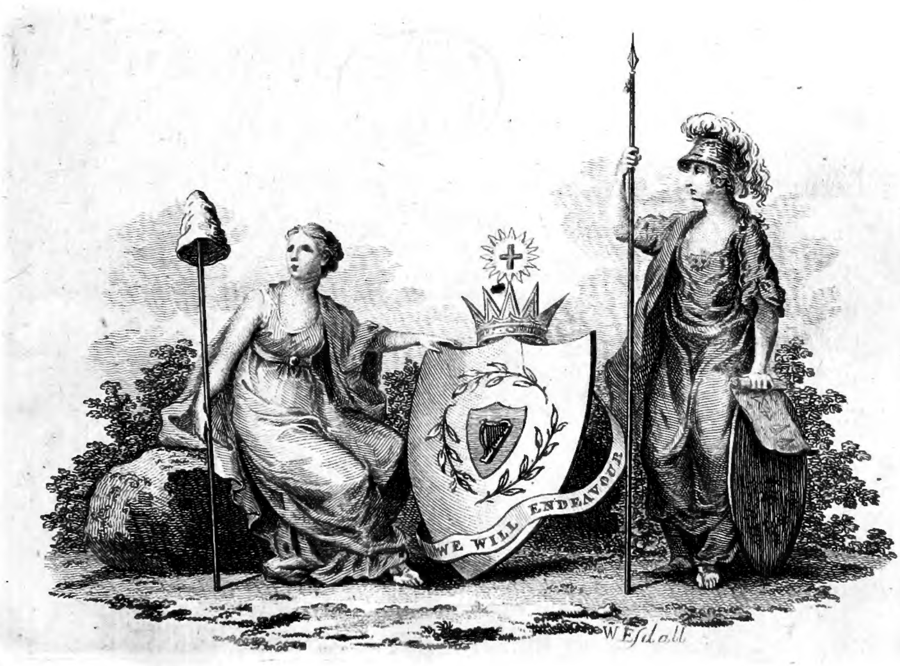
THE
TRANSACTIONS
OF THE
ROYAL IRISH ACADEMY.

M.DCC.LXXXIX.



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E R R A T A.

SCIENCE.

Page 11, Line 21, *for* 53, *read* 58.
Page 33, Line 3, *for* ſupernatent, *read* ſupernatant.
Page 35, Line 2, *for* mais, *read* maize.
Page 36, Line 3, *for* mais, *read* maize.
Page 43, Line 6, *for* Caſſup, *read* Caſhup.
Page 50, No. 4, *for* much, *read* minute.
Page 50, No. 5, *for* lanunated, *read* laminated.
Page 51, No. 23, *for* 1 9, *read* 0 9.
Page 52, Line 6, *for* on, *read* in.
Page 53, Line 2, *for* lay, *read* lie.
Page 58, Line 16, *for* Mooreſque, *read* Moreſque.
Page 82, Line 9, *for* height, *read* heights.
Page 103, Line 27, *dele* ætatis diem.
Page 118, Line 15, *for* ſides, triangles, *read* ſides of a triangle.
Page 151, Line 7, *for* calculation, *read* collection.
Page 152, Line 18, *for* proſperity, *read* property.

POLITE LITERATURE.

Page 8, Line 11, *for* has, *read* had.

ANTIQUITIES.

Page 3, Line 10, *for* Alceus, *read* Alcæus.
Page 5, Line 9, *for* apartment, *read* apartments.
Page 21, Line 15, *for* laſt, *read* late.
Page 31, Note *, *for* Lipſus, *read* Lipſius.
Page 35, Line 3, *for b, b, read i, i.*
Page 76, Line 4, *for* Saxum, *read* Saxhum.
Page 76, Line 13, *for* Maſſorabic, read Meſſarabic.
Page 78, Line 6, *for* Sexons, *read* Saxons.
Page 79, Line 5, *for* papal bulls from, *read* papal bulls, and from.

☞ A Catalogue of the Donations to the Academy will be given in the next Volume.

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C O N T E N T S.

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Experiments on the ALKALINE SUBSTANCES *used in* BLEACHING, *and on the* COLOURING MATTER *of* LINEN-YARN. *By* RICHARD KIRWAN, *Esq;* F. R. S. *and* M. R. I. A.

SECTION I.

BLEACHING being one of those arts which consist in scarce any thing else than a particular application of some of the general principles of chymistry, it might be expected that the knowledge of the instruments it employs would keep pace with the progress and improvements of that science to which it is subordinate, and so much the more as the nature of alkaline substances in general, which are its proper instruments, has been in great measure explained by the celebrated Doctor Black upwards of thirty years ago: Yet it has so happened, that on a late occasion, when a scarcity of these saline substances, imported from foreign countries, unhappily prevailed in this kingdom, it was seriously questioned whether their place could be supplied by materials manufactured

Read April 4, 1789.

manufactured at home. In the courſe of this diſcuſſion, it evidently appeared, from the contradictory teſtimonies of many of the principal bleachers, that however they might excel in that art, when well provided with the inſtruments they employ, they were but little acquainted with the general agency of the inſtruments themſelves and their reſpective powers, or even with the moſt advantageous and œconomical method of employing them. To elucidate theſe points by an analyſis of ſome of the different ſubſtances employed by bleachers, and by giving a ſure method of diſtinguiſhing the relative powers of every ſaline ſubſtance they may uſe, together with an account of the beſt method of obtaining them, as well as of adapting them to the purpoſe of bleaching, is the object of this paper. This taſk, which I have impoſed upon myſelf ſolely with a view to the utility of the public, requires no ingenuity, and might have been long ago well executed by many others, if chymiſtry, which has ſo many votaries of the higheſt rank in the moſt civilized parts of Europe, had been more known and cultivated in this country, which perhaps of all others ſtands moſt in need of its aſſiſtance.

SECTION II.

BARILHA.

OF this ſubſtance there are ſeveral ſorts made of different plants*, but the beſt is that formed near Alicant, at a diſtance from the ſea, by the combuſtion of a plant called by the inhabitants *barilha*, and deſcribed by Juſſien in the memoirs of the academy of Paris for 1717, under the name of *kali hiſpanicum, ſupinum annuum, ſedi foliis brevibus.* It ſeems to be claſſed by Linneus under the *pentandria digyn*, by the name of *ſal ſola vermiculata fruteſcens foliis ovatis acutis carnoſis*, and ſhould carefully be diſtinguiſhed from the various kinds of *ſalicornia* which he ranges under the title of *monandria monogyna;* and alſo from other plants which he calls *chænopodia*, which yield an alkali, but leſs pure than the *ſalſola*. Theſe plants being dried to the ſame degree as hay, are burned in pits nearly as kelp is with us, the aſhes and ſalt run into a greyiſh blue maſs, which is the barilha †. The beſt ſort is here called *ſweet barilha.*

THE ſweet barilha which I examined was moſt obligingly preſented to me by Mr. Byrne, an eminent merchant of this city. It was of a bluiſh colour, covered over with a ſaline powder

* See Colonel Conyngham's letter, report of the committee of the houſe of commons of Ireland, 1788, p. 87.

† And by the French *ſoude*, as being employed in ſoldering metals.

exceedingly

exceedingly hard, and had a ſmart alkaline taſte. When broken it looked black in the fractured part, and viſibly contained pretty large pieces of charcoal.

To find the proportion of fixed air in this ſubſtance, having reduced a quantity of it to fine powder, I poured on an ounce of it a ſufficient quantity of marine acid, and found it to loſe by the action of this acid 80 grains of its weight, conſequently one pound troy of this ſubſtance contains 960 grains of fixed air, (mixed with a little that had an hepatic ſmell) that is exactly $\frac{1}{6}$ of its weight. Other parcels contained ſomewhat more, and others ſomewhat leſs.

As this ſubſtance evidently contained ſome parts that were ſoluble in water and ſome that were inſoluble therein, to diſcover the weight of each I poured on one pound of it reduced to fine powder thirteen pounds of water moderately hot, ſucceſſively; this water had previouſly been boiled and filtered, and contained no other impurity than a ſlight trace of common ſalt. This quantity of water was neceſſary to exhauſt all the ſoluble matter in the barilha.

The ſolutions were taken in ſix different portions, none of them betrayed the ſmalleſt mark of ſulphur, of which I was aſſured by trying them with the nitrous ſolution of ſilver, nor did the Pruſſian alkali diſcover any veſtige of iron.

By eighteen ſucceſſive evaporations and cryſtallizations, I obtained 4881 grains of ſaline matter, the different ſpecies of which I ſhall preſently mention, and 2903 of inſoluble matter.

It

It may at firſt ſight appear extraordinary that the ſaline matter and the inſoluble part ſhould weigh more than the pound of barilha that ſeemed to afford them, for this amounts only to 5760 grains, and the two former weigh 7784 grains; but it ſhould be conſidered that theſe products were obtained not from the barilha alone, but from the barilha and the water in which the ſalts were diſſolved, whoſe cryſtals retained a great quantity of it, and alſo from the air to which the ſolutions were expoſed, and which they abſorbed in large proportion.

As the quantity of the inſoluble matter was ſubject to no ſuch deceptive appearance, I began by examining the weight of that, for this being ſubtracted from 5760 grains neceſſarily determined the true weight of the ſaline part, and as the ſtate in which the ſaline part exiſts in barilha depends in ſome meaſure of the earths and charcoal with which it is united, as well as the moſt advantageous method of uſing it, I examined the nature and quantity of theſe very minutely.

Having therefore dried the inſoluble matter for a conſiderable time in a low heat until it appeared as dry as the barilha itſelf, and having found its weight in that ſtate to amount to 2903 grains or 6,04791 ounces, I took one ounce of it, and drying it in a heat little below redneſs, found it to loſe 38 grains of moiſture.

Another ounce of the ſame reſiduum being treated with dilute marine acid loſt 125,5 grains of its weight, and this loſs expreſſes the quantity of fixed air contained in it.

Another

ANOTHER ounce being calcined in a white heat for about one hour loſt 200 grains of its weight, and on repeating this experiment I found the loſs amount to 199 grains.

LASTLY, on the 281 grains which remained after this experiment I poured dilute marine acid, and found the quantity of fixed air to be 106 grains.

HENCE I deduce the weights of the ſeveral ſubſtances diſſipated by the calcination of an ounce of the inſoluble reſiduum of the barilha.

1ſt, The weight of the fixed air loſt was 125,5—106=18,5 grains.

2dly, The loſs of moiſture was 38 grains.

3dly, The loſs of the fixed air and moiſture amounted together to 56,5 grains. This deducted from the intire loſs, that is from 199 grains, gives the loſs ariſing from the combuſtion of the charcoal, and conſequently its quantity, 199—56,5=142,5 grains.

I NEXT proceeded to examine the fixed incombuſtible part that remained after the above calcination. On the 279 grains of this, which remained after the calcination of an ounce of the inſoluble part, I poured a quantity of diſtilled vinegar, whoſe ſpecific gravity in the temperature of 62° was 1,008, and digeſted that reſiduum therein for ſixteen hours in a heat little more than 100°. After edulcoration and deſiccation I found the weight of what remained undiſſolved to amount to 63 grains. Upon this experiment

riment I reasoned thus : 281 grains of a residuum of this sort contained 106 grains of fixed air, therefore the 279 grains subjected to the vinegar in this experiment must have contained 105,24 which were dissipated by the action of the acid ; there remained therefore of mere earth only 173,76 ; but of these 63 escaped the action of the acid, therefore there were dissolved 110,76. And as distilled vinegar can act only on calcareous and muriatic earth, (the barytic being not expected) the 110,76 that were dissolved must have consisted of either or both of these, and the undissolved 63 grains must have been argillaceous or siliceous.

To determine the first point I distilled in a glass retort the acetous solution, which was very voluminous, until no more than about four pints remained. During the distillation some earth was deposited, which when dried in a red heat amounted to 4 grains ; this I re-dissolved, and finding it precipitable by caustic volatile alkali judged it to be magnesia. I then took as much of the acetous solution as amounted to $\frac{1}{6}$ of the whole, and pouring caustic volatile alkali upon it, obtained nearly 3 grains or more exactly 2,83 grains of magnesia precipitated by the alkali. Whence I concluded the whole solution to contain 17 grains, to which adding the 4 grains deposited, we have the intire quantity of magnesia dissolved by the vinegar =21 grains, and consequently the remainder of 110,76 grains, namely 89,76, must have been calcareous earth.

I ALSO examined the quantity of this earth in another manner ; to the $\frac{5}{6}$ of the acetous solution that remained I added gradually vitriolic acid, whose specific gravity was 1,463, as long as any precipitation appeared to take place, then pouring off the vinegar I edulcorated the residuum, and having dried it found it to weigh 240 grains, and consequently if the whole acetous solution had been used, the resulting selenite would have weighed 288 grains: Now 100 grains of selenite contain 32 of calcareous earth, therefore 288 grains contain 92,16 grains, which differs inconsiderably from the former determination.

LASTLY, the 63 grains which eluded the action of the acetous acid being digested in spirit of salt, left a residuum of 41,3 grains, which therefore were siliceous; the remainder not being precipitable by the vitriolic acid was consequently argillaceous earth; hence the quantities of these ingredients in 480 grains of the insoluble part of barilha were found to be

		And in the whole insoluble part.
	Grains.	Grains.
Fixed air	125,5	759
Water	38	229,82
Charcoal	142,5	861,82
Calcareous earth	89,76	542,86
Muriatic	21	127
Argillaceous	21,7	131,23
Siliceous	41,3	249,58
	479,76	2901,31
Error	,24	1,69
	480,00	2903,00

I NOW

I now return to the foluble part of the barilha, which neceffarily amounted only to 2857 grains, as 5760—2903=2857.

In the firft place I obtained 4213 grains of pure cryftallized mineral alkali, but thefe cryftals are known to contain but $\frac{1}{5}$ of real alkaline fubftance, the remainder of their weight being fixed air and water of cryftallization, therefore one pound of barilha contains but 842 grains of pure real alkali.

Besides this I alfo obtained 127 grains of a mixture of mineral alkali and common falt, which I could not eafily feparate, and 346 grains of a mixture of vegetable and mineral alkali, with a fmall proportion of extractive matter, and fome digeftive falt, as I believe; this mafs conftantly attracted moifture. I weighed it hot and dry, but forgot to examine the portion of fixed air it contained; it could not be lefs nor much more than 28 per cent. and therefore this mafs contained about 250 grains of mere alkali.

