II. *On the performance of Fluid Refracting Telescopes, and on the applicability of this principle of construction to very large instruments.* By PETER BARLOW, *Esq. F.R.S. Cor. Mem. Inst. of France, of the Imperial Academy of St. Petersburgh, &c. &c.*

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IN the Philosophical Transactions for 1827, a paper of mine was published containing an account of a series of experiments which I had carried on with Messrs. W. and T. GILBERT on the curvature of object-glasses for telescopes. In the course of these experiments, I saw so much the difficulty which opticians experience in obtaining large pieces of good flint-glass, that I turned my attention to supplying this material by a fluid. Having, after several attempts, at length found an admirable substitute in sulphuret of carbon, I wrote a short account of my intended construction, addressed to HIS present MAJESTY, at that time Lord High Admiral, and, as such, President of the Board of Longitude, soliciting from that Board assistance in carrying forward my experiments. Having obtained this aid, the result of my first trial was the construction of an eight-inch fluid telescope, at that time the largest refractor in this country. A description of this instrument is given in the Philosophical Transactions for 1829, and some objects are pointed out which had been selected as tests of its performance.

I have however since had more time and better means of testing the instrument; first, through the kindness of Mr. HERSCHEL, who pointed out to me several objects that he had observed with his new twenty-inch speculum; and secondly, by direct observations on the same objects in Sir JAMES SOUTH's new twenty-feet refractor, and in my own telescope. A few of these, which serve to mark distinctly the progress I have made, are given below; but I will first state two or three of my own observations, which, I conceive, tend also to the same object.

In the paper last referred to, I have stated my observation on η Persei, marked as double in SOUTH and HERSCHEL'S catalogue, with a small star at a greater distance; this star is seen distinctly sextuple in my telescope. These stars I had the satisfaction of showing to M. STRUVE in his recent visit to England, and I have since seen them in Sir JAMES SOUTH'S telescope. Another good test of the light of my telescope is found in σ Orionis, marked in the above catalogue as two distinct sets of stars, each triple; whereas, in my telescope, both sets are quadruple, with a double star, or rather two very fine stars between them; the fourth star in the bright set, is a remarkably fine brilliant point, very near to the principal star, and in the same line as the nearest of the original small stars, on the opposite side, so that the three are in one line; or more accurately, the line joining the two small stars touches the margin of the bright star. I might mention several other cases of fine double stars which I have discovered, but I select the above because it is evident that both objects have been well examined with fine instruments, and that the stars I have mentioned had, notwithstanding, escaped detection.

Of the tests furnished me by Mr. HERSCHEL I shall only select two, one of which in particular serves to point out in a very precise manner the limit of power of my telescope. This is the star β Capricorni, which, in the finder, is a coarse double star of about $3'$; but between these two stars, nearly in the middle, but a little below the line of junction, is a very fine double star, discovered by Mr. HERSCHEL, and which he considers a very severe test; he says indeed that he requires no other, of the light of a telescope. This star I can see, and, under favourable circumstances, distinctly; but still I have not sufficient command of it to see it double. We have thus the exact limit defined at which the light of this splendid instrument surpasses that of my telescope. The other object to which I have alluded is ϕ Virginis: this he considers a very easy double star, although it had before escaped detection; it is however rather close. This star I could see very distinctly one evening (June 4th), the moon being very bright and full, on the meridian, and within an hour of the star. I mention this object because it requires a certain degree of defining power; in point of light it involves no difficulty. Mr. HERSCHEL could see it when his aperture was reduced to six inches.

Amongst the objects which I have seen in Sir JAMES SOUTH'S twenty-feet, there is also one in particular which forms a good test of the relative power of his instrument and mine; this is MESSIER's twenty-second nebula. This object, which in a good $3\frac{1}{2}$-inch refractor has only the appearance of a white cloud, I saw in the above instrument resolved into an immense number of brilliant small stars. In my telescope also it is resolved into apparently as great a number of stars, but the full power of the instrument is exerted, and still the resolution seems scarcely complete. In fact, my instrument appears to labour to effect what seems to be quite within the power of the other.

