ming Water, in Seas, and in Clouds, and in Pickle; yet not so frequent, as to escape always the suspicion of being Prodigies. But in the foresaid references more is said of Light, than I am able to express; I shall only add, That I gave full warning to observe, whether the Light in my two Instances had any blewish or greenish tincture; all that saw both, affirmed the Light to be as clear as the brightest Moon-shine, and so it appeared to my own eyes; and I can perfectly remember, that I really thought the beams which came from the Mackrel, and the stirred pickle, to be bright Moonshine, till a Servant brought me to the Vessel, to see the contrary.

Postscript. We had the report here (whether true or false, you may best know) of the shining Beef in the *Strand*, about the same time, when the Neck of Veal, first mention'd, shined here. And it was here observed, That the Stars had that night a glaring brightness and largeness, more than ordinary, and for some moneths before, and ever since, the weather hath been more gentle, warm, and dry, than is usual in those months; but 'tis above my skill to demonstrate, how this belongs to the matter in hand. Note, that the Mackrel-pickle was thick and not transparent, till it was stirred and flaming; the Pork-pickle was clear, or transparent, yet shined not in any part.

A Discourse concerning the Spiral, *instead of the supposed* Annular, *structure of the Fibres of the Intestins; discover'd and shewn by the Learn'd and Inquisitive Dr.* William Cole *to the* R. Society.

Discoursing (near two years since) with a very ingenious Person, concerning the Mechanical reason of the *Peristaltick* motion of the Intestines, which is by Anatomists deduced principally from *Annular* fibres, constituting, according to the received doctrine (with the right fibres immediately investing them, though, by the by, I take these to make a distinct coat) one of the coats of them; his sence was (which he told me was that likewise of some others of his acquaintance) that they might be rather numerous, though small, Sphincter-muscles, than single fibres, to which that motion is to be attributed; Muscles being in most, if not all, other instances owned to be the adequate instruments of motions analogous to this; and fibres, though absolutely necessary, yet being no otherwise so, than as (a number of them being collected, and fitly disposed) they constitute a Muscle.

The Conjecture seemed to me more probable than the vulgarly received

received opinion: but yet (with all reſpect to the abettors of either) ſeveral difficulties occurred to me, whether of the two ſuppoſitions ſoever were allowed.

For, *firſt*, I conceived it might be doubted (each of theſe, whether ſingle fibres, or muſcles, being ſuppoſed diſtinct, as I think they generally are, and, if annular, I conceive, muſt be) how the actuating matter, or impreſſion (according to the opinion of ſome learned men) ſhould be tranſmitted from one to another down along the whole tract of the Inteſtines; ſince Natures uſual way, for the propagation of Animal motion, is by a Continuation of veſſels, (or at leaſt fibres, whether they be concave or not) from the part where it begins to that to which 'tis imparted, either for the conveyance of ſome actuating ſubſtance, or (according to the other *Hypotheſis*) the communicating an impreſſion. But there being, in the *Annular* ſuppoſition, no ſuch continuation of veſſels or fibres, a lateral contiguity being all that can be pretended, it might perhaps be urged, that the influent and moving matter (according to that notion) might be tranſmitted by mutual inoſculations between the contiguous fibres along their ſides; which, if there be no Communication by veſſels, was the only way, I could gheſs at, to ſolve the doubt; for, the notion of an *Impreſſion* would hardly do the buſineſs, ſince it ſeemed not evident, that there could be, in that ſuppoſition of a Continuity of fibres, tenſity enough in the Inteſtins to carry on ſuch a motion. But to this I conſidered,

Secondly, That ſuch a ſuppoſition ſeemed not very agreeable to Natures methods, which ordinarily makes uſe of Veſſels (and thoſe both cloſe, and as direct as the deſign and organization of the part will bear,) for the tranſmiſſion of the fluid ſubſtances in the bodies of animals, not lateral emiſſaries; except where ſome great inconvenience is deſigned to be prevented by the help of ſuch conveyances; as, for inſtance, by the Anaſtomoſes, diſcovered to be between veins and veins, arteries and arteries, in which veſſels the bloud running with a large and rapid ſtream, ſhould any of them chance to be obſtructed, the Circulation, ſo neceſſary to life, muſt needs be intercepted, without ſome lateral conveyance of it into others of the ſame kind: Which inconvenience yet I ſuppoſed would hardly be alledged in the preſent caſe; *that* fabrick of thoſe veſſels ſeeming to be deſigned for extraordinary emergencies, but *theſe* being, according to the preſent ſuppoſition, the conſtant and neceſſary ducts of this actuating matter. But nevertheleſs,

