his reader's feelings that the author does not at once disclose the astounding discovery which is to revolutionise chemical science, but leads the imagination gently up to it—first informing us that he has spent his life in trying to practise the precepts of Newton and Watt—then indulging in a series of disparaging remarks on modern chemical theory, which remarks we may state *en passant*, perfectly bristle with mis-statements. The discovery which we are now approaching is, we are told, the result of an attempt to apply mechanical principles to chemistry—then the author lets us into a *trait* of his personal character, after which comes the *dénouement*. Here it is: "What I claim is the discovery of an element whose weight is 5; it combines directly with nitrogen, and forms fluorine, chlorine, bromine, and iodine; with carbon, and forms oxygen and silicon (*sic.*); and with hydrogen and forms sulphur and lithium." This extraordinary and pantogenic element "is best described as being the opposite of hydrogen," which "opposition constitutes its energy, and this energy constitutes, or rather will constitute the science of chemistry. Its combinations, direct and indirect, with hydrogen, carbon, and nitrogen, form all the other elements except phosphorus." In the new chemistry potassium is "ammonia acid," sodium, "ammonium acid," lithium, and sulphur, "hydrogen acids," &c. We find no evidence in the pamphlet of the method employed in discovering the new element—before listening to Mr. Coutie's vague speculations, chemists will be justified in calling upon him to produce his *Pantogen*, and describe its properties—at present it exists only on paper. We may appropriately conclude this notice with a quotation from Priestley, printed by the author in the preface to the present rhapsody. "For my own part I will frankly acknowledge that at the commencement of the experiments recited in this section, I was so far from having formed any hypothesis that led to the discoveries I made in pursuing them, that they would have appeared very improbable to me had I been told of them." Mr. Coutie lays this missile so very temptingly within our reach, that we may be excused for throwing it at his very fragile fabric.

The author's language does not properly fall into our province, but enough has been quoted to give our readers a fair specimen of its quality;—of Mr. Coutie's science no further criticism is necessary. R. M.

LETTERS TO THE EDITOR

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

Theophrastus versus Millington

In a letter in *NATURE*, vol. xiii. pp. 85, 86, Mr. Bennett appears to contend—

1. That if the credit of suggesting the true function of the stamens of flowering plants is to be attributed to either, it belongs to Grew rather than to Millington.

2. But that even the views of the former were not materially in advance of those of Theophrastus.

First, as to a bibliographical matter, Mr. Bennett speaks of a second edition of Grew's book. There is no such thing. The "Anatomy of Plants," 1682, contains four books; the first of these was published in 1671, the second in 1673, the third in 1675. The fourth book, not published till 1682, contains the celebrated passage on the sexuality of plants. This was read before the Royal Society, Oct. 26 [Nov. 9], 1676.

Turning to p. 172, we find Chap. V. "Of the Use of the Attire" (stamens).

In § 1 he alludes to the "Secondary use." This (book i. p. 39) he explains to be the provision of food for animals.

"We must not think that God Almighty hath left any of the whole Family of his Creatures unprovided for; but as the Great Master, some where or other carveth out to all; and that for a great number of these little Folk, He hath stored up their

peculiar provisions in the Attires of Flowers; each Flower thus becoming their Lodging and their Dining-Room both in one."

Having given this proper recognition to the pious teleology of the time, he proceeds to remark in § 2 that "the Primary and chief Use of the Attire is such as hath respect to the Plant it self." He infers this very conclusively from the constant occurrence of stamens, even when the more conspicuous parts of the flower are wanting.

Then in the § 3 he tells us how Sir Thomas Millington gave him the clue—

"In discourse hereof [not as Mr. Bennett puts it, but on the nature of the Primary use of the Attire] with our Learned Savilian Professor, Sir Thomas Millington, he told me he conceived, That the Attire doth serve as the male, for the Generation of the seed."

In § 4 he proceeds: "I immediately reply'd, 'That I was of the same opinion;' and gave him some reasons for it, and answered some objections, which might oppose them."

Surely it is hardly necessary to comment on this charming little history. Grew gets the hint of the true solution from his friend, immediately perceives its importance, and eagerly proceeds to apply it to facts and to explain away apparent difficulties in accepting it. Surely scientific historians can hardly set aside Grew's own modest pleasure in attributing the discovery to Millington.

