts of the posterior lobe of the pituitary gland, a number of other phenomena caused by injections of this material have been described; thus although in mammals the blood-pressure is raised, in birds it is reduced; in the former the extract also stimulates the uterus to contract, and produces an increased flow of

milk and an increased secretion of urine, under certain conditions, but under others the urinary flow is markedly diminished. In frogs the extract stimulates the pigment-carrying cells or melanophores to dilate, producing thereby a darkening of the skin of the animal. It may be noted that these active principles are all obtained from the non-glandular posterior lobe of the organ, which is composed chiefly of neuroglial