

that, with Ricardo's premiss, his conclusion is absolutely correct without any further assumption. If, on the other hand, we adopt constancy of *ratio* (instead of constancy of *difference*)—which was Mill's (not Ricardo's) supposition—then some further assumption must be made in order to demonstrate that improvement in fertility produces diminution of rent. In proving this point, the editor uses an unnecessarily complicated piece of mathematical reasoning.

Without further dwelling on these defects, it is only necessary to say that the explanatory footnotes are everywhere extremely helpful, and that the frequent references to Ricardo's "Letters to Malthus" will be found especially useful in further elucidating the great economist's doctrines. W. E. J.

### OUR BOOK SHELF.

*Photographic Pastimes: a Hand-book for Amateurs.* By Hermann Schnauss. Translated from the Second German Edition. (London: Iliffe and Son, 1891.)

MANY and varied are the effects that can be produced with the aid of the camera, and the present work gives a plain and popular account of the methods that have been adopted in producing them. The five chapters are headed, respectively—specialities, curiosities, photography by peculiar arrangements, photographic optical entertainments, and entertainments with photographic prints.

In carrying out the experiments contained under the first two headings, amateurs will find their time fully occupied, while the novel effects that can be obtained will afford both instruction and amusement. With reference to taking pictures by moonlight, we can quite agree with the author when he says that "if the moon is included in the picture, its track will make a straight band of light nearly half-way across the photograph, which, besides the peculiar illumination of the landscape, gives a *most characteristic effect*." The characteristic effect, we should think, would be very decided.

An excellent and easy method of producing ghosts, which may prove useful to amateurs, and which is not wholly described in this book, is as follows:—The ghost consists of a person completely covered over with a sheet, the latter being so adjusted as to give a dim outline of the head; when in position, a short exposure of about half an inch of magnesium is given: then, as soon afterwards as possible, without moving anything with the exception of the ghost (which now is no longer required), another exposure is made, by means of a magnesium flash light, of the other figures that are required for the picture. In this manner excellent results have been obtained, the pattern on the wall appearing through the ghost, giving it quite a realistic appearance.

In these and the remaining chapters, descriptions of many novelties too numerous to mention are given, of which the following may serve as types—caricature, composite, and pin-hole photographs, statuary portraits, kaleidoscopic and stroboscopic pictures, &c.

Altogether, amateurs will find in this hand-book much that will occupy them during the winter months, when out-door photography is more or less at a standstill.

*On Surrey Hills.* By a "Son of the Marshes." (Edinburgh and London: W. Blackwood and Sons, 1891.)

THE Surrey hills are so well known that an ordinary writer would find it hard to say anything fresh about them. The "Son of the Marshes," however, has an exceptionally good power of observation, and even familiar facts he is able to present in a way that seems to give them new

vitality. In all his books he is especially interesting in passages dealing with the habits of animals, and there are many such passages in the present volume. No secondhand information is offered; the author tells us only of things which he himself has had opportunities of noting. Most of the chapters have already appeared in *Blackwood's Magazine*, but many who read them there will be glad to possess them in their present form. The manuscripts of the "Son of the Marshes" have, as usual, been edited by Mr. J. A. Owen, who does not say precisely how much his editorial work includes.

*Heroes of the Telegraph* By J. Munro. (London: Religious Tract Society, 1891.)

THE author of this book desires that it shall be regarded as in some respects a sequel to his volume on "Pioneers of Electricity." He begins with a short account of the origin of the telegraph, and then sketches the lives and principal achievements of those discoverers and inventors to whom we owe the electric telegraph and the telephone—Charles Wheatstone, Samuel Morse, Sir William Thomson, Sir William Siemens, Fleeming Jenkin, J. P. Reis, Graham Bell, Thomas Alva Edison, and D. E. Hughes. In an appendix, Mr. Munro gives brief accounts of various other investigators whose names are intimately connected with his subject. He has a plain, straightforward style, and the book will give much pleasure to young readers who take interest in the practical applications of science.

### LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

#### The Koh-i-Nur.

DR. BALL, in his reply (NATURE, vol. xlv. p. 592) to my criticisms on his "true history" of the Koh-i-Nur, feels aggrieved that I "smite him in season and out of season," and considers me in the light of a partisan for doing so. I can assure him that my criticisms were absolutely impersonal, as I have never, to my knowledge, seen him in my life, and bear no kind of ill-natured feeling towards him; indeed, I said whatever I was able honestly to do in favour of his work. But of course, where I considered his arguments to be groundless or illogical, I met them. If he has read into my remarks an asperity I did not desire to impart to them, surely he should blame himself somewhat for the style of his attacks on those who went before him, and of whom I have shown that they knew not less, but more, of the subject than he did himself.

I have pleasure in withdrawing my expression of an accusation that Prof. H. H. Wilson was one of those against whom Dr. Ball threw out a sneer in relation to the earlier history and traditions attaching to the Koh-i-Nur. I supposed that, as he has laboured to make his knowledge of the authorities on the subject complete, he would certainly have known what was of common knowledge at the time as to the authorship of by far the most interesting notice ever penned on the Koh-i-Nur. But that was long ago. It was that notice, however, that brought me into such contact as I have had with the subject. As a young Professor at Oxford, I had the honour of knowing the great master of Sanskrit and of Indian lore: and as I had been interested in Indian history I ventured to approach him now some thirty-five or thirty-six years ago on the subject of the values assigned by him to certain weights referred to in his article. I drew his attention to Babar's valuation of the mishkal in ratis, and I further pointed out the probability of the retention by Shah Jahan of the Mogul diamond in his captivity. He received my suggestions in the kindest spirit, and offered me every help in further inquiry; and at the East India Company's Library he placed all the documents before me.

I shall not weary your readers with thrashing out and again winnowing the various statements involved in this controversy.

I could say more about the Garcias-De Boot matter, but I am satisfied with having shown that it was not Dr. Ball, but Mr. King, who, twenty-five years ago, explained the misprints in De Boot, and declared the very great improbability of the 140 mangelin diamond of Garcias, estimated by De Boot at a weight of 187½ carats, not being the Koh-i-Nur. Dr. Ball alludes to inaccurate figures in Mr. King's treatise. That Mr. King was inaccurate, was hasty, no one knows better than I. Nor did any of his many warm friends lament more than I did the unhappy infliction of advancing blindness which explains so much of the former's demerit, as no one admired more than I the boy-like enthusiasm which often gilded in his imagination what seemed to others metal of a less precious order than gold. He had a splendid memory, and he trusted too much to it in drawing out from it, rather than throwing on his impaired eyesight, the verifying the records of his enormous reading and varied knowledge. I had controversies with him over a thousand subjects, but while he kept singularly isolated, and let no one come between him and his printer, he never resented a friend's criticism or difference from him.

As regards the scene before the throne of Aurungzebe, it can never, perhaps, be determined whether the view first put forward by Prof. H. H. Wilson, that Tavernier weighed the diamond, but with weights and scales supplied by Akil Khan its custodian, is the correct one; or the view I have held—namely, that Tavernier's account of the transaction given in his tenth chapter was barely compatible with his having weighed the stone, as he asserts he did in the twenty-second chapter of his book, which was avowedly a retrospective one written long afterwards, and near the end of his life. That I have reason for adhering by preference to the latter view is confirmed by what Dr. Ball himself says of another passage referring to the Great Mogul diamond. Dr. Ball condemns the passage as "in part spurious if not altogether so, . . . as the statements are in contradiction with others made elsewhere in the 'Travels'; and there is the strongest reason for attributing them to an erroneous editorial interpretation, and not to Tavernier himself." The delinquent he supposes to have been a M. Samuel Chappuzeau, the reputed editor of Tavernier's works.

