have had in the process has made me more alive to these dangers than even he is. I need only say here that I have not yet come across a case where I should feel warranted in stating that a break existed on the evidence of one curve only where the break depended on differentiation for being clearly visible. In my own work I have never considered any breaks as being more than "suggested" unless they were shown by at least two different properties of the substance under examination; the majority of the breaks which I insist on are shown by more than two, in some cases by as many as seven different properties.

As to the examination of the curves by means of empirical formulæ, nothing of the sort has been done, and it is difficult to understand how Prof. Lodge, even though he speaks under correction, should have so misunderstood the methods adopted. If Mendeleeff's paper may have been open to misinterpretation, Crompton's certainly was not, for he gives in a tabular form the results of the direct differentiation of the experimental numbers themselves; an abstract only of my own paper has as yet appeared, and I have not got it by me to refer to, but I do not think that the terms "formula" or "equation" occurred throughout it. The impossibility that seems to exist of getting either chemists or physicists to understand that the method of examining curves which we have employed does *not* involve the use of any equation at all is indeed extraordinary. My own opinion on the use of equations will be best illustrated by the following extract from my paper:—"It is necessary to say a word at starting to correct an erroneous opinion which is prevalent as to the method of examining curves which I have adopted. . . . It is imagined by many that this method consists in fitting sundry equations to the curves, and, on the strength of their concordance with these equations, to conclude that they are continuous or otherwise. Now, it is quite true that if a curve differentiates into a straight line after a certain number of differentiations, an equation of a certain form must represent that curve, and if it yields several straight lines there must be as many different equations applicable to different parts of it; but it is one thing to find equations empirically, and prove (?) their truth by a display of those most fallacious of arguments known as tables of 'found' and 'calculated' values, and another thing to apply an ordinary process of mathematical analysis to the curves, letting them speak for themselves, and tell us whether they are continuous or not. On the former of these methods I would place absolutely no reliance, and so far have I been from making use of it, that I have not found the equation for any single curve here depicted, and have purposely avoided finding any. The mathematical argument on which this work depends is, that a curve, if it be continuous, will on differentiation give either a straight line or another continuous curve, whereas, if it be not continuous, but be made up of different curves, will yield on differentiation a series of straight lines or curves. This, I think, is an incontestable fact."

That the majority of chemists are not mathematicians I willingly admit; this painful fact is shown only too clearly by their blind acceptance as gospel truth of everything which is "proved" mathematically. But Prof. Lodge must do us the justice to admit that we have occasionally some glimmers of common-sense, glimmers which would be inconsistent with our assuming that a certain curve was a parabola, and then being pleased, or even surprised, that it behaved after the manner of parabolas.

However much I may envy the powers of a mathematician, and however firmly I may believe that chemical facts will eventually be translated into mathematical expressions, I feel that at the present day the introduction of mathematical formulæ into chemistry almost invariably involves the exclusion of common sense. It is curious that Prof. Lodge's letter should have been immediately followed by an article on chemical affinity, which, I think, will be found to give a striking illustration of this dictum. What may be termed the x and y theory of chemical action, studied on paper by Guldberg and Waage, and followed up in the laboratory by Ostwald, has led unfortunate chemists into a labyrinth of cumbrous mathematical expressions for erroneous facts, where the common-sense of Berthollet would have given them a simple explanation of all the true facts of the case (see Trans. Chem. Soc., 1889, 26).

Harpenden, July 22. SPENCER PICKERING.

P.S.—Since writing the above I have obtained the most absolute justification of my method of differentiation which could possibly be obtained. I have isolated in the solid crystalline form a new hydrate of sulphuric acid, the existence of which I had predicted from an examination of the density and heat results of solutions of the acid. A few further details on the subject will, I believe, be found in the last issue of the *Chemical News*. S. P.

Ilfracombe, August 4.

PHOTOGRAPHIC STAR-GAUGING.

THE mere equal-surface counting of the stars visible with the same instrument in different sections of the sky gives results open to misinterpretation. Admirable in itself, the method fails because it encounters what we may call "systematic errors" in the distribution of the stars. With incidental anomalies it is fully competent to deal; they should, on a large average, be mutually compensatory; but it breaks down before the clustering tendency which pervades, more or less markedly, the entire sidereal system. Not only are certain parts of space more crowded than others, but the crowded parts are related according to an obvious plan. They do *not* occur casually. Their effect is then heightened, instead of being eliminated, by multiplied observations.

