21L.016 / 21M.616 Learning from the Past: Drama, Science, Performance

Spring 2009

nine experiments which the examiner in the 11th and 12th chapters reckons up as urged by his adversaries; yet do not thereby declare my acquiescing in his explications of those phænomena, but only leave both them and some other things he delivers about siphons and the *Magdeburg* experiments, to be discoursed by those that are more concerned to examine them, contenting myself to have sufficiently disproved the Funiculus which his expositions suppose, and cleared the grounds of explicating such experiments aright.

CHAP. V.

Two new Experiments touching the measure of the force of the spring of air compressed and dilated.

Page 12. THE other thing, that I would have considered touching our adversary's hypothesis is, that it is needless. For whereas he denies not, that the air has some weight and spring, but affirms, that it is very insufficient to perform such great matters as the counterpoising of a mercurial cylinder of 29 inches, as we teach that it may; we shall now endeavour to manifest by experiments purposely made, that the spring of the air is capable of doing far more than it is necessary for us to ascribe to it, to solve the phænomena of the Torricellian experiment.

WE took then a long glass-tube, which, by a dexterous hand and the help of a lamp, was in such a manner crooked at the bottom, that the part turned up was almost parallel to the rest of the tube, and the orifice of this shorter leg of the siphon (if I may so call the whole instrument) being hermetically sealed, the length of it was divided into inches (each of which was subdivided into eight parts) by a streight list of paper, which containing those divisions, was carefully pasted all along it. Then putting in as much quicksilver as served to fill the arch or bended part of the siphon, that the mercury standing in a level might reach in the one leg to the bottom of the divided paper, and just to the same height or horizontal line in the other; we took care, by frequently inclining the tube, so that the air might freely pass from one leg into the other by the sides of the mercury (we took, I say, care) that the air at last included in the shorter cylinder should be of the same laxity with the rest of the air about it. This done, we began to pour quicksilver into the longer leg of the siphon, which by its weight pressing up that in the shorter leg, did by degrees streighten the included air: and continuing this pouring in of quicksilver till the air in the shorter leg was by condensation reduced to take up but half the space it possessed (I say, possessed, not filled) before; we cast our eyes upon the longer leg of the glass, on which was likewise pasted a list of paper carefully divided into inches and parts, and we observed, not without delight and satisfaction, that the quicksilver in that longer part of the tube was 29 inches higher than the other. Now that this observation does both very well agree with and confirm our hypothesis, will be easily discerned by him, that takes notice what we teach; and Monsieur *Pascbal* and our English friend's experiments prove, that the greater the weight is that leans upon the air, the more forcible is its endeavour of dilatation, and consequently its power of resistance (as other springs are stronger when bent by greater weights). For this being considered, it will appear to agree rarely-well with the hypothesis, that as according to it the air in that degree of density and correspondent measure of resistance, to which the weight of the incumbent atmosphere had brought it, was able to counterbalance

and

and reſiſt the preſſure of a mercurial cylinder of about 29 inches, as we are taught by the Torricellian experiment; ſo here the ſame air being brought to a degree of denſity about twice as great as that it had before, obtains a ſpring twice as ſtrong as formerly. As may appear by its being able to ſuſtain or reſiſt a cylinder of 29 inches in the longer tube, together with the weight of the atmoſpherical cylinder, that leaned upon thoſe 29 inches of mercury; and, as we juſt now inferred from the Torricellian experiment, was equivalent to them.

WE were hindered from proſecuting the trial at that time by the caſual breaking of the tube. But becauſe an accurate experiment of this nature would be of great importance to the doctrine of the ſpring of the air, and has not yet been made (that I know) by any man; and becauſe alſo it is more uneaſy to be made than one would think, in regard of the difficulty as well of procuring crooked tubes fit for the purpoſe, as of making a juſt eſtimate of the true place of the protuberant mercury's ſurface; I ſuppoſe it will not be unwelcome to the reader, to be informed, that after ſome other trials, one of which we made in a tube whoſe longer leg was perpendicular, and the other, that contained the air, parallel to the horizon, we at laſt procured a tube of the figure expreſt in the ſcheme; which tube, though of a pretty bigneſs, was ſo long, that the cylinder, whereof the ſhorter leg of it conſiſted, admitted a liſt of paper, which had before been divided into 12 inches and their quarters, and the longer leg admitted another liſt of paper of divers feet in length, and divided after the ſame manner. Then quickſilver being poured in to fill up the bended part of the glaſs, that the ſurface of it in either leg might reſt in the ſame horizontal line, as we lately taught, there was more and more quickſilver poured into the longer tube; and notice being watchfully taken how far the mercury was riſen in that longer tube, when it appeared to have aſcended to any of the diviſions in the ſhorter tube, the ſeveral obſervations, that were thus ſucceſſively made, and as they were made ſet down, afforded us the enſuing table: *See Fig. 53*

A table

A table of the condenſation of the air.

