

1593. *Solanum Campechianum calycibus echinatis*
H. Elt. 360.
1594. *Soldanella Hort. Cliff.* *Alpina rotundifolia*
C. B. P. 295.
1595. *Sonchus floribus folitariis, foliorum laciniis ex-*
trorsum flexis Hort. Cliff. *Sonchus Tin-*
gitanus papaveris folio T. 475.
1596. *Teucrium foliis lanceolatis integerrimis petio-*
latis, spicis laxis subrotundis Hort. Cliff.
Polium lavendulæ folio C. B. P. 220.
1597. *Thevetia Hort. Cliff.* *Ahovas major Pif.*
1598. *Tordylium umbellulis remotis, foliis pinnatis*
pinnis subrotundis laciniatis Hort. Cliff.
1599. *Veronica spicata lanuginosa & incana floribus*
cæruleis Amman. Ruth.
1600. *Zigophyllum foliis subseffilibus Hort. Cliff.*
Fabago Africana arborescens flore sulphureo,
fructu rotundo Com. Rar. 10.

LXX. *An Account of some Experiments*
upon a Machine for measuring the Way
of a Ship at Sea. By Mr. J. Smeaton,
F. R. S.

Read April 4, 1754. **I**N the *Philosophical Transactions*, N^o
391. for November 1725. Mr. Henry
de Saumarez gives an account of a machine for mea-
suring a ship's way more exactly than by the log.
This machine consists of a first mover, in the form of
the letter Y. Upon the two arms of the Y are fast-
ened two vanes, inclined in such a manner, that when
the

the Y is hauled through the water by a rope, fastened to the stem or tail thereof, it may turn round, and, of consequence, endeavour to turn the rope round. The other end of the rope, being fastened to the end of a spindle capable of moving freely round, will be made to do so by the rotations of the Y, communicated to the rope. A motion being thus communicated to a spindle within the ship, this spindle may be made to drive a sett of wheel-work, which will register the turns of the Y; and the value of a certain number of these turns being once found, by proper experiments, they are easily reducible into leagues and degrees, &c. The only difficulty then is, whether this Y will make the same number of rotations in going the same space, when it is carried through the water fast, as when it is carried slow. Upon this head Mr. de Saumarez, as well in the paper above-cited, as in a subsequent one published in *Philos. Transf.* N^o 408. for March 1729. has given an account of several trials, which he has made of it, from which it appears, that this machine, in part, answers the end proposed; and is, in part, defective: The errors of which he supposes to proceed from the sinking down of the Y into the water, upon a slow motion; the axis of its rotation being then more oblique to the horizon than in a quick one.

In a machine, constructed like this, it is evident, that the end of the spindle, to which the rope is fastened, must be of sufficient strength and thickness, not only to bear the force or stress, that the hauling of the Y through the water will lay upon it, in the greatest motion of a ship; but also to bear the accidental jerks, that the waves will superadd thereto.

The

The thickness of the spindle then being determined by these conditions ; it is also manifest, that, to prevent the spindle from being pulled out of its place by the draft of the rope, there must be a shoulder formed upon it, which must be greater than the part of the spindle before described, for the spindle to bear against. The size, that Mr. Saumerez proposes to give to his Y, is 27 inches the whole length ; 15 inches for the length of the arms (which are to be opened to a right angle) ; 8 inches for the length of each vane ; 4 inches and an half broad, and the stems and shank to be two-thirds of an inch thick. According to these dimensions, the resistance, that this part of the machine will meet with, in passing thro' the water, will, in the swift motions of the ship, be very considerable : consequently, the necessary bulk of the pivot-end of the spindle, and its shoulder, will occasion a considerable friction in the turning thereof, and retardation to the rotation of the machine.

To cure these defects, as much as possible, instead of the Y before described, I made trial of a single plate of brass, of about 10 inches long, 2 and an half broad, one-thirtieth of an inch thick, and cut into an oval shape. This plate being set a little atwist, and fastened by one end to a small cord, in the manner of the Y, is likewise capable of making a rotation, in being drawn through the water ; but with this difference, that as this is but a small thin plate drawn edgeways through the water, its resistance, in passing through it, is much less ; of consequence, a much smaller line is sufficient to hold it, which again considerably diminishes the resistance ; and this, of course, proves a double diminution of friction in the spindle :

spindle: First, as the pressure upon it is less; and, secondly, as it allows the spindle and shoulder to be of a less diameter. To break the jerks of the waves; next to the end of the spindle I fixed a spiral spring of wire, to which the cord was fastened; which, by this means, was capable of playing backwards and forwards, and giving way to the irregularities of the sea: and, lest the plate should lay fast hold of any thing, or any extraordinary jerk should damage the spindle or spring, a knob, or button, was fastened upon the cord, at a small distance from the spring, which stopped upon a hole in a piece of wood, and prevented the spring from being pulled out to above a certain length; so that all addition of force, beyond this, could only tend to break the cord, and carry away the plate. The spindle, being thus guarded from accidents, will allow of a still further diminution of its size; so that, at last, I ventured to make the spindle-pivot no more than one-twentieth of an inch diameter, and that of the shoulder one-eighth; being of tempered steel, and sufficiently smooth. The hole, in which the pivot, and against which the shoulder worked, was of agate likewise, well polished.

