of the former $66\frac{1}{2}$. This excess of angle is a false quantity, because it does not come as a radiant from one single position in the axial focal point, but from other lateral rays of the marginal foci.

From this it may be inferred that unless precautions have been taken to exclude these lateral pencils, all measurements for ascertaining large apertures have hitherto been erroneous, and far in excess of the true pencil from a single radiant point; and this source of error will exist from the same cause, whether the measurement is taken by means of the usual convenient sector, or what may be termed the telescope method, of obtaining distinct distant images with a suitable eye-piece arrangement.

I disclaim invidious intentions in putting forth the expression, that the apertures of high-power object-glasses published in the lists of opticians are all wrong, and shall not quote any particular one as an example. If this is a scientific fact, let it be fairly discussed as such, without rancour or personality. The question is a simple one, and I cannot at present see that I am in error in a theory clearly proved by point of fact measurement.

The result may always be verified by the measurement of the focal distance (suitably corrected by the adjustment) and size of the working portion of the front lens, through which rays are admitted. Many will be surprised to find how small this portion is. In high powers it may be defined by screwing the object-glass in the place of the achromatic condenser, and the diameter of the spot measured with a micrometer, used with a low power. The front of the object-glass should have a drop of milk dried upon it to serve as a screen, or if the lens is flush with the setting, a piece of thin covering glass, greyed on one side, may be laid upon it, by means of which the proper light area can be determined having a definite margin.

V.—*On Zeiss' $\frac{1}{25}$ th Immersion.* By W. J. HICKIE, M.A.

ABOUT the 18th of last October I obtained from Zeiss, of Jena, one of his $\frac{1}{25}$ ths (No. 3 immersion), and during the first week in January the same maker forwarded two others, which he had completed only a few days before. On the evening of the same day all three were submitted to a trial of their capabilities. In the account I am about to give, I shall indicate the three $\frac{1}{25}$ ths then examined by the letters A, B, C, where A denotes the objective first sent, while those sent in January are denoted by the letters B and C. As A had been in constant use for more than two months, I had had ample opportunity of making myself acquainted with all the peculiarities of this description of lens, as also with the

exact adjustment of the correction-screw required for a considerable number of the most trying tests. Having carefully compared these glasses, one with another, and likewise with Seibert's $\frac{1}{4}$ th and Schieck's (so-called) $\frac{1}{10}$ th, both immersions, I left off with a strong impression that the one I have called C was markedly superior to the other two, reserving the more rigid scrutiny for the following morning. As my habit is, when I have any task of this kind requiring more than ordinary carefulness, to work during the morning hours, and in a darkened room, it gave rise in this instance to a somewhat curious accident. Having arranged the lenses on a sideboard, and, as I thought, in the order in which I have named them, i. e. A, B, C, I renewed the examination of the preceding evening, but this time with the utmost care and exactness. On this occasion, however, to my great disgust, the decidedly best glass appeared to be B; and repeated trials only made its superiority more and more evident. Unwilling to believe that I could have been so maladroit the night before in a matter so straightforward as this, I proceeded to light a candle, and the mystery was solved at once. The glass I had taken up second on each occasion turned out to be C, which had somehow got placed second in order instead of third. As a matter of course, I tried them all again the morning after that; but the result remained the same, and C again proved itself to be the best glass all through. A came next in order of merit, and then, but at a considerable interval, B. It is therefore of C that I shall have to speak in this present paper, as the other two passed immediately afterwards into other hands. I may add, that the maker had himself compared them very carefully, and had been unable, as he declared to me, to detect any difference whatever. Before proceeding further, I will mention briefly the most salient features of this class of objective. The workmanship is conspicuous above all German objectives which I have seen for its extreme neatness, and may be examined all over with a hand-magnifier. There is no extravagant disparity between the breadth of the front lens and the breadth of the hindmost lens; upon which point I shall have something to say later on. Its working distance is remarkably great. But this is characteristic of all Zeiss' objectives. It has abundance of light, even with deep eye-pieces. This C now in my possession bears Ross' E eye-piece, with very little ill effect beyond becoming more difficult to manipulate. It also has complete flatness of field, and is wondrously sensitive to the slightest change of the correction-screw. The following instance will suffice to show this. On *Stauroneis spicula*, when placed vertical, and with the correction-screw set at $11\frac{1}{2}$, the transverse striæ are altogether invisible; but when the correction-screw is at $12\frac{1}{2}$, the same striæ are brought into view with perfect clearness! The screw-collar, instead of moving the front lens, as