I wish particularly to direct attention to this object and that of β Capricorni, because, where the same object can be seen without any very apparent difference in two instruments, or where it can be only seen in one, the great test of comparison is lost; but in those I have mentioned, the exact limit of power is defined.

Amongst the objects I have examined in Sir JAMES SOUTH's telescope, and repeated in mine, for the purpose of comparing the defining powers of the two instruments, were the planets Jupiter and Mars. These were both more sharply defined in Sir JAMES SOUTH's than in my telescope, but the superiority was by no means so great as I had expected. I fortunately saw the shadow of Jupiter's third satellite pass over the disc, on the 8th of August, at Kensington, and it exhibited a fine black round spot, extremely well defined; and on the 13th of the same month, I witnessed precisely the same phenomenon in mine, and, as far as the definition of the shadow was concerned, with an effect in which I could not distinguish an inferiority; it had the appearance of a small black wafer on a sheet of white paper; still, however, the edge of the planet was certainly sharper in the twenty-feet. Both evenings were amongst the finest this climate affords, and the powers employed as nearly as possible equal; viz. about 260 and 450 in both instruments. We also, at Kensington, observed Mars with various powers from 260 to 1400, and it carried 1200 well; with this the white spot near its south pole was seen beautifully distinct, as also a long dark spot on its apparent eastern limb. The bright spot at its south pole I saw also remarkably well defined in my own telescope on the 13th, and a dark spot on its disc very distinct; but it occupied the centre of its disc. The highest power however I used was 500, and it was probably best seen with 260.

There can be no question of the superior defining power of the twenty-feet, and the light is of course also greater; still, however, when I consider that I have been comparing with two telescopes, one of twenty inches aperture, and the other of twelve inches, and each of them twenty feet in focal length, or nearly, while my telescope is barely eight inches aperture, and only twelve feet in length, I cannot but consider the comparison as highly satisfactory.

In addition to the above observations, which have been certainly highly gratifying to myself, I have also had the honour of showing the instrument to many persons, both Englishmen and foreigners well acquainted with astronomy, and in every instance the practicability of the principle of construction has been admitted; a point by no means generally granted when the suggestion was first advanced.

Other obstacles also, independent of the arrangement of the lenses, were foreseen, which time is gradually dissipating; such as the difficulty of permanently securing the fluid, and then, admitting this to be effected, the probability of a decomposition of the glass by the fluid, &c. &c. I have, however, now the satisfaction to state, that the lens of my 3-inch telescope, filled August 5th 1827, continues in precisely its original state, no perceptible change having yet taken place in either the quantity or quality of the fluid, or in the transparency of the glass.

As far as the above observations and remarks extend, therefore, it appears that the essential properties of the flint lens are supplied by the fluid. I beg now to state a few particulars in which the sulphuret of carbon has advantages which the glass has not:—these are, first, that in consequence of the very high dispersive power of this fluid, the correcting lens is placed so far behind the principal plate or crown lens, as to require to be only one half as much in diameter; a highly important consideration in the construction of a very large telescope.

Secondly, the combination is such as to give a focal power one and a half times the length of the tube, or, which is the same, the telescope may be reduced to two thirds the length of a glass telescope of the usual kind, without incurring a greater amount of spherical aberration in the front lens.

Of the latter advantage, however, I have not ventured fully to avail myself in my 8-inch, because, as I knew the general opinion was against the success of the experiment, I was fearful of failing in the beginning by attempting too much.

I have therefore made the length twelve feet, to an aperture of eight inches, which, although shorter than opticians would choose to work in the usual achromatic, is not so short as this principle of construction would admit, and which in any new case I should not hesitate to adopt. Indeed, according to the form of construction I am now about to propose, a telescope of two feet aperture and twenty-four feet in length would not have more spherical aberration to contend with, than a telescope of the usual construction of six inches aperture and twelve feet length, which is fully within the range of the usual practice; at the same time I will not undertake to say that I could on so large a scale confine the length to twelve times the aperture, although I should certainly attempt it in the first instance. But if the length extended to even fifteen or eighteen times the aperture, I have little doubt of making the instrument manageable by one person, by adequate mechanical arrangements, and of producing a telescope which would as much exceed the most powerful telescopes of the present day, as these exceed the refractors of highest repute at the close of the last century.

