f. 1

Camera Club,
Charing Cross Road,
W.C.

Decr 6th 95

Dear Sir

I have heard from our Secretary & Manager of the visit you paid to the Club yesterday, & can only regret that I also was not present to welcome you. I hear also with very great pleasure that there is a possibility of our having the honour, & pleasure

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of hearing a Paper contributed by you –

I can only hope most sincerely that this good news is true –

And remain

Dear Sir

Yours Faithfully

R W Cox

Hay Sept 7.

Francis Galton Esq^re
&c &c

42 Rutland Gate

S.W.

f. 1r

ALL LETTERS TO BE ADDRESSED TO

J. H. Dallmeyer, Limited.
OPTICIANS & LENS MANUFACTURERS.
Telegraphic Address, "DALLMEYER, LONDON"
25, Newman Street,
LONDON, W.

14 Dec 1895

Francis Galton Esq F.R.S.,
42 Rutland Gate

Dear Sir,

I regret that by an oversight I have not yet fulfilled my promise of informing you as to the particulars required with regard to the distances necessary in taking your photographs.

For an error of $\frac{1}{100}$" you will require a distance, from the nearest portion of your horizontal plot, of 33 feet using a 5 in lens with an aperture of f/6. The error will be halved, viz: $\frac{1}{200}$" if you focus practically at a spot midway in the diagonal of the plot.

On the slip of paper on which

F. 1v

we roughly considered the subject the other day you wrote down an error "·02 in". This is of course 1/50th, but I think I understood you to mean that the error should not be greater than 1/200th = ·005 in

Trusting this may be satisfactory to you, I am

Yours faithfully:

Thomas R. Dallmeyer

from Mr Dallmeyer Jan/96

The Journal and Transactions

OF THE

PHOTOGRAPHIC SOCIETY

OF

GREAT BRITAIN.

NEW SERIES. VOL. XII., No. 6.] FRIDAY, MARCH 30, 1888. [PRICE 3d.

PATRONS.

HER MAJESTY THE QUEEN

AND

HIS ROYAL HIGHNESS THE PRINCE OF WALES.

COUNCIL AND OFFICERS.

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For hood of Lens; size, 1½ in. and under, **25/-**; for 2½ in. and under, **33/-**; for 3¼ in. and under, **40/-**. This Shutter reduces the sky exposure to one-third of that of the foreground, hence the clouds are beautifully rendered as they appear in nature.

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For Plates 3¼ square. It may be used either in the hand, or fixed on a stand. It is light, strong, and compact. There is a new arrangement to the Dark Slide, and other novelties of construction.

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This addition to the Academy Camera makes the instrument much more handy to use. It can be held to the body, and the focus is reflected from the mirror through a hole cut in the top of the Camera; the general make has also been much improved, and, in the hands of one who will use a little care, no Hand Camera offers the same advantages. Made in four sizes, for plates 1¼ in. square, 2 in. square, 3¼ in. square, and 4¼ × 3¼ in.

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The PRESIDENT showed some remarkable photographs of lightning taken by Mr. H. Schleussner and Mr. E. B. Vignolles, at Tours, during storms on May 26th, 1886, and one by Mr. Davis, at Dover, July 18th, 1887 He thought they were of so striking a character that he had asked that they might be sent to the Society for the inspection of the Members, and afterwards be sent to the Meteorological Society.

Mr. SAMUELS: With the permission of the Chairman, I should like to call attention to a great omission in the Report of the Council. There is in this Report no reference to the exhibition of apparatus. This appears to me so important a matter that I think its omission is likely to be detrimental to the future of the Exhibition, and to those who have taken part in it.

The PRESIDENT: I think if you address these remarks to the Council any suggestion will be taken into consideration, and discussed, with a view to action, if possible.

Mr. SAMUELS said this was what he had been advised to do last year. He had done so, but nothing had come of it.

The PRESIDENT was sure the Council did not neglect any communication sent to it. If Mr. Samuels again wrote it would be attended to.

The PRESIDENT then called upon Mr. T. R. Dallmeyer to read the following paper:—

ON SO-CALLED "DEPTH OF FOCUS" AND "DIFFUSION OF FOCUS."

By T. R. DALLMEYER.

IN plane geometry a *point* is defined as that which hath position but no magnitude; a *straight line* is such that any part will, however placed, be wholly on any other part, if its extremities are made to fall on that other part; and a *plane* is a surface in which any two points being taken the straight line that joins them lies wholly in that surface.

I mention these three definitions to make clear what I wish to describe as an ideally perfect photographic instrument in so-called "depth of focus." If you will allow, in imagination, a point to be the section of a straight line, and let that point be the aperture of an imaginary pin-hole camera, then every point in whatever plane (near or distant) in the object would be represented by a corresponding point in the image upon the plane on which it was received. In such a case you would have *perfect depth of focus.* In reality, however, with the pin-hole camera, because it is not a point, you obtain "diffusion of focus," but with uniform "depth of focus."

The object of my paper is to point out, in the first place, that absolute depth of focus in any lens, perfectly or imperfectly corrected for spherical aberration, does not exist at all; secondly, to explain clearly what is meant by so-called "depth of focus," in the ordinary acceptation of the term, and a method of calculating it for any lens; thirdly, the meaning of "diffusion of focus" with its bearing on so-called "depth of focus."

First. Absolute depth of focus does not exist in any lens. There is a well-known law in optics, known as the "law of conjugate foci," with which I anticipate you are all well acquainted; but as I shall have to refer to this again, I will call your attention to Figure 1 for a moment. Any one point in a given plane in the object O can only be absolutely defined by one corresponding point in the plane of the image I determined by the distance of the object from the optical centre of the lens, and the focal length of the lens employed. Object and image are interchangeable; but however little the plane of the object be changed in position, either nearer to, or further from the lens, there must be a corresponding change in the plane of the image, *i.e.*, further from or nearer to the lens, respectively.

f. 5

Fig. 1.

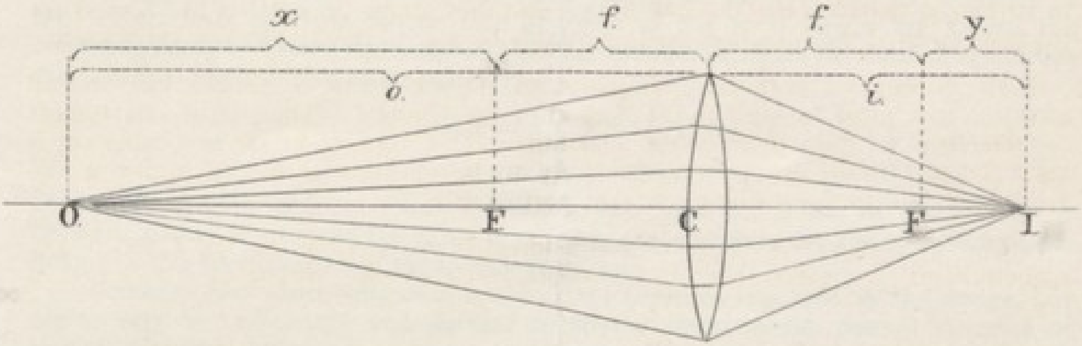
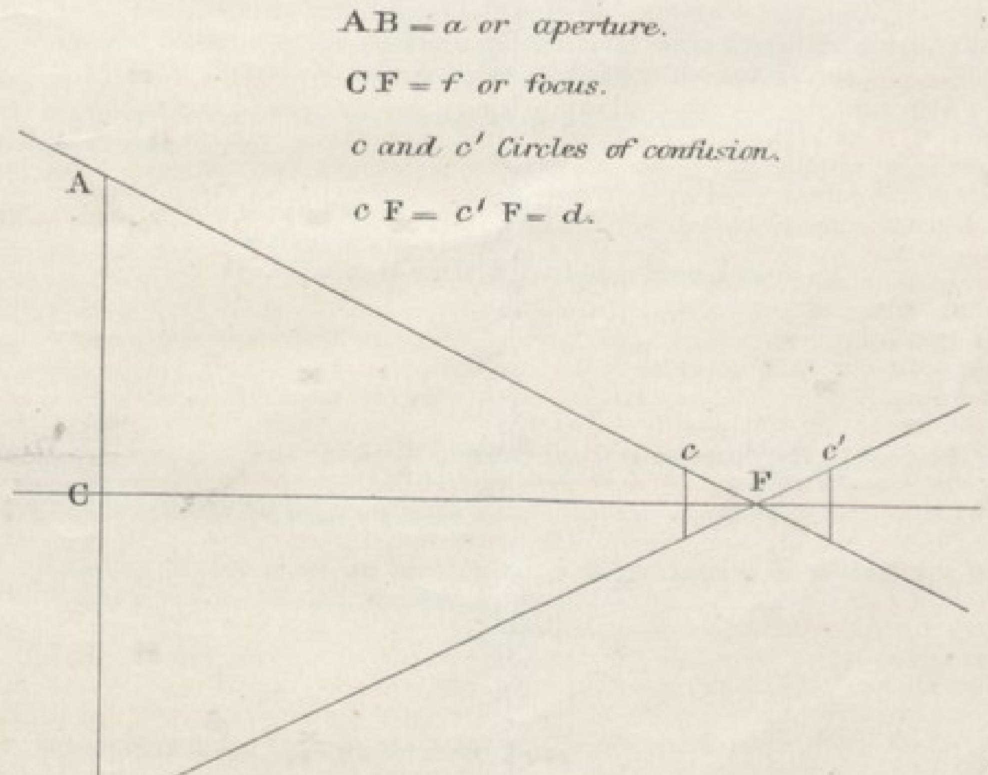


Fig. 2.



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To determine these positions definitely, I think one of the most ready and practical uses of the law referred to is the one I have given in the "Choice and Use of Photographic Lenses." When the focal length of the lens and the distance of the object from the lens on one side in terms of the focal length are known, the conjugate focus is distant the reciprocal of that number of times the focal length from the principal focus on the other side, *e.g.*—

$$\text{If } x = n\,f,\ y = \frac{1}{n} f, \text{ \&c.}$$
$$x\,y = f^2.$$

Hence it is impossible to focus absolutely two separate points of the object in different planes at one and the same time, upon the one plane for the reception of the image, in this particular case, the focussing screen.

Points in one plane only of the object are perfectly rendered in the image, but others may be sufficiently well defined to appear sharp at the normal distance of vision; and this leads me to the *second* point. This allowable difference between absolute sharpness and apparently visual sharpness is what is commonly termed "depth of focus."

It is generally granted that a circle of confusion of $\frac{1}{100}$ inch in diameter is sufficiently small, when viewed at the normal distance of vision (say, 12 to 15 inches, or subtending an angle of less than one second of a degree), to be practically sharp, and it is upon this assumption that the tables published in the "Choice and Use of Photographic Lenses" were calculated. I will now explain, as simply as possible, how these figures are arrived at, and give simple formulæ for calculating—

a. The nearest point in the object for any lens beyond which every point will be sufficiently well defined or "in focus."

b. For determining the distances of objects on either side of the point focussed upon, that will be sufficiently well defined in the image.

a. As before stated, the permissible circle of confusion is not to exceed $\frac{1}{100}$ of an inch, or (see Fig. 2).

If—

a = aperture.
f = focal length.
d = distance of circle of confusion from f, or $= c\,f$.

Then, by similar triangles—

$$\frac{a}{f} = \frac{c}{d};$$

or $c = \frac{a}{f} d$, where c is a constant (viz., $\frac{1}{100}$ inch) and $\frac{a}{f}$ (or the intensity of the lens) is known.

Referring to Fig. 1, all we have to do, y being known, is to find x. It is evident that $y = d$ in this case, and hence

$$y = \frac{\frac{1}{100}''}{\frac{a}{f}}$$

Substituting R for $\frac{a}{f}$, or the intensity, then

$$y = \tfrac{1}{100}\,\text{R}$$

But—

$$x\,y = f^2$$
$$\therefore\ x = 100\ \text{R} f^2 \text{ inches,}$$

And hence the distance sought, viz.—

$$x + f = 100\,\mathrm{R} f^2 + f.$$

Take a simple example, to illustrate this, before proceeding. Focal length of lens = 6 inches, working at an intensity $\frac{1}{10}$. Here—

$$\mathrm{R} = \tfrac{1}{10}$$
$$f = 6$$

Hence—

$$x = 100\,\mathrm{R} f^2$$
$$= 360$$

Or the distance sought—

$$x + f = 366 \text{ inches or } 30\tfrac{1}{2} \text{ feet.}$$

From this it is apparent already that greater depth of focus in a lens can only be obtained by loss of rapidity. The depth varies inversely as the diameter of the aperture, but the exposure requires to be increased by the square of the corresponding gain in depth, *e.g.*, twice the depth can only be obtained by four times the exposure; and, again, the depth varies inversely as the square of the focal length. This disposes of the first and simpler head.

b. To determine the distances of objects on either side of a given point that will be sufficiently well defined in the image, or the depth of focus on either side of the point focussed upon.

This explanation is given for application to portrait lenses, more especially where the ratio of aperture to focus is large, and the distance of the nearest object inconsiderable.