Next, however, we are told it is no discovery at all. It will be sufficient, in answer to this, to quote from p. 172 a few lines, omitting unnecessary analogies.

"The Globulets [pollen grains] and other small particles . . . in the Thecae [anther-cells] are as the vegetable sperm: which . . . falls down upon the seed-case [ovary] or womb, and so touches it with a prolific virtue."

To say that Theophrastus in the fourth century B.C. possessed all the detailed information necessary to come so near the truth as Grew did in this statement, shows a wanton disregard for facts which it is sad to contemplate even though it be a momentary *lapsus* in the case of Mr. Bennett.

As to attributing all the credit in the matter to Camerarius, I cannot do better than appeal to an authority of which no one can question the impartiality.

Sprengel, in his "Historia rei Herbariæ," vol. ii. pp. 14, 15, has the following remarks:—

"Una fere cum Grewio Jacobus Bobartus, horti Oxoniensis prefectus, experimenta cum *Lychnide dioica* instituit (1681), quæ ovula in capsula obvia haudquaquam fecunda esse, dam filamenta apicibus suis seu antheris careant, docuerunt. Id e Sherardi ore accepit Patricius Blair ("Botan. Essays," p. 243). Mox etiam Jo. Raius eandem sententiam, de fecundandi functione antherarum (1686) . . . uberrime et optime defendit. Unde elucet, quantopere aberrant à veritate, qui Rud. Jac. Camerarium inventorem credunt, licet plura hic argumenta pro sexuum differentia adduxerit. Cum historia spermatationum arrogantiam, fatendum etiam esse, Anglorum gloriam esse, quod primi tum phytotomiae tum doctrinæ, quam sexualem dicunt, fundamenta jecerint."

There really is no case for discussion. Everyone is familiar with the fact that a large amount of time, paper, and ink may be wasted in contentions of this kind, and it is to be regretted that Mr. A. W. Bennett should employ his energies in furnishing additional experimental proof on this head. That the writer in *NATURE* and the reviewer of Sachs were each and severally justified in their allusion to Millington is also clear enough.

A. B. C.

Estimation of Fractions

THE question of "Personal Equation in the Tabulation of Thermograms," &c., which was recently considered in this journal, is but a portion of the general subject of the estimation of fractions, and the various influencing causes connected with it. Having made some experiments on this matter some time ago, and more fully of late, I subjoin some of the results, as it is a subject on which there seems but little accurately known in general, and which is important in thousands of readings made every day.

The experimental readings were made with appliances varying in each case, but in all cases the whole space or division was perfectly free from any visible marks besides the movable index, as they might bias or help the division; also the reader avoided noticing during each set of readings whether they were mostly + or — the truth, as this would bias the judgment. Moderate sunlight shining on white paper led to more accurate results than

diffuse daylight. Each result stated below is the mean of from ten to fifty readings under fairly favourable circumstances. d is used for the whole division or space on which the position of the index was estimated; and i is the inch, which is the unit used throughout.

(1) The first question is the average amount of error made by different persons with regard to the fraction of *division*; in fact, the personal variation of average error. This on a 10-inch space was—

By A	mean error	$d \div 112$
" B	"	$d \div 60$
" C	"	$d \div 50$
" D	"	$d \div 36$
" E	"	$d \div 21$

(2) The average amount of error with regard to the fractions of an *inch* (the *divisions* being too small to see the above fractions of them) were—

By A	on space of	$i \div 41$	mean error	$i \div 1890$
" B	"	$i \div 139$	"	$i \div 3200$
" C	"	$i \div 41$	"	$i \div 1640$

Under the best circumstances and reading with the right eye, A's mean error is only $i \div 5000$ on the space of $i \div 139$; and as the distance was about six inches, this is $\frac{1}{10000}$ of the distance; agreeing with the size of the smallest line visible, which is about $\frac{1}{10000}$ to $\frac{1}{20000}$ of the distance according to circumstances, but without any *shadow* to the wire examined. As to the observers, A has had practice lately in measuring; B had much practice some years ago; C and E are ladies of artistic tastes but unmetrical; D is moderately accustomed to measuring, but has a difference between the focus of his eyes. It would be highly interesting to have a good collection of statistics on this question, from among surveyors, mechanics, and the unmetrical classes. I proceed to readings for other points, made by A.