These folutions, and particularly the laft portions, afforded alfo 125 grains of Glauber's falt and 70 of common falt, but the Glauber's falt at leaft did not exift in a cryftallized form in the barilha; and as 100 grains of it are reduced to 42 by expelling the water of cryftallization, no more than 53 grains of it can be deemed to have pre-exifted in the barilha.

These folutions alfo depofited 20 grains of earth.

Hence the weights of the different ingredients contained in one pound of fweet barilha are as follows:

Fixed air	960	
Charcoal	861,82	
Calcareous earth	542,86	1050,67
Muriatic earth	127	
Argillaceous	131,23	
Siliceous	249,58	
Mineral alkali pure	842	1219
Mineral ditto impure	250	
Mineral ditto mixed with common ſalt	127	
Glauber's ſalt	125	
Common ſalt	70	
Earth depoſited	20	
	4306,49	
Water	1453,51	
Total	5760,00	

Hence we ſee that the alkaline part of barilha is nearly in a cauſtic ſtate, for the intire pound of barilha contained but 960 grains of fixed air, and of this quantity we have ſeen that 759 were contained in the earthy part. Therefore only 201 grains were contained in the ſaline part. Now 960 grains of this (and the mere alkaline part did not certainly amount to leſs) require for their ſaturation at leaſt 700 of fixed air, therefore they wanted at at leaſt $\frac{2}{3}$ of the quantity requiſite to ſaturate them. And hence bleachers ſhould not uſe boiling water to extract the ſaline ſubſtance of barilha, for the alkaline part being in a cauſtic ſtate diſſolves part of the coaly matter with which it is united, which ſullies the ſolution, gives it a dark hue, and afterwards is depoſited on the linen, and cannot be ſeparated by acids.

SECTION III.

OF DANTZIC PEARL ASH.

THIS ſalt was alſo ſent to me by Mr. Byrne. It is exceeding white, and if not expoſed to the air, very hard, and poſſeſſes an alkaline taſte.

THE quantity of fixed air and earth contained in different parcels of this ſubſtance is variable; in ſome ounces I found the quantity of fixed air to amount to 100 grains, in others to 115; and therefore at a medium it may be rated at 107,5 grains, or 1290 grains, in one pound troy. The earth remaining after the ſolution of one pound amounted to 20 grains.

ONE ounce of this ſubſtance gradually heated to redneſs, and kept in that heat for three quarters of an hour, loſt 70 grains of its weight; and being then diſſolved in ſpirit of ſalt, loſt 72 grains; therefore the quantity of moiſture in one ounce of this ſubſtance was 70—107,5—72=34,5 grains, or 414 in one pound.

AGAIN, after ten evaporations I procured from one pound of this ſubſtance 505 grains of tartar vitriolate, the laſt portions of which appeared by the teſt of the nitrous ſolution of ſilver to contain ſome digeſtive ſalt, and alſo 36 grains of this laſt containing a portion of tartar vitriolate; about 18 grains of earth were depoſited during the evaporations. The remainder of the pound, after all theſe deductions, muſt have conſiſted of pure mere alkali. Hence the ingredients in a pound muſt have been nearly in the following quantities:

Fixed air - -	1290	
Moiſture - -	414	
Tartar vitriolate - -	505	
Digeſtive ſalt and ditto -	36	
Earth - - -	38	
	2283	5760
Mere alkali - -	3477	—2283
	5760	3477

DISGUSTED by the tedioufneſs of theſe experiments, and recollecting that the alkaline part of theſe ſalts was that alone with which bleachers had any concern, I bethought myſelf of an eaſy practical method of diſcovering the preſence of this principle, and determining its quantity in all ſubſtances in which it exiſts, either uncombined, or combined only with fixed air or ſulphur.

SECTION IV.

To difcover whether any quantity of fixed alkali worth attention exifts in any faline compound, diffolve one ounce of it in boiling water, and into this folution let fall a drop of a folution of fublimate corrofive; this will be converted into a brick colour, if an alkali be prefent, or into a brick colour mixed with yellow, if the fubftance tried contains lime.

But the fubftances ufed by bleachers being always impregnated with an alkali the above trial is in general fuperfluous, except for the purpofe of detecting lime. The quantity of alkali is therefore what they fhould chiefly be folicitous to determine, and for this purpofe:

1ft, Procure a quantity of allum, fuppofe one pound, reduce it to powder, wafh it with cold water, and then put it into a tea-pot, pour on it three or four times its weight of boiling water.

2dly, Weigh an ounce of the afh or alkaline fubftance to be tried, powder it and put it into a Florence flafk with one pound of pure water, (common water boiled for a quarter of an hour, and afterwards filtered through paper, will anfwer) if the fubftance to be examined be of the nature of barilha or pot-afh; or half a pound of water if it contain but little earthy matter, as pearl-afh; let them boil for a quarter of an hour, when cool let the folution be filtered into another Florence flafk.

3dly,

3dly, This being done, gradually pour the ſolution of allum hot into the alkaline ſolution alſo heated; a precipitation will immediately appear; ſhake them well together, and let the effervefcence, if any, ceaſe before more of the aluminous ſolution be added; continue the addition of the allum until the mixed liquor, when clear, turns ſyrup of violets or paper tinged blue by raddiſhes, or by litmus, red; then pour the liquor and precipitate on a paper filter placed in a glaſs funnel, the precipitated earth will remain on the filter; pour on this a pound or more of hot water gradually until it paſſes taſteleſs; take up the filter and let the earth dry in it until they ſeparate eaſily, then put the earth into a cup of Staffordſhire ware, place it on hot ſand and dry the earth until it ceaſes to ſtick to glaſs or iron, then pound it and reduce it to powder in the cup with a glaſs peſtle, and keep it a quarter of an hour in a heat of from 470° to 500°.

4thly, The earth being thus dried, throw it into a Florence flaſk and weigh it, then put about one ounce of ſpirit of ſalt into another flaſk, and place this in the ſame ſcale as the earth, and counterbalance both in the oppoſite ſcale: This being done, pour the ſpirit of ſalt gradually into the flaſk that contains the earth, and when all effervefcence is over, (if there be any) blow into the flaſk, and obſerve what weight muſt be added to the ſcale containing the flaſks to reſtore the equilibrium; ſubtract this weight from that of the earth, the remainder is a weight exactly *proportioned* to the weight of mere alkali of that particular ſpecies which is contained in one ounce of the ſubſtance examined; all beſide is ſuperfluous matter.

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I HAVE ſaid that alkalies of the *ſame ſpecies* may thus be directly compared, becauſe alkalies of *different* ſpecies cannot, but require the intervention of another proportion; and the reaſon is, becauſe *equal* quantities of alkalies of different ſpecies precipitate *unequal* quantities of earth of allum. Thus 100 parts by weight of mere *vegetable* alkali precipitate 78 of earth of allum; but 100 parts of *mineral* alkali precipitate 170,8 parts of that earth. Therefore the precipitation of 78 parts of earth of allum by vegetable alkali denotes as much of this, as the precipitation of 170,8 of that earth by the mineral alkali, denotes of the mineral alkali. Hence the quantities of alkali in all the different ſpecies of pot aſhes, pearl aſhes, weed or wood aſhes, may be immediately compared by the above teſt, as they all contain the vegetable alkali, and the different kinds of kelp or kelps manufactured in different places, and the different ſorts of barilha, may thus be compared, becauſe they all contain the mineral alkali; but kelps and pot aſhes, as they contain different ſorts of alkali, can only be compared together by means of the proportion above indicated.

THE application of this teſt is founded on the following principles:

1ſt, That a hot ſolution of a free alkali, or of an alkali combined only with fixed air or ſulphur, can hold no *terreno* or *metallico neutral* ſalt in ſolution; though it may *alkalino neutral* ſalts or quick lime, if the alkali be free from fixed air.

2dly, That earth of allum cannot be precipitated either totally or partially by the hot ſolutions of any *alkalino neutral* ſalt, and therefore that its precipitation is always due to the preſence of a

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free alkali, or at leaſt of an alkali combined only with fixed air or ſulphur, to whoſe quantity it is always proportional. It is true quick lime will alſo decompoſe allum, but the preſence of quick lime is eaſily diſcovered by the addition of a few drops of any mild alkaline ſolution, and by the ſame means as eaſily ſeparated.

3dly, That if the earth of allum takes up fixed air, (which would increaſe its weight) this air will be ſeparated by the heat employed in drying it, or at leaſt by the ſpirit of ſalt poured upon it, and ſo may alſo another heterogeneity which will hereafter be mentioned.

I CAN ſee but one inaccuracy attending this teſt, and that of little moment; it is this, if the alkali contains ſulphur, this will alſo be precipitated with the earth of allum and increaſe its weight. The limits of this inaccuracy, at leaſt in common caſes, ſcarcely reach 2 or 3 grains, as we ſhall preſently find.

SULPHUR is eaſily detected in any alkaline ſolution by ſaturating it with an acid; hepatic air is generally developed, and the liquor becomes troubled.

NOT only the proportion but alſo the *abſolute* weight of alkali in different alkaline ſubſtances or aſhes may be found by this teſt, as will appear by the following experiments:

SECTION V.

Of the Quantity of mere Alkali in different Alkaline Subſtances as exhibited by the aluminous Teſt.

CRYSTALLIZED SODA.

I BEGIN with this as being the pureſt mineral alkaline ſubſtance in a dry form produced by art. Though it contains only $\frac{1}{5}$ of its weight of real alkali, the remainder being water and fixed air, but the proportion of alkali being invariably the ſame, it is the fitteſt for a ſtandard with which other ſubſtances containing the ſame ſort of alkali may be compared. I found that as much of this ſubſtance as would contain 480 grains of mere alkali would precipitate 725 grains of earth of allum dried and treated as already mentioned, and conſequently that 480 grains of *mere* mineral alkali precipitate 725 of earth of allum.

NOTE, That in this and all the ſubſequent experiments a little more earth of allum is precipitated than is mentioned, becauſe a little always remains in the filtering paper that cannot be had out of it, and I have reaſon to think by weighing the paper before and after, that this quantity amounts to 3 or 4 grains; but as this defect is the ſame in all caſes, it does not invalidate the compariſon.

Sweet Barilha.

The ſolution of one ounce of barilha precipitated 174 grains of earth of allum; therefore, ſince 725 grains of earth of allum require for their precipitation 480 of mere mineral alkali, 174 grains of that earth require 115,2 of mere mineral alkali. And conſequently one ounce of barilha contains but 115,2 of mere alkali; and one pound of barilha ſhould contain 1382,4 grains. This quantity exceeds by about $\frac{1}{8}$ the quantity I found by direct analyſis, but poſſibly one pound may contain more than another, for it could ſcarcely happen that I ſhould commit a miſtake of that magnitude.

I must not here omit an odd appearance that occurred in this experiment: The earth of allum in drying acquired a bluiſh colour, and when ſpirit of ſalt was poured on it, to diſengage the fixed air it might contain, the blue colour was more developed, and ſome blue particles floated in the liquor. This ſeems to proceed from the tinging matter of Pruſſian blue which has been found in barilha. The weight of this I have not examined, but it could not exceed 1 or 2 grains.

Cunnamara Kelp.

This was manufactured by Mr. Martin Mealy, and ſent to me by Mr. Francis French, an eminent merchant in this city. It is a hard porous black ſubſtance, mixed with white and grey ſpots, its ſmell ſulphureous, and its taſte mixed, being that of common ſalt and alkali. One ounce of it diſſolved in marine acid

acid loſt 24 grains of its weight, which eſcaped in an aerial form. This air was hepatic.

Another ounce diſſolved in boiling water left an inſoluble reſidue, which being heated in a crucible to redneſs weighed 165 grains; this reſidue effervefced with acids, and ſeemed for the moſt part calcareous.

The ſolution by the teſt of the nitrous ſolution of ſilver evidently contained ſulphur, and the Pruſſian alkali gave manifeſt ſigns of iron.

This ſolution precipitated 25 grains of earth of allum, and therefore contained 16,5 grains of mere alkali. During the precipitation of the earth of allum much hepatic air was emitted, and the earth was ſullied by the ſulphur, though only a few grains of this can be preſumed to be mixed with it.

To find the quantity of ſulphur in this kelp I diſſolved two ounces of it in pure water, and ſaturated the ſolution with marine acid; the liquor became turbid, and partly by filtration and part by ſpontaneous depoſition, (for ſome of the ſulphur paſſed through the filter) I obtained 8 grains of ſulphur, which gives 4 grains for each ounce, beſides what exhaled in hepatic air.

In order to eſtimate the quantity of ſulphur which a given quantity of mineral alkali is capable of containing, I diſſolved 400 grains of cryſtallized mineral alkali in ſix times its weight of water, (this quantity of the cryſtals contained 80 grains of mere alkali) and to this I added 80 grains of ſulphur, and boiled them

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for half an hour; only 60 grains of ſulphur were diſſolved, by which I found that this alkali can take up nearly $\frac{3}{4}$ of its weight of ſulphur in the moiſt way; I ſay nearly, as ſome earth remained with the undiſſolved ſulphur. With this ſaturated liver of ſulphur I precipitated a ſolution of allum, and found the precipitate to amount to 130 grains. Now 80 grains of mere mineral alkali can precipitate only 120,8 grains of earth of allum; therefore 9 grains of the above precipitate were ſulphur. Yet this ſmall proportion of ſulphur was very viſible in the earth of allum when heated to 500 degrees, by its ſtrong yellow colour; therefore in the precipitation of the earth of allum by kelp, in which no ſulphur was viſible, the proportion was incomparably ſmaller, and no deduction need be made on that account.

THERE are three methods of deſulphurating kelp, or any other alkalino ſulphureous compound: The firſt is by calcining it in an open fire by expoſing it to a rapid blaſt of air; and for this a very ingenious contrivance was deviſed by my much reſpected friend Mr. WILLIAM DEAN. The only inconvenience attending it is that much of the ſulphur will be converted into vitriolic acid, and thus combine with the alkali. The ſecond is by ſaturating it with a vegetable acid, and afterwards calcining it, by which means the vegetable acid will be decompoſed; if this method could be cheaply executed it would be the beſt. The third is by ſaturating a ſolution of kelp with fixed air: This I have endeavoured to effect by putting a ſolution of two ounces of kelp into Doctor Nooth's machine for impregnating water with fixed air; the liquor ſoon became turbid, and emitted a ſtrong hepatic

ſmell;

ſmell; after the ſulphur had ſubſided I drew off the liquor, and with one half of it precipitated a ſolution of allum. No hepatic ſmell was now perceptible, and the precipitate amounted to 40 grains. I dare not ſay that this great increaſe of power in the alkali was intirely owing to the deſulphuration, but ſome part undoubtedly was; yet the quantity of ſulphur I could collect was very inconſiderable, and mixed with coal duſt. Kelp may alſo be deſulphurated by nitre, as ſhall hereafter be ſhewn. According to Doctor Watſon, 30 ounces of kelp afforded him 12 ounces of cryſtallized mineral alkali, conſequently 1 ounce would afford ,4 of an ounce, that is 192 grains, of which $\frac{1}{5}$, that is =38 grains, muſt have been mere alkali. His kelp might have been better than that I uſed; but it is impoſſible that his alkali was pure, as mineral alkali, when mixed with ſuch a quantity of common ſalt as is in kelp, can never be thoroughly ſeparated from it, but by proceſſes which he certainly did not uſe, namely, by precipitating a ſolution of ſilver in ſpirit of nitre, eſtimating the quantity of luna cornua, and afterwards decompoſing the cubic nitre, or by ſaturating the alkali with diſtilled vinegar, and diſſolving the neutral ſalt thus formed in ſpirit of wine, which leaves the common ſalt behind.

