XVII. *Some account of the transit instrument made by Mr.* Dollond, *and lately put up at the Cambridge Observatory. Communicated April* 13, 1825. *By* Robert Woodhouse, *A. M. F. R. S.*

Read May 19, 1825.

As I am inclined to hope that the observations to be made at the Observatory lately established at Cambridge may, at some future period, be useful to astronomical science, I beg leave to send a brief description of our transit telescope, the only large instrument which we are at present possessed of.

The annexed drawings, which I have caused to be made of the instrument, will explain its construction.

Its dimensions are nearly the same as those of the Greenwich transit made by Mr. Troughton.

	Ft.	In.
Its focal length is - - -	9	10
Its aperture - - -	0	5
The length of the axis between the piers	3	6

The weight of the instrument is 200lbs.

The instrument is counterpoised; and the whole lengths (2 inches) of the pivots rest on the Y *s*.

Seven fixed wires are placed in the focus of the object glass, and two other wires moveable by a micrometer screw; the interval of which wires is equal to the interval between any two of the fixed wires, and, *equatorially*, is 17^s.88.

The two small graduated circles (see the figure) with their spirit levels, fixed near to the eye-piece, are for the purpose of finding a star's place in the meridian. Each circle is furnished with two verniers; one for polar, the other for zenith distances.

I wish to add a few words respecting the determining the place of the transit room, and the adjusting the instrument to the plane of the meridian; which, as we had in the beginning no astronomical point to stand on, was a matter of some trouble.

Our first object was, if possible, so to fix the site of the transit room, that its meridian mark might be placed on the steeple of Granchester church, distant to the south about $2\frac{1}{2}$ miles from the field on which the observatory was to be built.

The first approximations to such site were made by adjusting the middle wire of a small transit telescope (18 inches long) to the spire, or iron rod of the steeple, and by comparing the sun's transit with the time brought up by chronometers from Mr. Catton's observatory at St. John's College. Our second approximations were made by observations of *high and low* stars with the small transit instrument above mentioned.

According to the results thus obtained the piers of the transit were placed; and when, in June 1824, the instrument was put upon them, were found to be placed with considerable exactness. From the above time observations have been constantly made with the instruments described in this paper, and with a clock made by Molyneux and Cope.

The first operation was to determine the clock's *rate*, which was done by observations of the same stars on successive days: the next, to determine the clock's *error*, which was found in the usual way, by deducting the observed passages of stars from their tabulated, or computed right ascensions.

The clock's error, as it was to be expected, was found, after allowing for its rate, different with different stars; which is a sign of the instrument being out of adjustment in some of its parts. The error might be in the line of collimation; in the axis not being horizontal; or, which was probably the chief cause of error, in the transit deviating from the plane of the meridian. Any one, or two, or all of these circumstances might occasion the noted difference in the clock's errors.

For instance, the clock being before sidereal time, its error from α Cygni was found to be less than from α Aquilæ. This might arise from the western end of the axis being too high, or from the line of collimation deviating to the east, or from the transit deviating to the west. A single observation such as this, or any number of the same stars, would leave us in doubt respecting the causes of the want of adjustment; but a third star would lessen this doubt. Thus, if, the clock's error after allowing for its rate being from α Cygni $4^s.721$, and from α Aquilæ $4^s.854$, we attributed the difference of errors to a defect of horizontality in the axis, the quantity of such defect would become known. Let it be expressed by H, the clock's error by ε; then for the latitude of Cambridge we should have two simple equations between H and ε, from which both may be found

$$-4.721 + \varepsilon = 1.39\,H, \ \alpha \text{ Cygni}$$
$$-4.854 + \varepsilon = .75\,H, \ \alpha \text{ Aquilæ};$$

and, accordingly, $H = 0^s.2$, nearly.

With this value of H, the error of time for α Urs. maj.

would be $0^s.4288$ ($= 0^s.2 \times 2144$); which not being found to agree with the observed error (or rather the difference of its observed passage and its computed right ascension), showed that the difference of the errors of the clock had been wrongly, or partially, assigned.