(3) The mean errors on spaces of various lengths stand thus:—

10 i space,	mean error	$d \div 112$
1 i "	"	$d \div 89$
($i \div 41$ " "	"	$d \div 70$ read by right eye only)
$i \div 41$ " "	"	$d \div 106$ if read by both eyes.

Thus the size of the division does not seem to much affect the estimation, the extreme difference being 4 : 5. If the angular width of a space exceed 5° it is not so easily grasped by the eye, though the error is not perceptibly increased till it exceeds 20° . The $i \div 41$ space was read with a magnifier, ($\times 3$), so it was equivalent to $i \div 14$.

(4) The relative error of the first guess (or the numerical idea produced instantaneously at the first glance) and the most carefully considered readings stand thus:—

First sight.	Carefully considered.	Ratio.	Distance of observer.
10 i space, $d \div 55$	$d \div 114$	2.1 : 1	110 i or 11 d
1 i " $d \div 54$	$d \div 78$	1.4 : 1	18 i " 18 d
$i \div 139$ " $d \div 13.6$	$d \div 13$	1 : 1	6 i " 840 d

apparently indicating that the first guess is worst where the space is rather too wide angularly to be grasped by the eye at once; and therefore that the want of grasp is the defect corrected by consideration.

(5) The effect of looking at the space askew was tried by viewing it at 30° to either side; shifting sides ten times so as to avoid any gradual change in the quality of the reading, and so as to call up the contrast more strongly. The results of careful readings were:—

	In sum of errors.	per cent.
10 i space to right hand	31	69
10 i " left "	45	55

and it was noticeable that the fraction nearest to the reader was invariably guessed smaller at first sight, than it appeared on careful examination, showing that the tendency to diminish the nearest side which is noticeable in the above percentage of the space to right hand, was still stronger at first, and was corrected by consideration.

So far the results have been independent of differences between the eyes, but it would seem that these are considerable from the following:—

(6) Comparison of the percentage of + and - errors of the two eyes separately; the sum of the amounts of errors being taken, and not the number of errors.

	Right eye.	Left eye.
1 i space ...	55	45
1 i " ...	75	25
$i \div 139$ " ...	92	8
Mean ...	73	27

The elements of this mean, and all others after, are weighted by the number of observations. As in all these readings the zero was to left hand, it would seem that each eye imagines the fraction on its own side smaller than it really is, this being in exact accordance with the effect produced by both eyes viewing the object from one side. See (5).

(7) When both eyes are used together the readings appear to be well balanced on an average:—

	per cent.
10 i space ...	42
1 i " ...	59
Mean ...	53

The variations, however, of different series of readings are as wide as + 80, - 20; and + 20, - 80; this may be partly due to temporary rest of one eye, involuntarily; and to a slightly skew view of the space.

(8) The comparative amount of error of the two eyes stands thus:—

	Right : Left ::
1 i space ...	100 : 109 white light
1 i " ...	100 : 120 coloured lights
$i \div 139$ " ...	100 : 150 on silver
Mean ...	100 : 121

So 5 : 6 is the relative error of the right and left eyes.

(9) The relative errors of each eye separately and of both together are:—

	Right eye.	Left eye.	Mean of both.	Both used together.
1 i space ...	1.52	1.64	1.59	1.00

So that the advantage of two eyes over one is more than $\sqrt{2} : 1$, which it would be, if regarded as the mean of two readings; the excess of 1.59 over $\sqrt{2}$ may be due to the restraint of keeping one eye shut. The accuracy seems to be about $\sqrt{1/2}$ the number of eyes.

(10) A most important practical question is that of the influence of colour on the error of estimation. On trying this with coloured glasses the results were most unexpected.

	Relative amount of error.
	No glass. Blue. Green. Red.
1 i space ...	1.0 .81 .06 .70
1 i " ...	1.0 .17 .88 .23
Mean ...	1.0 .99 .97 1.46

The two series (each of ten readings with each colour) are given to show how far uniform the results are; they were read on different days, completely reversing the order of the four columns; and they were equally divided between the eyes, five by right and five by left.