A	*A*	*B*	*C*	*D*	*E*
48	12	00	Added to 22¼ makes	29 2/16	29 2/16
46	11½	01 7/16		30 9/16	33 6/16
44	11	02 12/16		31 15/16	31 14/16
42	10½	04 6/16		33 8/16	33 7/7
40	10	06 3/16		35 9/16	35- -
38	9½	07 14/16		37	36 15/19
36	9	10 2/16		39 5/16	38 7/9
34	8½	12 8/16		41 10/16	41 2/17
32	8	15 1/16		44 3/16	43 11/16
30	7½	17 15/16		47 1/16	46 3/5
28	7	21 3/16		50 5/16	50- -
26	6½	25 3/16		54 5/16	53 10/13
24	6	29 11/16		58 12/16	58 2/3
23	5¾	32 3/16		61 5/16	60 18/23
22	5½	34 15/16		64 1/16	63 6/11
21	5¼	37 15/16		67 1/16	66 4/7
20	5	41 9/16		70 12/16	70- -
19	4¾	45- -		74 2/16	73 11/19
18	4½	48 15/16		77 14/16	77 2/3
17	4¼	53 11/16		82 12/16	82 4/11
16	4	58 8/16		87 4/16	87 3/8
15	3¾	63 15/16		93 1/16	93 1/3
14	3½	71 5/16		100 7/16	99 6/7
13	3¼	78 11/16		107 13/16	107 7/11
12	3	88 7/16		117 9/16	116 4/5

AA. The number of equal ſpaces in the ſhorter leg, that contained the ſame parcel of air diverſly extended.

B. The height of the mercurial cylinder in the longer leg, that compreſſed the air into thoſe dimenſions.

C. The height of the mercurial cylinder, that counterbalanced the preſſure of the atmoſphere.

D. The aggregate of the two laſt columns *B* and *C*, exhibiting the preſſure ſuſtained by the included air.

E. What that preſſure ſhould be according to the hypotheſis, that ſuppoſes the preſſures and expanſions to be in reciprocal proportion.

For the better underſtanding of this experiment, it may not be amiſs to take notice of the following particulars:

1. That the tube being ſo tall, that we could not conveniently make uſe of it in a chamber, we were fain to uſe it on a pair of ſtairs, which yet were very lightſome, the tube being for preſervation's ſake by ſtrings ſo ſuſpended, that it did ſcarce touch the box preſently to be mentioned.

2. The lower and crooked part of the pipe was placed in a ſquare wooden box, of a good largeneſs and depth, to prevent the loſs of the quickſilver, that might fall aſide in the transfuſion from the veſſel into the pipe, and to receive the whole quickſilver in caſe the tube ſhould break.

3. That we were two to make the obſervation together, the one to take notice at the bottom, how the quickſilver roſe in the ſhorter cylinder, and the other to pour in at the top of the longer; it being very hard and troubleſome for one man alone to do both accurately.

4. That the quickſilver was poured in but by little and little, according to the direction of him that obſerved below; it being far eaſier to pour in more, than to take out any, in caſe too much at once had been poured in.