If we suppose the difference of the errors of the clock to arise from two causes—the want of horizontality and the deviation of the transit from the plane of the meridian, then, calling the latter deviation H, we have, instead of the former, these equations:

$$-4.721 + \varepsilon = 1.39\,H + .185\,Z$$
$$-4.854 + \varepsilon = .73\,H + .7\,Z$$

to which a third similar equation must be added for α Urs. maj.

If from such three equations we determined H and Z, we might proceed as before, and examine, by means of a fourth star, whether it were necessary to suppose the existence of a third cause (an error in the line of collimation for instance) to account for the differences in the clock's error.

If C should denote the error of collimation, dt the error of time, c the colatitude of the place of observation, δ the star's north polar distance, the general form of the equations for determining H, Z, &c. is

$$-dt + \varepsilon = H\,\frac{\cos.\,(c-\delta)}{\sin.\,\delta} + Z\,.\,\frac{\sin.\,(c-\delta)}{\sin.\,\delta} + \frac{C}{\sin.\,\delta}$$

In this way we might consider the subject in *all its generality* (as foreign writers express themselves); and from observations alone, arrive at a knowledge of the defects of the instrument. And this mode of considering the subject is not without its use, since it may be applied to recorded and ancient observations; as BESSEL has done in the case of

BRADLEY'S Observations. But no practical astronomer, I apprehend, can be so fond of encountering difficulties,* as to adopt this mode of adjusting his instrument; for, if from one set of equations he deduced the values of H, Z and C, he could not, by reason of his imperfect knowledge and management of the screws of his instrument, at once adjust it; but would be again and again obliged to repeat his observations, and the solutions of the resulting equations.

But this is not all. The differences of the clock's errors are the differences of the differences of the observed culminations of stars, and their tabulated or computed right ascensions, and therefore must partake of the uncertainties to which the latter quantities are subject. The point to be aimed at in adjusting an instrument is, to adjust it by means that do not rest on the results of astronomical science.

* As a kind of proof of the great *uncertainty* of determining the deviations of the instrument by the method of equations, I subjoin the following instance:

October 13, 1824.	Time by Clock.	Æ	Errors.
	h. m. s.	s.	s.
α Aquarii	21 56 52.86	48.72	4.14
α Pegasi	22 56 8.74	4 .4	4.34
α Andromedæ	23 59 27 .7	23.25	4.45

whence, the axis being horizontal, and the clock going sidereal time, we have these equations:

$$-4^s.14 + \varepsilon = .8028\,Z + C$$
$$-4.34 + \varepsilon = .6347\,Z + 1.032\,C$$
$$-4.45 + \varepsilon = .4627\,Z + 1.114\,C$$

from which, $Z = 1^s.48$, $C = 1^s.52$; but if an error of $0^s.1$ had occurred in the observations, or if we suppose the tables to be erroneous to that degree, and the second equation had been

$$-4^s.44 + \varepsilon = .6347\,Z + 1.032\,C,$$

then, instead of the preceding values of Z and C, we should have had these:

$$Z = 2^s.935$$
$$C = 6.033$$

The old methods of adjusting a transit instrument do not rest on such results; and the old method of proceeding seems to me the most sensible one, that of separately and successively correcting each cause of defective adjustment.

The axis can be made horizontal, or its defect of horizontality known, by the level, the plumb line, or by reflection.

The line of collimation can be adjusted by means of a small object in, or near to, the horizon. In this operation a small defect in the horizontality of the axis will have scarcely any effect on the accuracy of the operation. If the mark should, for instance, be 2° above the horizon, and one end of the axis $5''$ higher than the other, the error in collimating from that cause would, in the latitude of Cambridge, be only $0''.1075$. The error in the same operation with the pole star, supposing it be fixed, would be $1'\ 11''.5$.

The third adjustment, which is the most troublesome, is to place the transit instrument in the plane of the meridian; and there are two methods of effecting this: one, by *high and low* stars; the other, by circumpolar stars, or, as it almost always happens in practice, by the pole star.

