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Mr. Thomas Reid

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and motion will prevent congelation even in water, if not already too near the freezing point. But observing that the snow passed off in breadth in proportion to the spreading and shading of the branches; I fixed a long stick in the ground, and tied an umbrella to it, which should, when spreading, be about six or seven feet from the earth. It was then going to snow; and when it was already pretty deep round the stick, I expanded the umbrella, and before the next morning, the whole extent of the circle of the parasole was totally melted before nine o'clock. It could not therefore be any effect of the roots of plants that caused the snow to dissolve; but was it not rather the dews and vapours falling and sinking among the dead leaves, putrid vegetable matter, or calcareous earth, which became nitrate of potash or saltpetre, and of course increased the cold, and preserved the congelation of the rest of the snow? Many other trials and experiments have since that time confirmed my *conviction of its propriety*; though not of consequence enough to be *submitted to the reader*. I cannot help thinking that we are *little aware how many juices*, and various *liquids* and compounds, are bestowed on us by the atmosphere. The hairs of the plants make that subject *most visible*, though they may, like the salts aforementioned, not descend to us in a *perfect state*; yet they certainly (with the liquid already in the hairs, or rising immediately *after in them*) receive *affinities* which produce most of the scents and juices that form our fragrant oils and resins.

I am, sir,

Your obliged servant,

Sherwood, Feb. 3, 1815.

AGNES IBBETSON.

XXXI. *On the Rate of going of two Clocks, with Remarks on HARRIS'S Pendulum Clock erected in 1641. By Mr. THOMAS REID.*

To Mr. Tilloch.

SIR, — **T**HE circumstance of two clocks keeping the same time so closely together, and that for periods of considerable lengths of time, appearing extraordinary and unexpected to me, is the reason why I have transmitted the case to you; and should you deem it worthy an insertion in your valuable Journal, it is at your service.

Both the clocks were going in an imperfect state, and were intended for further improvement: but their assuming the appearance of keeping so near together, made me delay the taking them down to make any alterations, till I saw how long they would continue to do so; which they did for such a length
of

of time, and might have done so *for how long* I shall not pretend to say; had it not at last become necessary for me to put an end to it. One of the clocks was a month one, with a recoiling scapement, and a compensation pendulum made after Ward's construction. The other was an eight-day one, having a detached scapement, and a zinc tube compensation pendulum, both made some years ago, and of my own contrivance.

During a period of four months previous to the middle of November 1813, they kept so constantly and close together, that at no time could I perceive the tenth part of a second of difference between them; indeed no sensible difference could be perceived, either by myself or by another person who had occasion frequently to see them expressly for this purpose. A pretty sharp cold taking place on one of the nights in this November, the month clock made a little deviation from the other: this was imputed to the influence of the cold upon the oil, as the arc of the vibration of the pendulum was followed by being shortened a little at this same time. However, very soon after, they again went together, allowance being made for the difference or deviation made by the one from the other, and this difference was kept up to for *more* than a period of 164 days afterwards. The mean daily rate of these clocks, during the four months previous to the middle of November 1813, was $+0.2$ per diem. The mean daily rate of them during a period of 164 days, viz. from the 2d of February 1814 to the 16th of July, was also $+0.2$ per diem, the same as before. I shall give an abstract more in detail, of their going during these 164 days, not with the view of showing any great perfection either in their rates or in the compensation; yet at the same time it may be allowed that in these respects, considering the incorrect state in which they were, few clocks, even in their best and most correct condition, have, *if any thing*, much exceeded this. On the 2d of February, the clock having the detached scapement was fast by mean time $1^m\ 35^s$. The month clock was about 53 seconds faster than it, and this difference it maintained during 164 days, when the month clock was taken down to be corrected and to get another pendulum.

			No. of Days.	Total Gain.	Mean daily Rate.	Mean of the Thermo.
February 2.	+ 1 ^m 35 ^s					
March 7.	+ 1 46	33	11	+ 0.33	40	
	29. + 1 50	22	4	+ 0.18	42	
May 11.	+ 1 55	48	5	+ 0.11	55	
June 10.	+ 2 4	30	9	+ 0. 3	52	
July 16.	+ 2 10	36	6	+ 0.16	60	

The intermediate times of trial gave them for the greatest
M 2 mean

mean daily rate $+0^s.33$, and for the least $+0^s.11$, and the mean of these five periods is $+0^s.216$ as the mean daily rate.