is usually the case, moves the hindermost combination; in which arrangement there are certain advantages: it decentres the combinations much less, as is plainly apparent when this species of objective is compared with objectives on the old plan; the object also remains in sight while the adjustment is being altered, and there is at the same time less danger of smashing the covering glass of the slide. It is also well seen how carefully it has been corrected generally, and through all the divisions of the adjustment, from 5 to 16 inclusive; and that even at 15,—and I have only two slides (duplicates) that require so extravagant an adjustment,—its definition is both clear and beautiful. Beyond that it begins to show a little colour. This range, however, is amply sufficient for all reasonable purposes.

I must not omit to mention that certain mechanical alterations have been introduced into his objectives of this year, which did not appear in his earlier issues. The figures and division-lines connected with the correction-screw have been enlarged in size, to suit those who work by lamplight. In his earlier objectives these were so minute, as to make it often a difficult task to discover what was the precise adjustment one had fixed upon as best suited for the particular object under examination. The part containing the figuring and correction-screw has been slightly depressed, so as to keep it more out of the way of one's fingers; and the screw-collar itself has been made to move more stiffly. In his older objectives this part was sometimes so loose, that the proper adjustment often got shifted in handling, which made the glass appear to work badly. These alterations I cannot but regard as improvements, the more so as they were introduced at my suggestion.

I am here reminded of certain recent statements respecting the magnification of Zeiss' objectives, which seem to require some notice. Dr. Griffin* states the magnification of Zeiss' $\frac{1}{6}$ inch, with a B eye-piece, to be $\times 640$, and the magnification of his $\frac{1}{4}$ inch (dry), with the same eye-piece, to be $\times 875$; whereas, if we turn to Zeiss' catalogue, we find the magnification of the $\frac{1}{6}$ th, with the second eye-piece, set down at $\times 330$, and that of his dry† $\frac{1}{4}$ th set down at $\times 500$. So, again, one writer in the 'M. M. J.' states the initial magnification of Zeiss' $\frac{1}{25}$ inch to be $\times 1250$, and is contradicted by another, who appeals to Zeiss' own catalogue in proof that its initial magnification is only $\times 680$. In both cases

* 'M. M. J.,' No. lxxxvi., p. 96.

† I am sure so dispassionate a writer as Dr. Griffin will excuse me for reminding him, that to compare a dry lens with an immersion lens, is not usually considered a fair proceeding; and that he would have done better to have made the comparison with Zeiss' No. 2 immersion ($\frac{1}{25}$ inch); not that I expect the result would have been much different, if the $\frac{1}{25}$ th had been matched against so unique a glass as Powell and Lealand's new $\frac{1}{25}$ inch, but it would have been a fairer course to adopt.

the statement has been made in good faith, and the contrariety is more seeming than real. Magnification is the product of objective, length of tube, and focal length of eye-piece. Now, Zeiss' figures are based on a length of tube of only 6 inches (while the length in England is about 10 inches), and on a focal length of eye-piece somewhat more than 2 inches, while our A eye-piece is, I believe, about $1\frac{1}{2}$ inch focal length.

One quality in this glass (designated as C) which attracted my notice very strongly is its extraordinary resolving power. Its "grip" of an object is something enormous. And, as regards its definition, I may here briefly say, that I am perfectly contented with it; which is saying a good deal; as few are so hard to satisfy in this respect. This I infer from having observed the (in my opinion) very moderate performance which has satisfied my microscopical friends. Indeed, I have often been called upon to admire what (in my own work) I should call a failure. In this connection, a very singular remark is put forward by Otto Müller: * "The resolving power of Zeiss' objectives is nothing specially great. On the other hand, their definition is surpassed by none of the objectives I have examined, and only equalled by few: the clearness and repose of the picture is a perfect pattern."