I had expected that red would hold about the above relation to white; but that green, and especially blue, would have been worse; the colours were spectrally tolerably pure, except the blue, which contained some yellow. The badness of blue on the second trial in diffuse daylight is accounted for by a want of sufficient light; blue containing such a small proportion of light-rays.

So far I have remarked on those points which are perhaps, or probably, not due to idiosyncrasies; the question of partiality for certain digits is clearly a personal error, as has been demonstrated in NATURE; so it is not worth noting further, as its interest in individual cases is confined to the observer, who should try (being "forewarned") to be "forearmed" and so allow for it in reading; of course a series of statistics on it would have much interest.

(11) One point, however, which is probably alike in all is the relative amount of error in different parts of a scale; the mean I find to be thus:—

0 to 1	1 to 2	2 to 3	3 to 4	4 to 5	5 to 6	6 to 7	7 to 8	8 to 9	9 to 10
.65	1.53	1.36	1.52	1.47	1.54	2.2	1.30	1.09	1.14
-	+	-	+	+	+	-	-	-	-

The + and - below show the mean error in that digit to exceed or fall short of the mean of all (1.44). The readings are on a 1-inch space. Thus it is apparent that the errors exceed the mean in the four middle digits, and fall short of it in the others with but one exception.

Now, the practical results which may be deduced from these data are as follows:—

From (1), that there is considerable variation in accuracy of estimation; yet, in persons fit to estimate, it may be put at $d \div 50$ up to $d \div 100$; and therefore as the maximum error in some hundreds of readings is only about three times the average error, it is useless (and, worse than that, confusing) to have divisions smaller than $20 \times$ the quantity required to be read. The application of this result to surveyors' rods, &c., would be very conducive to ease in distant reading.

From (2), that for ordinary eyes about $i \div 2000$ is the mean error of reading (with one eye only), and therefore that $i \div \frac{8000}{3}$ is the smallest size useful for reading with the naked eye; consequently any closer divisions than these ($\frac{1}{25}$ of an inch) are detrimental because of the greater confusion they introduce.

From (4), that in small divisions no consideration is necessary, as the first instinctive impression is nearly, or quite, as accurate as careful estimation.

From (5), that it is important not to look at a space askew; and that if unavoidable, careful consideration is desirable.

From (6), that in reading a series of observations of one amount, the right and left eyes should be used alternately to equalise the + and - errors.

From (8), that the right eye should be used in preference to the left.

From (9), that both eyes should be used wherever possible.

From (10), that red is the worst colour for measures, and that green or blue are as good as white; so that blue and white would probably be the best practically, and more distinct from vegetation, &c., than the green at a distance.

From (11), that more care is required in readings in the central portions of a space than at the ends. This is as true on spaces of $i \div 41$ as on $10 i$ spaces.

Of course some of these results may be *personal errors*, and not generally true, but at least this notice will perhaps call the attention of those accustomed to estimation to the subject, and so elicit some definite statistics. There are many other interesting questions which I have not touched on here as being of less consequence, and not affecting practical questions so much as the foregoing results; these results it is to be hoped may lead to a better acquaintance with a subject of such practical importance.

W. M. FLINDERS PETRIE

Bromley, Kent

The late Dr. von Willemoes-Suhm

IN the list of papers given in Prof. Wyville Thomson's obituary of the late Dr. von Willemoes-Suhm (*NATURE*, vol. xiii., p. 88), is omitted—"Notes on some young stages of Umbellularia, and on its Geographical Distribution;" (*Ann. and Mag. Nat. Hist.*, 4th ser., vol. xv., p. 312, and pl. xviii. A.) This animal is a Pennatulid, and was obtained during the *Challenger's* cruise at depths from 1,375 to 2,600 fathoms. It is usually associated with decidedly deep-sea forms.

To this may be added two papers which were read at the meeting of the Royal Society on Dec. 9th, "On the Development of *Lepas Fascicularis*, and the '*Archizoëa*' of Cirripedia;" and "Preliminary Remarks on the Development of some Pelagic Decapoda."