Thirdly,

Thirdly, It ſeemed difficult (to me at leaſt) to ſolve this Inteſtinal contraction, though theſe lateral apertures were ſuppoſed: For, if fibres (whether conſidered as ſingle, or as conſtituting a muſcle) be contracted according to their length from ſome influent matter, it muſt be (according to my ſence) from a diſtenſion of them in breadth; and, in order to that, this matter muſt undergo ſome confinement in the part to be diſtended; but if they have lateral perforations (and thoſe in the oppoſite part proportionate to thoſe in that which admits this matter, which muſt, I conceive, be granted, ſince the contraction is all along the Inteſtines proportionate,) how can it be ſuppoſed, a diſtenſion (at leaſt ſuch a one as is here required) can happen, when the matter deſigned to effect it has ſo ready a paſſage forth, eſpecially its determination from the impelling cauſe being in right lines downward? If it were objected, that the motion of this ſubſtance might be ſuppoſed to be lateral as well as direct, in regard there would be a paſſage for it into the fibres as well as through the Anaſtomoſes, and that in proportion larger than through theſe, whence nothing ſeems to hinder but that a diſtenſion of them might follow; I ſuppoſed, it might be replyed, that, by reaſon of ſuch a diſtorſion of part of the impelled matter, it ſeems, that the impreſſed motion would be ſoon loſt (according to the laws of motion) unleſs the impelling cauſe were more violent than I ſee reaſon in this caſe to imagine it to be. But indeed I think, no Anatomiſts have obſerved, that muſcles (ſuppoſing theſe ſuch) receive their actuating matter in at their ſides, or, when their motion ceaſes, ſend it forth that way; but all, ſo far as has been obſerved, are fenced with a conſiderably compact, and (comparatively) impervious membrane.

Fourthly, I conſidered, that all muſcles are obſerved to have two tendons, one at each extremity, by the approach of one whereof toward the other, its motion, which is contraction, is performed; but it ſeems hard to conceive, that theſe tendons ſhould coincide (as in this ſuppoſition they muſt) and, if they do, I preſumed it would be difficult to determine, what part of theſe circular muſcles (if ſuch) the tendons are, and where the motion ſhould begin in each; it being obſerved, that all muſcles are faſtned to ſome, either ſimply or comparatively, unmovable part, toward which (ordinarily) they move, and by which the inſtinct of motion is from the nerves conveyed to them: But no Anatomiſts, (ſo far as I had obſerved) having diſcovered, that any one part of

theſe

theſe muſcles, or moving *fibres*, whichſoever they be, has any ſtricter coheſion than other with any of the adjacent parts, I conceived, I might be allow'd the liberty to doubt of the *Hypotheſis*, eſpecially if I could ſatisfie my ſelf better by another.

For inſtead of theſe ſolutions there occurred to my thoughts a third way, which (provided experience would countenance it) ſeemed more mechanically adjuſted to ſolve the *Phænomenon*; *viz.* That thoſe *fibres*, which have been eſteemed *annular*, might perhaps be *ſpiral*, and ſo be continued down in one tract to the loweſt extremity of the inteſtines; withal, that their ſinalneſs, compared with the compaſs they fetch about the inteſtine, might very eaſily, I conceived, impoſe upon any, who made not thoſe reflections, or tried not to unravel them; their declination being, for that reaſon, not eaſily diſcernible: Which if true, it ſeemed probable to me, that when either a bare motion ſhall be impreſſed on them at their beginning, or any ſubſtance impelled into them, they being to be ſuppoſed *in ſtatu naturali* moderatly tenſe, ſo long as the moving cauſe continues, the motion muſt be ſucceſſively continued all along their tracts, and, that being in *ambitum*, muſt therefore, whilſt it laſts, by abbreviating theſe *fibres*, ſtraiten the inteſtine, and ſo thruſt forward what is contained in it, eſpecially if they proved to have a muſcular fabrick. The conjecture as 'twas not diſreliſhed by the perſon to whom I propoſed it, ſo gratified me the more for the ſeeming eaſineſs of the performance; Nature's operations being the moſt eaſy and ſimple that can be imagined, though for that reaſon very often, I doubt, overlook'd. But the notion lay afterward long *dormant*, till, about half a year ſince, being revived by I know not what occaſion, I conſider'd 'twas too unphiloſophical to acquieſce in bare ſpeculation, when *autopſy* might be conſulted; and therefore I ſet upon the experiment, which I firſt made in a portion in the upper inteſtines of an Ox, which, by reaſon of their largeneſs of proportion to thoſe of moſt other *ſpecies* of animals, ſeem'd fitteſt for the tryal; afterwards in thoſe of Sheep and Calves, beſide the repetition of it in Oxen, and not only in the ſmaller inteſtines, but in the *colon* and *cæcum* alſo. The circumſtances and reſult of which tryals are as follows.