As a fact, the travelled Frenchman seems to have been a person somewhat illiterate, as he had to call in extraneous aid in putting his memoirs into shape. He must be supposed to have picked up some colloquial Persian, but otherwise seems to have been dependent on interpreters throughout his travels. The treatment Chappuzeau received during a year of editorial service at the hands of Tavernier and his wife is recounted by Dr. Ball as a sort of "mortification, if not martyrdom." Chappuzeau appears to have described the notes of the traveller, on which he had to depend, as a chaos, and to have attributed the only written part of them to the penmanship of one Father Gabriel. I think I am justified, then, in asking whether the account of the weighing in the later chapter may not have been an editorial afterthought; but whether it were so or was historical, in the sense assigned to it by Prof. Wilson, really very little affects the question.

The logical issue of this discussion is involved in the acceptance of one of two alternatives, the one a series of astounding coincidences and improbabilities, the other one of simple probabilities. Garcias saw a diamond weighing 140 mangelins; Le Cluze estimated its weight at 700 apothecary grains (= 573.8 grains troy, or 180 carats). De Boot assigned to it a weight of 187½ carats. The Koh-i-Nur weighed 589½ grains, or 186 carats. Misinterpreting a note of Le Cluze, Dr. Ball throws scorn on this having anything to do with the Koh-i-Nur.

Tavernier sees a diamond to which a weight is assigned of 319.5 ratis. Babar's diamond (the Koh-i-Nur) weighed 8 mishkals, or 320 ratis, equivalent to about 186 carats. Dr. Ball says this diamond was that known as the Great Mogul, that it is the Queen's Koh-i-Nur, but that it was whittled down by necessitous princes—to find them, in fact, in pocket-money—from 280 carats to 589½ grains, or 186 carats, the identical weight of Babar's diamond and of the Koh-i-Nur. Dr. Ball finally declares the Darya-i-Nur to have this same weight of 186 carats.

In opposition to this impossible recurrence of coincidences I have endeavoured to show that the stone Garcias saw may have been the Koh-i-Nur, that the one Tavernier handled was in all probability—I believe was certainly—the Koh-i-Nur. I say there is no evidence whatever of the Koh-i-Nur having been whittled down by cleavage, accidental or intentional; that its form in 1851

was more probably its original form rudely faceted (and I think, perhaps, I may not be without a mineralogist's experience when I say this); I further say that the Darya-i-Nur is undoubtedly the "Golconda table" diamond.

Finally, I assert the probability that the Great Mogul, unwhittled down and entire, is in the jewel chamber of the Shah of Persia to this day.

Of the great diamond which I would identify with this stone I append a tracing, in which it is seen in its carcanet of ruby-enamel. In the original drawing it is accompanied to right and left by two large diamonds, similarly girdled; while, above and below, is a row of three enormous rubies encircled by emerald-enamel. Ten pearls above and ten below, some of them ¾ of an inch in diameter, form a fringe to this gorgeous ornament. It

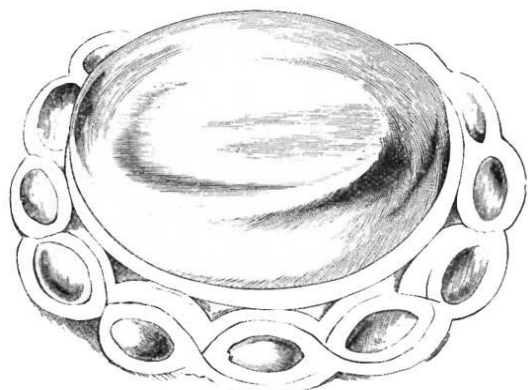


FIG. 1.—Great Mogul.

is, however, only one half of a cylindrical cap the corresponding half of which is its counterpart in splendour and wealth of stones, only the Darya-i-Nur is in, that other half, the central ornament.

I leave the great stone to speak for itself in the tracing, and I furthermore for comparison give a tracing from a drawing of the Koh-i-Nur, taken from a somewhat similar point of view—that is to say, looking down on it.