The essential difference in these two methods is, that the former rests on the results of astronomical science, whilst the latter does not so rest; and this circumstance gives the latter a decided advantage over the former, when it is necessary to make a nice adjustment. Yet there is not wanting considerable astronomical authority for placing the two methods on a level, the one with the other. Baron de ZACH, for instance, views each as an equally good method; and in his Tabulæ speciales Aberr^{s}. et Nut^{s}. gives instances of the adjustment of a transit instrument (a 5-feet one by DOLLOND); the first, by the comparison of the passages over the meridian of

Capella and Rigel; the second, by the passages of Capella above and below the pole: and the result, equal to a deviation of 12″.685 to the east, is in each case the same: a coincidence of marvellous accuracy; and which, if the observations were exactly noted, we must suppose to have arisen from a fortuitous balancing of the errors of the observations with those of the tables.

In the method of *high and low* stars I suppose, which is almost always the case, that the clock's error is found by subtracting from the observed passage of the star its computed right ascension. The error may indeed be found by *equal altitudes*, should the observer possess an altitude and azimuth instrument of sufficient accuracy for that purpose. But should he not, the adjustment of the transit instrument by high and low stars must partake of that uncertainty to which we are subject in computing the true apparent right ascensions of stars.

We have only to look at the catalogues of stars by different astronomers to be convinced of the existence of such uncertainty. If, indeed, the tabulated right ascensions differed only by a constant quantity, the *difference* of the errors of the clock, on which the method of high and low stars depends, would be the same, whether we employed BESSEL'S or the Greenwich catalogue. But it is otherwise: to instance this, on the 8th October, 1824, the clock going very nearly sidereal time, the passages of Arcturus and β Urs. minˢ. were as follow:

	Time by Clock.	Æ by N.A.	Error.	Æ by SCHUMACHER.	Error.
	h. m. s.	s.		s.	
Arcturus -	14 7 44.56	40.18	4.38	39.98	4.58
β Urs. min. -	14 51 20.43	14.67	5.76	14.95	5.48

Hence for determining the deviations of the transit instrument we have, respectively, the following equations:

By Nautical Almanack.

$$\left.\begin{aligned} -4.38 + \varepsilon &= .5657\ Z \\ 5.77 - \varepsilon &= 1.48\ Z \end{aligned}\right\} , Z = 1^s.52$$

By SCHUMACHER'S Tables.

$$\left.\begin{aligned} -4.58 + \varepsilon &= .5657\ Z \\ 5.48 - \varepsilon &= 1.48\ Z \end{aligned}\right\} , Z = 0^s.98.$$

The value of the deviation (Z) is uncertain then to the amount, and more, of half a second of time. From such kind of uncertainty, the method of circumpolar stars is entirely free; its characteristic excellence, as it has been already said, consists in its being independent of the results of astronomical science.

In what I have said, I must be supposed to speak of the exact adjustment of large instruments. The method of high and low stars is very convenient, and easily practised: it informs us, in the space of a few hours, of the nature and degree of the deviation of the instrument; and in some cases, when the transit instrument is prevented by its situation from being directed to stars beneath the pole, it is almost an indispensable method.

I wish to add a few words respecting the adjustment of the line of collimation by means of the reversion of the transit instrument during the passage of the pole star. This method has indeed the air of being philosophical; but, according to my opinion, is neither so easily practised, nor so certain as the old method. It is liable to the uncertainty of the times of the pole star's passages over the wires; and always requires, before and after the observation, the examination of the horizontality of the axis. Without attention to this latter circumstance the method is worth nothing; for, if H should be the error in horizontality, the corresponding error in time would, in the latitude of Cambridge, be equal to about 28.6 H. When we adjust according to the old plan, the collimation by means of an object near the horizon, the operation of levelling is not required; which in large instruments is rather a troublesome one; and certainly is not, what M. DELAMBRE states it to be, "the affair of an instant."*

The level indicating the degree of the defect of horizontality, enables us to correct the time;† and this correction is made on the supposition that the instrument is in the same state when the star is observed, as it was during its examination by the level. It is therefore, other things being equal, expedient to examine by the level, as nearly as it is possible, at the time of observation. But this I am unable to do; as I will show, by stating a circumstance rather deserving of attention. The tube of the telescope is braced to the axis (see the figure) by four tubes. The stations of the two

* Il ne faut pas commencer d'observations sans avoir rectifié l'horizontalité de l'axe, ce qui est l'affaire d'un instant. Astron. tom. i, p. 431.