These two clocks coming to keep time so close and near together, and for such a length of time, was a matter merely accidental: I should consider it a very vain pursuit, to attempt to make any two clocks do so, nor do I think that there is any probability of my seeing the like to take place again. It is not impossible to make two clocks to go very nearly together, and for a considerable length of time *too*, but in this *very nearly*, and in that *quite close*, there is a very wide difference.

It has been said in some of the numbers of your Phil. Journal, where I think that I have seen it, that a clock with a dead beat scapement was made by the late Mr. Grignion, and given by him to the Society for the Encouragement of Arts, Manufactures, &c. "that any addition or diminution of the motive force would produce no alteration in the time-keeping of the clock." Now, sir, being well acquainted with the principle of the dead beat scapement, I cannot easily admit this. When this circumstance was mentioned, it should surely have been stated in what manner this was obtained by Mr. Grignion, in the dead beat scapement. Where this takes place in any scapement, I humbly presume it can then no longer have the properties of that of the dead beat scapement. However, if you or any of your correspondents will have the goodness to take the trouble to explain this, it will be esteemed a considerable favour.

From the same respectable authority of Mr. Grignion, we are informed that in the year 1641 a clock was made and put up in the church of St. Paul's, Covent-garden, by a Richard Harris, who had applied a pendulum to it. This is rather singular, considering the cavilling which took place many years afterwards, as to the priority or right of having first applied the pendulum to a clock. Father Alexander tells us, that there were no clocks made with pendulums to them in Paris, till after the year 1660; and yet there was a duodecimo pamphlet of Galilei's, On the nature and properties of pendulous bodies, translated from the Italian into French, published at Paris in 1639. May we not hazard a conjecture for Richard Harris in this case? Our countryman Inigo Jones travelled twice into Italy, and was at Venice, about the time that Galilei was there. It is not improbable that Mr. Jones when there may have heard of what Galilei had suggested regarding the pendulum, and of the propriety of applying it to a clock, and afterwards may have communicated these ideas to Richard Harris on his returning to London. It must be observed, that Inigo Jones was not in life at the time
when

when the dispute on this subject took place between Hooke, Huyghens, and others. Whether Richard Harris was, I know not.

I am, sir,

Your most obedient humble servant,

THOMAS REID.

XXXII. *On Steam-Boats.* By ROBERTSON BUCHANAN, Esq.
of Glasgow.

To Mr. Tilloch.

DEAR SIR,—AGREEABLY to your request. I now send you some account of the steam-boats on the Clyde.

I am, dear sir,

Your most obedient servant,

Glasgow, January 9, 1815.

ROBERTSON BUCHANAN.

So early as the year 1801, a vessel propelled by steam was tried on the Forth and Clyde inland navigation, but was laid aside, among other reasons, on account of the injury it threatened the banks of the canal by the agitation of the water: and as far as I can learn, the same objection still subsists to the use of steam-boats on artificial canals so narrow as those usual in Great Britain. That objection, however, I should think, does not apply to some of those of Holland and other countries on the continent.

The first attempt on any scale worthy of notice, to navigate by steam on the river Clyde, was in the year 1812*. A passage boat of about 40 feet keel and 10½ feet beam, having a steam-engine of only three horses' power, began to ply on the river. Since that period the number of boats has gradually increased.

Besides three vessels which have left the Clyde, there are six at present plying on the river, two of which carry goods as well as passengers. They have on the whole been gradually increased in tonnage as well as in the power of their engines; and still larger boats and more powerful engines are now constructing: among others, one of about 100 feet keel and 17 feet beam with an engine of 24 horses' power; and one of equal burthen, having an engine of 30 horses' power†. These boats are all neatly fitted up, and some of them even elegantly decorated.

On board all the passage steam-boats are newspapers, pam-

* The first steam-boat in America was launched at New-York on the 3d of October 1807, and began to ply on the river between that city and Albany, a distance of about 120 miles.

† For the value of a horse's power, see Buchanan's Essay on Mill-work, Teeth of Wheels, p. 130.