Proceeding as before—

The distance of the object $= x + f$ (x and f are both given); hence y is known, for since

$$x\,y = f^2$$
$$y = \frac{f^2}{x}.$$

Then, for a given distance of object (see Fig. 1)—

$$\frac{a}{f + y} = \frac{c}{d};$$

Or, $$d = \frac{c\,(f + y)}{a}.$$

The two new values of y, say, y_1 and y_2, are

$$y_1 = y + \frac{c\,(f + y)}{a},$$

$$y_2 = y - \frac{c\,(f + y)}{a};$$

Or, $$y_1 = \frac{f^2}{x} + \frac{c\left(f + \frac{f^2}{x}\right)}{a},$$

$$y_2 = \frac{f^2}{x} - \frac{c\left(f + \frac{f^2}{x}\right)}{a};$$

$$y_1 = f\,\frac{\{\,a f + c\,(x + f)\,\}}{a\,x},$$

$$y_2 = f\,\frac{\{\,a f - c\,(x + f)\,\}}{a\,x}.$$

From these quantities, y_1 and y_2, to find the values x_1 and x_2, which are the quantities required—

$$x_1 y_1 = f^2 \text{ or } x_1 = \frac{f^2}{y_1},$$

$$x_2 y_2 = f^2 \text{ or } x_2 = \frac{f^2}{y_2}$$

$$x_1 = \frac{f^2}{f\{af + c(x+f)\}} = \frac{f a x}{af + c(x+f)}$$

$$x_2 = \cfrac{f^2 a x}{\cfrac{f\{af - c(x+f)\}}{a x}} = \frac{f a x}{af + c(x+f)}.$$

$$\text{For depth in front of object, } x_1 + f = f + \frac{f a x}{af + c(x+f)},$$

$$\text{,, behind ,, } x_2 + f = f + \frac{f a x}{af - c(x+f)};$$

Where a, f, x, and c are all known, or can be written, by substituting R for $\frac{a}{f}$ and $(\Delta - f)$ for x

$$\text{Depth in front or behind the point of focus} = f \times \frac{f^2 R x}{f^2 R \pm \frac{1}{100}(x+f)},$$

Expressing Δ = distance of object.

$$\text{Depth in front} = \text{difference between } \Delta \text{ and } f + \frac{f^2 R(\Delta - f)}{f^2 R + \frac{1}{100}\Delta} \quad (2)$$

$$\text{,, behind} = \text{ ,, ,, ,, } f + \frac{f^2 R(\Delta - f)}{f^2 R - \frac{1}{100}\Delta} \quad (3)$$

Example.—Take a lens of 15 inches focal length, intensity $\frac{1}{4}$, and let distance of object focussed for be 24 feet. To find the depth of focus in front of and behind the point focussed upon. Here

Δ = 24 feet, or 288 inches.
f = 15 inches.
R = $\frac{1}{4}$.

Substituting these values in equations (2) and (3), we get—

$$\text{For depth in front} = 15 + \frac{\frac{288}{4} + 273}{\frac{288}{4} + 2\cdot 88} = 274\cdot 7.$$

$$\text{,, behind} = 15 + \frac{\frac{288}{4} + 273}{\frac{288}{4} - 2\cdot 88} = 302\cdot 7.$$

The distance of object focussed for was 288 inches. Hence

Depth in front = 13·3 inches.
,, behind = 14·7 ,,

Now, as it is evident from these results (equations 2 and 3) that depth of focus or permissible indistinctness is entirely dependent upon focal length and ratio of aperture to focal length, it follows that, in comparing lenses of identical foci and intensities, it is absurd to speak of one having more depth of focus than another, if both are perfectly free from spherical aberration, and used under the same conditions. However, I would point out that other defects, particularly curvature of field, are very misleading to photographers generally, and in judging the capabilities of lenses, may have led them to ascribe greater depth of focus to one lens as compared with another, even when examined under similar conditions.

Thirdly and lastly, I wish to explain the meaning of diffusion of focus and its relation to depth of focus.

At the commencement of my paper I spoke of a pin-hole as giving diffusion of focus with uniform depth of focus, resulting from the fact that no one point in the object is represented by one point in the image, but by a small circle, and the resulting picture is produced by a number of these small circles running one into another, and if the aperture can be made sufficiently small, a presentable picture is obtained.

Diffusion of focus in a photographic lens is produced by a certain amount of outstanding positive spherical aberration. This has been explained on many previous occasions; but I hope you will excuse the repetition, as I wish to demonstrate clearly the advantages that do exist when a certain amount of outstanding positive spherical aberration is *obtainable* in portrait lenses of great intensity. I purposely use the word "obtainable" in the sense that no lens of any practical utility should be sent out by an optician, no matter for what purpose, with inherent positive spherical aberration; but there are distinct advantages in employing a lens for portraiture where the spherical aberration referred to as *obtainable* can be *regulated, produced to a definite extent*, and reproduced again *at will*. As before stated, a point in the object can only produce a definite point in the image if the lens be entirely free from spherical aberration; but if the subject chosen be such that the circumstances under which it is to be photographed render it impossible to work within the limits of indistinctness permissible, to produce a general and equal sharpness throughout the whole of that subject, then the advantages of the introduction of positive spherical aberration to produce a general *equality of indistinctness* are apparent, and moreover exceedingly valuable in the hands of an artist.

To make this clearer, suppose Fig. 3 to represent the reproduction of an object O at the image I in a lens entirely free from spherical aberration. The point O will be reproduced by a point at I. Suppose it necessary that two other points in two other planes O_1 O_2 on either side of the object O should be in focus, it is apparent that in the image they must be produced by two circles of confusion i_1 i_2, considerably out of focus. On the other hand, Fig. 4, O being the same object, and I the sharpest possible image produced by a lens possessing a certain amount of positive spherical aberration, a small circle of confusion will be produced at I, two similarly situated points on either side of the object O, namely O_1 and O_2, will produce two other circles of confusion i_1 i_2, similarly to the lens entirely free from spherical aberration. Comparing these two cases, I contend, and I think it is evident, that for artistic and pictorial effect, as in portraiture, that the extreme contrast between absolute sharpness and confusion of image in the first case, as compared with the general softness or equality of indistinctness in the second must be apparent. In the former case there must be a lack of harmony, and the resulting picture would be as incongruous in tone and effect as would be a portion of the careful work of one great artist, such as Meissonier, placed in the centre of a work by a wonderful impressionist, such as Velasquez!

Mr. W. E. DEBENHAM: Depth of focus is a subject upon which photographers have been very much mistaken. It should be recognized that depth of focus is not a function of the lens at all, it is simply a question of aperture. This Mr. Dallmeyer has now admitted. Some twenty-one years ago the late Mr. Dallmeyer claimed for one of his lenses that he had obtained in it greater depth of focus and greater sharpness in the out of focus planes, not relatively, but absolutely, by introducing a certain amount of spherical aberration. The late Mr. T. Grubb, who, I suppose, may be considered an authority of the highest rank, in a paper and diagram on this subject, which appeared in the *British Journal of Photography* of February 8th, 1867, showed that by the introduction of spherical aberration not only was there no greater defini-

Fig. 3.

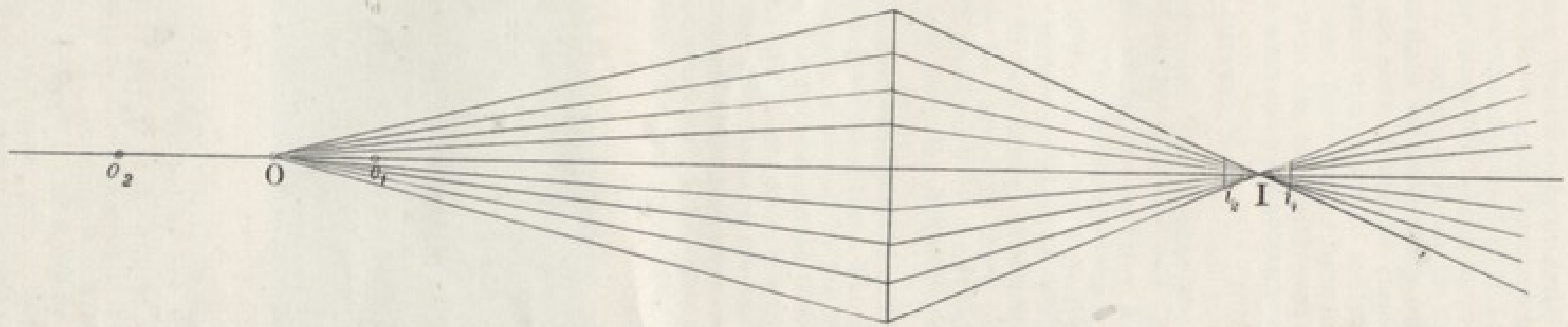
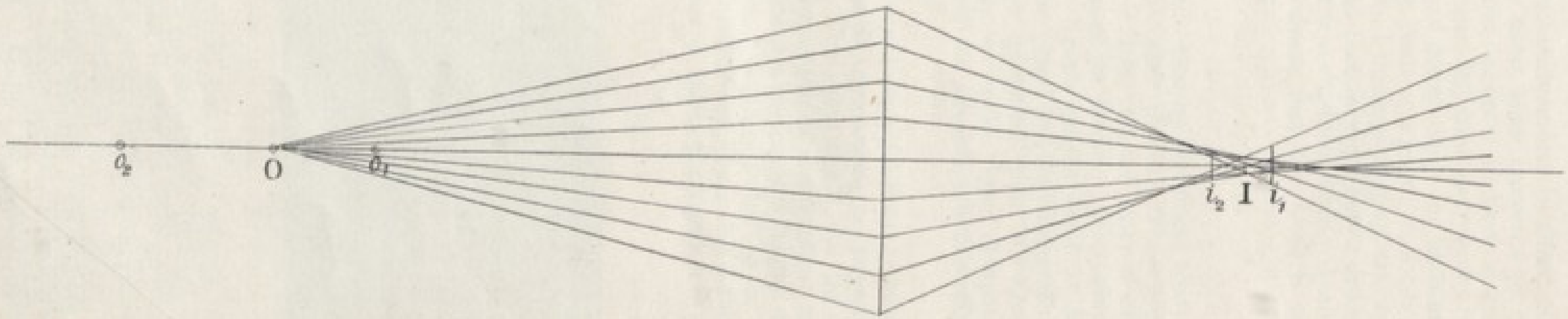


Fig 4.



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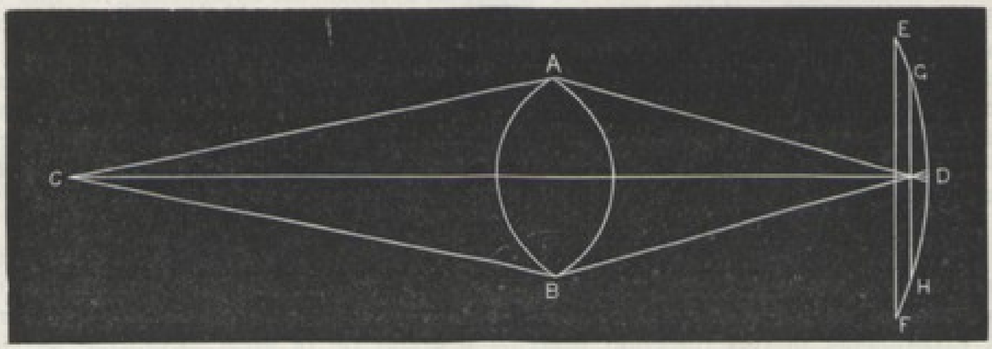
f. 8

tion obtained in the out-of-focus parts, but the definition was less in these out-of-focus planes. Mr. Grubb's statement has, I believe, never been answered or questioned. It consequently comes to this: Is it desirable, in order to have a greater equality in the image throughout, to put up with a worse definition everywhere? In other words, so that we shall not be struck by the bad definition of the parts not in focus, shall we make the whole thing worse—both the parts which are in focus and those which are not? If this be considered desirable, I think photographers should clearly understand it, and not be misled by being told that they got better definition in the use of out-of-focus planes by sacrificing sharpness at the focus itself. I should like to know from Mr. Dallmeyer—yes or no—if he admits that the focus is everywhere inferior when spherical aberration is introduced.

Mr. DALLMEYER: No.

Mr. DEBENHAM: That is what Mr. Grubb showed—that by sacrificing sharpness at the focal point you also sacrificed sharpness in the other planes. There are two things we have to do: one to bring forward any new facts, and the other to expose any old fallacy. The latter sometimes is quite as useful as the former. Photographers have long held the fallacy that there are lenses which have a greater depth of focus, by which they understand depth of definition, than others, and Mr. Dallmeyer has now admitted this to be a fallacy. I would like to read a quotation from Mr. Grubb's paper, wherein he says—"In short, in whatever way we make these comparative measurements, provided it be done fairly, we shall find the advantage with respect to depth of focus to be on the side of the corrected lens."

Mr. G. L. ADDENBROOKE said there was always a good deal of difference between theoretical and practical optics. The depth of focus, as represented on paper, may be quite differently expressed by the photographer. This idea might not be right, but it was in use, and he should like to explain what depth of focus meant to the photographer. While, of course, it is true technically that a perfect lens has no depth of focus in the strict sense of the term, yet the term "depth of focus" is in such general use amongst photographers to express certain qualities of a lens that I think it will hardly be given up. I will try and point out what I think photographers mean when saying that a lens has great depth of focus.



If A B is a convex lens and C an object in front of it, the image will be found at D. Now, as Mr. Dallmeyer has shown us, there is a small space through which E D F may be moved backwards or forwards without the circles of confusion becoming too large to show a blurred image. Now, with a perfect lens of good figure this would permit of the focussing screen coming as far forward as E F, let us say when the whole area between E and F will be in good definition, whereas with a poor lens the figure of which was not perfect like many of the French lenses in the market at present, the focussing screen could only be brought to G H, and so perfect definition,

f. 9v

even if secured in the centre, would not be so good over a large area. To put it simply, photographers, I think, consider the depth of focus of a lens the amount the focussing screen can be moved forwards or backwards after perfect definition has been obtained with full aperture without blurring the image as seen by the eye. This is of course a very long explanation, not taking into account several important factors which go to make up the defining power of a lens.

Mr. DALLMEYER: Do I understand, that you consider depth of focus to be a question of the field over which the lens will work with moderate sharpness?

Mr. ADDENBROOKE: It is curvature of field.

Mr. DALLMEYER: But the depth of focus will come in.

Mr. ADDENBROOKE: Several things come in. It does express something to the photographer when with a perfect lens you get a much larger range than with an imperfect one.