While accepting Müller's happily-chosen words "Klarheit" and "Ruhe," as exactly conveying my impression of the performance of my own glass (C), under which the object appears, as I have heard it described, like a sunlit landscape, with all the details vividly delineated, and the edge of the striæ, to the outermost margin, as sharp as a knife, I consider the former sentence so singularly in conflict with fact, that I must confess my inability to understand it as having been written by him, unless I am at liberty to suppose that he wanted to see how facts look when turned topsyturvy. Here in this country, if anyone were asked to mention the most noticeable feature in Zeiss' objectives, he would unhesitatingly single out precisely this resolving power, as the one that struck him most. Or may Müller's remark find its explanation in the fact, that he knew only Zeiss' older issues? He himself adds in his Preface: "I ought to mention further, that Zeiss' objectives have in these latest times [i. e. subsequently to the year 1870] been considerably improved, in consequence of which the results I have recorded no longer correspond to the present standard of performance of Zeiss' objectives." I notice also that his list mentions no higher power of Zeiss' than his $\frac{1}{4}$ inch (dry). Some, indeed, among ourselves profess to have detected in Zeiss' objectives a lower scale of definition when compared with those of Hartnack; but, as they omit to mention what lenses of each maker were compared, the remark need not detain us. On one occasion I spent

* 'Modern Objectives Compared,' p. 4.

the better part of a long summer's day at Hartnack's establishment in Potsdam, and that particular superiority in definition was just what I failed to discover! *Chacun a son gout*. But it is of this definition, and its supposed dependence on perfect centering, that I now wish to speak.

The popular persuasion with regard to definition would seem to be, that an objective's ability to bear deep eye-piecing is a sure proof of good definition; and, *vice versâ*, that good definition is a sure proof of its ability to bear deep eye-piecing; and, that failure in these respects is a sure proof of bad centering; and this is the belief of many, of whose opinions I may not speak otherwise than with respect.

It may be briefly formulated as follows: "There can be no more certain test of good definition * than that a glass bear deep eye-pieces exceedingly well."

These I believe to be the prevalent views with respect to the intimate connection between imperfect centering and imperfect definition; so that it becomes an interesting point to inquire "What are the real effects of bad centering?"

Let me first give an illustration. I have in my possession one of Gundlach's earliest $\frac{1}{8}$ ths, brought out when he had his reputation still to make, the definition of which (with a low eye-piece) has always been greatly admired. Now, according to the above theory, this glass ought, by virtue of its excellent definition, to bear deep eye-piecing extremely well; whereas it breaks down utterly even under the light burden of a C eye-piece. Nor will the logic of results be bettered, if we say, that the inability of this glass to bear a C eye-piece is a plain proof that it can have no pretensions whatever to fine definition; for "facts are stubborn chieils, and winna ding."

If definition were possible only under the condition of absolutely perfect centering, then definition would be something still unknown to us; for absolute perfection in this respect is a physical impossibility.

Nägeli and Schwendener † have gone into this matter at some length.

They remark: "With regard to perfect centering, the highest that can be attained by the optician in this direction is at best only an approximation; and it is especially in the stronger objectives that he comes farther short of perfection."

"According to common opinion, exact centering is the condition of aplanatism; and this is the view taken by Harting and Mohl; and yet it is contradicted both by theory and observation."

* Dr. Branwell expresses himself ('M. M. J.,' lxxxv., p. 43) more cautiously: "Deep eye-piecing is, above all, the surest test of correction."

† 'Das Mikroskop,' p. 69.

"Let us then inquire what will be the effect of a slight displacement of the combinations, whereby these, though remaining in the same plane, are pushed aside laterally in any direction."

The reader will see on pp. 70–74 of the work referred to how the editors have carried out their demonstration.

They further remark: "With every increase of displacement a corresponding marginal portion of the object is effaced, until at length the entire margin suffers more or less, leaving only a central portion of the field still retaining its original clearness, which portion, however, may take any eccentric position."

Their final summing up is: "That imperfect centering does, indeed, exert an injurious effect upon the optical image, but that this injurious effect limits itself (if we except the case where the axes of the combinations are not coincident with the axis of the tube), to the margin of the field, and only reaches the centre when the defects of centering are abnormally excessive."

Therefore the effects of imperfect centering are "to limit the area of definition," not to materially affect its absolute quality.