STRANGFORD KELP.

THIS was ſent to me by my worthy friend Mr. BRAUGHALL. It was much denſer, leſs porous, and in appearance approached more to that of a vitrified maſs than Cunnamara kelp; it was at leaſt equally ſulphureous. The ſolution of one ounce of it precipitated only 9 grains of earth of allum, and this earth was much more

more diſcoloured than that precipitated by Cunnamara kelp. The inſoluble reſiduum of an ounce amounted to 174 grains.

Vegetable Alkali.

I found that 480 grains of the pureſt and drieſt ſalt of tartar (making allowance for the quantity of fixed air it contained) precipitated 331,5 grains of earth of allum.

Dantzic Pearl Ash.

The ſolution of one ounce of this ſalt precipitated in one experiment 200 grains of earth of allum; and in another 220 grains; at a medium 210 grains. Then if 331,5 grains of this precipitate require 480 grains of mere vegetable alkali, 210 grains of this precipitate require 304; therefore at a medium an ounce of this ſubſtance contains 304 grains of mere alkali, and a pound contains 3648. By my analyſis it contained 3477 grains; the difference is 171 grains.

We may now determine which of two or more ſaline ſubſtances, one poſſeſſing the mineral, the other the vegetable alkali, is beſt in its kind; for that ſubſtance is beſt in its kind which approaches moſt to its proper ſtandard; 725, that is, the precipitation of 725 grains of allum being the ſtandard of the goodneſs of an ounce of a ſubſtance containing the mineral alkali, and 331,5 being the ſtandard of the richneſs of an ounce of a ſubſtance containing the vegetable alkali. Thus, if we compare barilha and Dantzic pearl aſh, as the ſtandard of barilha is to the quantity of earth of allum, an ounce of it precipitates, ſo

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is the ſtandard of Dantzic to the quantity an ounce of it precipitates; or 725 : 174 : : 331,5 : 79,5, by which we ſee that an ounce of Dantzic ſalt that would precipitate 79,5 grains of earth of allum would be as good in its kind as an ounce of barilha that precipitates 174; therefore ſince an ounce of Dantzic ſalt precipitates 210, it is richer in its kind by the difference between 79,5 and 210.

With reſpect to antacid powers the mineral alkali is ſtronger than an equal quantity of the vegetable, that is, will ſaturate more acid, nearly in the proportion of 48 to 22; yet it attracts acids leſs, that is, with leſs force and activity, ſince the vegetable will take them from the mineral alkali. But if the quantities of real alkali be unequal we may compare their antacid powers in this manner: As the precipitate by an ounce of a ſubſtance containing the mineral alkali is to 48, ſo is the precipitate by an ounce of a ſubſtance containing the vegetable alkali to a number expreſſing its comparative antacid power. Thus with reſpect to barilha and Dantzic ſalt, as 174 : 48 : : 210 : 58 nearly; therefore the antacid power of Dantzic ſalt is greater than that of barilha, when taken in equal quantities in the ratio of 58 to 48.

Cashup.

The beſt ſort, namely, that marked with the croſs arrows, is of a bluiſh grey colour, exceeding hard and of a ſemivitrified appearance, its ſmell ſulphureous, its taſte ſcarcely alkaline, and does not attract the moiſture of the air. With marine acid, one ounce of it afforded 31 grains of hepatic air. When diffolved in water the refiduum of an ounce was 357 grains of a grey earth that appeared to be calcareous for the moft part. The folution itfelf was of a yellow colour and ftrongly fulphureous. With the folution of allum it did not effervefce ftrongly until a good deal was added. The precipitate was of a dirty white, and amounted to 66 grains, of which two appeared to be fulphur. Hence its quantity of vegetable alkali is nearly 93 grains per ounce.

Mr. Clarke's Refined Ash.

This I obtained from Mr. Clarke himfelf. It is of a yellowifh white colour, with greenifh fpots; many pieces are externally white and internally green; it is moderately hard, of a very fharp tafte, and effervefces with acids.

An ounce of this fubftance diffolved in twelve ounces of boiling water did not effervefce with acids, but precipitated the folution of fublimate corrofive yellow and red as lime water does, and left a refiduum of 17 grains, which was evidently calcareous. A folution of two ounces of this fubftance being impregnated with fixed air in Doctor Nooth's machine, depofited 5 grains of mild calcareous earth; but a folution made in three or four times its weight of water, or without the affiftance of heat, contained no lime, and effervefced flightly with acids; and when this falt is fome time expofed to the air, its folution contains no lime.

A solution of one ounce of this falt precipitated 89 grains of earth of allum, and therefore contained 129 grains of mere vegetable

vegetable alkali, to which if we add 17 grains of inſoluble earth, we ſhall find that the remainder of the ounce, namely, 334 grains, conſiſted of neutral ſalts, namely, digeſtive ſalt, and perhaps tartar vitriolate, in ſmall quantity. To prove the exiſtence of theſe I ſaturated an ounce of the ſolution of this refined aſh with the nitrous acid, and then dropped into it the nitrous ſolution of ſilver; this latter was immediately precipitated in a curdy form, which as the alkaline part was ſaturated could proceed only from the marine acid contained in the digeſtive ſalt.

In the report of the committee of the houſe of commons, dated April 1788, Mr. Clarke delivers an account of his method of manufacturing this ſalt. He mixes five parts weed or wood aſhes with one part quick lime, and ſuffers them to lie together in a heap for ſix, nine or twelve months, and then extracts a ley from them which he evaporates to dryneſs. By ſuffering the lime and aſhes to ſtand together for ſo many months, he imagines that the common ſalt contained in the aſhes is decompoſed, and the quantity of alkali thus increaſed; but though it is poſſible to decompoſe common ſalt by quick lime, as Mr. Scheele has ſhewn, yet this decompoſition is effected by a very different management; and if in the firſt part of Mr. Clarke's proceſs ſuch a decompoſition were obtained, a recompoſition would ſpeedily be effected in the ſecond part of his proceſs; for ſuppoſing the marine acid to quit its alkaline baſis and unite to the lime, yet when the alkaline ſalt and marine ſelenite are both drawn off into the ley, the alkali immediately decompoſes the marine ſelenite and reunites to its acid, according to the well known laws of chymical affinity. So that by this long *maceration* (as he calls it) no advantage whatſoever is gained. However, Mr. CLARKE's ſalt is undoubtedly a valuable preparation for the purpoſe of bleaching, and may be obtained in a ſpace of time incomparably ſhorter than he requires.

THE neutral ſalts contained in the ſolution of Mr. CLARKE's refined aſh do not proceed from any error in his proceſs, but from the bad quality of the aſhes he employs.

COMMON IRISH WEED ASHES.

I OBTAINED a parcel of theſe aſhes from Mr. CLARKE; it was of a looſe texture, dark grey colour and ſalt taſte, mixed with charcoal, brick duſt and other impurities. I choſe the cleaneſt, and ſifted it. One ounce of it loſt by gentle drying 47 grains, and in a red heat 72 grains more.

TWELVE ounces of the undried aſhes being lixiviated, left a reſiduum, which when dried weighed 4214 grains; the ſolution was reddiſh, replete with extractive matter; it afforded a large quantity of digeſtive ſalt, and ſome tartar vitriolate, and very little alkali.

TWO ounces of the ſame aſhes being gently heated to a ſlight degree of redneſs loſt 186 grains of their weight. One ounce of this calcined aſh being boiled in ſix ounces of water left a reſiduum of 344 grains, and conſequently contained 136 grains of ſaline matter; but of this ſaline matter only 22,4 grains were pure alkali, for the ſolution precipitated only 15,5 grains of earth of of allum; an hepatic ſmell was perceived during the precipitation of the allum, and the earth was of a dirty colour.

I TRIED alſo another ſort of aſhes which I had from a chandler; it was of a whiter colour and cleaner. The ſolution of an ounce of it in ſix ounces of water precipitated only 5,5 grains of earth of allum, and therefore contained but 8 grains of mere alkali.

THERE is a remarkable circumſtance attending theſe aſhes, namely, that if they be much calcined they ſeem to loſe their alkaline properties, and the ſolution no longer precipitates that of ſublimate corroſive reddiſh, as alkalies not thoroughly aerated do. What this circumſtance depends on I have not as yet examined, but am almoſt certain it proceeds from the preſence of common ſalt, as fixed alkalies and common ſalt melt very eaſily, and thus unite to the earths.

To eſtimate the goodneſs of different aſhes, ſome have recommended the uſe of an hydrometer, whereby to diſcover the ſtrength of ſolutions of equal weights of theſe aſhes in equal quantities of water; but as this inſtrument is equally affected by the preſence of neutral ſalts, as of alkali, it becomes uſeleſs.

Table

Table of the Quantity of mere Alkali in one hundred Avoirdupois Pounds of the following Substances by the aluminous Test:

One hundred pounds,	Mineral Alkali.
Cryſtallized ſoda - - -	20 lbs.
Sweet Barilha - - - -	24
Mealy's Cunnamara kelp - -	3,437
Ditto deſulphurated by fixed air - -	4,457
Strangford kelp - - -	1,25

One hundred pounds,	Vegetable Alkali.
Dantzic pearl aſh - - -	63,33 lbs.
Clarke's refined aſh - - -	26,875
Caſhup - - - -	19,376
Common raw Iriſh weed aſh - -	1,666
Ditto ſlightly calcined - - -	4,666

SECTION

SECTION VI.

Of the beſt Manner of procuring Alkaline Salts.

1ſt, OF THE METHOD OF PROCURING MINERAL ALKALI.

MINERAL ALKALI may be procured more or leſs pure from the combuſtion of the various ſpecies of kali or ſalſola of Linneus, or from that of the different ſpecies of ſalicornia and chænopodia, mentioned by the ſame author. The compounds thus formed are called *barilhas* or *ſoudes*.

THE cultivation of the ſalicornia may be ſeen in the fifth volume of the *Memoires des Scavants Etrangers*. A French acre (1,261 Engliſh) produces one tun of this weed; and this tun when burned produces but 100 weight of barilha, and this of a kind inferior to ſweet barilha. A ſmall quantity of this alkali is alſo contained in kelp. I am inclined to think that much of the alkali is loſt by its union with the earthy parts during the fuſion effected in the common manner of fabricating this ſubſtance; and therefore the proceſs ſuggeſted by Mr. CADET may be uſeful. He adviſes a trench two feet deep, ſeven feet long, and eighteen inches broad, to be made, lined with clay mixed with ſand, and over this iron bars two inches diſtant from each other to be laid; upon which a wall 2,5 feet high is to be conſtructed, of limeſtone if poſſible; over the bars the dry ſea weed is to be laid and ſet fire to; the aſhes will fall into the trench,

trench, and when it is full the fire is difcontinued *. I believe alfo that wafhing the fea weed in frefh water, to carry off the fea falt that adheres to it, would be ufeful.

To defulphurate kelp, Abbé MAZEAS recommends projecting on it while in a red heat $\frac{1}{3}$ of its weight of nitre; but this procefs feems too expenfive to be practifed in the great.

2dly, This alkali is found native in Ægypt and feveral parts of the Ruffian empire, and perhaps may be cheaply imported.

3dly, Common falt may be decompofed after Mr. TURNER's method, by trituration with litharge, as the calx of lead here employed is afterwards converted into a yellow pigment. This method is very beneficial.

I HAVE alfo contrived another procefs for decompofing common falt. The particulars of my experiment were as follows:

1ft, I rendered the common falt pure by adding to its folution a folution of mineral alkali until all the earthy matter was depofited.

2dly, To a folution of three ounces of this purified falt in nine ounces of water I gradually added a faturate folution of 4,75 ounces of fugar of lead, both hot, until the folution of lead fcarce excited any whitenefs in that of the common falt. After one

* Mem. Paris, 1767.

one night's reſt part of the ſugar of lead cryſtallized in the bottom of the veſſel, by which it is plain that too much of it had been uſed. Theſe cryſtals weighed 240 grains; the ſupernatent liquor I again evaporated to nearly $\frac{2}{3}$, and after two days obtained large pellicles of acetous ſoda, which I ſeparated; they weighed 325 grains; to the reſiduum, which ſtill had a ſweetiſh taſte, I added a ſolution of mineral alkali, until no further precipitation appeared; a very ſmall quantity of the alkali was ſufficient for this purpoſe. I then evaporated the remainder nearly to dryneſs; and afterwards heated it in a crucible to redneſs: In this heat it inflamed, and when calcined nearly to whiteneſs, I took it out and diſſolved it in twelve ounces of water, filtered it, and on adding an hot ſolution of allum obtained a precipitate, which when dried weighed 169 grains, and indicated the quantity of pure alkali to be 112 grains nearly. In this proceſs nothing is loſt, for the lead may be either revived or turned into a pigment.

Laſtly, Glauber's ſalt may afford the mineral alkali, but moſt eaſily in the form of liver of ſulphur: I endeavoured to decompoſe it by the above proceſs, but the quantity of alkali obtained from a large quantity of it was very inconſiderable.

Of the Vegetable Alkali.

It is univerſally known that this alkali may be extracted in greater or leſſer quantity, by lixiviation, from the aſhes of almoſt all vegetables, and it is *now* well eſtabliſhed that it preexifts in vegetables before combuftion; not indeed in a feparate uncombined ftate, but united partly with the vitriolic or marine acids, and fometimes the nitrous, but generally and for the greater part with a vegetable acid or oil, with which it forms effential falts, as they are termed. Thefe vegetable acids and oils are decompofed during combuftion, and thus the alkaline part is fet free; but the vitriolic by contact with inflamed matter is converted into fulphur, part of which unites to the free alkali, which protects it from combuftion, and thus forms what is called *liver of fulphur*, a product found in moft afhes, efpecially when the air has not had free accefs to them during combuftion.