Mr. SEBASTIAN DAVIS observed that definition had much to do with curvature of field in connection with depth of focus. He remembered, many years ago, having several communications from the late Andrew Ross on the subject of lenses. In the first instance he (Mr. Davis), like most practical photographers, took advantage of the ordinary French lenses, and was able to obtain presentable portraits much easier than with the more highly finished instruments of Ross. He found with the half-plate lens, $3\frac{1}{2}$ inches aperture, of Ross, that only certain parts could be obtained perfectly sharp, the focus having to be altered in order to obtain sharpness at the margins, and he had some correspondence with Mr. Ross before they could arrive at a clear understanding, the lenses of this maker being constructed to give extreme definition at one particular point. As we advanced, Dallmeyer succeeded in producing a lens which gave diffusion of focus, which could be increased by placing the front and back combinations further apart. One lens which gave a great diffusion of focus with flatness of field was the triplet, and this was brought about by putting a negative lens in the centre between the two others. The real fact of the case was, as Mr. Addenbrooke had pointed out, that a lens of this kind threw the image nearer the margin in the form of a plane, and with the triplet an approximate approaching to this was obtained. He (Mr. Davis) had occasion to notice the subject for one special purpose, in connection with the exhibition of transparencies on a large scale. Two lenses he had tried—one Dallmeyer's well-known portrait lens B 2, and the other a specially constructed lens, and the result was a better picture, having marginal definition as well as centre definition, was obtained with the former than with the latter. His impression was that by the use of a "negative" lens it was quite possible to throw the image towards the margins further back than was obtainable at the present time.

Sir DAVID SALOMONS pointed out that the result of Mr. Dallmeyer's calculations could be arrived at in a simpler form and without any second manipulation. He did not quite agree as to the advantage of diffusion of focus. He had made a large series of experiments in this direction, thinking it would be of great advantage for large heads, but the moment he attempted to obtain this diffusion, something appeared all over the head which he did not like. When you had an object to deal with approaching the natural size, the only plan was the old-fashioned method of moving the plane of the plate backwards and forwards during the exposure. If the nose were first focussed, and then the eyes, the focus could be kept moving between these two planes, and though the size of the images were not exactly the same, the difference could not be appreciable if suitable stops were used. He had also been told that the same result could be obtained by moving the lens, with the advantage of not increasing the size of the figure. He hoped before long to read a paper at the Society on the subject, from another point of view.

Mr. V. BLANCHARD: I shall not attempt to go into the mathematical aspect of the question, but shall deal with the lens as an artistic instrument, capable of producing artistic results. In regard to depth of focus I am bound to say that from long experience I have found an immense difference in lenses in their power artistically to render the face. Some years ago, anxious to make some large heads, I employed for the purpose the front combination of an old French lens reversed and unstopped. These pictures, taken by a single lens, were shown to Mr. Dallmeyer, and considered very remarkable by him, and he thought the tube, being very long, acted as a stop. I then instanced a lens made by a celebrated maker, Andrew Ross, and I said this lens does not please me. I can understand, I said, its being perfect from an optician's point of view, but it is too perfect for me. It was sharp in certain parts, but by no method of focussing, by dividing the distance between the extreme points, could I get an artistic picture. However it was focussed, that peculiar sharpness was obtained which was so disagreeable. The experience of Mr. T. R. Williams, Mr. Blanchard went to say, was the same, and it was a fact that results could be obtained with a common French lens which were impossible with the more perfect instrument which gave such microscopical sharpness. In regard to perfect lenses, Mr. Blanchard said on one occasion to Mr. Dallmeyer—"If it were not for the enormous exposure, the triplet would be the most perfect of all. Is it not possible to so enlarge the aperture of the central lens so that it may be used as a portrait lens?" Mr. Dallmeyer made the experiments with a large triplet in Mr. Blanchard's possession, and the result was the most perfect instrument he (Mr. Blanchard) had ever seen in his life. This lens gave place to the D series, with arrangement for separating the lenses in back combination. "I am bound to say," Mr. Blanchard continued, "that in doing some 5-inch heads for the Crawshaw Competition, I drew out the dispersing arrangement to its utmost extent, in order to get these heads. However, I carried off the prize, which was more to the point. I speak on behalf of many photographers who are not anxious to regard this as an optician's question. The artist wants to obtain an artistic instrument, and I have found a decided difference between what I received as a lens having depth of focus and what I understand as depth of focus.

Mr. T. R. DALLMEYER, in replying, said: With the exception of the points raised by Mr. Debenham, the discussion seems to have turned on the question as to whether curvature of field is included in the definition of depth of focus, as ordinarily understood. What Mr. Addenbrooke said (as Sir David Salomons pointed out), related to flatness of field and not depth of focus. The views of Mr. Debenham and Mr. Blanchard are entirely in opposition. I was pleased to hear Mr. Blanchard refer to the lenses made many years ago by my father for him and for Mr. T. R. Williams. It is true that my father constructed lenses which gave a considerable amount of spherical aberration to produce this diffusion of focus. The results speak for themselves, and have been widely appreciated. One point with these lenses under discussion is that *used intact* they are *perfectly corrected*, *i.e.*, free from spherical aberration, but they will give the diffusion of focus, *if necessary*, and it can be produced to any extent *at will*. As for Mr. Debenham's quotation from Mr. Grubb, it would not become me now to enter into that matter. I may say that if the plate be drawn out of focus, as Sir David Salomons suggested, a circle of confusion entirely out of focus is produced; but if you have *positive* spherical aberration, you still retain your *point*, though as a halo of softness. That is the difference between the two methods. All these halos run one into another, and produce a general softness, which, I believe, to be a better form of obtaining the desired effect than any other.

The PRESIDENT remarked that it was always a great thing to have papers which contained the results of practical experience brought under the notice of the Society. He was very much struck by the reference made by Sir David Salomons to shifting the focus during the exposure. He recollected that the method was practised by Claudet, but thought it had been quite forgotten.

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A vote of thanks was then passed to Mr. T. R. Dallmeyer, for his paper.

The PRESIDENT then drew special attention to the fact that the next Meeting would take place on the third Tuesday in April, and adjourned the Meeting to—

TUESDAY, APRIL 17TH.

MONTHLY TECHNICAL MEETING.

Tuesday, February 28th, 1888.

Captain ABNEY, R.E., F.R.S., Vice-President, *in the Chair.*

The following Paper was read :—

SILVER-GOLD PRINTING BY DEVELOPMENT.

By E. HOWARD FARMER, F.C.S. F.I.C., and H. KNEEBONE TOMPKINS, B.Sc.

IN reviewing the progress which has been made in photography during the past ten years, it is apparent that whilst a complete change has been effected in the method of obtaining negatives so that the photographer is comparatively independent of the amount of light at his disposal for this purpose, yet the method of printing, by which at least nine-tenths of the positive prints are obtained, remains the same as that in vogue forty years ago, with the result that during the dull weather, of which we in England have so large a share, the photographer is compelled to keep his clients waiting one, two, or three weeks before completing their orders.

We think it will be conceded that this loss of time is a great detriment to business; but notwithstanding this, very few, if any, serious attempts appear to have been made to effect in positive printing what has been done so well for negative taking.

Some time back it occurred to us that a careful investigation of the subject would reveal a method or methods by which this delay could be overcome, and after some preliminary experiments it became evident to us that the emulsion method, which has given such valuable results in the negative process, was also the method to employ for getting over the difficulty. And experience has confirmed this.

At the present stage of our work we are acquainted with at least four distinct emulsion processes by which prints similar to those on albumenized paper can be obtained, but more expeditiously. These are :—

1. An albumen process for printing out from four to eight times as sensitive as the ordinary method.

2. A rapid albumen process for printing by development and also giving prints identical in appearance to ordinary albumen prints.

3. A very rapid gelatine process for printing by development.

4. A collodion emulsion process for printing out and for development.

As may be gathered from the title of the paper, we have the honour to bring to your notice "Silver-Gold Printing by Development."

It is the second of these processes we propose to demonstrate this evening, as it is the one to which we have given the largest attention, but before proceeding with the process we should like to explain why we call it Silver-Gold Printing.

At a very early stage of our inquiry we found that ordinary or so-called silver prints, *i.e.*, albumen prints, at any rate as far as their permanence, beauty, and colour are concerned, are very much more gold than silver prints (a print was passed round from which the silver had been removed), and in our process, although the reduced silver compound is probably in a more stable condition than in an ordinary print, the gold when it is required to print the exact photographic tint is equally important. Hence, to distinguish it clearly from the bromide paper process, which is truly silver-printing by development, and as the gold is equally important for our process as for albumen prints, we term it silver-gold printing by development. We were aware at starting of other processes for obtaining albumen photo-tints by development.

Examples :—

Hardwicke's process.

In this process salted albumenized paper is floated on weak silver nitrate and developed by acid development.

Mr. Wilkinson's process.
Mr. Wm. Brook's collodion emulsion.
Commercial gelatine papers.
Doubtless emulsion processes.

We carefully examined these processes alongside preparations of our own, including both bath and emulsion methods, with the following results :—

That in no case were we able to obtain anything like the brilliancy and purity of surface colour so characteristic of ordinary prints, but that along with the development of the red compound of silver there was always a tendency to a muddiness or fogginess of tone when examining the results by reflected light.

And here we wish to draw attention to the remarkable differences in result given by these processes when examined by transmitted and reflected light respectively, a point which we believe has not formerly been noticed. That whereas it was comparatively easy to obtain gorgeous hues by transmitted light, yet in proportion to this gorgeousness by transmitted light the prints were dull and muddy by reflection, and in many cases when the colour by transmitted light was weak and dull the reflected colour was brilliant and vigorous (examples were passed round).

There appeared to be three explanations of these contrary effects :—

I. The reduced compound was dichroic, or in other words reflected rays complementary to those transmitted, a compound in fact similar in properties to that which gives pink and green fog in dry plates, and in many cases the appearances obtained made this theory appear the correct one.

But an ordinary print is red both by transmitted and reflected light, and we considered and do consider that the red compound obtained by development is practically the same substance as that produced by light, and therefore that what was done by the one was possible also by the other. Our experiments have confirmed this.

There is, it is true, in ordinary printing an effect sometimes obtained very similar to the one I have been describing. I mean what is called bronzing—when you get a greenish metallic surface by reflected light instead of the usual red, although by transmitted light the colour is bright red.

We can now, understanding more about the subject, obtain bronzing in the shadows by development identical in appearance to that produced by exposure.

II. A second possible explanation was that the want of surface colour was due to the image not being sufficiently on the surface of the film, as by development one can readily understand that the image would have a greater tendency to extend through the thickness of the film than by simple exposure.

A modification of this theory was want of homogeneity in the film or of exposure of the sensitive salt to light, a plausible explanation from the fact that collodion films give the muddiness in an extreme degree. A great many experiments were made on the basis of this theory and its modification, with the result that while demonstrating the importance of keeping the image on the extreme surface of the film, they did not confirm it as an explanation of the difficulty.

III. A third explanation which occurred to us was that the two actions were separate and distinct from one another, that is to say, two actions went on during development, one giving the red colour and the other the muddy colour, and experiment has confirmed this theory.

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Amongst others we made and confirmed the following observations bearing on this point :—

1. That the colour was usually red at the early stages of development and then passed into the muddy stage.

2. That if the development was prolonged the image became muddy by transmitted light as well as by reflected light.

3. That the longer the exposure the further could the development be carried without getting the muddiness, but (as a note to this observation) an over-exposed appearance was at the same time given to the image.

4. That salts, such as soluble chlorides, bromides, citrates, &c., added to the developer acted powerfully in retarding the appearance of the muddy stage, and also counteracted in great measure the over-exposed appearance previously mentioned.

5. That neither the red colour nor the muddy colour were distinctly marked stages of development, but that the colour was first a light yellow and gradually darkened as the development proceeded.

We believe that Mr. W. H. Burton and others have made similar observations.

From the first of these observations, that the muddiness came at the end of development, we concluded that the muddiness in development is the analogue of bronzing in ordinary printing, and we further concluded from its appearance and from analogy that the muddiness was due to a portion of the film being reduced to metallic silver, or a very low sub-compound (a print was shown developed half red and half green). Moreover, we found that the development could not be kept at the red stage, but usually passed into the dark or muddy stage. With a long exposure the image was weak and red, with a short exposure the image was more vigorous, but passed into the dark stage. This even held good with a large quantity of restraining substance.

6. We finally observed that even when the red colour was obtained it had the drawback of disappearing almost entirely in the fixing bath, the residue being of a bright mustard-yellow colour, quite unsuitable for the production, in conjunction with gold, of the ordinary photographic tone. In connection with this observation we obtained the interesting result that the shorter the exposure with which the red colour could be obtained the more perfectly it withstood the fixing bath.

It appeared apparent from the effects described that if during the development the appearance of the muddy or dark deposit could be prevented or lessened, a much more successful result could be obtained; for obviously a shorter exposure being given, the image would be bright and vigorous as well as red, and at the same time much less, if at all, soluble in the fixing bath, and we experimented with this object in view with emulsions and preparations of various kinds.

Amongst the numerous salts of silver tried were silver chloride, bromide, citrate, phosphate, and carbonate. We found that with the haloid silver salts a red colour could generally be obtained if sufficient exposure were given, the strength of the developer being reduced in proportion. Also that, at least in the case of silver chloride, the state of aggregation of the sensitive compound had very little influence on the colorific effects, grey digested chloride giving a colour only slightly darker than that of the undigested salt.

Of the remaining salts the citrate gave a yellow-red image, whilst the phosphate gave a not unpleasing sepia tint. This last salt has the peculiarity of not being fixable by hypo but of being readily fixed by dilute ammonia or nitric acid, the print having in the latter case the appearance of a toned print. The carbonate gave a fogged image.

Amongst developers tried may be mentioned ferrous salts and pyrogallol, hydrokinone and some of the tannic acids in conjunction with ammonia, caustic potash and soda, and their carbonates, and the hydrates of some of the alkaline earths.

The reducing agent employed has not much influence on the colour, the effect depending more upon the strength used. Difference of temperature of the developer had also very little effect except at temperatures below 37° F, when the development was arrested.

Various restrainers were used, viz., potassic chloride, bromide and nitrite, ammonium oxalate, sodium acetate, potassic citrate, potassic chlorate, and salicylic acid.

We found generally that the more the developer was restrained the redder the colour, the general effect being, as might be expected, to increase the necessary time of exposure, time of development, and harshness of image, the particular restrainer used being unimportant.

Finally, we did not observe any distinct difference by exposure under similar conditions to different coloured rays.

We tried various substances for suspending the sensitive salts, viz., albumen, gelatine, gum, gluten, and collodion. No great differences were observed except such as were due to the mechanical differences of the substances; thus, albumen was obviously most suitable for prints, collodion developed and toned readily, whilst the gelatine preparations were the most sensitive.