† Mr. DOLLOND considers the value of 1 division of the scale of the level to be equal to 1″. I have determined its value astronomically. Previously to a star's culmination, I lowered the eastern end of the axis 10 or 12 divisions, and observed the star's passages across the four first wires. I then caused the western end to be lowered, and observed the star's passages across the three remaining wires, and then examined the level. The following are the results:

δ Cephei - -	″1.014	
α Cygni - -	0.9	
δ Draconis - -	1.005	
α Cephei - -	.855	
Polaris - -	0.9516	Mean 0″.9451.

persons who level are opposite, and contiguous to the south-west and north-east braces. Being in the constant habit of examining the meridian mark, in order to know what degree of stability the instrument possesses, I found, after levelling, that the south meridian mark was to the east of the middle wire. In about 10 minutes the middle wire returned to the meridian mark, and bisected it. I noted this circumstance a second, third, and fourth time, and then began to inquire whether I had conjectured rightly in attributing it to the expansion of the tubes or braces. For this end, I placed a heated blanket across the south-west and north-east braces, and found the meridian mark deviating to the east of the middle wire: a contrary effect was produced by placing the blanket across the south-east and north-west braces. In these trials the object glass was towards the south: contrary effects took place when it was turned to the north.*

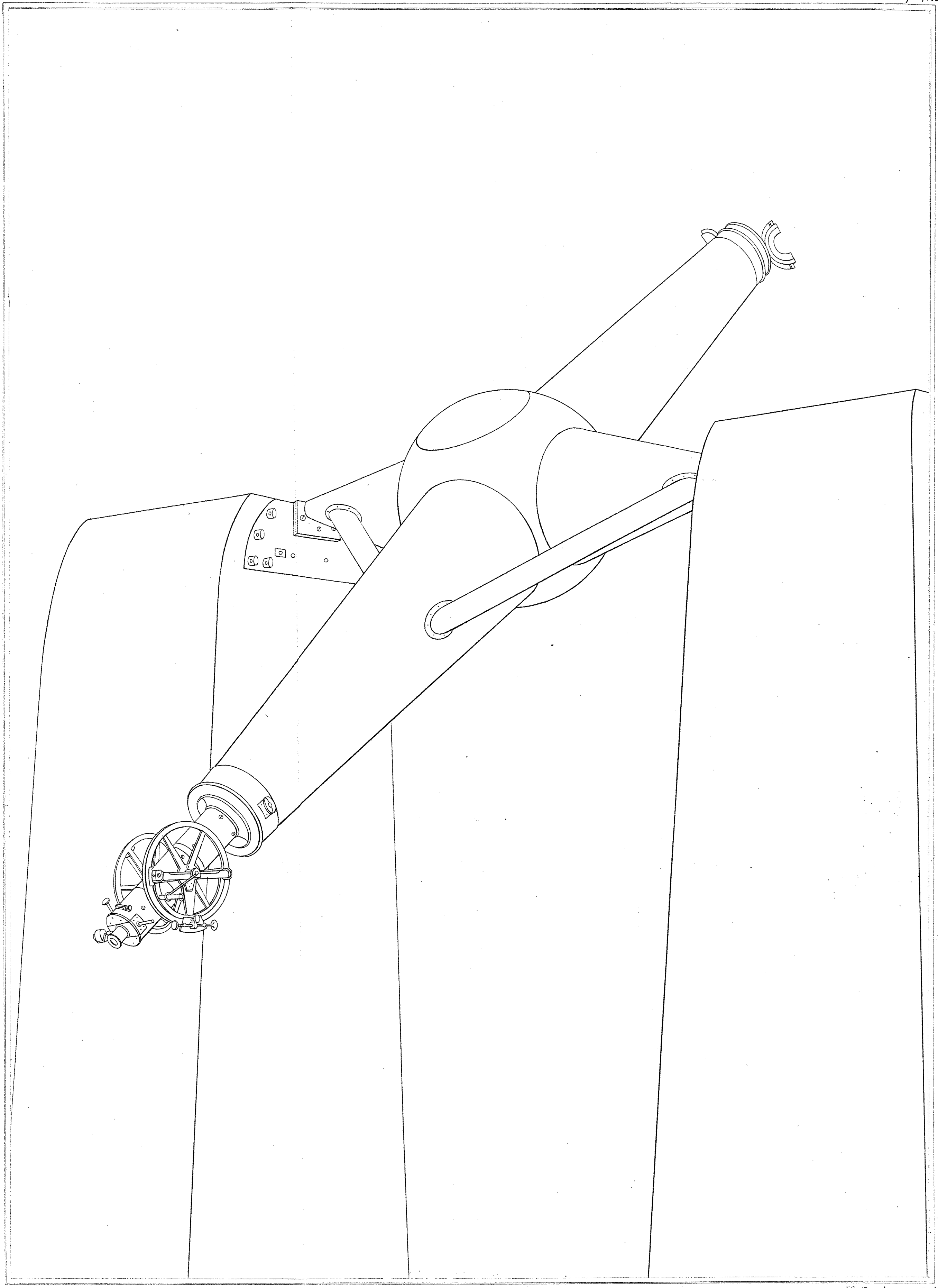
As yet I am unable to say whether or not the sun's rays falling on the braces, during an observation of his transit, affect the accuracy of the observation. I am enquiring into that point, and have ordered a screen to be made to protect the braces from the rays of the sun.