The present resources of science, however, seem to offer the means of discriminating, to some extent, between real crowding and the simple extent of star-strewn space. Although the total number of the stars visible in each case with the same telescope might be precisely the same, their relative numbers, counted by magnitudes, would in all probability be very different. In a stratum, supposing the distribution of the stars equable, and their size uniform, their numbers should be nearly quadrupled at each descent of a magnitude. This of course is an ideal law of progression which we cannot expect to find anywhere strictly obeyed; but even approximate conformity to it must be held to indicate with tolerable certainty that the lessening ranks of the stars are, on the whole, at distances from us corresponding with their light. Now it *is* approximately conformed to by the stellar multitude down to about 8·9 magnitude over the general expanse of the sky, as well as over the zone of the Milky Way. But in that zone, stars of the ninth and higher magnitudes very much exceed their due numerical proportions; in other words, they are physically, no less than optically, condensed.

From these circumstances two very important inferences may be derived: first, that the lower margin of the galactic aggregations lies at a distance from us corresponding roughly to the mean distance of a ninth magnitude star, costing light some fourteen hundred years of travel; next, that the aggregated objects are average stars, neither larger nor smaller than those in our nearer neighbourhood. Both conclusions seem inevitable should the facts turn out, on closer investigation, to be as above stated. A regular increase in the numbers of the successive photometric orders of stars, tallying with the increased cubical contents of the successive spheres of which the radii are the theoretical mean distances of those same orders, affords strong, if not demonstrative, evidence of a corresponding real penetration of space.[1] And since the sequence continues unbroken down just to the ninth magnitude, we see that the galactic condensations of ninth magnitude stars cannot be situated nearer to us than their brightness would lead us to suppose—cannot, in other words, be stars on a lower than the ordinary level of lustre.

It is tolerably certain, however, that the denser star-clouds of the Milky Way lie far beyond ninth magnitude distance. The ground for this assertion is not the apparent minuteness of their components, but the singular fact, adverted to by Argelander, that, in the divided Milky Way, running from Cygnus to the Centaur, the

[1] The idea of determining distance by distribution seems to have presented itself to Dr. Gould in 1874. See *American Journal of Science*, vol. viii.

shining branches are nearly on a par with the dark rift separating them as regards the distribution of stars even fainter than the ninth magnitude. The nebulous effect to the eye distinguishing the branches is, then, presumably due to more remote collections. As to the further limits of these, we know as yet nothing, except that Herschel's gauge-numbers left it to be inferred that "thinning-out" had become marked before the attainment of fourteenth magnitude distance. On these, and similar subjects, enlightenment may be hoped for through the judicious use of means already at hand.

For simple star-counts, we have only to substitute star-counts by magnitudes over selected areas of the sky.[1] The relative numbers of the photometric ranks can hardly fail to give highly valuable indications as to real distribution; provided only that the assumption of a general uniformity in the brightness of the stars be valid. Not, it need scarcely be said, of a uniformity such as to preclude any extent of individual variety; all that need be supposed is, that the average size of a star remains constant throughout sidereal space. This hypothesis has far more probability in its favour than any other which could be set up instead of it; though it may receive corrections as our inquiries advance.

The photometric classification of small stars is one of the many branches of sidereal science which will henceforth be prosecuted only with the assistance of the camera. Visual methods are inadequate and insecure. Those by photography, it is true, have also their difficulties, not yet completely vanquished: they will, however, evidently prove manageable. Prof. Pickering is tentatively establishing methods in photographic photometry which will doubtless before long be brought to perfection. They depend mainly upon comparisons of stellar impressions upon any given plate, exposed under known conditions, with standard impressions of standard stars obtained with varied exposures or apertures. For the purpose we have in view, accidental errors of estimation, even if very large in amount, are of no importance. What is essential is, that the integrity of the series should be preserved—that the proportionate change of light from one magnitude to the next should remain invariable from the first term to the last. The realization of this aim, now virtually attained, is one of the most weighty services rendered to astronomy by the sensitive plate.

We may now describe the process of photographic star-gauging. It consists in the enumeration, by magnitudes or half-magnitudes, of the stars down, say, to the fifteenth magnitude, self-pictured from distinctively situated patches of the sky. Each such area should be wide enough to insure the elimination of minor irregularities in distribution; but a single large field would often suffice to show the characteristic grouping of the smaller telescopic stars.

The Milky Way would naturally be the first subject of inquiry; and the comparison of several plates taken in different sections of its course might be expected to yield data of great significance as regards its constitution. From simply calling over the muster-roll by orders of brightness of the stars contained in them, answers may be derived to the following questions:—

(1) How far does the regular sequence of increasing numbers extend? That is, down to what grade of brightness do the stars continue nearly to quadruple with each additional magnitude?

(2) Is the progression interrupted by defect or excess, or by each alternately? In other words, does the stellar system embrace systematic vacancies, as well as systematic groupings?