As alkaline falts are of great importance in feveral arts, the proportion of afhes afforded by different vegetables, and that of alkali by the afhes of each fort of vegetable, has of late been accurately attended to, I fhall here prefent the beft authenticated refults of the experiments made with this view.

One thoufand weight of the following vegetables, perfectly dry and burned in a clean chimney and open fire, afforded the quantity of afhes and faline matter, exhibited in the annexed table:

One thousand pounds.	Pounds of Ashes.	Pounds of Salt.	
Stalks of Turkey wheat or mais	88,6	17,5	
Of Sun-flower	57,2	20	
Vine branches	34	5,5	
Box	29	2,26	
Sally	28	2,85	
Elm	23,5	3,9	
Oak	13,5	1,5	
Aspin	12,2	0,74	
Beech	5,8	1,27	
Fir	3,4	0,45	
Fern in August	36,46	4,25	Home.
Wormwood	97,44	73	Wiegleb.
Fumitary	219	79	Id.

Table of the saline Product of one thousand Pounds of Ashes of the following Vegetables.

	Saline products.
Stalks of Turkey wheat or mais -	198 lbs.
Ditto of Turnsel or Sun-flower -	349
Vine branches - -	162,6
Elm - - -	166
Box - - -	78
Sally - - -	102
Oak - - -	111
Aspin - -	61
Beech - - -	219
Fir - - -	132
Fern cut in August -	116, or 125 { according to Wildenheim.
Wormwood - -	748
Fumitary - -	360
Heath - - -	115 Wildenheim.

HENCE

HENCE we ſee that in general weeds yield much more aſhes, and their aſhes much more ſalt, than woods; and that conſequently as to ſalts of the vegetable alkali kind, as pot aſh, pearl aſh, caſhup, &c. neither America, Trieſte, or the Northern countries, poſſeſs any advantage over us.

2dly, That of all weeds fumitary produces moſt ſalt, and next to it wormwood; but if we attend only to the quantity of ſalt in a given weight of aſhes, the aſhes of wormwood contain moſt. *Trifolium fibrinum* alſo produces more aſhes and ſalt than fern.

MOST of the experiments on woods were made in France by order of government, under the inſpection of the overſeers of the ſalt-petre works; yet are to be read with caution by thoſe that attend to the quantity of alkali with reſpect to bleachers; for as tartar vitriolate (a ſalt uſeleſs to bleachers) is as ſerviceable to the makers of ſalt-petre as alkaline ſalts, they have conſtantly confounded one with the other; but the experiments made on weeds were inſtituted by perſons who carefully diſcriminated theſe ſalts: 100 grains of the ſalt of wormwood contain but ſix of tartar vitriolate, and 100 grains of the ſalt of fumitary contain 15. All alkaline ſalts, unleſs mixed with lime, contain alſo $\frac{1}{5}$ at leaſt of fixed air, which produces no other effect in bleaching than that of reſtraining their activity.

SECTION

SECTION VII.

Of the Proceſs for obtaining pot and pearl Aſh.

1ſt, The weeds ſhould be cut juſt before they ſeed, then ſpread, well dried, and gathered clean.

2dly, They ſhould be burned within doors on a grate, and the aſhes laid in a cheſt as faſt as they are produced. If any charcoal be viſible it ſhould be picked out and thrown back into the fire. If the weeds are moiſt much coal will be found. A cloſe ſmothered fire, which has been recommended by ſome, is very prejudicial.

3dly, They ſhould be lixiviated with twelve times their weight of boiling water. A drop of the ſolution of ſublimate corroſive will immediately diſcover when the water ceaſes to take up any more alkali. The earthy matter that remains is ſaid to be a good manure for clayey grounds.

4thly, The ley thus formed ſhould be evaporated to dryneſs in iron pans; two or three at leaſt of theſe ſhould be uſed, and the ley as faſt as it is concentrated paſſed from one to the other; thus much time is ſaved, as weak leys evaporate more quickly than the ſtronger. The ſalt thus procured is of a dark colour, and contains much extractive matter, and being formed in iron pots is called *pot aſh.*

5thly,

5thly, This ſalt ſhould then be carried to a reverberatory furnace, in which the extractive matter is burned off and much of the water diſſipated; hence it generally loſes from 10 to 15 per cent. of its weight. Particular care ſhould be taken that it ſhould not melt, as the extractive matter would not be thoroughly conſumed, and the alkali would form ſuch a union with the earthy parts as could not eaſily be diſſolved. I added this caution, as Doctor Lewis and Mr. Dossie have inadvertently directed the contrary. This ſalt thus refined is called pearl aſh, and muſt be the ſame as Dantzic pearl aſh *.

For the moſt œconomical conſtruction of a laboratory and furnaces for the above operations I refer to the deſcription given in a French tract, called *l'art de fabriquer le ſalin & la potaſſe*; and ſhall only add, that if the ſalt were extracted by a fire ſupported by vegetables whoſe aſhes might afterwards be employed, no inconſiderable advantage would be gained. Pearl aſh is frequently tinged green or blue; this colour it acquires during fuſion, not from any union of the ſalt with phlogiſton, as was formerly ſuppoſed, but by reaſon of the mangameſe contained in the aſhes of almoſt all vegetables, as Mr. Scheele has ſhewn. When it is calcined without melting it is perfectly white as Dantzic pearl aſh.

* The French call the refined aſh *pot aſh*, and the unrefined *ſalin*.

SECTION VIII.

Of the Colouring Matter of Linen Yarn and its Solvents.

HAVING, through the obliging attention of Mr. ARBUTHNOT, procured a ſufficient quantity of alkaline ley ſaturated with this colouring matter, or, as the workmen call it, *killed*, and which they are in the habit of throwing away; I found it to be a turbid liquor, of a reddiſh brown colour, a peculiar taſte and ſtrong ſmell, affording no ſign either of acidity or alkaleſcence. On five quarts of this liquor I poured two ounces of weak marine acid; there was no efferveſcence, but a copious depoſition inſtantly took place of a greyiſh green colour, and the liquor freed from this depoſit was of the colour of red amber.

THE next day I drew off the liquor with a ſyphon, and poured two quarts of pure water on the depoſited matter, and having agitated the whole, ſuffered this matter again to ſubſide, drew off the water, and added two quarts more; this liquor gave manifeſt ſigns of acidity, and continued ſomewhat reddiſh. Preſuming that after the addition of ſo much water this acidity could not proceed from the ſmall quantity of marine acid I had uſed, more eſpecially as the liquor originally contained an alkali, in the ſaturation of which the greater part of the acid muſt have been employed, I began to ſuſpect that this ley contained an acid of its own, which was diſengaged and ſeparated from the alkali by the marine acid as the more powerful of the two; and hence I reſerved

reſerved the two quarts of liquor, laſt added, for ſubſequent experiments.

After repeated affuſions of cold water, when the characters of acidity were ſcarcely any longer perceptible, I threw the depoſited matter on a filter and ſuffered it to dry for ſome time; it was then of a dark greeniſh colour, ſomewhat clammy like moiſt clay. I took a ſmall portion of it and added to it ſixty times its weight of boiling water, but not a particle of it was diſſolved. The remainder I dried in a ſand heat; it then aſſumed a ſhining black colour, became more brittle, but internally remained of a greeniſh yellow, and weighed 1½ ounce.

By treating eight quarts more of the ſaturated ley in the ſame manner, I obtained a further quantity of the greeniſh depoſit, on which I made the following experiments:

1ſt, Having digeſted a portion of it in rectified ſpirit of wine, it communicated to it a reddiſh hue, and was in great meaſure diſſolved; but by the affuſion of diſtilled water the ſolution became milky, and a white depoſit was gradually formed; the black matter diſſolved in the ſame manner.

2dly, Neither the green nor the black matter was ſoluble in oil of turpentine or linſeed oil by a long continued digeſtion.

3dly, The black matter being placed on a red hot iron, burned with a yellow flame and a black ſmoke, leaving a coaly reſiduum.

4thly, The green matter being put into the vitriolic marine and nitrous acids communicated a browniſh tinge to the two former, and a greeniſh to the latter, but did not ſeem in the leaſt diminiſhed.

Hence it appears that the matter extracted by alkalies from linen yarn is a peculiar ſort of *reſin*, different from pure reſins only by its inſolubility in eſſential oils, and in this reſpect reſembling lacks. I now proceeded to examine the power of the different alkalies on this ſubſtance: 8 grains of it being digeſted in a ſolution of cryſtallized mineral alkali ſaturated in the temperature of 60°, inſtantly communicated to the ſolution a dark brown colour; two meaſures (each of which would contain eleven penny weights of water) did not intirely diſſolve this ſubſtance. Two meaſures of the mild vegetable alkali diſſolved the whole.

One meaſure of cauſtic mineral alkali, whoſe ſpecific gravity was 1,053, diſſolved nearly the whole, leaving only a white reſiduum.

One meaſure of cauſtic vegetable alkali, whoſe ſpecific gravity was 1,039, diſſolved the whole.

One meaſure of liver of ſulphur, whoſe ſpecific gravity was 1,170, diſſolved the whole.

One meaſure of cauſtic volatile alkali diſſolved alſo a portion of this matter.

Though

Though thefe experiments were fully fufficient to refolve my own doubts, yet to render ftill more fatisfaction to bleachers, I repeated them with the falts they generally ufe, and alfo with foap.

I therefore diffolved one ounce of fweet barilha, Dantzic pearl afh, Cunnamara kelp, caffup and Clarke's pearl afh, each in fix ounces of pure water, and putting in one ounce meafure of each folution, 8 grains of the green colouring matter, fet them to digeft in a heat of about 180° for $3\frac{1}{2}$ hours. At the end of this time I found that

The Dantzic diffolved more than the barilha.

The kelp as much as the Dantzic.

The cafhup and Clarke's diffolved the whole.

Hence I added half an ounce more of the folutions of Dantzic, barilha and kelp; the Dantzic and kelp then diffolved the whole, but of the folution of barilha two ounces were requifite to perform this effect.

I also diffolved half an ounce of Windfor foap in eighteen ounces of water; the folution was turbid, and could not be rendered tranfparent but when it was near boiling, and then it was very unmanageable, for when boiled it fpouted three feet high out of the bottle. Three ounces of this folution were requifite to diffolve 8 grains of the colouring matter.

Now to compare the powers of these different solvents we must remark, that as an ounce of barilha contains 115 grains of mere alkali, the solution of it being made in six ounces of water, each ounce of the solution must contain the sixth part of 115, that is 19 grains; and in the same manner it will be found that an ounce of the solution of Dantzic salt contains 50 grains of mere alkali, that of Cunnamara kelp 2,8 grains, cashup 15, CLARKE'S 21.

THEREFORE 4,2 grains of saline substance of kelp performed the same effect,

As 75 of that of Dantzic,

38 of that of barilha,

15 of that of cashup,

21 of that of CLARKE,

213 of soap.

I ALSO tried the power of lime water, but found that three ounces of the strongest dissolved very little of the colouring matter as should be naturally expected, for the three ounces did not contain above 3 grains of lime, nor did the mixture of sulphur render it more active.

FROM the foregoing experiments we may now deduce the following practical propositions:

1st, Liver

1ſt, Liver of ſulphur is of all alkaline compounds the ſtrongeſt ſolvent of the colouring matter; next to this the cauſtic vegetable, and after this the cauſtic mineral alkali; the mild vegetable and the mild mineral alkali occupy the laſt place. Sulphur, it is ſaid, leaves a ſtain in linen; but if liver of ſulphur be uſed in the beginning, that is to ſay in bleaching the yarn, the ſtain will probably be removed by the purer alkalies afterwards uſed. Hence the ſolutions of kelp, caſhup and markoft are advantageouſly uſed in the firſt proceſſes of bleaching, for which Dantzic and ſweet barilha are leſs fit; but ſix tun of kelp will be neceſſary to produce the ſame effect as one tun of caſhup; yet as the former is manufactured at home it deſerves the preference.

2dly, As the alkali manufactured from inland weeds is more powerful than the mineral, Mr. Clarke's is more powerful, or may be rendered ſo, than any imported. It is already ſufficiently cauſtic, and may be converted into liver of ſulphur only by adding $\frac{1}{20}$ of its weight of ſulphur to it when boiling, and thus it is fitted for the firſt proceſſes of bleaching. In its primitive ſtate it is fit for the ſecond proceſs, and by rendering it milder, which may be effected by burning half a buſhel of charcoal in a pan in the ſame room in which its ſolution ſtands, it will be adapted to the laſt proceſſes, in which a leſs active alkali is required.

3dly, Clarke's ſalt converted into liver of ſulphur is preferable to kelp, becauſe this latter, by the preſent manner of manufacturing it, holds charcoal in ſolution; this coaly matter it depoſits on the

the yarn, and thus leaves a black tinge; whereas Mr. CLARKE's is free from this contamination, to ſay nothing of the far greater quantity of alkali it contains, inſomuch that one tun of CLARKE's is nearly equal to eight tun of kelp. Hence it clearly follows that the linen manufacture ſtands in no ſort of need of foreign ſalts or aſhes for the proceſſes of bleaching.

THE chief defect in Mr. CLARKE's manipulation is the loſs of time during what he calls the *maceration* of aſhes and quick lime; by barely moiſtening them the ſame effect may be produced in nine hours which he expects from their maceration during nine months, and much more lime is uſed than is neceſſary.

DANTZIC pearl aſh contains much more alkali than CLARKE's; this muſt proceed from the ſuperior quality of the aſhes from which it is extracted. Thoſe I received from Mr. CLARKE were exceeding bad; nor do I believe that any crude aſhes can be advantageouſly uſed in bleaching. But if ſome perſons in the different manufacturing counties would allot a few acres to the culture of wormwood and fumitary, I believe their own advantage, as well as that of the public, would thereby be conſiderably promoted. An acre will, I ſuppoſe, ſcarcely produce leſs than four tun of the dry weeds, and each tun will afford nearly 200 weight of aſhes, and each tun of wormwood aſhes will give nearly 1500 weight of unrefined ſalt, or 1300 of the refined.

THE alkali, manufactured after the manner I have indicated in the ſeventh ſection, may not be ſufficiently cauſtic for the

earlier

earlier operations of bleaching, but by the addition of half a pound of quick lime to every hundred of the ſalt, or of ten pounds for every tun, it will be rendered ſufficiently ſharp. There is no danger that any of the lime will remain in the ley; but if any ſhould it will immediately be diſcovered, and depoſited by the addition of a little of the unmixed ley.