That the Koh-i-Nur was valued beyond these greater stones I believe to have been in consequence of its being the reputed talisman of Indian empire. It was probably that last relic of his treasure surrendered by the miserable Muhammad Shah when he exchanged caps with Nadir, and the conqueror saluted this most historic of his spoils by the name it has since borne—

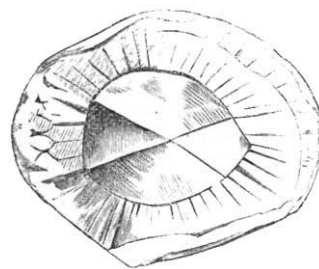


FIG. 2.—Koh-i-Nur.

the Koh-i-Nur. It was certainly the diamond that Shah Rukh, after yielding up all his wealth of jewellery, held to through every torture till he gave it to Ahmad Shah. Shah Zaman carried it to his prison, and secreted it in a crevice; whence Shah Shuja recovered it on information from his blinded brother.

Shah Shuja again clung to the old talisman not less fiercely than those who had preceded him, till he surrendered it to Runjit Singh under pressure which amounted to compulsion; and memorable was the answer of Shah Shuja to Fakir Nur-ud-din, who had been sent by Runjit to ask in what its value consisted. It is "good luck," said Shah Shuja, "for he who has possessed it has done so by overpowering his enemies."

I have put, I hope clearly, to my readers, the alternative and

conflicting interpretations of the portion of the accounts of the Koh-i-nur from Babar's time onward. There are still some interesting questions of a difficult kind regarding its history antecedent to the days of the Mogul Empire. But I believe I have said now my last word regarding the later history, and leave to my readers the decision as to the side in this little controversy on which the truth is more likely to lie.

N. STORY MASKELYNE.

Basset Down House, October 26.

### A Rare Phenomenon.

AURORAS were visible at Lyons, New York, on September 9, 10, and 11. That on September 9 was very fine, flickering streamers and arches forming at intervals from 8 o'clock to 10 o'clock p.m. A peculiar feature of this aurora was an arch similar to that described in NATURE of September 17 (vol. xlv. p. 475), as having been seen by Mr. Tuckwell at Loughrigg, Ambleside, on September 11. The arch seen at Lyons on September 9 was visible shortly after sunset, and remained in the same position throughout the evening. It consisted of a narrow band of light, which arose vertically from a point on the horizon nearly due west, and passed through the constellations of the Northern Crown and the Lyre, and just south of the zenith down to the eastern horizon. When it was brightest, at about 10 p.m., a few small streamers formed in connection with it nearly in the zenith; otherwise it consisted simply of a narrow band of white light separated by a wide interval from the auroral coruscations and streamers in the northern heavens. This seems to have been very similar to the band seen by Mr. Tuckwell. Other instances have been noted by the writer in which some peculiarity of form or colour characteristic of an outbreak of the aurora has attended its appearance in localities remote from each other.

M. A. VEEDER.

Lyons, N.Y., October 17.

Two instances of the occurrence of the rare phenomenon referred to in your issue of September 24 (vol. xlv. p. 494), by Prof. R. Copeland and Mr. W. E. Wilson, will be found recorded in the Transactions of the Nova Scotian Institute of Natural Science, vol. vi. p. 100. The dates of these occurrences were July 31 and September 5, 1883. The general appearance and position of the luminous arch were the same in both cases as in those described by Prof. Copeland and Mr. Wilson. Two additional points were noted, however, which are worthy of mention, viz. (1) that the arch of September 5 had a slightly marked rayed structure, which, when first observed, was in the direction of its length, but which gradually changed to a direction inclined about 45° to the longitudinal, and (2) that the spectrum of this arch, as determined by one of Hilger's pocket spectroscopes, consisted of two lines in the green, one quite bright and the other faint.

On Tuesday, September 1 of this year, I again observed the same phenomenon at Halifax, N.S. I was unable to make accurate observations, but noted the following facts:—The luminous arch was quite bright when first observed, at 11.30 p.m., and extended from horizon to horizon. Fifteen minutes later it had completely faded away. It was about 4° or 5° in width throughout its whole length. It met the horizon at points about 10° or 15° to the north of the east and west points, and passed through a point a few degrees south of the zenith. When first observed, it was approximately uniformly bright throughout, except at the edges, where its brightness diminished rapidly outwards. To the eye its light seemed to be white, and stars were visible through it. In fading away, the east and west ends disappeared first, and the main body of the arch became gradually fainter, wider, and more variable in width. The night was bright and clear, and the temperature lower than it usually is in the beginning of September, and there was no appearance of aurora in other parts of the sky.