(3) Supposing an accumulation of stars to set in at a definite stage of space-penetration, where does it stop? Down to what magnitude is the augmented ratio of increase maintained?

(4) Are there symptoms of approaching total exhaustion of the stellar supplies beyond?

These should be found in a concurrent decrease of density with brightness, "density" being understood as the proportion of the numbers present to the space *theoretically* available for stars of a given magnitude. For one of two things seems certain: either the thinning fringe of stars is composed of really small objects interspersed among larger ones; or of average stars at average distances from us, but further and further apart from each other. In the first case, the system ends abruptly; in the second, it is, as it were, shielded by outliers from the absolute void.

Particular attention should be paid to the differences of stellar distribution upon plates of the Milky Way proper, and of the dark aperture between its cloven portions. That this really forms an integral part of the galaxy is shown by the far greater profusion of small stars there than in the general sky at the outer margins of the galactic branches—a fact in itself fatal to the "spiral theory," by which the rift was interpreted as a *chink* of ordinary sky-background left by the interlacing, to the eye, of two great streams of stars, one indefinitely more remote than the other. From photographs we may now hope to learn what is the nature of the distinction between rift and branches—what are the magnitudes, relative numbers, and presumable mean distances, of the clustering stars present in the latter, but absent from the former.

Gauges taken in the neighbourhood of the southern "coal-sack" ought to prove instructive as to the nature of the nebulous stratum out of which it seems as if *scooped*. If the Milky Way be there shallower than elsewhere, a greater uniformity of lustre may be looked for among the stars composing it. No background profusely stored with lessening ranks will come into view, and stars below the average of those grouped in bright masses, representing their genuine companions, will be but scantily present.

Outside the Milky Way, two points suggest themselves as likely to be settled by photographic gauges. Argelander found that the faintest stars in the *Durchmusterung* were everywhere in excess of their due proportion.[1] Even at the galactic pole, their increase, as compared with the class next below, was sextuple instead of quadruple; in the undivided galactic stream it was 9½, in the rift 8½ times. If this semblance of crowding *in all directions* at about the mean distance of a ninth magnitude star be no accident of enumeration, then the Milky Way is only the enhancement of a phenomenon universally present, and the fundamental plan of the sidereal system must be regarded as that of a sphere with superficial condensation intensified in an equatorial ring. The counts, to settle this question, will have to extend over a considerable area.

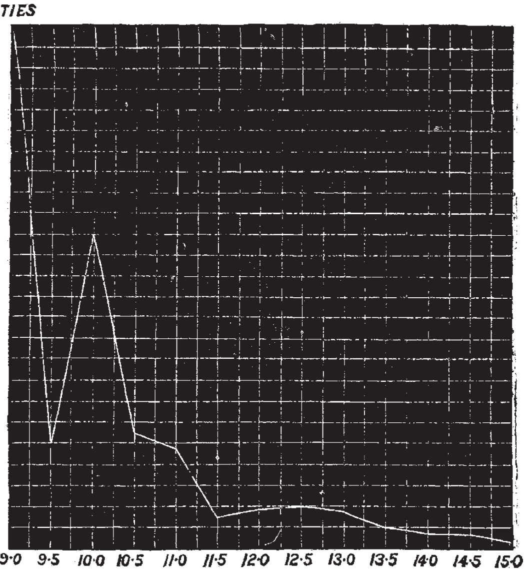
The second point for photographic investigation refers to the limits of the system towards the galactic poles. There is reason to believe them comparatively restricted. M. Celoria, of the Milan Observatory, using a refractor capable at the utmost of showing stars of eleventh magnitude, obtained for a "mean sounding," at the north pole of the Milky Way, almost identically the same number given by Herschel's great reflector.[2] That is to say, *no additional stars were revealed by the larger instrument*. Should this evidence be confirmed, the boundary of the stellar scheme should here be placed at a maximum remoteness of 3500 years of light-travel.

As a specimen of a photographic gauge-field on a small scale, we may take Prof. Pickering's Catalogue, from the Harvard plates, of 947 stars within 1° of the north celes-

[1] This plan was first suggested by Prof. Holden in 1883, as a mode of investigating the composition of star-groupings ("Washburn Publications," vol. ii. p. 113). Counts with varied telescopic apertures gave him the numbers in the successive photometric ranks. We believe that a photographic method of determining them has since been adopted by him.