J. C. G.

Seasonal Order of Colour in Flowers

I HAVE often noticed during spring and summer that flowers appear to follow the spectrum from the blue of the wild hyacinth to the deep scarlet of our summer flowers. Will any of your correspondents tell me if during spring the actinic rays of the sun are in greater force, and whether these chemical rays are those which revivify seeds and plants after their winter's hibernation? During autumn I have noticed the same gradations in the colours of flowers.

C. E. HERON ROGERS

Retford, Notts, Dec. 7

OUR ASTRONOMICAL COLUMN

ATLAS PLEIADUM, Σ 453.—Has anyone provided with a large telescope examined this object of late for duplicity? It appears very improbable that Struve's epoch of

1827 can have been put upon record through an illusion. We find in "*Mensuræ Micrometricæ*," p. 2:—

1827.16 Position $107^{\circ}8$ Distance $0''.79$ Mag. 5 and 8.

It is then termed "duplex difficillima." On subsequent examination, 1830.25, the position was found to be 29.2 , and the distance only $0''.35$. In 1831 and 1832 it was not seen double with the Dorpat refractor under a power of 600. Struve remarks, "*si justa est anni 1827 notatio, quod vix dubitare potest, cum nullum observans dubium expresserim, motus in hoc systemate egregie celer inesse debet.*" From 1840 to 1847, Mädler observing with the same instrument, says he always found the star "quite round," and to our knowledge a few years since the star was pronounced single after close scrutiny with more than one of the larger telescopes.

THE "GEGENSCHIN" OF THE ZODIACAL LIGHT.—The last number of the "Monthly Notices of the Royal Astronomical Society" contains some interesting remarks by Mr. Backhouse, of Sunderland, bearing upon this phenomenon of the zodiacal light, as observed by him between the years 1871 and 1875, which are the more notable, as the observer does not appear to have been aware of its previous observation, and consequently the particulars he has placed upon record afford a very satisfactory confirmation of the existence of this feature of the light. The "gegenschin" consists in an elliptical patch of light almost directly opposite the sun's place, with the greater axis nearly in the plane of the ecliptic. Attention was first directed to it in the year 1854 by Herr Brorsen (the discoverer of the comet of short-period—February 1846)—while observing at Senftenberg in Bohemia. It may be inferred that it is generally difficult to distinguish or trace, with certainty, as even Prof. Heis, of Münster, with his excellent vision and long practice, did not succeed in verifying Brorsen's observations for several years (*Zodiacallicht-Beobachtungen*, Heis, 1875). Observations of the phenomenon will be found in various numbers of the "*Astronomische Nachrichten*." Brorsen thought this opposition-glow of the zodiacal light was more distinctly seen near the vernal than the autumnal equinox; the observations of Heis are in January, April, and in December. There are also observations by Schmidt at Athens, Schiaparelli at Milan, &c.

THE COMET OF 1729.—This very remarkable body presented a case which is quite unique in the history of these interesting denizens of the solar system. It was visible without telescopic aid, or with little optical assistance, at distances from the earth and sun not greatly inferior to those to which any other comet has been followed even with such an instrument as the Poulkova refractor, and it may be inferred was of very different constitution.

The comet was first detected by Sarabat, an ecclesiastic of the Jesuit order, at Nismes, on the 31st of July; the moonlight, which was then on the increase, prevented his seeing it on the following evenings until Aug. 8, when the moon being totally eclipsed, he recovered the comet and gave intimation of his discovery to Cassini at Paris, by whom it was observed on forty-four nights between Aug. 31 and Jan. 18 following. Cassini saw the comet without fixing its position until the end of the same month. While it was visible with difficulty to the naked eye, in a 16-feet telescope it appeared "*en forme d'une petite étoile nébuleuse avec une chevelure autour d'elle, dont l'étendue paraissait au moins aussi grande que le diamètre de Jupiter, vu avec une pareille lunette.*" This is about the extent of our information relating to the aspect of the comet.

The best orbits are those of Burckhardt (hyperbola and parabola), published in the "*Connaissance des Temps*" for 1821. If we employ the parabolic elements we find the following distances of the comet from the sun and earth expressed in units of the earth's mean distance from the sun:—