After repeated trials, I have been obliged to abandon the counterpoises;† instead of relieving the instrument, they render it unsteady. It has happened with them (as it has happened in cases of a different nature), they have overpowered what they were meant only to assist.

* The effect I have noted is somewhat of the same kind as that which was complained of in HALLEY's transit. See BRADLEY's Observations, vol. i. p. 2.

† They are now with Mr. DOLLOND, who is endeavouring to remedy their defects.

My chief study, since the fixing up of the instrument, has been to obtain a thorough knowledge of it: to find out its defects, should it have any, their nature and degree. The observations of stars have been chiefly made for, and have served that end; but they are not, I think, otherwise useful, nor worth registering.



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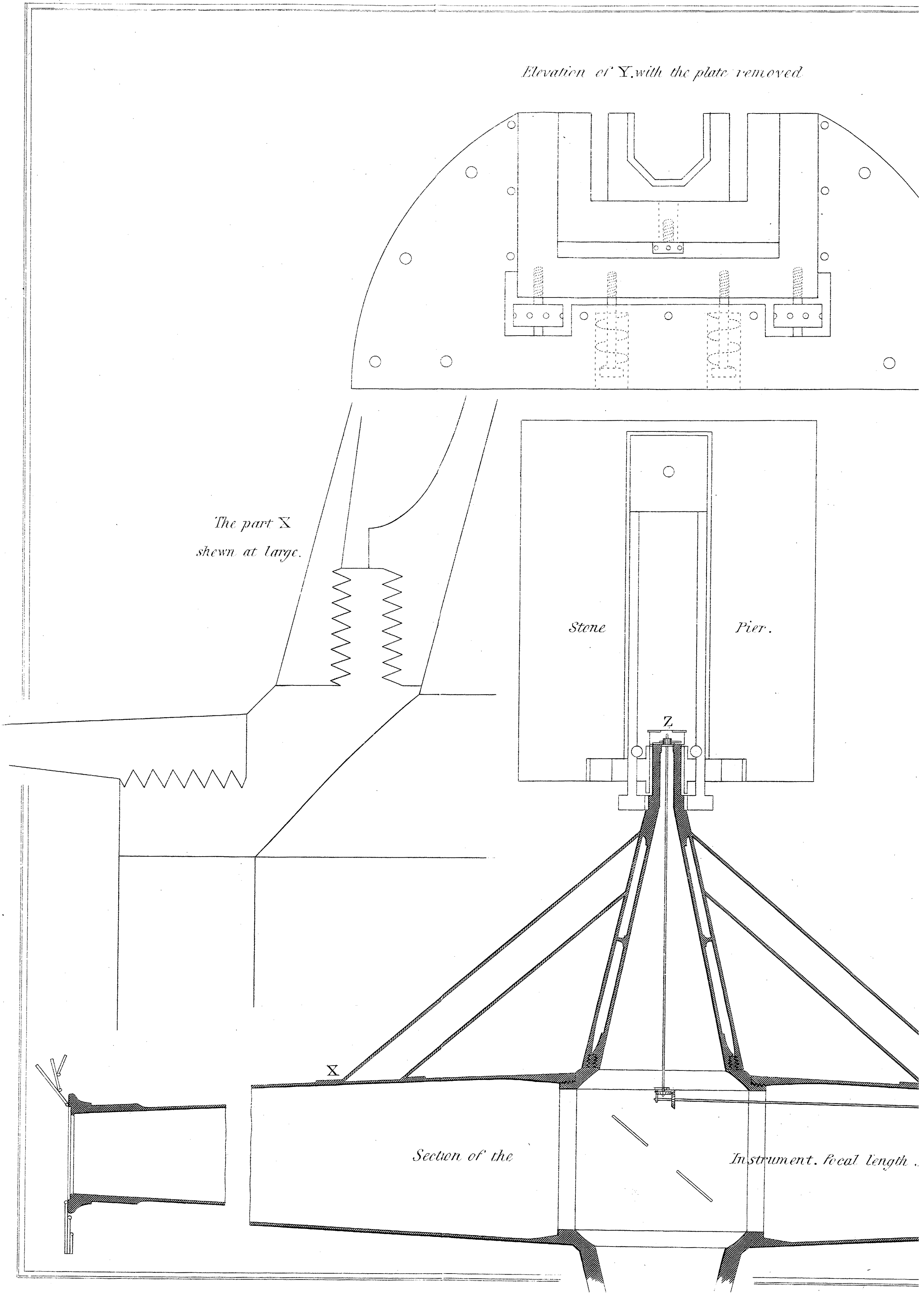
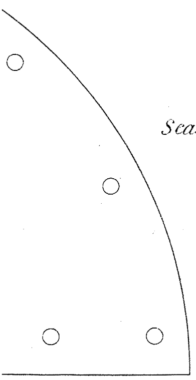
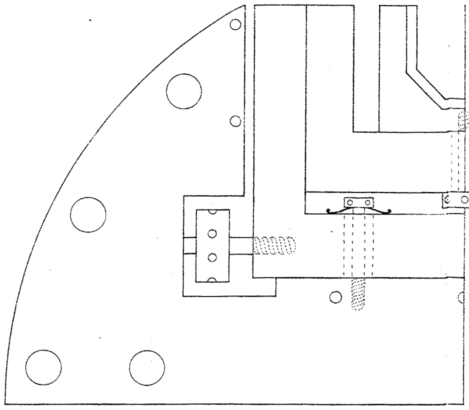


Elevation of Y. with the plate removed.
The part X
shewn at large.
Stone
Pier.
Z
X
Section of the
Instrument. Focal length

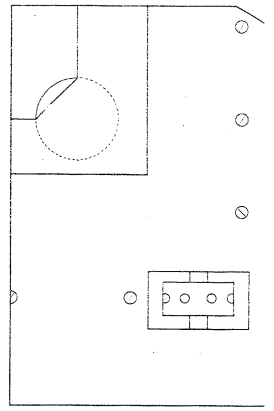


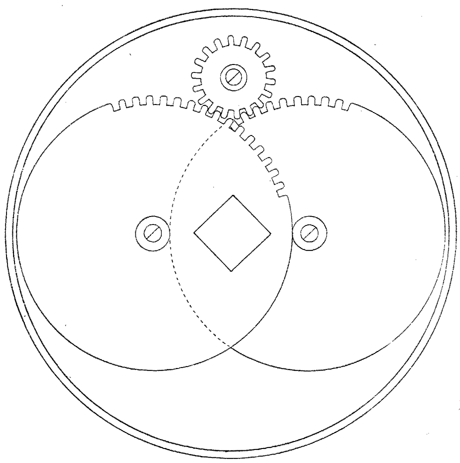
Scale, 3 Inches to a foot.

Elevation of one half of
the opposite Y.