Whether such an instrument will be undertaken at present, depends upon circumstances which I cannot command. I can only say, that if such a construction were entrusted to my direction, no exertion should be wanted on my part, to render it complete and worthy of the present state of English science. At all events I cannot doubt that the spirit of scientific enterprise will lead ultimately to the attempt; and in order to facilitate the accomplishment of it, as far as lies in my power, I have in the following pages described the nature of the arrangements which in my opinion would most contribute to success.

In my former paper I have given a formula expressing the relations between the length, foci, and distances of the lenses, and have remarked upon the almost infinite variety of forms to which it leads; some of them, I have stated, would probably be found in practice preferable to others, although they are all equally correct in theory. Of these cases, some have since suggested themselves to me; and others will also probably be detected, by a due examination of the formula and tables, which Professor Littrow, of Berlin, has recently presented to the Astronomical Society, relative to this form of telescope; with tables of curvatures, both direct from the formulæ of Euler (reduced to the case of open lenses), as also indirectly from principles of his own. I have not as yet had an opportunity of examining these cases, but, from the well-known ingenuity of their author, I cannot doubt of finding in this memoir many *useful suggestions.*

The great change, however, which I propose to make in the construction of this giant telescope, is to have two front lenses, which will be attended with advantages not involved in the above considerations. At present, in consequence of the diameter of the fluid lens being only half that of the front lens, it is difficult to get a sufficient quantity of spherical aberration in the former, to correct that of the latter ;—for although we give to the plate lens the curvature requisite for reducing its aberration to a minimum, yet the fluid lens is obliged to be made considerably concavo-convex (a form not to be used when it can be avoided), in order to produce a sufficient aberration in the fluid to correct it. Moreover I have hitherto employed parallel meniscus cheeks to contain the fluid, which present a practical difficulty, if not a positive impediment, to good centering. This will be seen immediately when we consider that when a lens is double-concave or convex, and also when it is concavo-convex, if the radii of curvature are very unequal, the centering may always be effected: for the line joining the centres of the two spheres, or this line produced in the latter case, must pass through the lens, and indicate its true centre: but when the lens has parallel surfaces, or the radii equal, if the two spherical centres be not coincident from the tool itself (a very improbable case) the line which joins them can never cut the lens, and consequently it can have no true centre. All these evils will, however, be avoided in the proposed application of two front lenses, which, by being placed each in what opticians call their best position, will at once reduce the spherical aberration of the front lens to about one third of its present amount, and thereby enable us to correct it by the fluid lens without adopting the distorted form rendered necessary under present circumstances.

Another important consideration is also involved in this form, relative to the facility it affords of obtaining the plate-glass. If the front lens were single, the thickness would be such as would require the glass to be made specifically for the purpose, and of course all the delay and expense of previous experiments would be incurred; whereas, by dividing the whole amount of curvature between the two, the usual thickness of plate-glass, as at present manufactured, would be sufficient, and we might have the selection from large stores of the best glass at a trifling expense; and as to that of the correcting fluid, or substitute for the flint-glass, it is so very inconsiderable as not to deserve being mentioned; although, if it were possible to obtain a piece of flint-glass large enough for such a purpose, scarcely any price, however great, would be thought exorbitant. In the instrument proposed, nearly the whole expense would be the workmanship, and I must think it very inconsiderable in comparison with the magnitude and importance of the undertaking.

I had intended to have concluded this paper by giving the curvature, foci, &c. which I have computed; but as they are merely supposititious, as far as they are dependent on the index and dispersion of the front glass, it is perhaps better to withhold them. The only object I had in making them was to form some idea of the requisite curvature, thickness of glass, &c. They can only of course be permanently made after the plate-glass has been selected.