Being thus provided, in May 1751. I procured a boat, upon the serpentine river in Hyde-park, to try how far the turns of the machine would be consistent with themselves, when the same space was measured over with the same, and with different velocities. The course was determined at each end, by observing the coincidence of two trees, in a line nearly at right angles to the river. We, however, rowed beyond the

mark, that the machine might be in full play when the course was begun : The spindle was stopped at the beginning and end ; the numbers read off, and were as follows :

The space between the marks was, by estimation, about half a mile.

			Revol ^s
1st	rowing up the river,	in 11 min.	the plate made 615
2d	down	14	645
3d	up	18 and an half	612
4th	down	9 and an half	603
5th	up	18	620
6th	down	10	600

It is observable, that the greatest difference, among the above observations, is between the 2d and 6th, being 645 and 600 ; the difference being about one fourth part of the whole ; the times being 14 minutes and 10, both in going down the river : Whereas those observations, which differ most in point of time *viz.* the 3d and 4th, being performed in 18 minutes and an half, and 9 minutes and an half, respectively ; have their revolutions more nearly alike, being 612 and 603 ; which differ only by one sixty-eighth of the whole. From these observations I was led to think, that the different velocities, wherewith a vessel moves forwards, would make no material difference in the number of rotations of the plate ; or, at least, that those differences would be less than the irregularities arising from other causes, even in trials nearly similar.

The next trial of this machine was on board a small sailing vessel, in company with Dr. Knight, and Mr. William Hutchinson, an experienced seaman, and master of a considerable merchant-ship. Our expedition was upon the river Thames, and some leagues below the Nore. The intention of the trial here was, to find, in general, how far it agreed with the log, and how it would behave in the swell of the sea; a comparison with the measure of a real distance being here impracticable, on account of the tides and currents.

The method of trial was this: We suffered the whole log-line to run out, being 357 feet between the first knot and the end. The person, who hove the log, gave notice, at the extremes of this measure, that the person, who attended the dial of the machine, might stop the spindle at the beginning and end; while a third observed, by a seconds-watch, the time taken up in running these 357 feet. By these means, we were enabled to ascertain the comparative velocity, wherewith we moved, and the number of turns of the plate at each trial, corresponding to 357 feet by the log; which, if the machine and log were both accurate, ought to have been always the same. The particulars of these experiments are contained in the following table.

Y y y

Turns

Turns of the
Plate.

83	In the river at anchor by the tide	124	} Seconds of time during the run- ning out of 357 feet of log-line
82	The same repeated	134	
81	Sailing in the river	98	
79	In the river at anchor by the tide	135	
76	Sailing in the river	115	
74	At sea upon a wind	64	
74	The same repeated	69	
71	Sailing in the river	71	
70	The same	66	
70	Before the wind at sea	77	
70	The same	56	
70	The same	52	
66	Before the wind in the river	55	
64	The same	53	
64	The same	60	
64	The same	43	
63	At sea upon a wind	53	
62	The same	52	
62	Sailing in the river	45	

It appears from these trials, made in different positions of the vessel with regard to the wind, both in the river and at sea, as well by the tides at anchor, as in sailing, that the turns of the plate, corresponding with the space of 357 feet by the log, were from 62 to 83; and the times, in which this space was run, were from 45 to 135 seconds; the greater number of revolutions answering to the greater number of seconds, or slower movement of the vessel. Upon finding this considerable disagreement between the log and plate, when swift and slow motions are compared, I did not suppose, that they proceeded from a retard-

retardation of the plate in swift motions, but from the hauling home of the log in slow ones. As for instance; the log, to do its office accurately, ought to remain at rest in the water, whatever be the motion of the vessel. But even the keeping the line strait, and much more the suffering the log to haul the line off the reel (as practised by many), will make the log, in some measure, follow the vessel, and will be greater, in proportion as the time of continuance of this action is greater; and therefore the log will follow the ship twice as far in going one knot, when the ship is twice as long in running it. The consequence of this is, that a vessel always runs over a greater space than is shewn by the log-line; but that this error is greater, in proportion as the vessel moves slower. It is this reason, I suppose, that has induced the practical seamen to continue the distance between their knots shorter than they are directed by the theory.