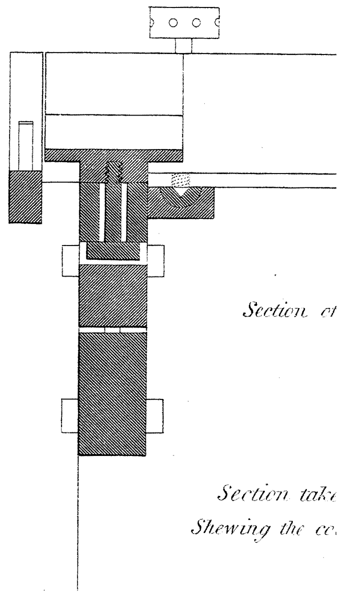


Elevation of one half o
with the plate added.

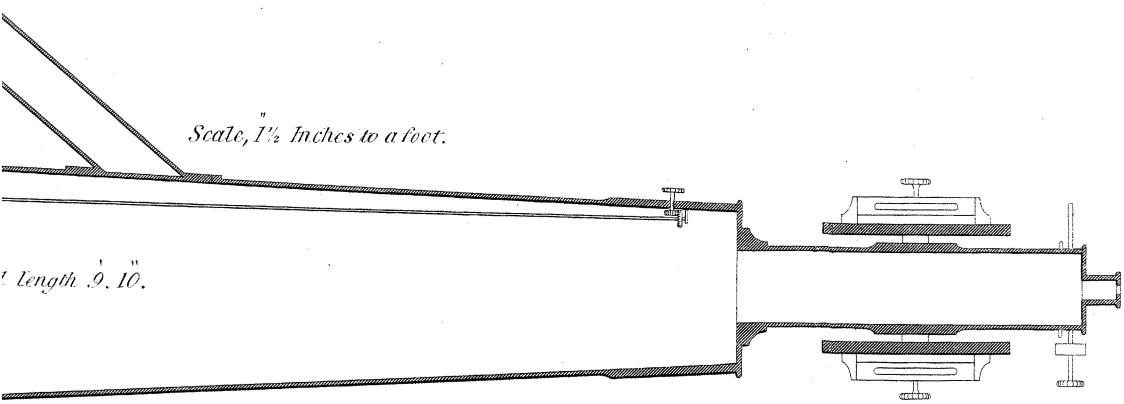




The part Z. shewn at large.



Scale, 1½ Inches to a foot.



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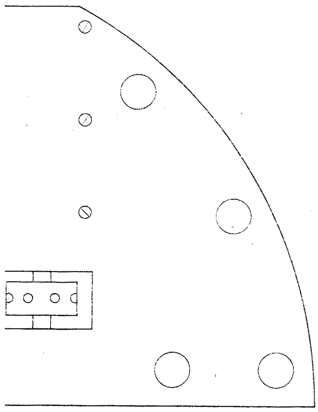

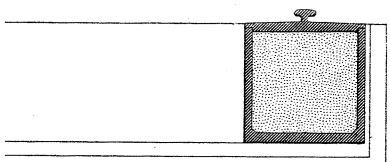

Section of Stone Pier.

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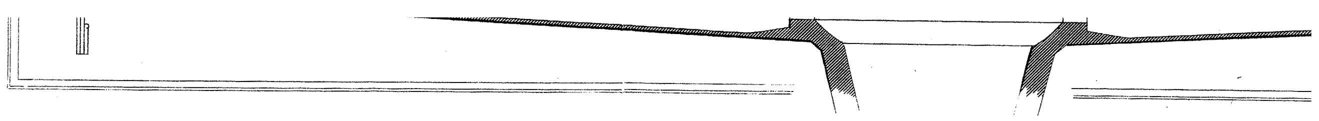
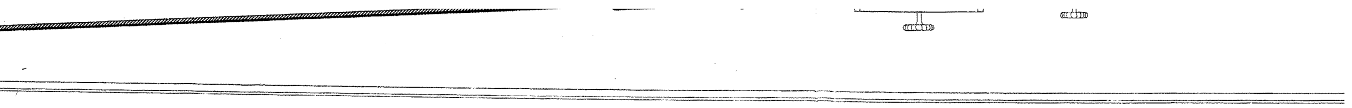
ving the counterpoise &c.