To effect a due disjunction of the membranes and *fibres* (which I found 'twas hard, if not impoſſible, for me to make while 'twas raw,) I was fain to cauſe the inteſtine of Oxen to be boiled 5 or 6 hours, of Sheep 4; whereby the compages of the parts was ſo looſned,

looſned, that the two outward coats, *viz.* the common one, and that conſiſting of right *fibres* were eaſily ſeparated (if it were attempted ſoon after it was taken out of the water) from that to which my ſearch was deſtined, and left thoſe reputed *annular* ones naked; (though, by the way, too long coction would prove prejudicial on the other hand, by too much intenerating the *fibres.*) Theſe at the top of the inteſtine I attempted to ſeparate from one another; and when thoſe, which had been decurtated by the unequal cutting of the knife, were taken off, I found,

Firſt, that I could not ſeparate a ſingle *fibre* from his fellows to any conſiderable diſtance, all of them (to my obſervation) being very ſmall, and in the ſeparation running ſmaller and ſmaller, and withal by reaſon of their implication or ſtricter coheſion one with another eaſily breaking; but a congeries of them (to be obſerved eſpecially, though not preciſely alwaies, in thoſe places, where by gently extending the inteſtines ſeveral times, and then letting it return again, the coheſion of the ſeveral *ſeries* of them became looſned) which at firſt view would reſemble a pretty large *fibre*, would without much difficulty riſe together; the very ſmall conſtituting *fibres* of which cluſters yet, if the boiling had been very long continued, whereby the compages was very much relaxed, would in the raiſing be very apt to ſeparate from one another, and appear diſtinct, by reaſon of their inſertions, by and by to be mentioned.

Secondly, that when, beginning at the top, I attempted the ſeparation of one of theſe (ſuppoſed *annular*) cluſters of *fibres* towards my right hand (on that ſide of the inteſtine, I mean, which was turned towards me) a whole ring would come off together, (excepting that ſome *fibrillæ*, which, riſing from contrary parts, decuſſated one another at the top in that *phaſis*, would a little retain it) which at firſt ſtagger'd me as to my forementioned conjecture; but endeavouring it towards my left, I found, for the moſt part, I could eaſily enough unravel that cluſter to a conſiderable length, *viz.* that of ſometimes more than two or three ſpans, before ruption (of the whole cluſter I mean,) which yet at laſt 'twould be ſubject to. For,

Thirdly, though thoſe convolutions, as to the greateſt part of them appeared diſtinct, yet I found, that from every one of them at ſhort diſtances ſome *fibres* did obliquely, and the moſt of them, to my beſt obſervation, according to the courſe of thoſe I have mentioned, inſert themſelves into the next convolution, and become a part of it; though withal ſome I obſerved to have a contrary ten-

dency, or rather seem'd to ascend from the lower to the upper convolution, and help to constitute it, and so to observe the course mentioned; nay, sometimes would go farther than the next convolution, and, running under it, apply themselves obliquely to some higher, which yet being in a smaller number than the rest that lay in the order contrary to them, did not very much hinder the dissociation of the main ones: which *fibres* breaking off, and that in some places in greater numbers than in others, would at last (and the sooner if the intestine began to grow dry, which 'twould quickly do) cause the whole cluster to break off.