LETTER *from* RICHARD KIRWAN, *Efq*; *F.R.S.* *and* *M. R. I. A.* *to the Right Honourable the Earl of* CHARLEMONT, *P.R.I.A.*

My Lord,

I BEG leave to lay before your Lordfhip a copy of a letter which I juft received from Mr. Mills, an eminent miner in Chefhire: it tends to reftrict the generality of an obfervation I had made on the coal-mines in the neighbourhood of New-caftle and Whitehaven, and had unwarily extended to all the coal-mines in England. As the Academy has already done me the honour to publifh my paper on coal-mines, I feel myfelf obliged to fubmit to its confideration, whether it may not be proper to publifh this rectification of the miftake I had fallen into, and the curious and intelligent remarks that accompany it; for this purpofe I commit it to your Lordfhip,

Read Dec. 5, 1789.

And am, my Lord,

Your moft obedient and humble fervant,

RICHARD KIRWAN.

H

S I R,

I HOPE you will excufe the liberty I take in pointing out an error which occurs in your remarks on coal-mines, in the fecond volume of *The Tranfactions of the Royal Irifh Academy.* At page 161, it is faid, " In England beds of coal of lefs than " two feet and an half in thicknefs are judged not worth " working;" but I affure you that in this neighbourhood we work much thinner feams and at confiderable depths, as you may perceive by the fubjoined account of the ftrata at Blakelow colliery, fituated about one mile fouth eaft from the town of Macclesfield.

	Feet.	Inches.	
No. 1.	15	0	Clay and gravel.
2.	102	0	Black argillaceous fhale.
3.	—	1	Smut feam of coal.
4.	25	6	Rock. A pale grey fine-grained filiceous grit-ftone, containing much particles of mica.
5.	2	3	Grey beds. A foft, lanunated, pale grey grit-ftone, containing mica between the lamina.
6.	—	4	Shale.
7.	1	6	Grey beds.
8.	1	6	Shale.
9.	—	9	Grey beds.

No.

	Feet.	Inches.	
No. 10.	2	3	Shale.
11.	1	2	Middle ſeam of a good quality.
12.	—	9	Cap. Grey clay.
13.	36	0	Rock. A compact fine-grained, brown, ſiliceous grit-ſtone, containing ſmall particles of mica.
14.	3	0	Grey beds.
15.	—	4	Shale.
16.	2	3	Grey beds.
17.	1	0	Shale.
18.	1	6	Grey beds.
19.	2	3	Shale.
20.	—	4	Nodules of iron ſtone or ſhale.
21.	2	3	Shale.
22.	1	4	Gank, or principal ſeam of coal of a good quality. *Note*, Higher on the riſe this ſeam has been proved 22 inches thick.
23.	1	9	Cap. Grey clay.
24.	30	0	A medley of grey beds and ſhale, blended together without any regularity.
25.	1	4	Harper ſeam. A very ordinary coal, full of pyrites, and not worked. *Note*, The beds dip N. E. and riſe S.W. one yard in three yards and an half.

THE

The middle and gank ſeams alone are worked. The ſmut is too ſmall and the harper too bad to be raiſed.

The method uſed for getting the coal is, firſt, to drive a level or ſough from ſome low ground to cut the coal as deep as poſſible; by this ſough the water drains off, and the ſeam being cut, the level is continued on the coal until interrupted by a fault, (which is a ſlide or fiſſure in the ſtrata, uſually filled with clay, ſtone, coal and ſhale, and abounding with water) or until it is deemed neceſſary to ſink a pit from the ſurface to the level; which being done, coal is immediately raiſed by driving on the riſe as far as neceſſary; after which each man turns out, taking his drift or hole before him upon the level of the coal, and parallel to the firſt level. Commencing at the upper part of his hole, he cuts out the coal about four inches in thickneſs from the floor as far as his pick will reach, then with wedges breaks it down from the roof, and continues his work in the ſame manner to as great a width as the nature of the roof will admit. In the middle ſeam above mentioned the coal is cut out three yards in width, and pillars are left three quarters of a yard wide to ſupport the roof: in the gank ſeam the coal is cut out five yards in width, and pillars of half a yard wide are left ſtanding; but theſe pillars are in both ſeams occaſionally broke through from hole to hole to promote a circulation of air, and for the more conveniently drawing away the coal.

To

To perform this kind of work the collier is neceſſarily obliged to lay on his ſide; on his knee he wears a piece of leather called a cap, on his thigh a piece called a pilch, and on his arm another piece called an elbow patch; and uſually works without any other clothing than a pair of flannel drawers; and after having cut his coal is obliged to draw it in a baſket on a little kind of ſledge, going on his ſide feet foremoſt, dragging the coal after him to the foot of the ſhaft (or pit) at which it is drawn to the ſurface by an horſe whimſey. The drawing the coal to the ſurface is contracted for at eight pence per quarter, and the colliers get the coal from the middle and gank ſeams for five ſhillings and eleven pence per quarter, containing twenty-eight hoops, each hoop equal to one Wincheſter buſhel, (the twenty-eight hoops weigh upon an average about 19 C. 2 qrs.) and in either ſeam the colliers can earn from two ſhillings to half a crown, in working only ſix hours.

In places where there is a great conſumption of coal and a proportionable price, ſeams of the thickneſs I have mentioned may be worked to advantage, provided the roof above the coal is good, that is, if it will ſtand for a tolerable width without the ſupport of timber; but where much timber is required under ground, or where from the local ſituation of the colliery expenſive machines are neceſſary to draw off the water, but little hopes of profit can be entertained from ſuch very thin ſeams.

To

To elucidate what I have wrote, I inclose a plan and section of the workings in the bottom of a coal mine, and a section of the strata at Blakelow. I shall be happy if this attempt to promote the cause of science meets your approbation,

And am, Sir,

Your most obedient, humble servant,

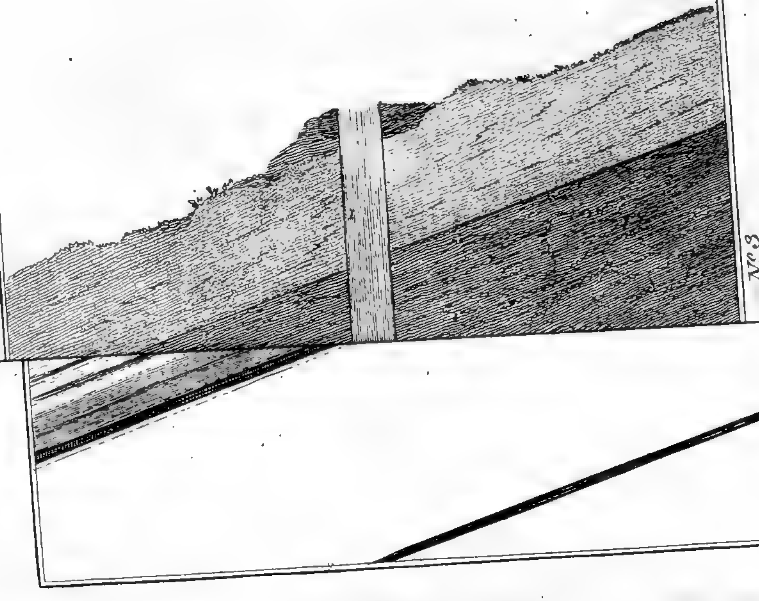
ABRAHAM MILLS.

Fence-house, near Macclesfield, Cheshire,
Oct. 30, 1789.

RICHARD KIRWAN, Esq;

Strata at Blakelow

No. 1



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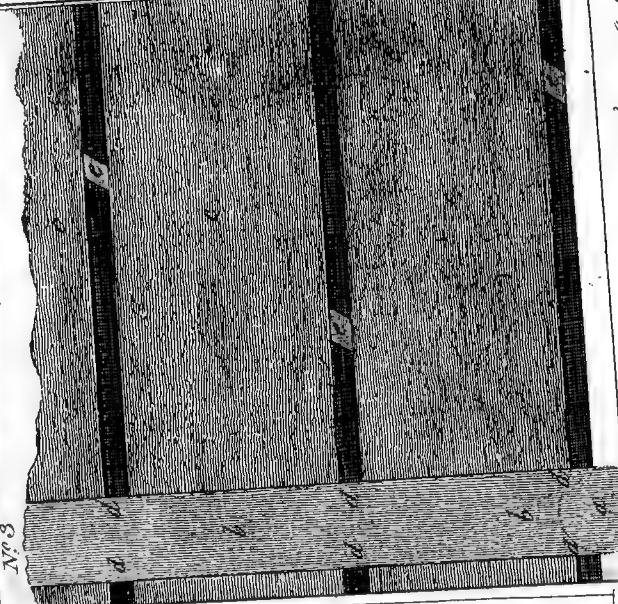
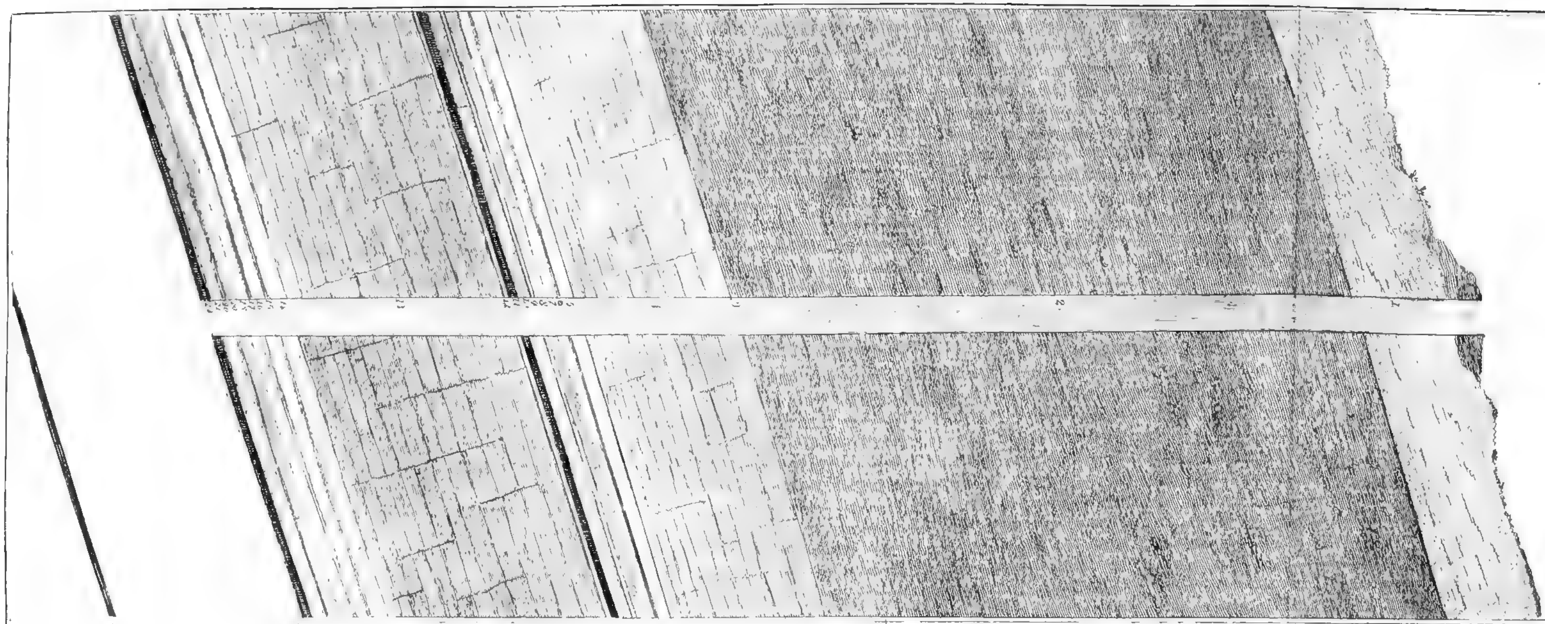


Level or Sough

Section of the G[...] ifts on the rise of the Coal; c,c,c,c,c,c, Holes, where the Coal
[...] e,e,e,e,e, Holes for air, and for Drawing the Coal to the Shaft, a.

Strata at Blakelow

No 1



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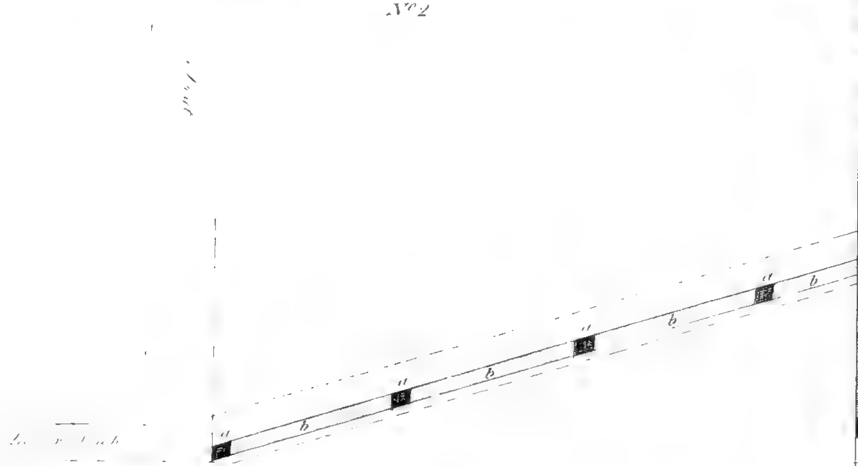
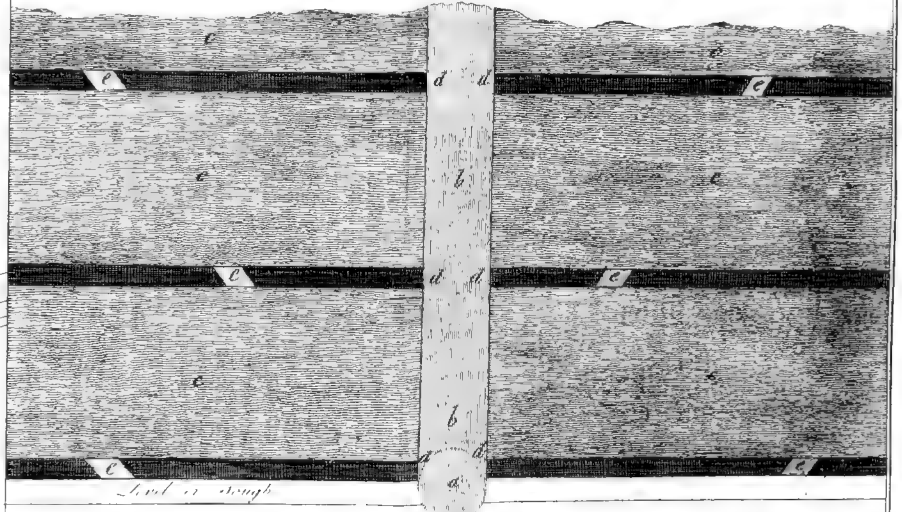


...the Gank Seam ... a a Pillars to support the roof b b b b Coal cut out

No 3



Plan of the Gank Seam, a, Shaft, b, b, Drifts on the ... of the Coal, c c c c c c, Holes where the Coal has been cut out, d d d d d d, Pillars of Coal, e e e e e e, Holes for air, and for Drawing the Coal to the Shafts

The ORIGIN *and* THEORY *of the* GOTHIC ARCH.