We will now demonstrate the manipulation of the paper. This is made of two kinds, one with a matt surface, the other glossy (specimens passed round).

The paper is exposed under a negative in a printing frame in the usual way, from fifteen seconds to five minutes to diffused daylight, according to the state of the weather, being the necessary exposure. A faint visible image is produced, and this is a valuable guide, as the exposure is correct when all the details are just visible (specimens of exposed prints passed round). The prints are then soaked in water and developed (development of prints shown). The prints are then toned in any of the usual toning baths and fixed (specimens of fixed prints toned and untoned passed round).

In conclusion we would draw your attention to some prints produced by this process, which we think are sufficient to show that it is possible to produce by development prints in every way equal to the finest silver prints in as many hours as are now required days, and to thus remove, in our opinion, the greatest hindrance under which the photographer labours.

Mr. E. Howard Farmer, assisted by Mr. Tompkins, then passed some already exposed prints through the developer, which it was stated was hydrokinone (from a formula already published by Captain Abney). There was no especial advantage in this developer over any other, except in its cleanliness; and the strength was regulated in such a way that the prints should take from four to five minutes for its action.

With respect to the practical working of the process it was stated that the paper should be exposed until all detail was visible; the specimens shown and ready for development had been exposed to daylight from about four seconds to one minute. Particular attention was directed to the fact that the finished prints after rolling had precisely a surface similar to that of albumenized paper; also some examples were shown upon a matt surface, and of a colour so like a platinotype print that it was difficult to distinguish one from the other.

Mr. G. SCAMELL enquired what was the composition of the emulsion.

The CHAIRMAN observed it had been stated by Mr. Farmer that the developed image was of the same composition as that of albumenized paper, but he thought it would be a difficult matter to get a similar image by development. In the usual result on ordinary albumenized paper there was a combination of chloride and albuminate of silver, which can be tested by sulphuretted hydrogen.

Mr. E. HOWARD FARMER had not experimented in that direction, but he did not think that prints produced in this way would be acted upon by sulphuretted hydrogen.

The CHAIRMAN said if such a theory was good, then it could not be a fact that the composition of these developed images was similar to the ordinary albumen print.

Mr. W. COBB enquired if special negatives were necessary.

Mr. E. HOWARD FARMER said that in all printing processes the best results would be from good negatives; but by this process, by modifying the treatment, better results could be got from weak and over-dense negatives than by the old albumenized paper process.

Mr. W. E. DEBENHAM remarked that there was very little matter for discussion, as the details of the process were not before them. Mr. Farmer had said that the sub-division of the chloride had no effect upon the red colour. How could that be, as his (Mr. Debenham's) experience was quite the contrary.

The CHAIRMAN said he entirely agreed with Mr. Debenham; silver could be either in the red or blue state. When red, it did not arise from the fineness of the sub-division of the particles. His own theory was that in red chloride of silver the particles were not necessarily finely divided, but was a result connected with the absorption of light; pigments also possessed a specific absorption.

Mr. E. HOWARD FARMER said he did not know much about molecular differences but both himself and Mr. Tompkins found that digestion of the silver chloride made very little difference in the colour of the print.

Mr. G. L. ADDENBROOKE enquired about the keeping qualities of the paper.

Mr. E. HOWARD FARMER stated that some paper prepared three months since was still as good as when first made.

A flash lamp, designed by Mr. A. James, was then exhibited, where magnesium powder was propelled through the centre of a gas flame in an Argand burner, by blowing through a tube, the result being a rather large flash of actinic light, which appeared to be very effective.

NOTES.

PHOTOGRAPHIC CONFERENCE OF THE CAMERA CLUB.

The Second Annual Conference was opened on Tuesday, March 13th, in the Theatre of the Society of Arts, under the presidency of Captain ABNEY, R.E., F.R.S., the President of the Club.

After a few introductory remarks from the Chairman, *Dr. D. G Thomson* read a paper on "The Application of Photography to Medicine and Allied Science." The author said that it was only within the last ten years that photography had been applied to medicine. It was of very great use in securing a record of the appearance of sections of whole or parts of bodies. It had been used to obtain accurate drawings of the vocal cords in the larynx by Brown and Behnke, and the back of the eye has been photographed through the pupil. In surgery photography was extensively employed to illustrate deformities, injuries, and tumours in living subjects. In medicine the departments in which it was of the greatest service were skin disease, insanity, and the study of bacteriology. In conclusion the author strongly urged the advisability of all medical institutions being furnished with a photographic department, or at least a well-equipped dark room.

Mr. H. Trueman Wood, M.A., then read a communication entitled, "Some of the Applications of Photography to Scientific Purposes." In the course of his paper the author referred to the use of photography in astronomy, drawing special attention to Mr. Common's work on the nebula in Orion, and to the proposed photographic survey of the heavens. He then mentioned the uses of photography in meteorology, amongst them the determination of the distance of clouds and the measurement of solar radiation. He drew attention to the use photography has been put to in spectroscopy, anthropology, microscopy, and geography.

The next paper was "On Single Lenses corrected for Architecture," by *Mr. J. Traill Taylor*. In the course of his address the author said :—

Over thirty years ago the attention of opticians, both amateur and professional, was given to the best means for effecting a cure of distortion; but of all those no one, so far as he was aware, wrought so assiduously in this direction as James T. Goddard. A professional optician, he was also a devoted experimentalist, and introduced many different forms of lenses, which, owing to his commercial resources being terribly limited, caused by his deficiency in commercial tact, were not extensively manufactured or widely known, the more especially as in those days manufacturing opticians appeared to tacitly recognize a species of proprietary interest or copyright in the productions of contemporary producers.

The condition under which rectilinearity is obtained is this—that every ray which enters a lens shall emerge from it in a direction parallel to that of its entry. Goddard, in his compound landscape lens, discovered that by associating with the principal achromatic lens a convex and a concave of such relative power as to neutralise each other, while the focus was not altered, a cure of distortion was effected. This discovery and that of his double periscopic landscape lens dates from January, 1859. This latter lens, the double periscopic, although only a few were sold and now almost forgotten, has in it the element of goodness; a fact evinced from the circumstance that an eminent optician to whom its existence was evidently unknown has, within the past few weeks, introduced and obtained protection for one similar.

Mr. Taylor submitted two untrimmed large round periscopic spectacle glasses, one being a positive of eight inches focus, the focus of the other being similar, so that when placed together their power is *nil*, or practically so. By separating them to the extent of half an inch or an inch, with the convex side of the diminishing lens next to the concave side of the positive or magnifying one, it was at once perceived, when looking at any object through them obliquely, that a great degree of displacement of such object takes place; and when this combination is placed at the position of the diaphragm of the lens, the oblique rays are transmitted to the objective with such a degree of increasing expansion as to overcome or neutralise the contraction or compression that would otherwise take place in the absence of such an expedient.

The distance apart of the two correcting lenses should be determined by trial, for it is not arbitrary except for any one objective. It is desirable that they be mounted in the following manner:—Procure a piece of tube about an inch long, and of such diameter as to fit into the hood of the lens. Into the end of this is set the concave lens. Into the outer end fit the other lens, under such conditions as to provide for a slight movement, so as to increase or diminish the separation.

One such corrector will serve for a great variety of lenses of different foci, to which end it is desirable to have the means of effecting the separation just mentioned, because such is their power for correcting distortion that by their adjustment not only may the distortion of contraction—or barrel-shaped distortion—be cured, but even that of an opposite nature be induced.

The author did not advise the employment of this corrector for either landscape, portrait, or group work, when a single cemented lens was made use of for such purposes; for the fewer the reflecting surfaces in an objective the better, but only when the photographer was confronted with an architectural difficulty in the form of a building which is to fill, or nearly fill, his plate. Drawing his corrector from his pocket, and inserting it in front of the lens diaphragm, he thus extemporises a lens which is quite free from distortion. As the cost of these lenses is so little (2*d*. each was the price paid) one can afford to keep a few beside him for experimental purposes.

In working with them the author had not found any disturbance of the chemical and visual foci, but the resulting pictures have invariably been sharp. This, too, was the experience of the late Mr. Cole, the architect, who always employed lenses corrected in a manner analogous to that now recommended, and his work was noted for its sharpness and delicacy.

But it might be said this was merely a description of a triple lens, such as at one time was a commercial production. In one sense it is; but whereas the triple was an objective complete in itself, each of its components being achromatised, this fulfilled the function of the triple at a nominal cost, was applicable to any lens, and leaves the landscape lens always ready for fulfilling *its* special function in pure landscape work. Still, it was only an expedient for those who did not possess special appliances for special work, which, of course, are always to be preferred.

Mr. G. S. Waterlow then read a paper on "Modern Photographic Engraving and Printing." The author divided modern photographic engraving and printing processes into four heads :—

1. *Typographic Blocks*, which are etched in relief, and printed from the surface in an ordinary printing-press.

2. *Plates*, which are etched in intaglio, and printed from the depth, as in the case of an engraved copper-plate.

3. *Woodbury Blocks*, which are impressed with an intaglio image by a gelatine relief under great pressure, and printed from by a special press.

4. *Collotype or Albertype Plates—i.e.*, a surface of gelatine on glass or metal, which has been sensitised, acted upon by light through a negative, and is printed from in a hand or steam-printing press.

Mr. Waterlow subsequently described in detail the preparations of blocks of each kind for printing purposes.

The next communication was on "Weights and Measures and Ten per Cent. Solutions," by *Mr. G. Lindsay Johnson*. The author compared together the avoirdupois and apothecary systems of weights.

In favour of the apothecaries' weights he put forward the following points :—

1. The total absence of fractions.

2. The convenient weight of the drachm of 60 grains, standing as it does between the grain and the ounce, and preventing the grains from running into the hundreds.

3. The fact that the apothecaries' ounce is in official use in the United States and Russia.

4. Most of the photographic formulæ are given in the apothecaries' weights, and that these weights are recommended in most books.

5. The scales sold at photographic stores always contain apothecaries' drachm weights.

6. The drachm is very nearly four grammes (3·88), *i.e.*, an error in excess of only 1⅗ grains, or less than three per cent.

7. In making up ten per cent. solutions it is very convenient to adopt six grains to the fl. drachm, and six times eight, or 48 grains, to the ounce of 480 grains.

In favour of the avoirdupois weights he stated :—

1. That drysalters and wholesale chemists *always* sell drugs by avoirdupois, and never by apothecaries' weight.

2. That chemicals other than those made up in prescriptions are invariably sold by avoirdupois.

3. The solid and the fluid measured ounce (of distilled water) both weigh 437½ grains. This is especially convenient in *weighing* out fluids.

4. The ounce weighs exactly 7,000 grains.

5. In many shops, especially in the country, the only weights which are kept are avoirdupois.

He then suggested that the grain and the drachm (of 60 grains) should be retained and used for weighing out salts; but that in writing formulæ they should be expressed in grains and avoirdupois ounces (of 437½ grains) only; in other words, all solids should be weighed by the avoirdupois grain, ounce, and pound only. Also that the avoirdupois drachm of 27·34 grains be never mentioned, and when a drachm is mentioned it should signify 60 grains. In order to prevent possible confusion the author suggested for those who preferred to use apothecaries' weights when they are intended, the word drachm, or the signs ʒ and ℥ should be used, but when avoirdupois weights are intended, the sign oz. only be used, and never ʒ or ℥ or the word drachm. As regards the grain, there can be no mistake, as its value is always the same.

With regard to ten per cent. solutions he proposed that the salt should be weighed out in grains, placed in the measure and water added up to the mark, and he proposed that *whenever a ten per cent. solution is described it shall be understood to mean that each fluid-measured drachm of the solution shall contain* 5½ *grains of the salt, and that each fluid ounce by measure shall contain* 44 *grains, and that if the metric system be employed, that each* 10 *cc. of the solution by measure shall contain one gramme of the salt. And in*

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order still further to avoid confusion, he recommended that the word drachm be omitted when referring to solids, and that whenever ounces be mentioned, if fluid ounces, 480 *minims be understood, and if solid ounces,* 437½ *grains.*

The next paper, which we reproduce in extenso, was on "A Recent Improvement in the Platinotype Process," by *Mr. W. Willis.*

As it is abundantly evident that many to whom the practice of the Platinotype Process is familiar are unacquainted with the principles upon which it is based, and as, moreover, I am compelled in this paper to make frequent reference to these principles, it will, perhaps, be better to explain them at the outset, in the meantime claiming the indulgence of those to whom they are well known.

The discovery of the reduction of ferric oxalate by the action of light was made, I believe, towards the end of the last century. Most photographers are aware that if paper be coated with this salt, ferric oxalate, and then exposed to light under a negative, a visible image will be formed upon it. This image consists of ferrous oxalate, more or less mixed with unaltered ferric oxalate. Now, the ferrous oxalate forming this image is a powerful reducing agent, and its reducing action can be made apparent in an easy manner by applying to it solutions of salts of various metals, such as silver, gold, &c. These pieces of paper have been coated with ferric oxalate and exposed to light under a negative; they now exhibit faint images, consisting mainly of ferrous oxalate. On floating one of them in a solution of ammonia-nitrate of silver, you will observe that a blackish image of reduced silver occupies the place formerly held by the faint ferrous image.

In the same manner, by floating another piece on a solution of chloride of gold, it will be found that a gold image replaces the ferrous one.

Reasoning by analogy, it seems fair to conclude that on applying to a piece of this paper a solution of chloride of platinum in a similar manner an image will be produced in platinum; but on making the experiment it will be seen that such is not the case. Platinum requires stronger and more coercive measures. Now, if we could place some of the ferrous salt forming this image into test-tubes, we could more conveniently experiment upon it.

You will observe that we have hitherto obtained this ferrous salt in the form of an image by the action of light upon ferric oxalate; but there is a much simpler method of forming the salt in larger quantities, and entirely by chemical means—namely, by mixing solutions of ferrous sulphate and oxalic acid. This mixture immediately throws down a precipitate of the salt, as a lemon-yellow powder. I have here some of the salt thus made, and subsequently washed and dried.