Afterwards, in the same summer, I made such another expedition, in a sailing vessel, along with captain Campbell of the Mary yacht, and Dr. Knight. Having prepared two of these machines as near alike as possible, I determined to try, how far they were capable of agreement, when exposed to the same inconveniencies, and used together. During the trial of these machines, one made 86,716 revolutions, and the other made 88,184. During this space, they were compared at ten several intervals. The revolutions between each interval differed from the proportion of these numbers, in the first comparison, one-nineteenth of the whole interval. The errors of each interval, in the other comparisons, were, in order, two-seventeenths, one-nineteenth, one-twentieth, one-

fifty-fourth, one-fourteenth, one-eightieth, one-sixty-seventh, one-fourteenth, one-sixteenth; the greatest errors being where the spaces were the shortest. In other respects, the plates seemed to perform their duty, in the water, well enough, tho' the sea was as rough, in this voyage, as our small vessel would well bear.

Lastly, Being, for some time, on board the Fortune sloop of war, commanded by Alexander Campbell, Esq; in company with Dr. Knight, for the purpose of making trial of his new-invented sea-compasses, I had frequent opportunities of making use of these machines, by comparing them with one another, with the log, and with real distances; and having, by repeated trials, pretty well ascertained the number of turns of the plate, that was equal to a given space, by the help of the log, in the manner before described, when the ship was upon a middle velocity; I found the spaces, so measured, nearly consistent with themselves, and with the truth: But all this while the winds and weather were very moderate. It afterwards happened, that we run 18 leagues in a brisk gale of wind, which, tho' not fair for us (being before the beam), yet drove us sometimes at the rate of 8 knots an hour, as appeared by heaving the log. During this run I observed, that the resistance of the water, to the line and plate, was very considerable, and increased the friction of the spindle so much, as to prevent it from beginning to turn, till the plate had twisted the line to such a degree, that when it did set a going, it would frequently run 150 or 200 turns at once. I also observed, that the wind coming across the course of the ship, blew the cord a good deal out of the direction of the spindle, and
caused

caused the line to rub against the safeguard-hole, for the button to stop against, as above described; which undoubtedly occasioned considerable friction in that place. But the most untoward circumstance, that I observed, was, that being in a rough, but short chopping sea, and sailing obliquely across the waves, the plate would frequently be drawn from one wave to another through the air, without touching the water; and, as it appeared, would jump from one wave to another, the unevenness of the surface, joined to the quickness of the motion, not permitting the plate to follow the depression of the water. This evil I endeavoured to remedy, by placing upon the line, at a small distance before the plate, some hollow bullets, such as are made for nets, in order to keep the plate so low down in the water, as to be below the bottom of the waves. This, in part, I found they did; but they, at the same time, added so much resistance, in their passing through the water, that the inconvenience was as great one way, as the other.

Upon making up the account of this run, I found the number of rotations were less, by one full third, than they ought to have been, compared with former observations; which afforded me a convincing proof, that this instrument was considerably retarded in quick motions

The length of the line made use of was about 20 fathoms, which I found necessary, that the water, disturbed by the body of the ship, might be tolerably settled before the plate was drawn through it; but this length of line was also an inconvenience, as it met with greater resistance in the water.

Upon

Upon the whole, it seems to me, that an instrument, made as above described, is capable of measuring the way of a ship at sea, when its velocity does not exceed 5 sea miles an hour, to a degree of exactness exceeding the log. It therefore may be useful in the mensuration of the velocities of tides, currents, &c. and also in measuring distances at sea in taking surveys of coasts, harbours, &c. Thus far it seems capable of performing, upon the supposition, that it cannot be brought to a greater degree of perfection. But this I am very far from supposing: On the contrary, I do not despair, that it may be brought to answer the end of measuring the way of a ship at sea universally; and, for that reason, it may not be amiss to put down a few hints, concerning the cause and cure of the errors above-mentioned, for the sake of those, who may hereafter be inclined to prosecute these enquiries*.

It appears then from the preceding observations, that the rotation of the plate is considerably retarded in the quickest motions of the ship; and sensibly so, in all velocities exceeding 5 miles an hour. This may proceed, first, from the friction of the machine increasing in a greater proportion than the power to turn it round. Secondly, From the water's being put in motion by the ship, so as to follow it in the same

* Upon communicating these experiments and observations to my ingenious friend Mr. William Ruffel, he gave me an account of a machine, that he had made trial of in a voyage, some years since, from the Levant, so nearly agreeing with the above-described, that one would have imagined we had been of each other's council in designing them.

direction, and that to a considerable distance astern. And, Thirdly, from the plate's jumping from wave to wave, when their concavity is great, and distance little.

The first may, in some measure, be helped, by applying a loaded fly, of a proper size, to the spindle of the machine, which will prevent its sticking fast for a time, and then whirling round with great rapidity, as it is apt to do when the resistance is great; by which means, the motion will be rendered more equal and uniform, as was justly observed to me by my friend Mr. Ellicott of this Society.