By the Reverend M. YOUNG, *D. D. F. T. C. D.* and *M. R. I. A.*

1. NOTWITHSTANDING the ſurpriſing things that have been effected by the architects of the middle ages, in raiſing ſuch ſtupendous piles on ſo ſlight an apparent ſupport as the pointed arch and ſlender Gothic pillar; it ſeems that their ſucceſſors have not, with all the care which the ſubject deſerves, conſidered how far any part of theſe great effects was to be attributed to the ſtructure of the arch. Whether it has been, that the ſuperior excellence of the Grecian architecture has afforded their genius ſo thorough employment, that they have not had leiſure to examine, with ſufficient accuracy, the works of our anceſtors of the Gothic age; or that their contempt of any thing that could originate amongſt Barbarians, as perhaps they concluded of this arch, has produced a neglect of that to which they might otherwiſe have attended with advantage; certain it is, that few, whether architects or mathematicians, have paid much regard to this

Read Nov. 7, 1789.

this ſpecies of architecture. And thoſe who have incidentally made mention of it, have in general directed their enquiries to its origin rather than its principles.

2. The Saxon, Norman and Gothic ſtyles of architecture, though nearly related to each other, yet have their peculiar characteriſtics and diſtinguiſhing features. The Saxon and Norman architecture agree in this, that the form of the building is the ſame, the pillars round, ſquare or polygonal, and very ſtrong and maſſive, and the arches and heads of the doors and windows ſemicircular. Mr. Bentham thinks that the criterion of Norman architecture, by which it is diſtinguiſhed from the Saxon, is its ſuperior maſſiveneſs and enlarged dimenſions. The Saxon churches, he obſerves, were often elegant fabrics and well conſtructed, but generally of a moderate ſize, frequently completed in the ſpace of five or ſix years, or leſs time. The works of the Normans were large, ſumptuous and magnificent, of great length and breadth, and carried up to a proportionable height, with two and ſometimes three ranges of pillars, one over another, of different dimenſions, connected together by various arches, all ſemicircular, forming thereby a lower and upper portico, and over them a gallery. In the centre was a lofty tower, and ſometimes one or two added at the weſt end, as in the venerable cathedral at Hereford, which not long ſince has ſunk under the weight of years. But I am inclined to think there is a much more ſtriking difference between theſe two orders: I mean in the pillars. The Saxon columns, we know, were round, ſquare or polygonal, and very maſſive; but if we look into Doctor Ducarel's Norman antiquities, we ſhall find the Norman pillars, without exception, to be ſlender

and

and cluftering. In England, therefore, the buildings erected in the time of the Norman princes were in a ftyle compounded of both; to the fimple and primitive Saxon adding the rich decorations and enlarged dimenfions of the Norman ftructure. * The body or trunk of the pillars were ufually plain cylinders, or fet off only with fmall half columns united to them; but, to adorn them, they fometimes ufed the fpiral grove winding round them, and the net or lozenge work overfpreading them. The capitals were, in general, left plain without any manner of fculpture, though inftances occur, in fome cafes, of foliage and animals on them. As to the arches, though for the moft part plain and fimple, yet fome of their principal ones, as thofe over the chief entrance at the weft end, and others more expofed to view, were abundantly charged with peculiar fculpture, as the chevron work or zig-zag moulding; the embattled frette, the triangular frette, the nail-head, the billeted moulding, and the hatchet moulding. To adorn the infide walls below, they had rows of little pillars and arches; and applied them alfo to decorate large vacant fpaces in the walls without; they ufed alfo the corbel table; and the nebule, which was a projection terminating in an undulating line. To thefe marks of the Saxon and Norman ftyle, we may add, that they had no tabernacles or niches with canopies, or pinnacles, or fpires, or any ftatues to adorn the outfide of their buildings, though fometimes they had miniature relievo figures over the doors. † The marks which conftitute the character of what is called the Gothic architecture, are its projecting buttreffes, its pinnacles and fpires, large ramified windows, niches, canopies

 and

* Bentham's antiquities of Ely cathedral.

† Rious's architecture.

and ſculptured ſaints, the fretted roof, the cluſtering pillar, but above all, the pointed arch. And as plainneſs and ſolidity conſtitute the leading features in the Saxon and Norman buildings, ſo, on the other hand, the Gothic architecture is diſtinguiſhed by the lightneſs of its work, the boldneſs of its elevations, and the profuſion of its ornaments. So that the Saxon bears ſome ſimilitude as it were to the Tuſcan order; the Norman to the Doric and Ionic; and the Gothic to the Corinthian and Compoſite.

3. In our enquiries into the origin of the Gothic ſtyle, we meet with not leſs genius and fancy than has been diſcovered by the writers on the origin of the Grecian orders, but a much greater diverſity of ſentiment; there being not leſs than five different opinions held with regard to the riſe of this ſpecies of architecture: either that it was introduced from the eaſt by the Cruſaders, and ſhould therefore be called Saracenic; or borrowed from the Moors in Spain, and ſhould therefore be ſtyled Mooreſque; or derived from the ancient cuſtom of worſhipping in groves, where the eye being long accuſtomed to contemplate the arches formed by the branches of the trees that ſhaded their altars, it was natural, when covered buildings ſucceded to theſe groves of worſhip, that men ſhould endeavour to introduce ſome ſimilitude between them and thoſe places in which they had been accuſtomed ſo long to perform their religious ceremonies; and that accordingly we find not only the arches formed by the branches exactly imitated by the pointed arch, but the ſtems of the trees as accurately repreſented by the ſlender and cluſtering pillars. The elegance, ingenuity, and plauſibility of this opinion have

have not failed to procure it the moſt general approbation. By others however it is maintained, that this arch originated from the interſection of circular arches in Saxon architecture; and others, laſtly, from the like interſection of circular arches in Grecian architecture.

4. WITH regard to the firſt opinion, which has been countenanced, or rather firſt advanced by Sir Chriſtopher Wren, ſome weighty objections obviouſly preſent themſelves. And firſt; inſtances may be produced of the exiſtence of the pointed arch in Europe antecedent to the Cruſades. * Dr. Stukely is of opinion, that the antient ſanctuary at Weſtminſter, in which the arches were pointed, was built by Edward the Confeſſor: "Nevertheleſs," ſays he, "I ſhall not be averſe to think it much older." † The church of Kirkdale has alſo the pointed arch, and is of the age of the Confeſſor. Croyland bridge in Lincolnſhire, which conſiſts of three Gothic arches, and is ſuppoſed to have been built in honour of the Trinity, is of an earlier date than the Cruſades, the time of its building being determined to be the year 860. Beſides, it does not appear, that what we call the Gothic is the prevailing ſtyle of architecture in the Eaſt; on the contrary, the ſtyle which predominates in that country is a corruption of the Grecian. This is a point ſo univerſally admitted, that Dr. Warburton maintains, that the Saxon architecture is an imitation of the

 Eaſtern.

* Archæol. vol. i.

† See Ledwich on Churches, Archæol. vol. viii.

Eaſtern. "When the Saxon kings," ſays he, "became Chriſtian, "their piety (which was the piety of the times) conſiſted in "building churches at home, and performing pilgrimages to the "Holy-land; and theſe ſpiritual exerciſes aſſiſted and ſupported "one another: For the moſt venerable, as the moſt elegant mo-"dels of religious edifices were then in Paleſtine. From theſe "our Saxon builders took the whole of their ideas, as may be "ſeen by comparing the drawings which travellers have given "us of the churches yet ſtanding in that country with the "Saxon remains of what we find at home. Now the architec-"ture of the Holy-land was entirely Grecian, but greatly fallen "from its antient elegance." See Pope's Eſſays, vol. 3, Ep. 4. And Mr. Ledwich, in his learned eſſay on the architecture of our Engliſh churches, advances very ingenious arguments to ſhew that the Saxon ſtyle had an Eaſtern origin, and produces a ſtriking inſtance from a Syriac MS. of the Evangeliſts, written A. D. 586, preſerved in the Medico-Laurentian library at Florence, which contains drawings of arches ornamented with every characteriſtic of the Saxon ſtyle. * Cornelius Le Brun has publiſhed many views of Eaſtern buildings, particularly of thoſe in the Holy-land; and in all theſe only one Gothic ruin, the church near Acre, and a few pointed arches occur, and theſe were built by the Chriſtians when in poſſeſſion of the country. In Sales Koran, the temple of Mecca is repreſented as built with ſemicircular not pointed arches. Neither did Pocock, Norden, or Shaw diſcover any traces of this ſtyle, as far as we can argue from

* Groſe's Antiquities, Pref.

from the drafts which they have given us in their travels. It appears therefore, that there is little or no reafon for fuppofing our Gothic buildings imitations of the Eaftern; for although the pointed arch may in fome few places occur, yet it by no means is the characteriftic of the Eaftern ftyle.

5. The fecond opinion, that the Gothic architecture was derived from the Moors, who introduced it with their victorious arms into Spain, from whence it was imported into the other countries of Europe, appears to be lefs tenable than the firft; the form and decorations of the Moorifh arch being very different from the Gothic, as may be feen in the drawings which are given us of Moorifh buildings, particularly of the celebrated palace of Alhambra. See Swinburne's travels in Spain. See alfo in Houel's Picturefque Voyage through the iflands of Sicily, Malta and Lipari, a drawing of the prince of Palagonia's palace at La Bagaria, which is a Moorifh building. Neither are there the leaft traces of Gothic architecture in the drawings of the Moorifh palaces in Les Delices d'Efpagne *. In all thefe, and alfo in the Moorifh caftle of Gibraltar, the arches are either femicircles, or fegments greater than femicircles, approaching to the horfe-fhoe form, which is the genuine Moorifh arch.

6. The third opinion is that of Dr. Stukely, "The original "of all arts," fays he, "is deduced from nature; and affuredly "the idea of this Arabian arch, (fo he calls the Gothic) and flender

* Grofe's Antiquities, Pref.

" ſlender pillar, is taken from the groves ſacred to religion, of " which the great patriarch Abraham was the inventor." This hypotheſis was adopted by Dr. Warburton, who has ſet it forth to full advantage in his notes on Pope's Eſſays. See Ep. 4. Againſt which however there lie ſome objections that do not admit of very obvious anſwers.

1ſt, WHEN worſhipping in buildings of wood and ſtone ſucceeded to the cuſtom of worſhipping in groves, if the faſhion of theſe buildings had been ſuggeſted by the images preſented in groves, as this opinion ſuppoſes, then the moſt ancient ſtyle of religious buildings would have been Gothic. But this is contradicted by fact; for in the Eaſt, where worſhipping in groves firſt prevailed, the architecture is not of this primitive ſpecies, but is a corruption of the Grecian, in which the circular form of the arch is retained. And it is a known truth, that the moſt ancient religious buildings, of which we have any account, were not in that ſtyle which is called Gothic. But if ſo, this hypotheſis muſt be abandoned; for it is unreaſonable to ſuppoſe, that the arches in the firſt religious buildings which immediately ſucceeded the period of worſhiping in groves, ſhould not bear any reſemblance to thoſe which are repreſented by the interlacing branches of trees, when the minds of the architects were ſo ſtrongly impreſſed with the images of thoſe " Leafy Cathedrals," and yet, that the form of the arches, which were introduced ages after that cuſtom, and all its concomitant ſcenery had ſunk into utter oblivion, ſhould be derived from them.

2dly,

2dly, THOUGH it ſhould be granted, that the origin of Gothic architecture was derived from the cuſtom of worſhiping in groves, yet there does not appear to be any reaſon why this ſtyle ſhould therefore be introduced into military architecture. Yet we find, in fact, that, in England at leaſt, it has been introduced into military perhaps as early as into religious buildings. Edgar's tower at Worceſter, which is in the Gothic ſtyle, was the portal into the Saxon citadel, and was erected in the reign of Ethelred, the ſecond ſon of Edgar, anno 1005, as appeared from an ancient inſcription on a ſtone in the building which was taken down a few years ago: the inſcription may be ſeen in Mr. Green's hiſtory of that city. Doctor Littleton, dean of Exeter, in a diſſertation read before the ſociety of Antiquarians in 1757, has indeed ventured to affirm, that this tower is not older than the time of King John, on the authority of Mr. Habington; who ſays, "that in "*all likelyhood*, the figure of the King with croſſed legs repreſents "King John, who, anno 1215, took on him the ſign of the croſs "for the holy voyage; or King Richard the 1ſt, whoſe lyon's "heart ſo conquered the infidels." But he takes no notice, as is obſerved in Groſe, of the female figures on each ſide of the King, nor of the date. And as both theſe kings had each but one wife, the dean believes the figure really repreſents King Edgar, and the two female figures Ethelfleda and Ethelfrida. But if this be admitted, it ſeems to follow, that it muſt be of the antiquity which the date implied; for it is highly improbable, that in the reign of one of the Norman princes, a tower ſhould be erected to the honour of a Saxon king, who had died two hundred and fifty years before. But whether this building be altogether of as ancient a

date

date as is aſſerted, or not, it is evident that the introduction of this arch into military buildings could not have ariſen from any deſire of imitating the original ſcenes of religious worſhip; it muſt therefore have taken its riſe either from a ſuppoſition of its ſuperior ſtrength, beauty, or other peculiar advantages: And this being once admitted, there is immediately aſſigned a more probable and ſufficient reaſon for its introduction into religious architecture alſo.