Now, into each of these test-tubes is placed a little of this salt, ferrous oxalate. Into the first tube I will pour a solution of silver, into the second a solution of gold chloride, and into the third a solution of platinum chloride. You will notice that a very rapid reduction of the metals takes place in the tubes containing the salts of silver and gold, but no trace of reduction in the third tube containing the salt of platinum. Even on boiling the ferrous oxalate in contact with platinum chloride, little or no reduction takes place.

In 1873, when making this experiment for the first time, I was greatly puzzled by the obstinacy with which the metal refused to be reduced from the platinum chloride. After a time I came to the conclusion that could the ferrous oxalate be dissolved in some solvent, reduction of the platinum chloride would be effected. My efforts to find this were unsuccessful until a note by a French chemist led me to try the neutral oxalate of potash. I had to make this salt. It could not then be procured in London. On trying this salt my expectations were realized, and the platinum was instantly reduced. Into this tube containing ferrous oxalate a solution of potassic oxalate is placed. On heating the solution the ferrous oxalate is dissolved, and now, on dropping into the warm solution some platinic chloride, it will be seen that platinum black is thrown down.

This experiment shows that the ferrous salt, which by action of light is formed on paper which has previously been coated with ferric oxalate, if formed in larger

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quantities by chemical means and then treated in a test-tube in the manner described, is capable of reducing a salt of platinum. The problem, then, is to find out how to make the reaction take effect, not only in a test-tube, but on paper which bears a ferrous image, and to secure so rapid a reducing action that the platinum shall be reduced by the image before the latter has been dissolved away by the liquid applied. It was by a test-tube experiment identical with the one just shown you that the possibility of inventing a platinum printing process first presented itself to me. Indeed, before my test-tube was cool, more than one method of working suggested itself. A note is well placed here. I have suggested that the use of the potassic oxalate is merely as a solvent of ferrous oxalate, and this is the view I undoubtedly held at the time this experiment was made. But I am convinced that this is not its only office; it is, *per se*, a reducing agent, and it very probably acts in increasing the reducing action of the ferrous salt.

Previous to this date, 1873, several processes with iron salts had been discovered by Sir John Herschel and others; in these processes paper was coated with ferric oxalate, citrate, or tartrate, then exposed to light, and then developed in solutions of ammonia, nitrate of silver, chloride of gold, ferrocyanide of potassium, and others.

Two of these processes have just been shown to you—namely, those with ammonia, nitrate of silver, and chloride of gold. In all these processes, as far as I am aware, a solution of the salt of the metal in which the image was to be enveloped was applied to the iron-coated paper after the latter had been exposed to light. I then very naturally attempted my first experiment in platinotype in a similar manner by coating paper with ferric oxalate, exposing it to light, and then developing it in a solution of potassic oxalate containing a salt of platinum. But, except from a scientific point of view, my results were valueless, for I obtained a picture in which only the deep shadows were developed, and these very, very feebly.

This is the method which, now made a successful one, I wish to introduce later on. This failure led me to try every conceivable combination of the chemicals and sequence of the operations.

But before proceeding I must here allude to another discovery, but for which platinotype would not have possessed any practical value. All my early experiments were naturally made with platinic chloride ($PtCl_4$), but by its aid nothing but hard results could be obtained, quite devoid of half-tone. It then occurred to me that by using a salt of platinous chloride ($PtCl_2$) only two atoms of chlorine would have to be removed, instead of four atoms, as when ($PtCl_4$) is employed—or, to state it differently, the ferrous oxalate would have less work to perform on ($PtCl_2$) than on ($PtCl_4$). After a troublesome operation, I made some potassic chloro-platinite, a double salt of potassic chloride and platinous chloride, and, by substituting this for the platinic chloride previously used, the results obtained were, as anticipated, full of half-tone.

At this stage, in 1873, I had discovered* all the essential elements of all modifications of the platinotype process. These are:—

1. Ferric oxalate as a sensitising agent on which the light acts.
2. Salts of platinous chloride, from which the pigment, platinum black, was to be obtained.
3. Potassic oxalate, which conveniently may be termed the developing agent.
4. Salts of lead and of mercury as aids to reduction.

Now, as I before stated, that having failed in the oldest and most natural mode of working—namely that in which the paper is sensitized with ferric oxalate, exposed and then developed on a solution containing potassic oxalate and potassic chloro-platinite—I proceeded to try every possible combination.

The following are amongst the methods I tried:—

1. Coating with ferric oxalate and platinum salt, exposing to light, and then developing on potassic oxalate.

* This wording may be misleading. I should have stated that I had at this time recognized all the essential elements, and that I had discovered Nos. 2, 3, and 4.

f. 15r

2. Coating with ferric oxalate, platinum salt, and potassic oxalate, and printing out or developing on hot water.
3. Coating with ferric oxalate and potassic oxalate (or the double salt), and then developing on a solution of potassic chloro-platinite.
4. Coating paper in the same manner as last, and after exposure to light applying to it, under pressure and heat, another piece of paper which had been coated with potassic chloro-platinite, and then allowed to become almost surface-dry. By this method it will be seen that the image obtained on the iron paper was used for the purpose of producing an image in platinum on another piece of paper. And many other methods with which I will not now trouble you.

Of these methods the only one which seemed really promising was the first, for one form of which I took out a patent in 1873. This, however, was a very complicated process, and extremely difficult to work. This method in its perfected form became the method now almost universally practised.

As these methods, however, lie outside my present subject, I have merely alluded to them as helping to illustrate the historical development of the process. Having now finished the brief sketch of the principles underlying the process, and the earlier methods discovered for working, I will proceed to the main subject of my paper—namely, the new platinum-in-the-bath method. I have already stated that my very first experiment in platinum-printing was by this method.

Its failure resulted from the fact that the ferrous image formed on the paper was dissolved away when treated with the platinum-developing solution before it had time to reduce the metal from the platinum salt.

It is evident that to make such a method successful one of three things must be done—either the reducing power of the ferrous image must be increased, or a developer must be used from which the platinum is more easily reduced, or the ferrous image must be rendered partially insoluble in the developer until the reducing action is complete. In the modification about to be introduced I have effected the desired result by the first-named plan—namely, by increasing the reducing power of the ferrous image.

Now, in my note-book for 1879, I find the following experiment recorded *:—"Here success was probably due to the action of the mercury salt in increasing the reducing power of the ferrous image, or by causing the reducing action to take place so rapidly that it was completed before the ferrous image was dissolved away."

Between 1879 and 1886 I find two other records of experiments of a similar nature, and it now appears to me foolish that they were not followed up; but, as a matter of fact, my mind was so prejudiced against this method by the many early failures, that the experiments were almost forgotten. In 1886, however, I again made some experiments in the same direction, which were so successful as to set me to work seriously. In this paper I propose to describe only one form of the platinum-in-the-bath method, namely, the modification founded on the before-mentioned experiment made in 1879.

This method may be thus described :—Paper is coated with ferric oxalate and a small quantity of mercury salt, then exposed to light, and afterwards developed on a cold solution, containing potassic oxalate and potassic chloro-platinite.

The solution of ferric oxalate employed is the same as that used in the present process, both as to its strength and acidity.

In each ounce of this ferric oxalate is dissolved from 1 to 1½ grains of a salt of mercury, preferably the chloride.

Paper is then coated with this solution in such quantity that each square foot of surface will contain about 13 grains of ferric oxalate and one-tenth of a grain of mercuric chloride.

It is then very perfectly dried, exposed to light under a negative, and then

* Paper was coated with ferric oxalate and mercuric chloride, exposed to light, and then developed on a boiling solution containing oxalate of potash and potassic chloro-platinite. Results, vigorous and deep black in the shadows, but muddy in the high lights.

developed on a cold solution, containing from 30 to 120 grains of oxalate of potash, and from 5 to 15 grains potassic chloro-platinite. The development proceeds sufficiently slowly to allow of its being watched and stopped by immersion of the print in the acid clearing-bath as soon as the desired strength of the deposit has been attained.

It is certainly very strange that so small a quantity of mercuric salt should suffice. The quantity of this salt employed it is very important to limit to the proportion I have named, for if the amount be much increased, all artistic value is destroyed by the blocking up of the shadows, which become opaque and dead.

Now as to printing and development. The printing is done in a printing-frame in the usual manner. The image is at least as visible as in the present process, though it is not generally necessary to print so deeply or so strongly. That the requisite exposure is most certainly less than would be the case ordinarily with the ordinary paper is undoubtedly true, but it is impossible at present to form a correct estimate of the gain in this respect.

After the paper has been exposed, it may be kept several days before development, without any visible deterioration.

In developing these prints, many variations may be made, both in the constitution of the developer and in the method of applying it. Various proportions and amounts of oxalate and of platinum salt may be used. I have, however, found it advisable to use not less than six grains of the platinum salt to each ounce of the developer. A good average strength is nine grains per ounce. The strength of the oxalate of potash solution may be varied between 30 and 120 grains per ounce.

With a strong solution of the oxalate very cold tones are obtained; with weaker solutions, warmer tones. A good average is 50 grains of the oxalate to each ounce of water.

This bath may be used either acid, neutral, or alkaline, but my experience is not sufficient to enable me to state which is the best state; but when the bath contains only a small quantity of oxalate of potash, it seems to be very advantageous to use a strongly acid solution. The constituents of this developer, when mixed in solution, undergo a slow mutual decomposition; hence it is necessary to mix them not too long before use. But this decomposition does not appear to affect the action of the developer until after the lapse of many hours.

In order to prepare the developer in an easy manner, a good plan is to keep stock solutions of the oxalate of potash and of the platinum salt. A good strength for the former is one pound of the salt dissolved in 54 ounces of water, and for the platinum salt 56 grains dissolved in one ounce of water.

Of the many methods of applying this developer, perhaps the most generally useful is by floating. The print is floated in the manner usual with platinotype prints, and the print may be allowed to remain floating on the surface until complete development has been effected; but I prefer to remove the print as soon as it has been well wetted, and then to hold it in my hand, carefully watching the progress of development until the right point has been reached, when I immediately plunge it into the acid clearing bath.

Instead of holding it in the hand it may be laid on a piece of glass or other convenient support, and then by means of a brush wetted with the acid clearing solution, the latter may be applied to any parts which it may be advisable to prevent from reaching their maximum intensity.

For very large prints, perhaps the best and most economical arrangement is to apply the developer by means of flannel-coated rollers.

The developer may also be applied very well by means of a spray-producer, or it may be brushed on by a camel-hair brush. This brushing method might be available to an artist. The clearing, washing, and drying operations do not differ in any respect from those ordinarily employed. Prints made by this method are usually characterised by a much greater transparency in the shadows; indeed, as prints on matt paper, I might be permitted to say, a marvellous transparency. Now, this is perhaps one of the most important, and certainly the most difficult effect to obtain on matt surfaces. I attribute this transparency to the method in which the pigment, platinum black, is

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applied to the paper. In this process the ferrous image on the paper reduces the platinum from the platinum salt in solution in the bath, and thus the pigment is, as it were, brushed on, not developed *in situ*.

Another characteristic of these prints is the great purity of the whites obtained on paper which has been long or badly kept. This arises from the fact that the paper is coated with iron only, and does not contain any platinum salt.

In America, where large numbers of direct enlargements are made in platinum, difficulty is sometimes experienced, especially in hot and damp weather, by a staining of the whites. I have just received some examples of enlargements made by this new method in which a wonderful purity is obtained.

This method offers very great opportunities for modifying the character of the results. Prints showing wonderful delicacy and softness, or, on the other hand, great boldness and vigour, may be readily obtained by slight alterations in the sensitizing operations, and these variations may be still further affected by changes made in development.

I will conclude by a statement of the principal advantages secured by this method. They are—

1. Greater transparency in the shadows.
2. Cold development.
3. Tentative development.
4. Shorter exposure.
5. Easy variation in the character of the finish.

Mr. J. F. Mostyn Clarke then read a paper on the "Present Value of Art in Photography." In this paper the author urged that more attention should be paid to artistic composition in photography.

On the second day of the Conference the first paper read was one by *Captain W. de W. Abney*, on the "Theoretical Aspect of Ortho-chromatic Photography."

In this communication the author first drew attention to Dr. Eder's theory "that the dye unites with the silver bromide to form a molecular compound of the nature of a lake. The action of light on the dye and the silver bromide is simultaneous. The compound of the bromide and the dye absorbs the light rays, and the energy which existed as wave motion is communicated to the molecules of the compound. The molecules are thereby thrown into such energetic vibration that their equilibrium is disturbed, and the silver bromide is either decomposed into bromide and silver sub-bromide, or is brought into that state of unstable equilibrium in which it is readily acted upon by a reducing agent such as constitutes an ordinary developer. When the light rays are absorbed by the dye alone, the waves for the most part undergo *photo-thermal extinction*, and their energy is transformed into heat, a small proportion undergoing *photo-chemical* extinction, and being used up in producing chemical decomposition, since the majority of dyes are slightly altered by light. When, however, the rays are absorbed by the dyed silver bromide, the greater part of the wave undergoes photo-chemical extinction, and their energy is used up in decomposing the silver bromide, whilst only a small portion undergoes photo-thermal extinction."

Captain Abney then criticised this theory, pointing out that radiation (or light, if it be preferred to call it so) consists of undulations which are capable of doing work of some kind on a body on which it falls. The work may be heating the body on which it falls by the absorption of rays, or chemical action by decomposing such a body, or both. This is what is translated as photo-thermal extinction and chemical extinction. Now, from the above it is evident that the dye is considered first to receive the light, and there is plenty of photo-thermal extinction (or heating of the body), and a little photo-chemical extinction (or chemical decomposition of the body); but directly the dye is in contact with the bromide, the two extinctions are reversed, and that chemical action is in the ascendant and the silver bromide is decomposed. In other words, the heating of the dye—for the dye not being chemically combined with the bromide, the work done in it must be identical with that which would be done were it not in contact with the bromide—does chemical work in the bromide of silver. If this were the case, a hot iron applied to the back of the plate should be quite as effective. If the silver was a real chemical compound with the dye, such an explanation, with modifications, might be the case; but one is not dealing with a chemical compound in the case of a lake, but only with a mechanical compound—a very widely different thing. For this reason he could not accept this explanation."