5. That

meaſures of ſtrength to be met with in the air's ſpring, according to its various degrees of compreſſion and laxity. But, before I enter upon this ſubject, I ſhall readily acknowledge, that I had not reduced the trials I had made about meaſuring the expanſion of the air to any certain hypotheſis, when that ingenious gentleman Mr. *Richard Townley* was pleaſed to inform me, that having by the peruſal of my phyſico-mechanical experiments been ſatisfied that the ſpring of the air was the cauſe of it, he endeavoured (and I wiſh in ſuch attempts other ingenious men would follow his example) to ſupply what I had omitted concerning the reducing to a preciſe eſtimate, how much air dilated of itſelf loſes of its elaſtical force, according to the meaſures of its dilatation. He added, that he had begun to ſet down what occurred to him to this purpoſe in a ſhort diſcourſe, whereof he afterwards did me the favour to ſhew me the beginning, which gives me a juſt curioſity to ſee it perfected. But, becauſe I neither know, nor (by reaſon of the great diſtance betwixt our places of reſidence) have at preſent the opportunity to inquire, whether he will think fit to annex his diſcourſe to our appendix, or to publiſh it by itſelf, or at all; and becauſe he hath not yet, for aught I know, met with fit glaſſes to make an any-thing-accurate table of the decrement of the force of the dilated air; our preſent deſign invites us to preſent the reader with that which follows, wherein I had the aſſiſtance of the ſame perſon, that I took notice of in the former chapter, as having written ſomething about rarefaction: whom I the rather make mention of on this occaſion, becauſe when he firſt heard me ſpeak of Mr. *Townley*'s ſuppoſitions about the proportion, wherein air loſes of its ſpring by dilatation, he told me he had the year before (and not long after the publication of my pneumatical treatiſe) made obſervations to the ſame purpoſe, which he acknowledged to agree well enough with Mr. *Townley*'s theory: and ſo did (as their author was pleaſed to tell me) ſome trials made about the ſame time by that noble virtuoſo and eminent mathematician the Lord *Brouncker*, from whoſe further enquiries into this mattter, if his occaſions will allow him to make them, the curious may well hope for ſomething very accurate.

A table of the rarefaction of the air.

A. The number of equal ſpaces at the top of the tube, that contained the ſame parcel of air.

B. The height of the mercurial cylinder, that together with the ſpring of the included, air counterbalanced the preſſure of the atmoſphere.

C. The preſſure of the atmoſphere.

D. The complement of *B* to *C*, exhibiting the preſſure ſuſtained by the included air.

E. What that preſſure ſhould be, according to the hypotheſis.

A	*B*	*C*	*D*	*E*
1	00	Subſtracted from 29½ leaves	29¾	29¾
1½	10⅓		19⅓	19⅝
2	15½		14½	14⅞
3	20⅔		9⅔	9 15/16
4	22⅝		7⅛	7 7/16
5	24⅛		5⅝	5 9/10
6	24⅞		4⅞	4 7/10
7	25⅜		4⅜	4¼
8	26		3½	3 3/4
9	26⅓		3⅓	3 1/16
10	26⅝		3	2 9/10
12	27⅛		2⅝	2 3/4
14	27⅜		2⅜	2⅛
16	27⅝		2	1 5/64
18	27⅞		1⅞	1 7/12
20	28		1½	1 9/10
24	28⅛		1¼	1 3/16
28	28⅜		1⅛	1 1/16
32	28⅝		1⅛	0 15/16

To

To make the experiment of the debilitated force of expanded air the plainer, it will not be amiſs to note ſome particulars, eſpecially touching the manner of making the trial; which (for the reaſons lately mentioned) we made on a lightſome pair of ſtairs, and with a box alſo lined with paper to receive the mercury that might be ſpilt. And in regard it would require a vaſt, and in few places procurable quantity of quick-ſilver, to imploy veſſels of ſuch kind as are ordinary in the Torricellian experiment, we made uſe of a glaſs-tube of about ſix feet long; for that being hermetically ſealed at one end, ſerved our turn as well as if we could have made the experiment in a tub or pond of ſeventy inches deep.

SECONDLY, We alſo provided a ſlender glaſs-pipe of about the bigneſs of a ſwan's quill, and open at both ends; all along which was paſted a narrow liſt of paper, divided into inches and half quarters.

THIRDLY, This ſlender pipe being thruſt down into the greater tube almoſt filled with quickſilver, the glaſs helped to make it ſwell to the top of the tube; and the quickſilver getting in at the lower orifice of the pipe, filled it up till the mercury included in that was near about a level with the ſurface of the ſurrounding mercury in the tube.