[1] *Bonner Beobachtungen*. Bd. v. "Einleitung."
[2] *Memorie dell' Istituto Lombardo*, t. xiv. p. 86.

tial pole.[1] The region examined lies about 27° from the zone of the Milky Way, but is nearly reached by a faint extension from it Since only one eighth magnitude star, and none brighter, are included in it, the study of distribution, for which it offers some materials, may be said to begin with the ninth magnitude. A single glance at the synoptical table suffices to show that the numerical representation of the higher magnitudes is inadequate. The small stars are overwhelmingly too few for the space they must occupy if of average brightness; and they are too few in a constantly increasing ratio. Either, then, the diminishing orders form part of a heterogeneous collection of stars of all sizes at nearly the same distance from us (about that corresponding to ninth magnitude); or they belong to attenuated star-layers stretching to a much vaster distance. A criterion might be supplied by Prof. Holden's plan[2] of charting separately stars of successive magnitudes over the same area, and judging of their connection or disconnection by the agreement or disagreement in the forms of their groupings.



Distribution of 934 stars within 1° of the pole, showing the ratio of numbers to space for each half-magnitude.

The accompanying diagram shows graphically the decrease of density outward, deducible from Prof. Pickering's numbers on the sole supposition of the equal average lustre of each class of stars. Those of the ninth are the most closely scattered; the intervals between star and star widen rapidly and continuously (for the sudden dip at 9·5 magnitude is evidently accidental) down to 11·5 magnitude, when a slight recovery, lasting to the thirteenth magnitude, sets in. How far these changes are of a systematic character, can only be decided from far wider surveys.

A. M. CLERKE.

[1] *Harvard Annals*, vol. xviii. p. 138.
[2] Recommended in the *Century Magazine* for September 1888, as well as "Washburn Publications," vol. ii. p. 113.

TWO AMERICAN INSTITUTIONS.

I.—THE SMITHSONIAN INSTITUTION.

IN 1826, Mr. James Smithson, F.R.S., an English gentleman (a natural son of the first Duke of Northumberland), in a fit of pique at the action of the Committee of the Royal Society, who had declined to accept a scientific paper he had submitted, bequeathed to the United States of America a large sum of money, (£105,000), "to found at Washington under the name of the Smithsonian Institution, an establishment for the increase and diffusion of knowledge among men."

The question of how knowledge might be best increased and diffused with £105,000 then arose for discussion.

The President of the United States applied to a number of persons, "versed in science and familiar with the subject of public education, for their views as to the mode of disposing of the fund best calculated to meet the intentions of Smithson and be most beneficial to mankind."

The President of Brown University (Prof. Wayland) proposed a University to teach languages, law, and mental philosophy (Arts), without Science. Dr. Thomas Cooper, of South Carolina, proposed a University to teach science only, and to exclude Latin and Greek, literature, law, and medicine. Mr. Richard Rush proposed a Museum with grounds attached sufficient to reproduce seeds and plants for distribution; a press to print lectures, &c., and courses of lectures on physical and moral science, and on government and public law. The Hon. John Quincy Adams proposed the establishment of an astronomical Observatory, with instruments, and a small library. Prof. W. B. Johnson proposed the establishment of an institution for experimental research in physical science. Mr. Charles L. Fleischman proposed the establishment of an agricultural school and farm. The Hon. Asher Robbins proposed a literary and scientific institution; and memorials were presented to Congress in favour of appropriating the fund for annual prizes for the best original essays on the various subjects of the physical sciences; for the establishment of a system of simultaneous meteorological observations throughout the Union; for a National Museum; and for a Library.

For ten years the Congress of the United States wrestled with the interpretation of the words "the increase and diffusion of knowledge among men." The discussions were numerous and irritating; and it was repeatedly proposed to send the money back to England. Finally Congress was wise enough to aknowledge its own ignorance, and authorized a body of men to find some one who knew how to settle the question. Joseph Henry was chosen. His idea was accepted and acted upon. "To *increase knowledge* men were to be stimulated to original research by the offer of rewards for original memoirs on all subjects of investigation; to *diffuse knowledge* the results of such research were to be published;" and in addition it was decided to issue a series of reports giving an account of new discoveries in science, and of the changes made from year to year in all branches of knowledge not strictly professional; as well as to publish occasionally separate treatises of general interest; and all these were to be distributed amongst the public institutions of the world.

In the result the Smithsonian Institution was established for the promotion of original research, and the diffusion of the same, and it now distributes to 3700 public institutions in Europe, Asia, Africa, and America, the following publications:—

"The Smithsonian Contributions to Knowledge," of which twenty-six volumes in a quarto series have been issued, comprising memoirs and records of original investigations; researches in what are believed to be new truths; efforts to increase human knowledge. "The Smithsonian Miscellaneous Collections," an octavo series, already numbering thirty-four volumes, containing reports on the present state of our knowledge of particular branches of science; instructions for digesting and collecting facts and materials for research; lists and synopses of species of the organic and inorganic world; reports of explorations; aids to bibliographical investigations, &c. "The Annual Reports of the Board of Regents of the