The author considered that it is more probable that the dye acts as a developer, and described a large number of experiments which tend to support this view. Commenting upon his experiments, the author said:—"It will be seen that in order to render the action of cyanine visible, it is not necessary to dye the film of a gelatine plate. It is only necessary to have two films in contact, one containing dye alone, and the other the sensitive salt, and to expose through the dyed film. This is what might be expected, however, as development commences at the surface. I may say that by placing a dried film of dyed collodion in contact with a gelatine film under great pressure the same action is manifest, though in a minor degree. This is a somewhat remarkable result, and applies not only to cyanin but to erythrosin, and all the dyes I have tried which give sensitiveness to a plate; and to this may be added substances which increase the general sensitiveness of a plate. Thus, collodion or resinous varnishes containing dyes, and even an alcoholic solution of the dye itself, will give the necessary orthochromatic properties, and is a very simple way of proceeding. Plates thus need not be prepared in advance, but are ready for immediate use on application of the varnish or dye. The dyed coating may be rendered alkaline by passing ammonia through the alcoholic solvent; or the film may first be rinsed with ammonia in water and dried, or it may be flooded with an alcoholic solution of ammonia, and then the second film applied. The action is then more rapid. This method of applying colouring matters is very advantageous, since it enables those to be used, such as chlorophyll, which cannot be employed in an aqueous solution, but which can well be employed in an alcoholic solution."

The next paper, which we reproduce in its entirety, was on "The Metamorphoses of the Silver Image," by *Lyonel Clark, C.E.*

Believing the best form of printing process to be one in which slow development is employed, I have been trying lately to obtain some chemical method of replacing the blacks in the gelatine bromide development process by some warmer tones. These trials have let me into an interesting set of experiments on the silver that forms the image of a bromide print, which may not be uninteresting. My first experiments were, of course, made in the direction of varying the exposure and development, but no beneficial results were obtained in this direction; occasionally a brownish tone is produced, but the print nearly always suffers, becoming flat and mealy.

I then made some experiments in fuming the paper with hydrochloric acid, after Blanquart Evrard's process, but beyond rendering the paper very insensitive, it had no effect in altering the colour of the developed image.

I then took a new line of country, and after developing and fixing the print, acted on it with several different oxidizing substances, changing in this manner the nature of the haloid salt, of which the image was formed, and then re-developing it.

My first experiments were made by transforming the metallic image, or what, for the sake of argument, I will assume to be a metallic image, into a chloride of silver, or a double chloride of silver, and some other metal or base.

With this idea I treated the halves of various prints with ferric, cupric, mercuric chloride in strong solutions. The bleaching or oxidizing action is very rapid in all these cases. With mercuric chloride it is very complete, not a trace of the image being left; but with the other chlorides faint, browny-yellow images always remain.

On refixing these oxidized images, a further difference is noticed, whilst the mercury chlorized image reappears (a sulphide of mercury being formed), the iron and copper chlorized images disappear totally. I have had very visible images on glass plates, of which, after fixing, not a trace remained.

The image appears to be completely destroyed, as the strongest reducing agents fail to bring any trace to light.

The unfixed portions, however, are easily developed by any of the ordinary developing agents, such as alkaline pyro or ferrous oxalate. They re-develop up slowly and easily exactly to the same state that they were before being chlorized. There is no difference to be observed between the half treated with pyro and that treated with ferrous oxalate.

May I be excused if I make a slight digression here, and say that I think a very easy and reliable process of either intensification or reduction might be based on this process? I am aware that Messrs. Cosmo Burton and A. Laurie proposed and described some time back a process of intensification by means of mercuric chloride and re-development, and that cupric bromide has been used for the same purpose, but I am not aware that it has ever been proposed as a process of reduction.

Roughly, the process I should recommend would be as follows:—Supposing that directly after development, and before fixing, you are discontented with your development; the picture is too hard or too weak. In this case, after hardening the film with alum, and before fixing, I should chlorize it, and then, after well washing, proceed to re-develop it, taking care in this development to avoid the errors I had fallen into in my previous development. For this second development I should advise the use of ferrous oxalate; much less washing is required between the operations than when you use pyro, for, as you all know, if ferric chloride be the oxidizing salt you have used, the slightest trace left of this will give ink-stains in the presence of pyro.

As far as my idea of getting different colours by chlorizing and re-developing the film was concerned, these experiments were a failure. The colour of the deposit develops just the same. I tried also bromizing the film, with identically the same results. I further used an oxidizing mixture composed of bichromate of potash and hydro-chloric acid. But again the result remained unchanged. In fact, it is quite impossible to tell, from the colour and appearance of the image, whether the film has been chlorized, bromized, or simply oxidized. With chromic acid, I have got a slightly different colour, but the whole image had lost so in intensity that I am inclined to put it down to other causes.

My idea, therefore, of obtaining difference of colour had failed.

I then proceeded to use iodine as my oxidizing base, and treated fresh prints with iodide of mercury and iodine, or rather tincture of iodine diluted with iodide of potassium.

Here, for the first time, I thoroughly succeeded in altering the colour of the print when treated with mercuric iodide, and re-developed either with pyro or oxalate. The print has a rich brown colour, which to me, at least, is fairly pleasing; as to its permanency, I am afraid to say anything. I know that bromide prints bleached with mercuric chloride, and treated with ammonia, fade very rapidly. In one case under my notice, in two months the mounted print had all but disappeared; but whether these prints, which have been re-developed and refixed, will also suffer, I am unable to say. I shall watch their existence with great interest.

With the tincture of iodine I had a veritable surprise. The positive print, when placed in the solution, rapidly turned into a negative. The conversion is very pretty. A dark bluish deposit settles everywhere on the white paper, whilst the silver image rapidly bleaches to the well-known grey of silver iodide.

The darkened negative image appears to be an unstable substance; it is, I presume, a compound of iodine with gelatine. Iodide of mercury darkens it slightly; iodide of potassium turns into purple and green; chromic acid blackens it; mercuric acid chloride bleaches it, and leaves a faint image of the original positive; on treatment with ammonia, it gives a greeny-yellow image.

The bluish negative is destroyed by hypo very rapidly, and leaves a faint yellow positive. Dr. Vogel, I think it was, proposed a test for hypo on these grounds; in fact, the iodide of starch test is closely allied to this, in its *raison d'être*.

Sulphurous acid destroys the blue image, but not hydrochloric; and either the pyro or ferrous oxalate developer completely removes it, leaving a very delicate lemon-yellow positive behind. Whether it would be possible to tone this image, and so get greenish colours, I am unable to say, but the change of the black deposit into the lemon-yellow image is very curious.

Of one other metamorphosis I would speak, and that is the use of the sulphantimoniate of potash, or Schlippe's salt, by means of which the image is turned into an antimoniate of silver.

The usual way to use it is in connection with mercuric iodide—Mr. Debenham's system of intensification; but I find a large gamut of tones, from brown to red-brown, can be obtained by either chlorizing, bromizing, or oxidizing the image in any of the above-mentioned ways, and then treating with the Schlippe's salt. If the oxidation be carried on very carefully, almost a scarlet image can be obtained.

I have tried to obtain a Bartolozzi red by these means, but without great success, but for future operators would point out some of the neutral chromates of the alkalies as likely to give the desired tone. Once, whilst treating a print with chromic acid, I got a bright-red deposit of chromate of silver. In fact, this metamorphosis or change of one silver salt for another might be useful in many ways. By transforming the image into a fluoride, and treatment with sulphuric acid, we might be able to form hydrofluoric acid, and so make the silver image engrave itself on the glass.

One other point I should like to mention, and that is the action of these oxidizing agents on the exposed and undeveloped film. I find that they, at any rate, considerably slow the action, if they do not altogether destroy it. I have exposed a bromide paper to the light till the image became visible, and after treating it with cupric chloride, have successfully developed it. When the exposure is short or normal, I am almost inclined to believe that the image is entirely destroyed. If it is entirely destroyed, we ought to expect the first action of the oxidizing agent on the developed image to bring oxidation to the developable stage, and a prolonged treatment should then in its turn destroy even this developable image. I can only say that a prolonged sojourn in the oxidizing solution very considerably weakens the image, it being impossible to obtain the same vigour as when the time has been shorter. On the other hand, the image never entirely disappears—it is always more or less visible. This may certainly be due to the fact that some of the metal of the chlorizing solution combines with the silver of the image, as we know mercuric chloride forms a double mercurous and argentous salt, either or both of which can be reacted on. It would be interesting to try the action of bromine or chlorine gases on the developed image, or some such agent as peroxide of hydrogen, in which case the advent of extraneous elements or compounds would be avoided.

These few experiments that I have described do no more than open up the subject, but if they will only persuade some one more competent than I to enlarge on them and deduce some facts from them, I shall feel that they have not been in vain.

The next paper was by *Mr. T. Dallmeyer*, on "A Further Development and Simplification of the Standard of Comparative Exposures proposed by Dallmeyer." The author said:—

Speaking of photographic lenses generally, as regards their rapidity under the conditions in which they are used, every intelligent photographer speaks of his lens as working at such and such an intensity— $\frac{1}{\text{so and so}}$. This fact led him to adopt the standard referred to; for a standard, to be of any use or intelligent meaning, should have direct reference to the intensities of the various stops or lenses under notice for comparison. It can be easily understood that whatever intensity be chosen as the standard unit for reference (if the fraction selected for intensity be neither too large nor too small to give cumbersome figures in the standard numbers themselves), a convenient set of figures *can* be obtained, as is the case in the P.S. of G.B. standard—viz., $\frac{1}{4}$. However, no standard unit can intelligently express intensity at the same time unless it be a decimal system, in which the relative exposures are an exact expression of the intensities to be compared.

To compare any two intensities for finding their relative exposures, it is only necessary to square the denominators of the fractions expressing those intensities; and the simplest of all standards that could be adopted is to leave these numbers exactly as they are, except for one reason—namely, that they are somewhat cumbersome. To obviate this, divide by 10, and a direct, clear, and intelligible reference to the intensities themselves is still maintained. This necessitates the adoption of a somewhat ugly-looking standard unit—namely, $\frac{1}{\sqrt{10}}$.

The author also explained a table and diagram he had devised for quickly determining standard numbers.

Sir D. Salomons then described "A Ratio Slide Rule."

The next paper, which we also give in full, was on "A New Form of Sensitometer," by *Mr. W. F. Donkin.*

The principle of the sensitometer I am about to describe is that of the pinhole camera. The idea of applying this principle to sensitometry is due to Mr. A. Vernon-Harcourt, who suggested it to me last April. I then made a few rough experiments, which seemed promising, and have lately continued them more carefully; and, though they are by no means complete, I have arrived at results which prove the applicability of the principle.

If we take a camera with a pinhole instead of a lens, and place a lighted candle in front of it in an otherwise dark room, so that the image of the candle-flame falls on the sensitive plate, we shall obtain a visible image on development of greater or less intensity, the intensity varying with several conditions. Thus, it is obvious that the intensity of the image will be increased (1) by a longer time of exposure; (2) by a brighter light; (3) by a larger pinhole; (4) by a shorter distance between the pinhole and the plate; (5) by variations in the development; and lastly, by greater sensitiveness of the plate. It is plain, also, that we may take values for the first five conditions such that with a plate of a certain sensitiveness an image is obtained only just strong enough to be visible. If, now, we make another pinhole a very little smaller, keeping all the other conditions constant, we shall obtain no visible image at all. A more sensitive plate, however, would give a visible image with the smaller pinhole. Suppose, then, we make a number of pinholes—say 25—in the front of the camera, of graduated sizes, a series of images of the single candle-flame will be obtained by one exposure, and the number of images visible on development will be proportional to the sensitiveness of the plate, provided all the other conditions are kept constant. Such an apparatus—a simple pinhole camera with twenty-five holes—constitutes the sensitometer I have the honour to bring before you. It is covenient to make the smallest hole about ·004 inch in diameter, and the remaining twenty-four of sizes increasing in a geometric series, such as that adopted by Mr. Spurge in his sensitometer, in which every third hole doubles in area. The convenience of this system is that it gives results comparable with those obtained by means of sensitometers depending on absorption of light in passing through layers of a medium the thickness of which varies in arithmetical progression, such as Warnerke's. The following table gives the actual diameters in inches of the holes (approximately circular) in my sensitometer:—

No.	Diameter.	No.	Diameter.
1	0·0640	14	0·0143
2	0·0570	15	0·0127
3	0·0508	16	0·0113
4	0·0452	17	0·0101
5	0·0403	18	0·0090
6	0·0359	19	0·0080
7	0·0320	20	0·0071
8	0·0285	21	0·0064
9	0·0254	22	0·0057
10	0·0236	23	0·0051
11	0·0202	24	0·0045
12	0·0180	25	0·0040
13	0·0160		

By making the distance between the holes and the sinister plate 2·5 inches, by giving a uniform exposure of thirty seconds in all cases, by using candles of the same composition, and by developing always in the same way, very constant results may be obtained. It is to be observed that no mention is made of the distance between the candle and the sensitometer. It simply does not matter; the distance may be varied within wide limits without affecting the result. It is of course true that the intensity of the light falling on any particular pinhole varies inversely as the square of the distance of the candle from it; but the size of the image on the plate varies in the same ratio, and consequently its brightness remains constant. Now it is only the *brightness* of this image that is measured, and not the total quantity of light. This is easily proved by using two candles instead of one, and placing them at different distances from the sensitometer, taking care, however, that the images do not overlap. Each candle will give the same number of images, and the same as if it had been employed alone. Hence all that has to be considered in regard to the source of light employed, is its intrinsic brightness. It would doubtless be possible to use magnesium; but the exposure would have to be inconveniently short, and the excess of blue rays is too great. For many reasons, candles are most convenient. Candles vary considerably as regards the quantity of light they emit, and this is true even of those parliamentary candles, which are still the only legal standard of light. I do not know whether the relative brightness of various candle-flames—as distinguished from the total light emitted—has been the subject of investigation, but it is at least probable that, with candles of the same composition, the brightness of the flame is more constant than the total light. I would suggest always using the best paraffin candles with this form of sensitometer, as being easily obtainable.