FOURTHLY, There being, as near as we could gueſs, little more than an inch of the ſlender pipe left above the ſurface of the reſtagnant mercury, and conſequently unfilled therewith, the prominent orifice was carefully cloſed with ſealing-wax melted; after which the pipe was let alone for a while, that the air dilated a little by thē heat of the wax, might, upon refrigeration, be reduced to its wonted denſity. And then we obſerved by the help of the above-mentioned liſt of paper, whether we had not included ſomewhat more or ſomewhat leſs than an inch of air; and in either caſe we were fain to rectify the error by a ſmall hole made (with a heated pin) in the wax, and afterwards cloſed up again.

FIFTHLY, Having thus included a juſt inch of air, we lifted up the ſlender pipe by degrees, till the air was dilated to aⁿ inch, an inch and an half, two inches, *&c.* and obſerved in inches and eighths the length of the mercurial cylinder, which at each degree of the air's expanſion was impelled above the ſurface of the reſtagnant mercury in the tube.

SIXTHLY, The obſervations being ended, we preſently made the Torricellian experiment with the above-mentioned great tube of ſix feet long, that we might know the height of the mercurial cylinder, for that particular day and hour; which height we found to be $29\frac{3}{4}$ inches.

SEVENTHLY, Our obſervations made after this manner furniſhed us with the preceding table, in which there would not probably have been found the difference here ſet down betwixt the force of the air, when expanded to double its former dimenſions, and what that force ſhould have been preciſely according to the theory, but that the included inch of air received ſome little acceſſion during the trial; which this newly mentioned difference making us ſuſpect, we found by replunging the pipe into the quickſilver, that the included air had gained about half an eighth, which we gueſſed to have come from ſome little aërial bubbles in the quickſilver, contained in the pipe (ſo eaſy is it in ſuch nice experiments to miſs of exactneſs). We tried alſo with 12 inches of air ſhut up to be dilated; but being then hindered by ſome unwelcome avocations to proſecute thoſe experiments, we ſhall elſewhere, out of other notes and trials (God permitting) ſet down ſome other accurate tables concerning this matter. By which poſſibly we may be aſſiſted to reſolve, whether the atmoſphere ſhould be looked upon (as it uſually is) as a limited and bounded portion of the air; or whether we ſhould, in a ſtricter ſenſe than we did before, uſe the atmoſphere and aërial

part

part of the world for almoſt equivalent terms; or elſe whether we ſhould allow the word atmoſphere ſome other notion in relation to its extent and limits; (for as to its ſpring and weight, theſe experiments do not queſtion, but evince them.) But we are willing, as we ſaid, to refer the matters to our Appendix, and till then to retain our wonted manner of ſpeaking of the air and atmoſphere. In the mean time (to return to our laſt-mentioned experiments) beſides that ſo little a variation may be in great part imputed to the difficulty of making experiments of this nature exactly, and perhaps a good part of it to ſomething of inequality in the cavity of the pipe, or even in the thickneſs of the glaſs; beſides this, I ſay, the proportion betwixt the ſeveral preſſures of the included air undilated and expanded, eſpecially when the dilatation was great (for when the air ſwelled but to four times its firſt extent, the mercurial cylinder, though of near 23 inches, differed not a quarter of an inch from what it ſhould have been according to mathematical exactneſs) the proportion, I ſay, was ſuitable enough to what might be expected, to allow us to make this reflection upon the whole; that whether or no the intimated theory will hold exactly (for about that, as I ſaid above, I dare determine nothing reſolutely till I have further conſidered the matter) yet ſince the inch of air, when it was firſt included, was ſhut up with no other preſſure than that, which it had from the weight of the incumbent air, and was no more compreſſed than the reſt of the air we breathed and moved in; and ſince alſo this inch of air, when expanded to twice its former dimenſions, was able with the help of a mercurial cylinder of about 15 inches to counterpoiſe the weight of the atmoſphere, which the weight of the external air gravitating upon the reſtagnant mercury was able to impel up into the pipe, and ſuſtain above twenty-eight inches of mercury, when the internal air, by its great expanſion, had its ſpring too far debilitated to make any conſiderable (I ſay conſiderable, for it was not yet ſo dilated as not to make ſome) reſiſtance: ſince, I ſay, theſe things are ſo, the free air here below appears to be almoſt as ſtrongly compreſſed by the weight of the incumbent air, as it would be by the weight of a mercurial cylinder of twenty eight or thirty inches; and conſequently is not in ſuch a ſtate of laxity and freedom as men are wont to imagine; and acts like ſome mechanical agent, the decrement of whoſe force holds a ſtricter proportion to its increaſe of dimenſion, than has been hitherto taken notice of.