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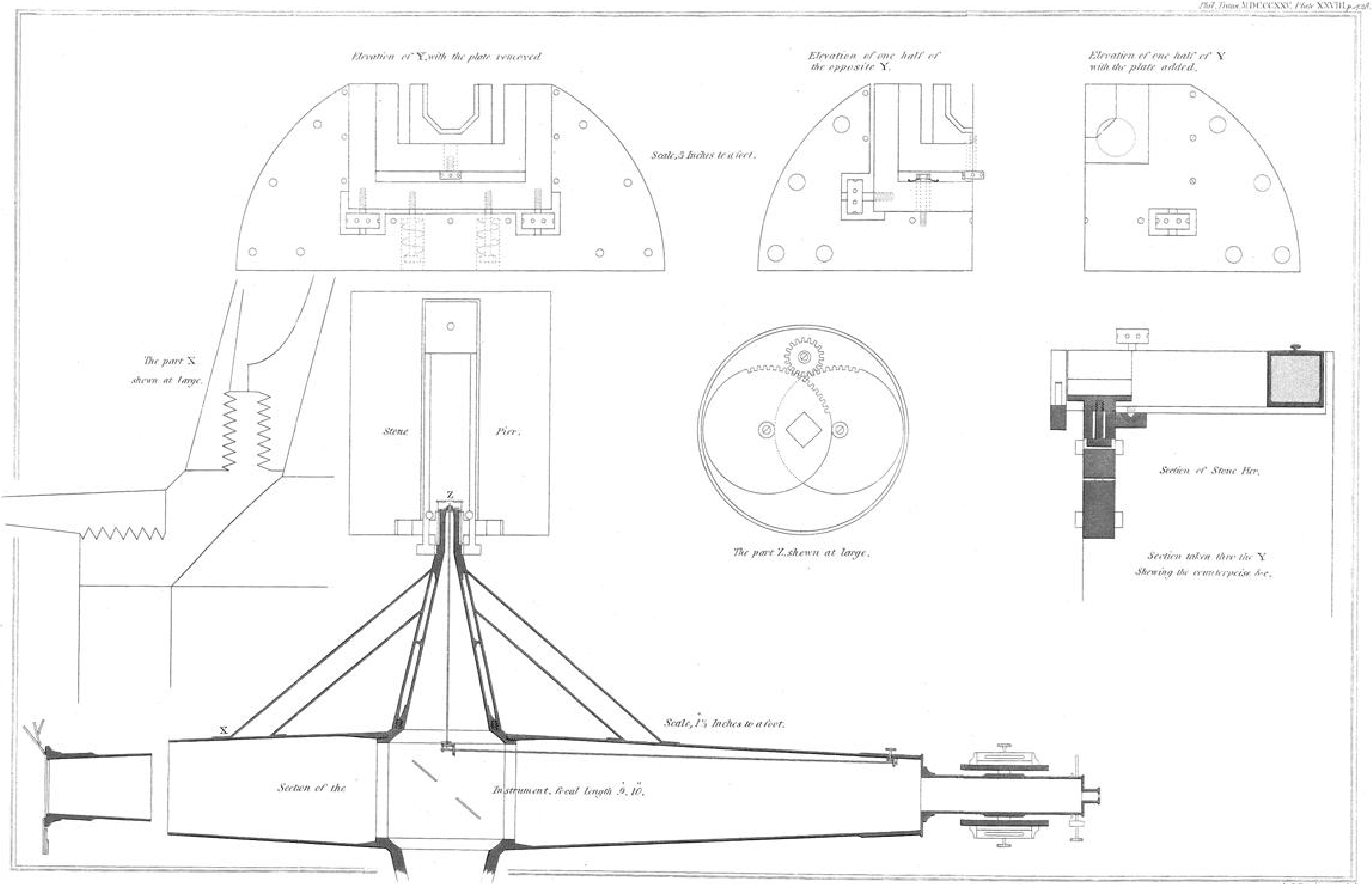

Jas. Basire, sculp.