The only part of the sensitometer that is difficult to make is the perforated plate. After trying various methods, I succeeded best with the simplest. I began at first with carefully-made little drills and a lathe, but now I find a few ordinary needles and an oil stone all that is necessary in the way of tools. A micrometer of some sort is, of course, necessary to measure the holes when made. I use tinfoil for the smallest holes, making a number of holes in little bits of foil, and picking out those of the right size and gumming them on a card with twenty-five larger holes punched in it. For the larger sizes I use thin aluminium foil, and find it an admirable material. It would doubtless serve equally well for the smaller sizes. Having got the plate made the next thing is to keep it clean and free from dust. It should be examined with a magnifier before use, and, if necessary, lightly brushed with a fine camel's-hair brush. Although the distance of the candle is theoretically of no importance, practically it is convenient to place it about eighteen inches or two feet from the sensitometer. Images of a sufficient size are thus obtained, while the divergence of the rays is not great enough to introduce sensible error.

The last paper was on "Centrifugal Force applied to Emulsion Making," by *Mr. Andrew Pringle.* The author pointed out the defects in the ordinary method of washing and draining the sensitive emulsion, and explained the process of centrifugal separation.

On the Oxidation of Silver. (H. le Chatelier, Bul. Soc. Chem. 48, 342.)—Since, according to the calculations of Thomson, the oxidation of silver disengages fourteen calories for one molecule of oxygen, the combination should take place directly, as is the case of most other metals. Heretofore this has not been accomplished. According to the laws of chemical equilibrium, the decomposition of silver oxide and the oxidation of the metal must be limited at every temperature by the fixed tension of oxygen. Hydrogen does not combine with oxygen at a temperature below 500°, and the direct oxidation of silver would require a calculated temperature of about 327° with a plus or minus error of 60°. The minimum temperature necessary for the oxidation of silver should be the same as the minimum temperature of decomposition

f. 19v

The bromine bulb was next broken and the chloride distributed as uniformly as possible over the inside of the tube, in order to expose it most advantageously to the action of the bromine. By means of an air bath the tubes were heated to about the boiling point of mercury in some cases, and to about 450° in others.

After heating, the tubes were freed from any adherent oxide of iron due to contact with the iron tubes of the bath by immersion in warm chlorhydric acid, washed, dried, opened by means of a hot wire on a piece of glazed paper, and heated at 120° till the free bromine was expelled and the weight constant. Lastly, the contents of the tubes were washed out and the empty tubes weighed. From the increase in weight of the chloride taken, the amount of bromine substituted, and hence the chlorine displaced could be calculated. The results of the experiments carried out in this way are given in the subjoined table. Chlorides marked with the same letter were heated simultaneously :—

It will be seen that only on one occasion—in the case of silver chloride—was a number got in anything like agreement with the figures demanded by Potilizin's law. In all other cases the amount of chlorine displaced was considerably less than that required by the law. The table, however, shows that although the amount of chlorine displaced stands in no definite relation to the atomic weight of the metal in the chloride used, yet in all cases most chlorine is displaced from the chloride of highest molecular weight when several are heated simultaneously. Further, in all cases, with the exception of silver chloride, the longer the time of heating the more chlorine was substituted, and hence it might be thought that if the time of heating were sufficiently prolonged a limit would be reached. On the other hand, in no instance was the amount of chlorine displaced in any one chloride proportional to the time of heating; even after heating sodium chloride for 120 hours scarcely half the quantity demanded by Potilizin's law was substituted. In the only case in which Potilizin gives definite data—viz., in the case of barium chloride, the percentage of chlorine displaced was 7·78 on heating from two to six hours at 400°, whereas Messrs. Thorpe & Rodger obtained only 4·8 on heating for seventeen hours at about 450°.

Messrs. Thorpe & Rodger consider that these observations altogether disprove the validity of Potilizin's law of displacement.

Chloride used.	Temperature of Bath.	Time of Heating.	Percentage of Chlorine Replaced.	Percentage given by the Law.
(*a*) NaCl	about 350°	8 hours	0·5	5·5
(*b*) NaCl	„ 450°	14 „	1·84	5 5
NaCl	„ 450°	120 „	2·6	5·5
(*a*) KCl	about 350°	8 hours	2·7	9·78
(*b*) KCl	„ 450°	14 „	3·4	9·78
(*a*) AgCl	about 350°	8 hours	23·3	27·28
(*b*) AgCl	„ 450°	14 „	24·7	27·28
AgCl	„ 350°	8 „	27·42	27·28
(*c*) $SrCl_2$	about 450°	17 hours	1·3	5·2
(*c*) $BaCl_2$	about 450°	17 hours	4·5	7·78
(*c*) $PbCl_2$	about 450°	17 hours	5·0	12·4

— *Proceedings of the Chemical Society.*

of the oxide, and it is known that the oxide begins to decompose at about 250°. One gramme of pure silver was sealed in a hard glass tube with some potassium permanganate, but separated from it by a plug of glass-wool. Under such conditions the oxidation takes place distinctly at 300°, as is indicated by the black colour of the oxide, while the tension of the oxygen does not exceed fifteen atmospheres. The oxidation cannot be made complete, the largest proportion of oxide formed being five decigrammes for one gramme of silver.—(From *Journal of the Chemical Society.*)

Potilizin's Law of Mutual Displacement of Chlorine and Bromine. (By T. E. Thorpe, F.R.S., and J. W. Rodger.)—On heating bromine with an equivalent quantity of an anhydrous metallic chloride in a sealed glass tube free from air at the temperature of the melting point of zinc, Potilizin found that the amount of chlorine displaced by bromine was greater the higher the atomic weight of the metal in the chloride. Experiments were made with the chlorides of sodium, potassium, silver, calcium, strontium, barium, lead, and mercury. Potilizin further found that if A be the atomic weight of the metal, ρ the percentage of chlorine displaced from its chloride when treated as above, and E its valency, the formula $\frac{A}{\rho E^2}$ = a constant, held good in the case of lithium, sodium, potassium, silver, calcium, strontium, barium, lead, mercury, bismuth, tin, and iron. He considered the quantity of chlorine displaced to depend on the atomic weight, valency and temperature. Arranging the metals in accordance with the periodic law in vertical series, the chlorine displaced was as the atomic weight, $\frac{A}{\rho}$ being a constant for each series. Berthelot pointed out that the displacement of chlorine by bromine was only possible if the heat of formation of BrCl was greater than the heat absorbed in the direct substitution. Such displacement might, he thought, take place at temperatures at which the chloride was dissociated, so that the bromine would be able to unite with the free metal. He repeated Potilizin's experiments, but found no action up to 400°. Potilizin, in answer to Berthelot, repeated his previous experiments, and further varied the mass of the bromine employed. He used sealed vacuous tubes, heating them at 400–515° or at 300–315°, and found that the chlorine displaced varied with the mass of the bromine, but at a given limit increase in the amount of bromine or of the temperature did not affect the quantity of chlorine displaced. He pointed out that the interaction was not in entire accordance with Berthelot's principle of maximum work, but that it depended partly on this principle and also on the atomic weight, mass, valency, and temperature of the interacting bodies. Berthelot attempted to reconcile Potilizin's statements with his principle by assuming that intermediate compounds—metallic per-bromides and chloro-bromides together with bromine chloride—were formed and dissociated. In the light of such a supposition he considered the displacement of chlorine by bromine if the latter is present in excess, and the small displacements of the two are in equivalent proportions, to be in accordance with the provisions of thermal chemistry. In answer, Potilizin stated that he verified the formula $\frac{A}{\rho E^2}$ = a constant, in the case of 14 chlorides, also in the case of nickel and cobalt. He denied that intermediate compounds could exist at the temperature of the interaction, and thought that the cause of the interaction must be sought for "in the intra-molecular conditions of the bodies being probably dependent on the varying rates of the molecules."

No further work appears to have been done on the subject, and in order to test the validity of the law stated by Potilizin, the following experiments were instituted :—

An amount of bromine equivalent to about one gram of anhydrous chloride was sealed off in a small glass bulb, and a quantity of the salt, exactly equivalent to the quantity of bromine taken, was weighed out into another bulb, and the two were placed in a piece of glass tubing closed at one end. At about 6 inches from the closed end the tube was thickened in the flame and drawn out into a thick capillary. Beyond this point it was drawn out and bent so as to be evacuated by the Sprengel pump. When the exhaustion was complete the tube was sealed up by directing a blow-pipe flame on to its capillary portion; the point was then annealed and the tube allowed to cool.

"NOW AND THEN."

The "Photographic Society" (now the Photographic Society of Great Britain) was founded thirty-five years ago, and although representing only a small portion of time, nevertheless it has been quite sufficient to disconnect the originators of the Society from the present members, both amateur and professional, who now crowd its ranks. Many of these, it may be assumed, are almost absolutely unacquainted with the work to which the early members of the Photographic Society gave such enthusiastic attention, consequently it may be desirable occasionally to bring forward some of the results arising out of their work, thinking that in so doing we are only rendering justice to those early pioneers who so freely made known their experiments and inventions.

Now this Society, as part of its undertakings, started a monthly journal, in which to record not only its own proceedings, but the result also of other investigations which were then going on in other places and other countries; it is from the pages of these past journals that our facts will be taken, and many a germ can there be found of several of those modern applications of photography which the advanced chemical and mechanical workers of the present time have, as it were, re-discovered and brought out. We take as our present matter for consideration one that is very interesting, seeing that it arises out of the *first original paper* read before this Society, the subject being the "Art Possibilities of Photography." The writer of this paper boldly introduces an artistic idea identical with that upon which a scientific paper has just been read.

As an historical fact, the Society's first paper was read by Sir William J. Newton, R.A., "Upon Photography in an Artistic Point of View, and in its Relation to the Arts." After stating that chemical photography fails to realize *at present* that harmony and union of parts which nature by its atmospheric veil produces, he goes on to say that whilst still more minute and perfect detail must be the study of the scientific photographer, for the artist it was not necessary or desirable to represent every minute detail, but to produce a broad and general effect, and for this purpose, to quote his own words, "I do not consider it necessary that the whole of the subject should be *in focus;* on the contrary, I have found in many instances that the object is better obtained by the *whole subject* being a little *out of focus,* thereby giving a greater breadth of effect, and consequently more *suggestive* of the true character of nature." And then he continues, "but for every other purpose, and especially for architectural subjects, &c., it was impossible to be too particular in getting the exact focus." It appears from the discussion which followed that at this early period the scientific photographer became somewhat startled, and took umbrage at this novel recommendation coming from the artist photographer, which seemed to do away with the necessity for all that fine detail and precision of form which is the very essence of scientific photographic representation, and at subsequent meetings the subject was again discussed, especially by artists, and papers were read by Mr. John Leighton, who wrote, "Minute detail is the attribute (and an invaluable one) of the sun-picture, but not so of the work of art; consequently many photographic pictures may be cut up into several pieces, all beautiful, but no particle can be removed from a work of art without detriment, since it possesses unity." Also by Mr. R. W. Buss, who after having stated his advocacy of Sir William's proposition, observed with singular clearness, "we thus early observe in the outset of the Photographic Society two distinct paths for its members to choose from—*one* in which the perfecting of the process of photography by subtle chemical investigation is proposed, the correcting of lenses, and various improvements in the form of the camera, affording great scope for the observation of the scientific members; while the *other* is that in which the professors and amateurs in art would re-create themselves;" and so—thirty-five years having passed since the artist-painter tried to effect an alteration on each separate photograph, by personal action upon scientific apparatus—*science* itself now proposes to effect a similar result without the artist; and thus, looking back on those early days when photography seemed to be received almost solely for its capacity for pictorial representation, we see what marvellous strides have been made in its

f. 20v

capability now to produce that which Sir W. Newton so much desired, viz.: "harmony and union of parts," and which he almost prophetically anticipated would be accomplished, when he used the words "at present."

Connected with this subject, in this "first Society paper" there is the germ of another matter, which thirty-five years have helped to carry to an extravagant outcome. Allusion is made by Sir W. Newton, to what he called "touching of negatives," and it appears that at first he attempted only the improvement of skies, where he used cyanide of potassium for making transparent clouds upon dense negatives, and Indian ink for opaque clouds upon transparent negatives, but he very soon extended the use of "touching" to the other parts of the negative; and it must always be remembered, that artists in those days gave educated talent to this work, so that the accepted deficiencies in those early negatives, and especially those on paper, must have caused the artist painter to try to supplement science, much to the apparent consternation of the photographic purist. But here again science has in these latter days produced highly sensitive surfaces, upon which nature's gradations and atmospheric charm have been so accurately rendered. It is then curious and instructive to note how, in the short period of time already alluded to, the advance of science more and more enables the artist to use photography for the production of pictures in artistic value so greatly beyond what could be produced in the first year of this Society's existence.

Edwin Cocking.

f. 21r

NOTICES.

Letters or original articles arising from the papers read before the Society, intended for insertion in the Society's Journal, should be addressed to the Editor, care of Messrs. Harrison & Sons, 45, St. Martin's Lane, W.C.

All communications for the Assistant Secretary (Edwin Cocking), to be addressed to the Gallery, 5A, PALL MALL EAST S.W.

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J. McE Fadyean MB f. 1
Principal & Dean

fix Monday Nov 4 3pm

ROYAL VETERINARY COLLEGE,
CAMDEN TOWN. N.W.

28th Oct. 1895

Dear Sir

Your letter to hand. I shall be very pleased to render you any assistance in my power. I think, however, it would be well if you could, as suggested, call and explain more fully what you wish. Unfortunately I am going out of town on Wednesday & may be away

f. 2

for the remainder of the week. If the matter admits of delay perhaps you could call some day next week – say Monday or Tuesday at 3 o'clock.

Yours faithfully
J. McFadyean

Francis Galton Esq F.R.S.
42 Rutland Gate
S.W.

Reid

f. 1r

CHARLES REID,
PHOTOGRAPHER.

The Studio.
WISHAW, N.B. 13th March 1899

Francis Galton Esq.,

Dear Sir

I have now got all the plates of horses developed, and am pleased to inform you that on the whole they are satisfactory. The first half dozen or so are not what could be wished, but the others will, I think, be quite suitable for your purpose.
Some of those developed on Saturday are now in the printers hands, + prints of the most of them could be sent before you leave London. if you so wish. I will number

f. 1v

them all on the back with the Catalogue Number, so that you will have no difficulty in identifying them.