I MUST not now ſtand to propoſe the ſeveral reflections, that may be made upon the foregoing obſervations touching the compreſſion and expanſion of air; partly becauſe we could ſcarce avoid making the hiſtorical part ſomewhat prolix; and partly becauſe I ſuppoſe we have already ſaid enough to ſhew what was intended: namely, that to ſolve the phænomena there is not of our adverſary's hypotheſis any need: the evincing of which will appear to be of no ſmall moment in our preſent controverſy to him that conſiders, that the two main things, that induced the learned examiner to reject our hypotheſis, are, that nature abhors a vacuum; and that though the air have ſome weight and ſpring, yet, theſe are inſufficient to make out the known phænomena; for which we muſt therefore have recourſe to his Funiculus. Now as we have formerly ſeen, that he has not ſo ſatisfactorily diſproved as reſolutely rejected a vacuum ſo we have now manifeſted, that the ſpring of the air may ſuffice to perform greater things than what our explication of the Torricellian experiments and thoſe of our engine obliges us to aſcribe to it. Wherefore ſince beſides the ſeveral difficulties, that incumber the hypotheſis we oppoſe, and eſpecially its being ſcarce, if at all, intelligible, we can add that it is unneceſſary; we dare expect, that ſuch readers as are not biaſſed by their reverence for *Ariſtotle*, or the Peripatetick ſchools, will hardly reject an hypotheſis, which, beſides that it is very intelligible, is

now

now proved to be ſufficient, only to imbrace a doctrine, that ſuppoſes ſuch a rarefaction and condenſation, as many famous Naturaliſts rejected for its not being comprehenſible, even when they knew of no other way (that was probable) of ſolving the phænomena wont to be explicated by it.

PART III.

Wherein what is objected againſt Mr. BOYLE's *Explications of particular Experiments, is anſwered.*

AND now we are come to the third and laſt part of our defence; wherein we are to conſider, what our examiner is pleaſed to object againſt ſome paſſages of our Phyſico-Mechanical Treatiſe. But though this may ſeem the only part, wherein I am particularly concerned; yet perhaps we ſhall find it, if not the ſhorteſt, at leaſt the eaſieſt, part of our taſk. Partly, becauſe our author takes no exceptions at the experiments themſelves, as we have recorded them (which from an adverſary, who in ſome places ſpeaks of them as an eye-witneſs, is no contemptible teſtimony, that the matters of fact have been rightly delivered): and partly, becauſe there are divers experiments which, together with their explications, the examiner has thought fit to leave untouched, and thereby allows us to do ſo too: and partly alſo, becauſe that (as to divers of thoſe experiments, upon which he animadverts) he does not pretend to ſhew, that our explications are ill deduced or incongruous to our principles; but only that the phænomena may be explained either better, or as well, by his hypotheſis; whereof he ſuppoſes himſelf to have demonſtrated the truth, together with the erroneouſneſs of ours, in the other parts of his book, eſpecially the third, fourth and fifth chapters. So that after what we have ſaid to vindicate the hypotheſis we maintain, and take away our author's imaginary Funiculus; it will not be requiſite for us, on ſuch occaſions, to examine his particular aſſertions and explications. Which advertiſement we hope the reader will be pleaſed to bear in mind, and thereby ſave himſelf and us the trouble of a great deal of unneceſſary repetition. Wherefore, preſuming he will do ſo, we ſhall not ſtay to examine the firſt and ſecond corollaries, which in this 17th chapter he annexes to the manner of emptying our receiver by our pump. Neither ſhould we ſay any thing as to his third and laſt corollary, but that we think fit to deſire the reader to take notice, that according to what he teaches in that place, the more the air is rarefied, the more forcibly it is able to contract itſelf.

A defence of our firſt and ſecond EXPERIMENTS.

AND to proceed now to his 18th chapter, which he intitles *De experimentis Boylianis*, we ſhall find, according to what we lately noted, that againſt the firſt experiment he objects nothing, ſave that if one of the fingers be applied to the orifice of the valve, when the pump is freed from air, the experimenter ſhall feel to his pain, that the ſucker is not thruſt inward by the external air, but, as the finger, drawn

aſſertion he ſays is eaſy: but alleges two or three arguments for it, which I think will be more eaſily anſwered than his aſſertion evinced.