3dly, ONE of the principal arguments in favour of Doctor Stukely's opinion is, that the Gothic ſtyle comprehends not only the pointed arch but the ſlender and cluſtering pillar alſo; the one repreſenting the ſtems of the trees, the other the arches formed by the interſection of their boughs. "Could the arches be other-" "wiſe than pointed," ſays Doctor Warburton, "when the work-" "man was to imitate that curve which branches make by their" "interſection with one another? Or could the columns be other-" "wiſe than ſplit into diſtinct ſhafts, when they were to repreſent" "the ſtems of a groupe of trees?" But this argument overturns the hypotheſis in confirmation of which it is advanced; for on this ſuppoſition the ſlender and cluſtering pillar ſhould be of the ſame date with the pointed arch; and they ſhould have been inſeparable companions in art as they are in nature, of which it is pretended, that this ſtyle is a cloſe copy. Now we know, that the pointed arch when firſt introduced into England, was not ſupported on ſlender pillars, but on the maſſive columns of the Saxon order, which were in faſhion at that time; and afterwards on the ſlender fluted pillars of the Normans, which were at length ſplit into

into diſtinct ſhafts, at a more advanced period of the Gothic ſtyle. So that, in England at leaſt, the pointed arch and cluſtering pillar have had different originals and periods of introduction. This obſervation ſeems to me effectually to overturn the hypotheſis we are at preſent examining, one main part of the fancied ſimilitude between Gothic architecture, and the ancient ſcenes of religious worſhip, being thus proved to have ſprung from a different ſource.

4thly, If the pointed arch had been intended to reſemble arcades formed by the branches of trees, we muſt ſuppoſe, that the ornaments of that arch would have had a cloſe affinity to their original. But on the contrary we find, that leaves or foliage conſtitute but an occaſional and accidental, not an eſſential part of the Gothic ornaments.

7. The fourth opinion on this ſubject is, that the Gothic arch has probably taken its riſe from thoſe arcades we ſee in the early Saxon and Norman buildings, where the wide ſemicircular arches connecting the alternate pilaſters, croſs and interſect each other, and form at their interſection a narrow and ſharp pointed arch. But there does not appear ſufficient grounds for our attributing the origin of this arch in general to the interſection of Saxon circles; for if ſo, the Gothic architecture would have had a Saxon original: but we find it in countries where no other traces of the Saxon ſtyle occur. Thus the pointed arch diſcovers itſelf amongſt the Mooriſh arches in the famous palace of Alhambra; and not only the pointed arch but fluted pillar alſo is to be found

in the church of St. Mamas, at Morfou, in the iſland of Cyprus. See Drummond's travels. Georgio Vaſari, in his proem to the lives of the Painters, ſays, that in the time of Theodoric, (that is, about the year 500) the architecture in Italy was of that manner called Tedeſco or Teutonic *. And the baptiſtery at Piſa, built in the year 1063, was ſubſequent to the period of Gothic architecture in that country, in which, though built in the Grecian ſtyle, we yet diſcover evident traces of the Gothic, which was now giving way to a better faſhion: But, at that period, we are certain, that the architecture in England was not Gothic. And as we find the uſe of the pointed arch in countries ſo widely diſtant, we muſt look for ſome more general principle of its introduction than the interſection of Saxon arches.

8. The

* After ſpeaking of the five Grecian orders, he thus deſcribes the ſtyle which he calls Tedeſco: "There is another ſpecies of architecture called Tedeſco, which in its ornaments and proportions is very different from the antique and from the modern; nor is it uſed at preſent by good architects, but is avoided as barbarous and monſtrous; being entirely deficient in order, ſo that it may be rather called diſorder and confuſion. The doors are ornamented with ſlender pillars, and twiſted like vines; which cannot have ſtrength to ſuſtain a weight, however light it may be. And thus by all their mouldings and other ornaments, they made a jumble of tabernacles one upon another, with ſo many pyramids, and points, and leaves, that they are not only unable to ſuſtain a weight, but ſcarcely even to ſtand; and they have more the appearance of being made of cards, than of ſtones and marble. And in theſe works they made ſo many ſudden tranſitions, breaks, little brackets, and turns, that they marred the proportion of their works: and often went to ſuch a height, that the top of the door touched the roof. This manner was invented by the Goths, who, after they had deſtroyed the ancient fabrics, and the architects were ſlain in war, introduced the arches of the fourth point, and filled all Italy with this monſtrous ſtyle of building." *Vaſari's Lives of the Painters and Architects, vol.* i. *p.* 65.

8. The fifth opinion is that which has been given by Mr. Barry, in his very ingenious and elegant *Enquiry into the real and imaginary obſtructions to the arts in England,* where he endeavours to ſhew us the ſucceſſive corruptions of architecture, as they grew out of one another, by which it was led inſenſibly from the Grecian to the Gothic taſte. " There are at the dome of Viterbo," ſays he, " and at St. Mark's, Venice, ranges of columns, from " which regular arches, of half a circle, interſect one another, " by being made to ſpring from every third column, which gives " a Gothic arch between every two." And he farther adds, that before the great nich, which has been cut through the attic of the pantheon, there are two columns which are remarkable, as the flutes are more than a diameter of a circle deep. This outrayed a little, by ſinking the flutes deeper, and leſſening their number, he thinks, led to the Gothic bundle of columns.

That the Saxon, Gothic, Mooriſh and Eaſtern ſtyles of building are but various corruptions of the Grecian, ſeems evident: The ſimilar diviſion of the column into pedeſtal, ſhaft, and capital, the imperfect acanthus, and the faint traces of the Ionic volute which we ſometimes meet with, will not ſuffer us to doubt of this; but that the Gothic arch derived its origin from the identical circumſtance in Grecian architecture here pointed out, ſeems by no means ſufficiently evident. For St. Mark's, Venice, according to the teſtimony of Vaſari, was built towards the concluſion, not commencement of the period of corrupt architecture. So that if the pointed arch was indeed ſuggeſted in this manner, which however is extremely uncertain, it muſt rather have been derived from the church of St. Sophia at Conſtantinople, which was built in the 20th of Conſtantine, that is, in the year 344, and was the original from which the plan of St. Mark's, Venice, was taken. Although the Gothic architecture commenced ſo early amongſt the Goths and Longobards, and that the true taſte began to revive about the year 973, yet attachment to inveterate cuſtoms and the beauty of the ſtyle itſelf prevented it from falling into entire diſuſe for ſome centuries after; according to the obſervation of Piraneſi, in his work *Della magnificenza d'architettura de' Romani*, "la "Greca (architettura) poi non potè eſſer portata in Egitto, ſe "non ſotto il regno de' Tolommei: quantunque per altro, ſic- "come difficilmente ci diſtacchiamo dai coſtumi inveterati, é "coſa piu probabile, che a que' tempi ſi confondeſſe l'una coll' "altra, di quel che l'Egizia foſſe affatto poſta in diſuſo. In quella "guiſa appunto che noi vediamo la chieſa di S. Agoſtino, tanto "tempo dopo eſſere ſtati ſcacciati d'Italia i Goti e i Longobardi, "cioé l'anno 1483 di Criſto, eſſere ſtata fatta alla Greca, e in- "ſieme di quell forma ſtraniera, la quale era ſtata in uſo in Italia "per tanto tempo." We are further to obſerve, that when it is ſaid, that the deepening of the flutes in the Grecian columns led to the Gothic cluſtering pillar, it is not neceſſary to ſuppoſe, that the Gothic ſtyle, in this reſpect, was directly derived from the pure Grecian, without paſſing through any intermediate gradations. In England it is more than probable that the fluted column, and its offspring the cluſtering pillar, was of Norman deſcent, as may be conjectured from the drawings of Doctor Ducarel; which Norman ſtyle, we muſt allow, was but a corruption of the Grecian, thus at length propagated into England.

And

And that we, of theſe countries, are not in any wiſe indebted to the Norman builders for the pointed arch itſelf, cannot I think be very poſitively aſſerted, when we conſider that the windows of the palace of William the conqueror at Caen were all pointed. So that although the pointed arch might have exiſted in England prior to this period, yet it was not till after the Norman conqueſt that it became the prevailing faſhion. This circumſtance of the turn of the arches in the palace at Caen Dr. Ducarel ſeems to have forgot when he tells us, *page 102, that the pointed arches were not introduced till near the end of the twelfth century. Italy has long been the ſchool for the fine arts, and as the Gothic architecture, from the teſtimony of Vaſari, ſeems firſt to have originated there, it is probable, that architects bringing home with them from thence the idea of the pointed arch, it aſſumed in different countries the garb of that into which it had been admitted. Thus the Gothic buildings in Germany, England, Spain, though they agree in the form of the arches, differ widely in their decorations.

" There iſſued from the hands of the maſters of thoſe times," ſays Vaſari, " thoſe fantaſtical and rude things which appear, even " at this day, in what remains of their works. The ſame thing " happened in architecture, for it being neceſſary to build, and " the form and good manner being entirely loſt by the death of " the artiſts, and the deſtruction of their works, they who gave " themſelves up to ſuch ſtudies, did not build any thing which for " order

* See his Norman Antiquities.

" order or meafure had grace, either in the defign or in any pro-
" portions. Whence new architects arofe, who from their barba-
" rous notions produced that manner of building which by us is
" now called *Tedefco*, who did fome things which afforded rather
" a fubject of laughter to the moderns, than praife to them;
" until afterwards better architects found out a better form, and
" fomewhat like unto the good antique. In this ftyle are the
" oldeft churches in Italy, but not antique, which were built by
" them, as by Theodoric, King of Italy, a palace in Ravenna, one
" in Pavia, and another in Modena, but in the barbarous man-
" ner, and rather rich and large, than well defigned or of good
" architecture. The like may be faid of St. Stephen's in Rimini,
" of St. Martin's in Ravenna, and of the temple of St. John the
" Evangelift, built in the fame city about the year of our falva-
" tion 438; of St. Vitale, which was built in the year 547, and,
" in fine, of many other monafteries and temples built after the
" Longobards. All which buildings, as has been faid, are great and
" magnificent, but of moft incorrect architecture; and amongft
" thefe are many abbies in France, built to St. Benedetto; and the
" church and monaftery of Mount Cafino; the temple of John
" the Baptift at Monza, built by that Theodolinda, queen of
" the Goths, to whom Pope Gregory wrote his dialogues; in
" which place fhe caufed to be painted the ftory of the Longo-
" bards. Afterwards, in Florence, architecture recovering a little,
" the church of the Holy Apoftle, which was built by Charlemagne,
" though fmall, was in a very beautiful manner. The architecture
" of this church was fuch, that Pippo di Ser Brunnellefco did not
" difdain to make ufe of it as a model for the church of the

" Holy

" Holy Ghoſt, and that of St. Lorenzo in the ſame city. The
" ſame may be ſeen in St. Mark's, Venice, which was begun
" under the Doge Juſtinian, and finiſhed in the Grecian manner
" in the year 973. And thus things went on till the year 1140.
" In the ſame Greek manner, and at the ſame time, were the
" ſeven abbies, which the Count Hugo, Marquis of Branden-
" burgh, cauſed to be made in Tuſcany; and other buildings,
" which ſhew, that architecture kept its ground a little, but
" much corrupted, and very different from the good ancient
" manner; of which many old palaces bear teſtimony, built in
" Florence, after the ruin of Feſole, of Tuſcan workmanſhip,
" but in the barbarous ſtyle, in the meaſure of the doors, and
" very long windows, and in the curviture of the ſharp arches
" of the fourth point, in the turn of the arches, according to
" the practice of the ſtrange architects of thoſe times. After-
" wards in the year 1013 the art ſeems to have recovered its
" vigor a little; in the rebuilding of the beautiful church of
" St. Miniato, in which it appears, that the Tuſcan artiſts en-
" deavoured to imitate in the doors, windows, columns, arches,
" cornices, as much as they could, the good ancient order, &c."
I have been thus full in this extract from Vaſari, as he is ſo expreſs as to the period and manner in which this Gothic ſtyle of building firſt originated, and is ſo competent a judge of ſuch a ſubject.

Mr. Ledwich indeed, in the eſſay above referred to, is of opinion, that the Gothic arch had been known and uſed many centuries before the Gothic power was eſtabliſhed. But the two

inſtances.

inſtances which he produces, as he himſelf obſerves, are not perfectly Gothic, but thoſe arches called *contraſted*, which have never been aſcribed to the Goths and Longobards: and we are further to obſerve, that they are not ſuppoſed to repreſent an actual building, but the ornaments of a cibarium only.

But in truth it ſeems extremely nugatory and fruitleſs to endeavour to aſcertain the ſpecific accident which gave birth to the pointed arch; many, almoſt innumerable different circumſtances could be aſſigned from which it might have originated; and who would venture confidently to adopt one in excluſion of the reſt? One may, perhaps, appear more probable than another; but diſcoveries do not always ariſe from thoſe circumſtances, which after they have been made, might have been ſuppoſed moſt naturally to have led to them. Who, for inſtance, after ſtudying the works of Newton, could have conjectured, that his great theory of the univerſal gravitation of matter had been ſuggeſted by the falling of an apple from a tree?—The mind indeed may ſometimes amuſe itſelf with excurſions into the aerial regions of fancy and conjecture; but to find reſt, it muſt return to the firm ground of fact. However entertaining therefore our ſpeculations may be on the poſſible cauſes which gave riſe to the idea of the Gothic arch, we can be ſaid to reaſon only when we direct our enquiries to the actual properties of the arch, which might have recommended its introduction into architecture, after the faſhion of ſuch an arch had been conceived. In other caſes, as in the circle, ellipſe, and catenaria, the nature of the curve alone is ſuppoſed ſufficient for that purpoſe; why then in the Gothic arch are all theſe conſiderations to be

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paſſed over? Certainly if it be poſſeſſed of any peculiar advantages, it ſeems unreaſonable to ſuppoſe, that they were entirelyun known or diſregarded; and that it is really poſſeſſed of ſuch, or at leaſt has been ſuppoſed to be ſo, which is ſufficient in the preſent caſe, we may conclude from what has been ſaid by Monſ. Gautier on this ſubject, in his *Traitè des Ponts et des Chemins;* " The Goths," ſays he, " who ſucceeded the good taſte of the Roman architecture, " erected bridges in many parts of France with pointed arches, " deſigning by that means to diminiſh the lateral preſſure, both " in public and private buildings, and alſo in the churches of " that period, which ſtill remain." And Sir Chriſtopher Wren obſerves, " that the ſharp-headed arch riſes with little centering, " requires lighter key-ſtones, and leſs butment, and yet will " bear another row of double arches riſing from the key-ſtone." The idea of the pointed arch might indeed have been firſt ſuggeſted in the manner deſcribed by Mr. Barry, or perhaps by the perſpective of groin arches; but the mere conception of ſuch a form, would not, I ſhould imagine, have been a ſufficient motive with architects to introduce it into their moſt magnificent buildings, and where it was to ſuſtain the greateſt weights. I ſhall therefore now proceed to enquire into the theory of this arch, and endeavour to aſcertain its relative ſtrength as compared with circular and elliptic arches, when in a ſtate of perfect equilibration; and alſo to determine the aberration from a true balance, which is generated by the horizontal termination of the ſolid building erected on it; from whence we ſhall be enabled to form ſome conjecture, whether the theory of the arch itſelf

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may not be juſtly enumerated amongſt the cauſes, to which we owe its introduction into architecture.