I note what you say on your post card.

Yours truly
Robert Reid.

Rec^d April 2
accepted April 3.

(Reid

f. 2

CHARLES REID,
PHOTOGRAPHER.

The Studio,
WISHAW, 28th March 1899
N.B.

Francis Galton Esq F.R.S.

Dear Sir

On getting home I found both your post cards & letter of the 14th ult. for which I thank you. I am glad to be assured that you are pleased with the photographs, and that they will serve your purpose. With regard to their use as newspaper illustrations, I have an enquiry about some of them for the "Live Stock Journal" and also from "Country Life" both of which papers may use some of them. As they might want them soon, & as a correspondence with you while abroad is attended with delays I wish to submit a general proposal for your consideration, viz. - that besides letting you have the first set of prints without extra charge, I supply an extra set to The Royal Commission on Horsebreeding, and a mounted cabinet to each of the 29 owners of Selected Horses, as from you, free; on condition that you grant me permission to supply copies to the

the illustrated press, which I would do at a modified rate. This arrangement would in no way interfere with your use of the photographs, as you would still retain your right to use them in the press or otherwise as you saw fit.
Should this proposal be agreeable to you please let me hear at your early convenience, and also say what letterpress you wish underneath, or to accompany, the photos, if published.
Perhaps:- By the kind permission of Francis Galton Esq F.R.S. The wording to be left to yourself.
I note that you refer me to your Nephew for your address after this month.

Yours truly

Charles Reid

Reid

f. 4

CHARLES REID,
PHOTOGRAPHER.

The Studio.
WISHAW. N.B. 12th May 1899

Dear Sir,
I have to thank you for sending cheque which my son says was acknowledged.
About the photos, I sent a set to "Country Life" but after consideration they returned them. The only other paper to which I sent a few is "The Live Stock Journal". They have used some of them & may use more.
At the time I wrote you last I was hopeful that "Country Life" would use the greater part of the photos, but although I made them a special - a tempting - quotation, they declined the offer.
I have not heard from the Dublin autority you allude to, but I had an enquiry from the Southdown Sheep Society the other day about taking 6 prize-winning Sheep at the Royal Show.
The secretary said he did this ~~at~~ on the very strong recommendation of Sir

f. 5

Sir James Duke. Since then I have had another enquiry and I am seriously thinking of going on this account & have written Sir Ernest Clarke to ascertain if I can obtain permission to do so. Of course I would be at liberty if I had a Stand in the yard, but I fear the time for applying for this is past.

I note what you say about the difficulty in obtaining satisfactory measurements of the Horses. I can easily imagine this to be the case, First attempts usually leave much to be improved upon.

I have two sets of the Horses ready, one for the Royal Commission on Horse breeding & another for the owners of the animals. Shall I send both sets to you unmounted, or would you prefer the owner's set mounted?

yours truly

Charles Reid

f. 1

TELEGRAMS,
ROSSANO LONDON.

TELEPHONE Nº 3540.

111, NEW BOND STREET, W.

London Dec 10th 1895

Dr. to Ross & Co. Opticians.

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	16	15

f. 1

30^A Wimpole Street
3rd November 1895

Dear Mr. Galton,

Enclosed is your photograph, with such points as I can find marked on the back. — I have also made a rough enlargement of him, on which the suggested measures are more clearly indicated.

It seemed best to try some sort of rough triangulation of the trunk, and then to get some measure of the long limb bones. — The head and neck I should feel inclined to ~~to~~ omit: because the neck, at least, is very greatly influenced by ~~—~~ castration.

In the trunk the obvious fixed points are a, b, c, f; and of these b, c, and f, are the best. — (b) and (c) are both projections of the ilium, where it is fixed to the vertebral column: and I would suggest whichever of these two may prove most generally conspicuous as the first fixed point. (f) is the root of the tail; (a), marked by the map of muscle in front of it, and the edge of the bone coming backwards from it, is the upper anterior corner of the scapula. The slope of the scapula is indicated by the line a d; and at the lower end of this line are two projections, marked d and e, of which e is higher than d, and a little behind it. I believe e to be the acromion process of the scapula, and d to be the great tuberosity of the humerus. From the prominent biceps which goes downwards and backwards from d, this seems the better of the two ~~points~~ points, because it is more likely to be always conspicuous in horses in

(1.)

good condition.

Measuring then a d, d b, and b a, along the lines in the drawing, the triangle a d b gives the distance between the limb supports, and the length and slope of the shoulder.

The girth of the trunk should be determined in the thorax, as measure of the lung-space, and in the abdomen, as measure of the room for the intestines: but I cannot make a suggestion which satisfies myself. — The best I can think of is to measure chest girth along a d, and waist along a perpendicular to the floor drawn through b. — Chest diameter then being r s in the drawing, and waist dimensions b p ~~pa~~ and b q recorded separately.

If these measures were adopted, then p f might be measured, and also q f: in this way the relation of f to b would be determined by the ~~direct~~ length b f, whose direction would be independently given ~~by~~ by drawing the triangles b q f and b p f. In the same way the relation between d and b might be checked by making both the triangles b a d and d b p.

[If only male horses (whether entire or not) were measured, the ~~pee~~ ~~edge of~~ corner of the retracted prepuce might be taken as determining p ?]

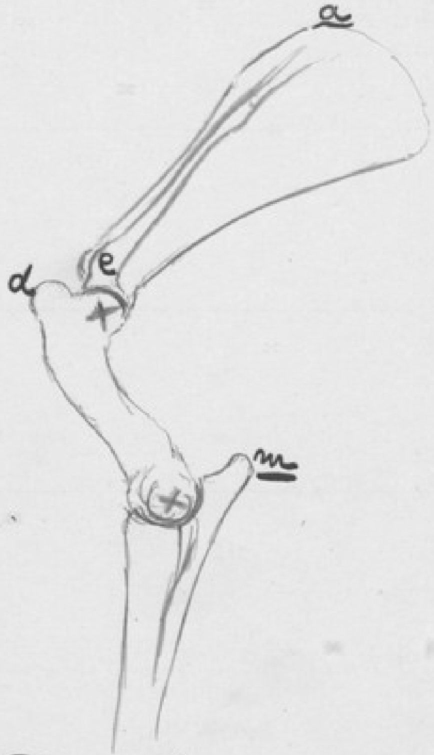
The line r s is very badly determined: it goes to the sloping root of the mane on one side, and to the swellings of the ~~deltoid~~ (pectoral) group of muscles at the other: but I cannot find a better!

The line bp rather pleases me; because I think it will not be affected, so much as any other waist measure, by such a thing as a change from corn to grass.

If more information about the back is wanted, I should feel inclined to draw a triangle on base d b, with its apex in the lowest point of the saddle, and another on base b f with its apex on the highest point of the rump ~~[illegible]~~: these triangles would certainly vary enormously in going from one breed of horses to another.

Limb measures:

In the fore-limb, the photograph seems to me to give few points of value. The direction of the humerus can be seen by the attachments of the muscles along the line dm: but with the limb vertical, no sign of the olecranon can be seen at m, — at least, in the shadows of the photograph. — In the bent limb, the olecranon can be seen at m; and perhaps the measure dm might then be worth making: but it would vary sensibly with the position of the limb, because neither d nor m is on an axis of rotation. — In the diagram on the next page I have marked the points a d e m as I believe them to lie on the bones. You will see that



Centres of rotation marked red.

both d and m change with position of the limb: but I do not believe that a horse in walking without doing much work moves its ~~humer~~ humerus so much that the change in d matters.

It certainly does move its radius and ulna: so that m is a bad point from which to measure length of upper arm.

If m were visible, the length of the fore-arm and finger in a straight line m t, from olecranon to the point where hair and horn join at the hinder edge of the hoof, would be a very good measure.

In a horse like your Bead Or, standing with his "knees" straightened, I cannot find any fixed point at the carpal joint: and although there is a well defined swelling above the fetlock, its position may surely be altered nearly an inch according to the taste of the groom who clips the horse: while it is invisible in many breeds, because of the bunches of hair. Also, one could not make the homologous measure in a cow.

On the whole, I should feel inclined to leave the humerus length undetermined, and to measure m t if possible. — If m (the olecranon) is usually as obscure as it is in the present photographs, there is a point, marked ⊙ in my drawing, and

(x) in the back of the photograph. You see that on the anterior ~~edge~~ side of the humerus is the swelling of the biceps and its set of muscles: on the posterior and upper side are the triceps, and the other muscles from the scapula to the humerus; these muscles lie close together between d and the point ⊙ in the drawing: at that point, they diverge, leaving a ∧ g space between them, filled by the origin of the long adductors & extensors of the hand. The apex of this ∧ is well defined: and for a given position of the limb, the line from this apex to t might be good. But the point is about half way between the centres of rotation of the humerus: so that it will change much with movement.

The most trustworthy measure of the fore-limb in a standing horse seems to me the perpendicular d x.

In the hind limb

I can suggest nothing more than gh and hk, where g is the hinder edge of the patella, — very near the joint; and h is the point where the tendons of the gastrocnemius & plantaris are in contact with the bone of the heel.

k is the point where hair and horn join at the back of the hoof, as in the fore limb.

There is no obvious way of getting at the length of the

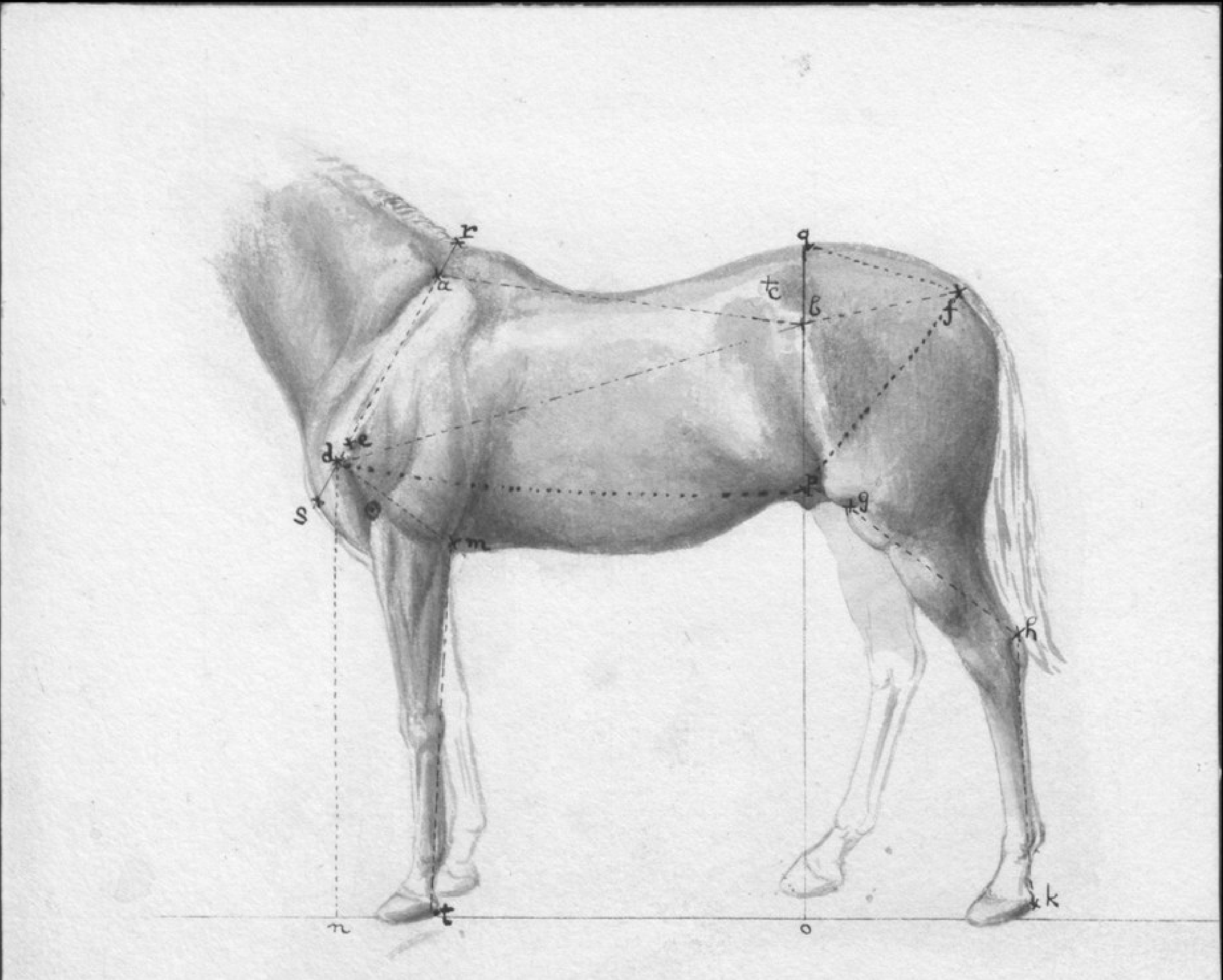
f. 6

femur, because its proximal end is buried so deeply among the muscles of the quarter, and is so far from any definite external mark.

And I am afraid here is an end of my suggestions.

Yours very truly

W F R Weldon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Bend Or
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1836 C.R. "Bend Or"

F.91

Francis Galton Esq., F.R.S.,

42, Rutland Gate,

S.W.

Nov. 2^d 1895

Do not acknowledge this

SWANSFIELD HOUSE,
ALNWICK.

Dear Uncle Frank,

I have typed out some, I fear, very inane remarks, in answer to your Enquiries; which I enclose together with your paper.

I know Graham pretty well, but not Housman. Our bailiff Patten, a very clever fellow knows both: The latter was his suggestion.

I have not answered your

f. 1v.

SWANSFIELD HOUSE,
ALNWICK.

4°. query as to possible "blunder".

There would I think most likely appear on technical points relating to special heads, & if I can be of any use in getting specialist advice, please let me know. Also if I can help in any other way.

Every yours affectly

S. G. Wheeler

Mother going on well. I got home from Clarendon last night.

f. 2

It might be interesting to collect measurements from a large number " of individuals of one breed, & see how nearly a prize animal approximated to the mean of the several measurements, allowing similar (or very approximate) weight or heights in all cases.