But this perfect centering is the product of several factors, and depends upon the fulfilment of more conditions than one. Granted that the axes of all the combinations are exactly coincident, there will still remain the exact coincidence of these with the axis of the tube,—which may be affected by the form of the tube itself, or even by the screw of the adapters,—and next, the coincidence of all the above with the axis of the eye-piece employed,—which latter, again, may be affected by looseness of fit or indifferent workmanship.

It may, indeed, happen that displacement in one direction is balanced by displacement in another direction; but these are elements that cannot generally be taken into account. Where the second defect comes into operation the injurious effects increase proportionately with the lengthening of the tube. As this defect sets the optical axes of the combinations at a slight angle with the axis of the tube, its immediate result is to introduce "want of parallelism," which is infinitely more destructive of good definition than imperfect centering. Anyone may demonstrate this to himself by looking at an object with a hand-magnifier, and holding the instrument slightly tilted. It will be noticed that I have omitted a case of possible occurrence; that is, where the lenses themselves are tilted in the setting. This, however, is not what is commonly included in the popular idea of "bad centering," but approximates to "want of parallelism." What is tested, then, by deep eye-piecing is "want of parallelism" *plus* chromatic and spherical aberration. Nägeli and Schwendener (p. 139) remark, in reference to this, that "coarse, blurred outlines, and coloured rims, which do not belong to the object, invariably point to defective correction of chromatic and spherical aberration;" and that "defective centering is betrayed

by the shifting position of the object when the objective is turned round" (p. 163). This operation, however, as it depends for its success upon the precision of the brasswork and the accuracy of the screw of the adapters, is seldom to be relied on. But the common practice of turning round the eye-piece in the tube is altogether illusory.

These considerations have suggested to me that possibly, where a foreign objective is tested on a microscope for which it was not constructed, and gets suspected of imperfect centering, the blame may be more justly apportioned between the maker of the microscope and the maker of the adapter. The chances, then, of perfect centering, as the phrase is popularly understood, are something infinitesimally small; and the generality of French and German opticians do not seem to distress themselves to any great extent about its attainment. Their *modus operandi* is pretty much as follows:—The back combinations are fixtures, put together and placed as fairly as is compatible with a moderate expenditure of labour. The workman then addresses himself to what he regards as the real business of the day, that is, to "marrying lenses," as he calls it. In other words, he tries on front after front, taken almost at random out of a great number, till he hits upon that one which exactly accommodates itself to the previous combinations, these fronts being, as an optician expressed himself to me, "made in bushels; yes, sir; made in bushels by girls." I could give, if it were desirable to do so, a goodly list of French and German opticians who follow this facile plan of manufacturing "first-class objectives." On the other hand, I could only mention two with any confidence, who, to judge by their workmanship, appear to follow a better advised method, and these two are Zeiss and Seibert. Glasses made on the first plan are easily recognized. Let the intending purchaser only hold them up to the light, and he cannot fail to notice the enormous disproportion between the breadth of the front lens and the breadth of the hindermost lens. There is, of course, a trick in this. As spherical aberration turns upon the fact that the focus of the peripheral rays is always shorter* than that of the central rays, which disparity increases, *pari passu*, with the distance from the axis,† it is quite conceivable that the adaptation of an extremely minute front lens to wide back combinations may shorten the labour of correction very considerably. Such glasses, when used with a low eye-piece, sometimes exhibit very brilliant definition; but then it is only within a certain limited range of the correction-screw. They will also generally be found, in spite of their good definition, to

* Thus, if we divide the periphery into a number of zones, the rays of the outer zone will have a shorter focus than those of the next zone, and the rays of the second zone a shorter focus than those of the third zone. See Nägeli, *l.c.*

† Nägeli and Schwendener, p. 43.

break down under deep eye-pieces, which popular theory says they ought not to do, and are further characterized by having almost no working distance, while their real magnification is always vastly below the nominal amount.

I propose to give now a tabular statement of the estimated performance of the three lenses I have mentioned; namely, Zeiss' $\frac{1}{25}$ inch (C), Seibert's $\frac{1}{24}$ inch, and Schieck's $\frac{1}{40}$ inch. My manner of procedure has been as follows: I have taken 1000 as the highest number of marks that could be assigned to any objective for its performance, and have made my estimate in each case with this number as the maximum. In some instances I have given the average of three different trials.