Also, if the body of the machine were hung, equally poised, upon cross-centres, like those used for sea-compasses, or in the manner of a swivel-gun, as captain Alexander Campbell well proposed; the spindle of the machine would readily place itself in the same direction with the line that draws it, and thereby avoid unnecessary frictions from the oblique direction of the cord.

The second may be helped by placing the machine upon the end of a pole, fastened near the fore-castle, over the side of the ship. By this means, a shorter line will be necessary, and the plate prevented from working in the more disturbed water at the stern.

Lastly, Its quitting the water, perhaps, might be helped by joining a shank of brass, of six inches long, and three-quarters of an inch diameter, to the fore-part of the plate, to which the cord must be fastened, the ends of the shank being formed into a figure most convenient for passing thro' the water with ease. The weight of this will cause the fore-part of the plate to

sink faster than the other, and endeavour to give it a direction down into the water*.

I had intended to have made trial of the effect of these alterations, but have been prevented, partly by want of opportunity, and partly from the indifference, with which I found such a contrivance as this, even if brought to perfection, was likely to be received by seamen; who, in general, do not seem to be over-fond of making trial of new instruments, especially if proposed by landmen, as, in derision, they are pleased to call us.

Indeed it may be objected, that, could we measure the way of a ship thro' the water ever so exactly, unless some method were found out, of ascertaining the currents, &c.; a ship's course, with respect to the globe, could not hereby be determined. But then it may be replied, with equal justice, that it is for want of a means of measuring the way of a ship thro' the water (and this compared with other check observations), that the drift and velocities of the principal currents have not already been determined.

Mr. de Saumarez, in his second paper, of March 1729. makes mention of another machine for this purpose, which he himself acknowledges to be inferior to his former, especially in rough weather at sea. But as several others have fallen upon, and proposed, a machine similar to this; it may not be amiss to add the following remarks upon it. The first mover, in this, is composed of four arms, fixed to the bottom of a
perpen-

* Mr. Ruffel's plate was joined to a shank, who never found it to jump out of the water, at any time, when he made use of it.

perpendicular spindle ; each arm is furnished with a vane, which opens one way, and shuts the other, as some have attempted the making of horizontal wind-mills. This, by being carried thro' the water progressively, will turn round, and the faster, as the ship moves faster : But to judge, whether it will do it proportionably in all velocities of the ship, let us consider,

1. That a good sailing ship will frequently fail at the rate of 10 sea miles (60 to a degree) an hour, which is at the rate of 17 feet *per second*.

2. Supposing the side of the fly, where the vanes are closed, to be retained by the water at rest ; the opposite side of the fly, where the vane is open, must meet the water with a velocity double to that of the ship, or at the rate of 34 feet in a second ; as would be the case with the upper part of a coach-wheel, whose velocity thro' the air is double to that, where-with the coach moves forward.

3. That a plane surface of 3 inches square, moving thro' the water with a velocity of 34 feet *per second*, will meet with a resistance, at least, equal to 70 pounds avoirdupoise.

4. That the resistance, which the open vanes will meet with in the water, will, in swift motions, be very considerable, and, of consequence, the fly will move much slower than it ought to do, if these resistances were less.

5. That from hence there is much reason to doubt, whether the resistance of the medium, and friction of the machine, taken together, will always produce such diminution, in the number of turns, as that the number of revolutions, actually shewn by the indexes,

may be the same, when the same space is gone over with a great velocity, as with a small one.

LXXI. *Observationes Eclipsium Satellitum Jovis habitæ Ulissipone in Regali Collegio Beatissimæ Virginis à necessitatibus, dictæ à Joanne Chevalier, Præbytero Seculari Congregationis Oratorii, Anno 1753.*

Read Feb. 14, 1754. **D**IE 30 Aprilis, cœlo clarissimo, observavi, telescopio Gregoriano 6 pedum longitudinis, emerisionem primi satellitis è umbra Jovis, hora postmeridiana temporis veri 9^h 57' 48".

Die 24 Maii, iterum cœlo purissimo, observavi, eodem telescopio Gregoriano, emerisionem tertii satellitis ab umbra Jovis, hora postmeridiana temporis veri 8^h 19' 6".

LXXII. *Observatio Solis defectus Ulissipone habitæ, in Æde Beatissimæ Virginis à necessitatibus, nuncupatæ à Joanne Chevalier, Præshytero Congregationis Oratorii, die 26^a Octobris 1753.*

Read Feb. 14, 1754. **H**UNC solis defectum commodè, ac exactè observare potui, cœlo clarissimo ac purissimo. Initium ac finem eclipsis telescopio 15 palmorum examinavi, digitos vero obscurationis