Fourthly, I observed, that as the most of these *fibres* would by degrees according to the order of the convolutions, insert themselves into the next, so some of them would (in the same order) pass over it, and more (so far as I have observed) would run under it, and either adjoyn themselves to some more remote, or elude my searching by hiding themselves under them. This insertion of these *fibres* seems to be the reason of the *annular phasis*, that I mentioned even now, in the contrary way of separation: For, the attempting it contrary to their order, must hinder in some measure the ready dissociation of the next convolutions upwards; especially near the severed extremity, where there is less resistance of the adjacent parts; the mentioned *fibres* also seeming somewhat bigger, and consequently stronger, in the upper, than after their insertion into the lower convolution: Though indeed

Fifthly, I found, that if I began at a lower part of the intestine, and try'd to unravel *upwards*, there was not much more difficulty in so doing, than when beginning above, I attempted it *downwards*; of which the reason, I suppose, might be the tenderness of the part occasioned by long boiling, whereby I could not perhaps judg of the degrees of renitency in those small *fibres*. In this contrary way of separation too, the operation, I observ'd, would not succeed, unless I attempted it in the contrary order, *viz.* towards my right hand.

Sixthly, when before boiling I caused the inside of the intestine to be turned outward, as I did in two tryals, and afterward by taking off the glandulous and vascular coats (which I think to be distinct from one another, as I said before of those consisting of right *fibres*, and the supposed *annular* ones,) endeavoured to unravel the *fibres*, I found they would come off in the contrary order, *viz.* from my left hand toward my right; which, I conceive, confirms the observation above deliver'd, in regard the intestine being inverted, the order of separation must be so too; though I found

found(or thought) the operation more difficult, by reaſon of ſome *fibres* lying in the oppoſite order (mentioned under the third particular) and in this appearance lying uppermoſt.

Seventhly, in one of theſe attempts of unravelling the *fibres* of the inteſtine of an Ox, ſo inverted, I found, that though the *fibres* I took up came off in the order I juſt now mentioned, yet running over ſome others, they made a more oblique excurſion, and for two or three convolutions left betwixt them a conſiderable *area* of *fibres*, amounting (according to my conjecture) to five or ſix times, or more, the bredth of thoſe that ſo came off, till going deeper and deeper among the other *fibres*, and at laſt running under them, they could be no longer traced, but brake off. Whether this be uſual, or only *luſus naturæ*, I cannot determine.

Eighthly, I found it much more difficult (in that one tryal I made) to unravel the *fibres* of the *Cæcum*, than the other inteſtine, which ſeemed more interwoven than thoſe of the reſt, and to have contrary tendencies one among another.

This is the ſum of my obſervations hitherto concerning this coat, which I take leave to think one concave and Helical muſcle (if I may ſo ſtyle it:) And that it might be ſuppoſed ſuch, the forementioned inſertions ſeem'd to evidence, they appearing to me in the ſeparating appoſitely enough to repreſent the fabrick of a muſcle delivered by the accurate *Steno*. Where the tendons of it are fixed, is not evident; but, if I may have the liberty to conjecture, I ſhould think the upper of them to be radicated (at leaſt) at the *pylorus* (if not as high as the *ſphincter gulæ* (if this be not it,) ſince, the carneous coat of the ſtomach being by the Learned Dr. *Willis* found to be a muſcular contexture, and there being a continuation of motion between that part and the inteſtines, it ſeems to me not altogether improbable they may be but one muſcle; and the *other* at the *anus*.

Whether the ſuppoſed *annular fibres* of the *veins* and *arteries* may not have the ſame fabrick as thoſe of the *Inteſtines*, ſince both theſe kinds of veſſels ſeem to have a *periſtaltick* contraction of their own, and not to be bare conduit-pipes to tranſmit the impelled bloud, I propoſe to be conſidered and examined by perſons of more acute hands and judgment; as I do all what I have here delivered, not daring too much to truſt even the informations of my own hands and eyes, till I find them confirmed by thoſe of others, more judicious as well as dextrous in making experiments.

Monsieur *Bullialdus* and M. *Richelts* account of the last Lunar Eclipse of *Januar.* 1. 1676. St. novo; whereby it appears that the *Rudolphin* Tables or *Hecker's* calculus made thereon, do considerably differ from the Heavens both for duration and magnitude, but the *Philolaique* Tables, less.