<i>Stauroneis spicula</i> (horizontal).									
OBJECTIVES COMPARED.								MARKS.	
Zeiss' $\frac{1}{25}$ inch	..	..	..	..	..	..	..	900	
Seibert's $\frac{1}{24}$ inch	..	..	..	..	..	..	..	730	
Schieck's $\frac{1}{40}$ inch	..	..	..	..	..	..	..	650	
<i>Stauroneis spicula</i> (vertical).									
Zeiss' $\frac{1}{25}$ inch	..	..	..	..	..	..	..	600	
Seibert's $\frac{1}{24}$ inch	..	..	..	..	..	..	..	0	
Schieck's $\frac{1}{40}$ inch	..	..	..	..	..	..	..	0	
<i>Frustulia Saxonica</i> (transverse lines).									
Zeiss' $\frac{1}{25}$ inch	..	..	..	..	..	..	..	970	
Seibert's $\frac{1}{24}$ inch	..	..	..	..	..	..	..	450	
Schieck's $\frac{1}{40}$ inch	..	..	..	..	..	..	..	800	
<i>Frustulia Saxonica</i> (longitudinal lines).									
Zeiss' $\frac{1}{25}$ inch	..	..	..	..	..	..	..	800	
Seibert's $\frac{1}{24}$ inch	..	..	..	..	..	..	..	200	
Schieck's $\frac{1}{40}$ inch	..	..	..	..	..	..	..	430	
<i>Navicula crassinervis</i> A (transverse lines).									
Zeiss' $\frac{1}{25}$ inch	..	..	..	..	..	..	..	950	
Seibert's $\frac{1}{24}$ inch	..	..	..	..	..	..	..	750	
Schieck's $\frac{1}{40}$ inch	..	..	..	..	..	..	..	700	
<i>Navicula crassinervis</i> B (transverse lines).									
Zeiss' $\frac{1}{25}$ inch	..	..	..	..	..	..	..	960	
Seibert's $\frac{1}{24}$ inch	..	..	..	..	..	..	..	600	
Schieck's $\frac{1}{40}$ inch	..	..	..	..	..	..	..	520	
Two of these were examined, the second somewhat finer than the first.									
<i>Amphipleura pellucida</i> (transverse lines).									
Zeiss' $\frac{1}{25}$ inch	..	..	..	..	..	..	..	600	
Seibert's $\frac{1}{24}$ inch	..	..	..	..	..	..	..	540	
Schieck's $\frac{1}{40}$ inch	..	..	..	..	..	..	..	500	
Lamplight was employed on each occasion.									

One word here about testing objectives. I have repeatedly heard it asserted, that the only proper course to pursue is to try them all, whatever their number be, under conditions exactly the same; that is, on the same object and with precisely the same

kind of illumination; and at first sight such a course appears to recommend itself to the judgment of all. But it will require no great amount of consideration to convince any reasonable being that no method could be more unfair or more productive of false results. Every high power has its own peculiar idiosyncrasy, has its own particular way in which alone it can be induced to do its best; and the illumination which is admirably suited for one glass may be very unsuitable for another. Or are we to expect all objectives to do equally well under all possible conditions of illumination? for that is pretty much what it amounts to, seeing that almost every microscopist has his own pet mode of illumination. To take an instance from the objectives in my own possession: Schieck's (so-called) $\frac{1}{40}$ inch immersion works best with an Abraham's prism, brought up as close as possible to the stage, the top edge of the prism being half an inch higher than the object, and set at a particular angle. Zeiss' $\frac{1}{25}$ th (C) is at its best when the angle formed by a line drawn from the object and one drawn along the bar of the mirror is exactly two degrees less than a right angle; and it shows itself very sensitive to any departure from this precise arrangement. Seibert's $\frac{1}{24}$ th, again, exhibits its best performance when the above-mentioned angle is some three degrees greater than a right angle. My plan therefore has been to find out under what conditions each lens performs best, and to let each be tried in its own way, and then to estimate the results: a troublesome plan, no doubt, but, as I think, the only fair one.

I may now briefly state what I have been able to do with this C lens.