Except on this occasion I have neither observed this phenomenon nor heard of its occurrence since 1883. But as it might readily occur without my either seeing it or hearing of it, I cannot say that I know it to be rare.

J. G. MACGREGOR.

Dalhousie College, Halifax, N.S., October 14.

It has twice been my good fortune to observe phenomena similar to that described in NATURE of September 24 (vol. xlv. p. 494). My recollections of the first occasion are some-

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what indistinct, but at all events the luminous band extended east and west almost through the zenith, and was preceded by an auroral display. It occurred in August or September of 1883 or 1884.

My attention was again directed to a similar appearance on the evening of September 9 of the present year, while near Toronto. The narrow band of light, as before, extended from the eastern almost to the western horizon, passing through the zenith, and was accompanied by an aurora.

It is worthy of note that I saw the phenomenon at Toronto on the evening of September 9, not September 11.

R. N. HUDSPETH.

Bishop's College, Lennoxville, P.Q.

### Apparent Size of Objects near the Horizon.

SOME years ago there appeared an account of an investigation into the cause of the sun and moon looking larger when low down than when high up in the sky. The theory advanced as the result of the investigation attributed the effect to a physiological cause. One could not expect an explanation of this kind to be applicable to all individuals, but rather that with different persons there would be different results; so I have made observations—81 in number—to find out what law applies to my own case. These observations were made by taking notice of two stars near the horizon, and then looking up near the zenith to see what stars in that situation appeared to be the same distance apart as those near the horizon. I took a great variety of different cases, the length of the compared arcs varying from 1° 4' to 100°. I observed them also in various angles of position, from horizontal to vertical; and sometimes had the two arcs at the same angle of position upon the retina, and at other times at different angles.

The result of this investigation is an unexpected one, showing that the length of the observed arc greatly affects the result of the estimation—short arcs appearing longer when near the horizon than when high up, and long ones appearing shorter.

The comparisons were made in either of two ways; according to one method, after I had carefully taken note of the apparent length of the arc near the horizon, and had fixed an idea of it in my mind, I then took a single glance at the stars near the zenith and fixed in a moment upon an arc that appeared to be of the same length; whereas in the other plan I made as deliberate and careful an estimation of the arc near the zenith as of that near the horizon with which it was compared, looking to and fro from one to the other till I was satisfied as to their apparent equality.

One would naturally expect that the instantaneous estimations would be less accurate than the careful ones, and this is found to be the case. Taking all the observations, I find the average deviation from the truth of a single estimation is 7.7 per cent. in the case of careful comparisons, and 10.3 per cent. in the case of the instantaneous ones. The following formula is based upon the careful comparisons—

$$L = l \left\{ 1 + \frac{A^\circ - a^\circ}{74^\circ} (.085 - .00321l) \right\},$$

where  $l$  and  $L$  are the lengths (in degrees) of apparently equal arcs at  $a^\circ$ , the lower altitude, and at  $A^\circ$ , the higher altitude, respectively. According to this formula, an arc 26° 48' long appears the same length at whatever altitude it is situated, but an arc shorter than 26° 48' appears longer at the horizon than at the zenith, and an arc in excess of 26° 48' would actually appear longer near the zenith than near the horizon: an arc 1° 4' long (the shortest in my observations), when at the horizon, would appear equal to an arc in the zenith 109.85 per cent. of its length; while an arc 100° (about the longest in my observations) at the horizon would appear equal to an arc of 71° 30' only in the zenith (i.e. with its middle point in the zenith). When the above formula is applied to all the observations, the average deviation (of the observed lengths from the computed) is reduced to 4.2 per cent. in the case of the careful comparisons, and 7.0 per cent. in the case of the instantaneous ones. If this formula can rightly be applied to objects of such small dimensions as the sun and moon, it (as will be seen) allows only a small increase for their apparent size near the horizon upon that when they are seen at a considerable altitude.

It would be easy to find a more complex formula which would satisfy the observations still better, but these are not sufficiently numerous to warrant the doing so.