Tabulæ Philolaicæ exhibent in Eclipsi visâ Januarii die 1 *mane*, 1676.

	Uraniburgi H. ′ ″	*Parisiis* H. ′ ″	*Decemb* 31. *St. novo.*
Initium	14. 53. 29	14. 13. 29	Digitos III. LI.
Max. obsc.	16. 0. 33	15. 20. 33	
☾ Veram	16. 9. 18	15. 29. 18	
Finem.	17. 7. 37	16. 27. 37.	

Sic verò illud deliquium observavit Ismael Bullialdus.

Capellæ Dist. à vertice. Parisiis manè

G. ′		H. ′ ″
39 36	Penumbra tenuis.	2 6 12
40 42	Penumb. crassior.	2 12 7
41 48	Obscurior adhuc.	
42 30	Initium sensibile è reg. sinus Hyperb. circa gr. 70.	2 23 32
42 50	Digiti fere $\frac{1}{8}$	2 25 48
44 25	Umbra attigit Atlantem minorem.	2 36 11
47 28	Umbra paulo supra Baronisum, supra Liguſtinum, occupaverat Macr. M.	2 56 27
48 56	U. attigerat fere catenam Mundi.	3 6 20
49 54	U. attig. Montunial.	3 12 54
50 30	Attigit sinum Perontic. Montem Pyram. & med. paludum Hyperbor.	3 17 51
52 7	Occupavit sinum Sagaricum & Peronticum, atque Promont. Lunæ.	3 29 1
54 52	Leucopetra extra umbram.	3 48 21
55 40	Sinus peronticus extra umbram.	3 54 6
55 48	Sinus Sagaricus extra umbram.	3 55 12
56 17	Sinus Cercinites fere emerserat.	3 58 29
57 16	Pars sub umb. æqualis fere latitudini Paludis Mæotidis.	4 5 38
58 30	Finis verus è reg. Mont. Macrocem. circa g. 355.	4 13 56
59 6	Penumbra.	

Corsicam non attigit umbra, neque Lacum Thrasymenum, propterea Eclipsis non excessit digitos III. XXX′. vel minus etiam.

Initium uno scrupulo primo vel 45″ *antecessit adnotatum, ita ut statui exactius possit H.* 14. ′22. ″32. *Hinc tota duratio satis præcise H.* 1. 51. 24. *Quare maxima obscuratio contigit H.* 15. 18. 14.

Parisiis *observatum.* Heckerus *Parisiis*

	H. ′ ″	H. ′ ″	
Initium	14 22 32	14 8 0	Totam durat. H. 2. ′24. ″36.
Max. obsc.	15 18 14	15 20 18	Digitos. IV. ′24.
Finis	16 13 56	16 32 36	″31.

Tabulæ ergo Rudolphinæ ostendunt durat. longiorem observatâ ′33. ″13. *Magnitudin. deliquii majorem* ′54 ″31. *unius digiti* ′60, *id est, uno fere digito.*

Argentinæ *sic observ.* D. Julius Richelt *Profess. Mathematum, utcumque, ut ipse dicit, ob nondum absoluta correctiora instrumenta:*

Alto Arcturo G. ′	H. ′ ″	
30 30	14 48 48	Initium.
36 0	15 20 8	U. per Montem Porphyritem & Promount. Lunæ.
39 50	15 45 44	U. Strinxit Lacum Thrasymen. Mont. Baronisum & sinum Cercinitem.
44 15	16 13 20	U. transiit per Prom. Lunæ & Montem Cimmerium.
46 25	16 27 36	U. tetigit Lacum nigrum minorem & M. Carpathum.
48 30	16 41 44	Desiit in Regione Hyperborea media ad Mare Hyperboreum.

Tota duratio observata Argent. H. 1. ′52. ″56. Merid. Parisiens. ab Argentoracensi distat O′ 22. ″48 ex fine.

Observationibus Londini *factis observatum est,*

	H. ′ ″	
Initium	14 16 0	Distat ex hac Eclipsi ad ortum meridianus Paris. a Londin. ′6. ″38. Quj ex observatione Eclipsis die vii. Julii 1675 apparuit ′10. ut etiam in Eclipsi xi. Januarii ejusdem anni.
Max. obsc.	15 11 37	
Finis	16 7 15	
Tota dur.	1 51 15	

An