- (1) Resolved Möller's (so-called) *Nitzschia curvula* into dots, and that too with the greatest prominence.
- (2) Resolved every three out of five frustules of *Amphipleura pellucida*.
- (3) Resolved the transverse lines of *Stauroneis spicula*, with the frustule lying vertical; that is, with the lines in the same direction as the illumination.

The first, I dare say, does not amount to much; and the second, perhaps, is not more than people usually expect from a first-class objective; but that there are *many* glasses able to do the third, and with the same means, is what I shall believe—when I see them do it.

I wish also to have it distinctly understood, that by "resolved" I do not mean "a wheen o' skarts" dimly visible through a dirty mist, but a complete resolution.

In doing the second I employed a silver mirror, with bright sunlight modified by blue glass. For the first and third I had merely a Bockett lamp, with a silver mirror assisted by certain paper shutters.

On the evening of the 7th of March I also saw with this lens, clearly and distinctly, the *longitudinal* lines of *Nav. crassinervis*, for the first time in my life. Did the same again on the 13th.

It will be inferred from the prominent place I have given to *Stauroneis spicula* throughout this trial, that I attach very great value to it as a test for high powers. And such, indeed, is the case. Of its extreme flatness, which recommends it for use with objectives of the very finest and most delicate construction, I need say little. There are other specialities connected with it. While a good $\frac{1}{8}$ th may be able to show the striæ pretty fairly, a still better $\frac{1}{8}$ th will reveal still more; and the best glass now in existence will probably leave something, either in matter or degree, to be shown by the still better objectives which may be used by our aftercomers. Or to put it thus: if a person possess half-a-dozen $\frac{1}{8}$ ths, such that the second surpass the first, and the third the second, and so on, each surpassing its predecessor by a specific degree of excellence, and he try them upon it in the ascending order, beginning with the lowest, he will hardly, when he has tried his last and best glass upon it, rise up with the persuasion that he has completely *exhausted* his test. To be sure, the same might be said, in a certain sense, of almost any test, but of none so truly, so emphatically as of the one here mentioned.

But after all it really does not matter very much what the particular test is, if the operator only observe these three conditions: (1) that the test employed be one that ought to be just within the capacity of the class of objective he is trying; (2) that the operator be sufficiently exacting as to what constitutes the best possible image; (3) that he recollect with sufficient keenness what that best possible image is.

Corrections in the President's Address.

By some unaccountable oversight, in copying out the data for calculating the number of molecules in liquid water, the factor expressing the specific gravity of the vapour of water was omitted, and afterwards overlooked. The number of atoms of a gas should

really be multiplied by $\frac{3}{2} \times 770 \times \frac{1}{.6239} \times \frac{1}{2} = 617$. But,

moreover, on reflecting on the relative reliability of the determinations by the various authors of the number of the atoms in gases, it appears that in taking the mean, greater weight ought to be allowed to that by Clerk-Maxwell, since founded on more recent and accurate data. If his results be considered to be equal in value to those of Stoney and Thomson combined, the mean would be

reduced to so nearly the same extent as the number of the molecules of liquid water is increased by the above-named correction, that the numbers given in the Address may be considered to be as good an approximation to the truth as can be determined in the present state of the question, and all the general conclusions need not be in any way modified.

In order to avoid any misunderstanding, it may however be well to give the corrected numbers, which are as follows:

The contraction of the vapour of water in condensing into a liquid should be to $\frac{1}{1234}$.

The number of atoms and molecules in a cubic $\frac{1}{1000}$ inch should be—

In a gas		6,000,000,000,000
In liquid water		3,700,000,000,000,000
In horn		65,000,000,000,000
In living albumen—		
Albumen		17,000,000,000,000
Water in molecular combination	..	923,000,000,000,000
		<hr/>
		940,000,000,000,000
In a sphere of $\frac{1}{1000}$ inch diameter—		
Albumen		10,000,000,000,000
Water in molecular combination	..	490,000,000,000,000
		<hr/>
		500,000,000,000,000

In the length of $\frac{1}{80000}$ inch there would be about 2000 molecules of water and 500 of albumen, which were the numbers previously adopted, so that the general conclusions are not at all modified by the corrections.