IN the firſt he ſays, that thoſe experiments concerning the adheſion of one's finger, *&c.* which he had mentioned in the foregoing chapter, *eodem modo ſe habent in loco clauſo ac in aperto.* But the anſwering of this we ſhall ſuſpend till anon; partly, becauſe it may then be more conveniently examined; and partly, becauſe our author ſeems not to build much upon it, his chief argument being that which he propoſes in theſe words: *Cum tota vis hujus Elaterii pendeat à refutato jam aëris æquipondio cum digitis* $29\frac{1}{2}$ *argenti vivi, ita ut nec plus, nec minus faciat hoc elaterium in loco occluſo, quam ſit per illud æquipondium in loco aperto; manifeſtum eſt, cum jam oſtenſum ſit fictitium planè eſſe hujuſmodi æquipondium, fictitium quoque eſſe tale elaterium.* Page 20. 'Being the whole power 'of the ſpring of the air depends upon the æquilibrium of its weight with twenty 'nine inches and a half of quickſilver, ſo that this ſpring doth neither more nor 'leſs in a ſhut place, than is done by that æquilibrium in an open place; it is mani- 'feſt, ſeeing we have ſhewed the æquilibrium to be plainly fictitious and imaginary, 'that the ſpring aſcribed to the air is ſo likewiſe.' Wherefore ſince all the validity of his objection againſt the ſpring of the air depends upon his former chapter, wherein he thinks he has diſproved the weight of the air; it will behove us to look back into the former chapter, and examine the four arguments which he there propoſes. But I muſt crave leave to vary from his method, and conſider the third in the firſt place, becauſe the removal of that objection will facilitate and ſhorten the anſwer to the reſt. His third argument therefore is thus ſet down: *Nam ſi tubus viginti tantum digitorum (quo uſi ſumus in primo argumento) non totus impleatur argento, ut prius, ſed ſpacium aliquod inter digitum ſuperiorem & argentum relinquatur in quo ſit ſolus aër; videbimus ſubtracto inferiore digito ſuperiorem non ſolum deorſum trahi, ut prius, ſed etiam argentum jam deſcendere, idque notabiliter, quantum nimirum extendi poteſt exigua illa aëris particula à tali pondere deſcendente. Unde ſi loco illius aëris ponatur aqua, aliuſve liquor qui non tam facilè extenditur, deſcenſus nullus erit.* Page 16.

Hinc, inquam, contra hanc ſententiam formatur argumentum: nam ſi externus ille aër nequeat vel hos viginti digitos argenti à lapſu ſuſtentare, uti jam vidimus, quomodo quæſo ſuſtentabit $29\frac{1}{2}$*? Certè hæc nullatenus reconciliari poſſunt.*

'FOR if a tube but twenty inches long (ſuch as we uſed in our firſt argument) be 'not quite filled with quickſilver, as before, but a little ſpace be left betwixt the 'mercury and the finger on the top of the tube, in which air only may abide; we 'ſhall find that the finger below being removed, the finger on the top will not only 'be drawn downwards, as before, but the quickſilver ſhall deſcend alſo, and that 'notably, *viz.* as much as ſo ſmall a parcel of air can be extended by ſuch a de- 'ſcending weight. So that if, inſtead of air, water, or any other liquor which is 'not ſo eaſily extended, be put in its place, there will be no deſcent at all.

'HENCE, I ſay, againſt this opinion an argument is framed: for if the external 'air cannot keep up thoſe twenty inches of quickſilver from deſcending, as we have 'proved; how ſhall it keep up twenty-nine inches and an half? Aſſuredly theſe can 'no way be reconciled.'

BUT to this argument, which he thinks ſo irreconcilable with his adverſary's hypotheſis, he has himſelf furniſhed them with an anſwer in theſe words; *Dices forte ideo argentum in hoc caſu deſcendere, quia deorſum truditur ab aëre illo ſeſe per ſuum Elaterium dilatante.* 'You will perchance ſay, that the quickſilver therefore doth in the 'alleged caſe deſcend, becauſe it is thruſt down by that parcel of air which dilates 'itſelf by its own ſpring.' Which anſwer I think ſufficient for the objection, notwithſtanding the two exceptions he takes at it.

FOR