9. The equilibration of arches, which has been ſo well treated of by Mr. Emerſon in his *Eſſay on the Conſtruction of Arches*, and Mr. Hutton in his *Eſſay on Bridges*, may be aſcertained in a very ſimple manner, by conſidering the vouſſoirs, or arch-ſtones, as ſo many wedges urged by the incumbent weight, and endeavouring to ſplit the arch. Now the efficacy of a wedge depends on the magnitude of its vertical angle, the impelling force, and the reſiſtance to be overcome. On the firſt account the force of the vouſſoir, conſidered as a wedge, at any point of the arch, is directly as the radius of curvature of the arch at that point; on the ſecond account, it is as the ſquare of the ſine of the angle formed by the tangent to the curve, at the given point, with a vertical line; and on the third account, it is directly as the ſine of the ſame angle.

10. The materials of which arches are compoſed are ſuppoſed to be of the ſame magnitude; and the joints of the ſtones muſt always be perpendicular to the curve, for every force acting againſt a ſurface acts in lines perpendicular to it; and therefore the force of the incumbent weight to rend the curve is to be reſolved into two, one of which muſt be perpendicular to the curve, and the other, of conſequence, in the direction of the tangent: now if this latter force were not perpendicular to the joints, that is, if the joints were not perpendicular to the curve, there would ariſe a lateral

a lateral preſſure, whoſe direction would not be along the tangent, and wanting a force to ſuſtain it, would deſtroy the equilibrium. Let E A B F, *r m n t*, repreſent two concentrical ſemicircles: alſo let A B, *m n*, be two vouſſoirs ſimilarly ſituated, whoſe ſides, being perpendicular to the curves, converge in the centre C: the forces of theſe vouſſoirs, conſidered as wedges, are inverſely as the ſines of their vertical angles, that is, becauſe of the equality of the arches, directly as the radii of curvature: And the like holds in any other curves whatſoever. Fig. 1.

11. Again, let H *h* be the invariable breadth of the vouſſoir, and G *g* H *h* the incumbent weight; this weight, becauſe the altitude G *h* is ſuppoſed given, is directly as the breadth *h k* of the incumbent column; that is, if *h* H be conſidered as radius, directly as the ſine of the angle *h* H *k*, formed by the tangent at the point H with the vertical line *g* H: but the force of this entire weight muſt be reſolved into two, one *g* K in the direction of the tangent, the other H K perpendicular to the curve, which is the force impelling the vouſſoir to ſplit the arch; and the line H K, which repreſents this latter force, becauſe *g* H is given, is the ſine of the angle H *g* K, which is equal to that formed by the tangent at H with the vertical line. Hence therefore, conjoining both theſe ratios, the force impelling the vouſſoir is as the ſquare of the ſine of the angle formed by the tangent to the curve, at the given point, with the vertical line.

12. Again, the wedge, impelled in a direction perpendicular to the curve, endeavours to ſplit the arch, and therefore move

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one ſegment about the fulcrum *r*, and the other about the fulcrum *t*; and therefore the force of the vouſſoir, acting on the levers H *t*, H *r*, is directly as the perpendiculars *t p*, *r q*, let fall from the fulcrums on the line of direction H *q*, that is, directly as the ſine of the angle *t* C *p*, or H *g* K.

13. Hence therefore if a weight, like a wall, be incumbent on the arch E A B F, ſtanding in a vertical plane, if the height of the wall on any point be in a ratio compounded of the reciprocal triplicate ratio of the ſine of the angle formed by the curve and vertical line, and the reciprocal ſimple ratio of the radius of curvature in that point, all the vouſſoirs will endeavour to ſplit the arch with equal forces, and therefore will be in perfect equilibrium with each other. On this general propoſition depends the theory of the conſtruction of arches.

14. But ſince the materials with which arches are conſtructed are not of infinite ſtrength, there muſt be a certain degree of preſſure, which will rend even an arch whoſe parts are in perfect equilibrium. Now the ſtrength of an arch, in its different points, is as the greateſt weight which it is able to bear on thoſe points without breaking; that is, in a ratio compounded of the reciprocal triplicate ratio of the ſine of the angle formed by the curve and vertical line in its different points, and the reciprocal ſimple ratio of the radius of curvature in the ſame points. On this principle the comparative ſtrength as well of different arches as of the ſame arch in its different points may be eaſily aſcertained.

15. Let

15. LET B D, confidered as variable, reprefent the fpan of the Gothic arch B E D, fpringing from the points B, D; and the invariable line C D the radius of the circle by which the Gothic arch is defcribed. On B D defcribe the femicircle B H D; the ftrength of the femicircular arch B H D at H is to the ftrength of the femicircular arch A G D at G, as C D to K D, by Art. 10. But, fince the relative ftrength of arches is to be determined by comparing them in their weakeft points, (for if an arch fails in any one point, the whole falls to ruin) the ftrength of the femicircular arch A G D will be to the ftrength of the Gothic arch, as the ftrength of the femicircle at G to its ftrength at E, that is, as the greateft weights thefe points are able to bear when in equilibrium; that is, reciprocally as the cube of the angle I E F contained by the vertical line I E and the tangent to the circle at E, or its equal E C K, that is, as the cube of E K to the cube of radius, Art. 14. Therefore, *ex æquo, &c.* the ftrength of the femicircular arch B H D at H, is to the ftrength of the Gothic arch of the fame fpan, defcribed with the radius C D, as $GC \times EK^3$ to $KD \times GC^3$, or as $EK \times AK$ to CD^2. Fig. 2.

16. SINCE the rectangle $EK \times AK$ vanifhes at both extremities when K arrives either at A or D, there muft be fome intermediate ftate where it is a *maximum*; to find this ftate, let $CD = a$, and $CK = x$; $EK = \overline{a^2 - x^2}|^{\frac{1}{2}}$, and $AK = a + x$, therefore

therefore the rectangle $EK \times AK = \overline{a^2 - x^2}|^{\frac{1}{2}} \times \overline{a+x}$; whose fluxion $\overline{a^2 - x^2}|^{\frac{1}{2}} \dot{x} - \frac{x^2 \dot{x} - ax\dot{x}}{\overline{a^2 - x^2}|^{\frac{1}{2}}} = 0$. Whence $x^2 + \frac{1}{2} ax = \frac{1}{2} a^2$, and $x = \frac{1}{2} a$. That is, the rectangle $EK \times AK$ is a maximum, when $CK = \frac{1}{2} CD$: or, becaufe CD is conftant, the ratio of the ftrength of a femicircular arch to a Gothic arch of the fame fpan is greateft when the fubtenfe of half the Gothic arch is equal to its fpan. In this cafe the ftrength of the femicircular arch is to the ftrength of the Gothic as 1299 to 1000, or 13 to 10, nearly.

If the radius of the Gothic arch be three-fourths of the fpan, in which cafe it is called the fharp arch of the fourth point, the ftrength of a femicircle will be to the ftrength of a Gothic arch of the fame fpan as 1257 to 1000. And if the radius be two-thirds of the fpan, in which cafe it is called the fharp arch of the third point, the ftrength of the femicircle will be to the ftrength of the Gothic arch of equal fpan as 1210 to 1000.

17. Because the rectangle $EK \times AK$ exceeds the fquare of the femidiameter when the point K bifects CD, and vanifhes when K arrives at D, there muft be fome intermediate pofition, in which the rectangle $EK \times AK$ is equal to the fquare of radius. To find the magnitude of KD in this cafe, let $CK = x$, and $CD = a$, as before; then $EK = \sqrt{a^2 - x^2}$, and $AK = a + x$; therefore $\overline{a+x} \times \sqrt{a^2 - x^2} = a^2$, and $x^4 + 2ax^3 - 2a^3x = 0$; and if $a = 1$, $x^3 + 2x^2 = 2$. Of this equation two roots are impoffible: The third,

third, or CK = ,839286755214, &c. That is, if the radius of the circle, by which a Gothic arch is defcribed, be to the interval of the capitals from which it fprings, as 1 to ,321426489572, the Gothic and femicircular arch of equal fpan will be alfo of equal ftrength.

18. Hence therefore, a Gothic arch whofe radius of curvature is equal to the interval between the pillars, which is the ufual manner of defcription, is the weakeft of all Gothic arches of that fpan. According as the centre from which it is defcribed moves on either fide, in an horizontal line, the ftrength of the arch increafes: if it moves towards the middle point of the interval of the pillars, the ftrength increafes till the centre arrives at K, when the Gothic arch becomes a femicircle. But if the centre moves in the contrary direction, the ftrength of the arch increafes without limit.

19. Mons. Gautier therefore is too general in his affertion when he fays, that the Gothic arch is ftronger than the femicircle; as this is true only in certain cafes, to wit, when the radius by which it is defcribed exceeds the fpan in the ratio of 1 to ,321426489572, very nearly; in all other cafes it is weaker. We may obferve alfo, that Sir Henry Wotton is not fufficiently warranted in his cenfure of this arch, when he fays, that "thefe "kind of arches, both for the natural imbecility of the acute angles, "as likewife for their uncomelinefs, ought to be exiled from all "judicious eyes, and left to their firft inventors, the Goths and "Lombards, amongft other reliques of the barbarous age." As to

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its beauty or deformity, it being a matter not ſubject to mathematical argument, his criticiſm on that head cannot be brought to the like deciſive teſt as the former, and therefore we ſhall not pretend to controvert it.

20. THE depreſſed or ſcheme Gothic arch is deſcribed from centres, lying below the right line joining the capitals of the
Fig. 3. pillars from which it ſprings: thus the arch B F D is deſcribed from the centres *m*, *n*, which lie below the horizontal line B D. Its ſtrength is eaſily computed by continuing the arch F D to the horizontal line R S which paſſes through the centres *m*, *n*: and then, proceeding as before, the ſtrength of the Gothic arch B F D will be to the ſtrength of a ſemicircular arch of equal ſpan, as $FG^3 \times KD$ to mS^4, that is, becauſe both F G and K D muſt always be leſs than *m* S, in a ratio of leſs inequality. And the ſtrength of the two Gothic arches B E D, B F D will be to each other in the direct duplicate ratio of their radii of curvature, and the inverſe triplicate ratio of the perpendicular heights of the arches above the lines of their centres. The ſtrength alſo of the Gothic arch of four centres may be eſtimated in the ſame manner.

21. BUT as the buildings erected on Gothic and ſemicircular arches are never ſo formed as to render them curves of perfect equilibration, but are generally terminated by an horizontal right line, it will be neceſſary to compare them in this reſpect; and we ſhall find, that though the ſtrength of the Gothic arch, when in

equilibrio,

equilibrio, is in certain cafes greater than that of a femicircular arch of equal fpan, yet a given altitude of folid building erected on the Gothic will in fome cafes, but not in all, produce a greater aberration from a perfect equilibrium than in the femicircular. Let B N E D, B H D, be the Gothic and femicircular arches, whofe common fpan is B D; let G R reprefent indifferently the extradoffo of either, from which let fall the perpendicular G M on the fpan, meeting the curves in N, n; and let a be the altitude of the folid building erected on each. If the Gothic arch were in perfect equilibrium, the height above N would be to a, as EK^3 to NM^3; and the height above n in the circle would be to the fame a as HK^3 to nM^3 (Art. 13.). Now if an ellipfe were defcribed with B D as a minor axis, and with E K as a femitranfverfe, it would fall without the Gothic arch, and meet the perpendicular G M fomewhere between N and G, for the radius of the circle ofculating the ellipfe in B is equal to $\frac{EK^2}{BK}$ (Ham. Con. Prop. 17. L. 5.) that is, equal to the greater abfciffa of the circle B N E, and therefore greater than the radius of the Gothic arch; and the ofculating circle falls without the ellipfe (Ham. Con. Prop. 15. L. 5.); but fince the ofculating circle touches the ellipfe fo intimately, that no circle can pafs between it and the ellipfe, the Gothic arch muft fall within it; therefore the ratio of E K to N M is greater than the ratio of E K to m M, that is, than the ratio of H K to n M (Prop. 30. L. 1. Ham. Con.); therefore the height of the folid wall over N, in the Gothic arch, muft be greater than over n, in the circle; and confequently, when the line bounding the wall is not the extradoffo of equilibrium but an horizontal line, there will be a Fig. 4.

M greater

greater deviation from a perfect balance in the Gothic arch. But the higher the Gothic arch the less will be the aberration; and it may, at length, be so far diminished as to become less than the error in the semicircle, for the ordinates E K, N M, vary in the subduplicate ratio of their greater abscissæ; and these abscissæ, their difference M K being given, approach towards equality, as the radius of the circle or height of the arch increases; but the ratio of H K to *n* M is the limit of their variable ratio; so that though the height of the wall to be erected over the corresponding points in the Gothic and semicircular arch, in order to render them curves of equilibration, continually approach to equality, and can be brought nearer to an actual equality, by elevating the point of the Gothic arch, than by any assignable difference, yet the height of the building to be erected on the points of the Gothic arch will always be greater than in the semicircle. Now as E *p*, the difference between the ordinates N M, E K, is always greater than E *r*, the difference between the ordinates *m* M, E K, and these are always in the constant ratio of *n* M to H K, it follows, that by producing the ordinates *m* M, E K, their difference may be made greater than any assignable quantity; and at the same time the difference between the heights of the building to be erected on the points N, *n*, of the Gothic and semicircular arch, may be made less than any assignable quantity. It is evident therefore, that the defect of equilibrium over the point N, in the Gothic arch, may be made less than the defect over the corresponding point *n* of the semicircle, arising from the horizontal termination of the building erected on them. Thus let the difference of the heights of the wall

wall over n and N, when the arches are in equilibrium, be equal to d; if Hv be to $Hv+d$, as HK to KE, Hv will be equal to $Er-d$. Then a being the altitude of the building over the crown of both arches, and Ng the height over the point n in the femicircle, when truly balanced, the defect of equilibrium in the Gothic arch will be $Ng+d-a-Ep$, and in the femicircle it will be $Ng-a-Hv=Ng+d-a-Er$; but Ep is greater than Er, and therefore the defect of equilibrium in the Gothic arch is lefs than in the femicircle. If Er be too fmall, produce EK till $Er-Hv=d$, which is greater than δ, the difference of heights when EK is fo produced: then, *a fortiori*, the deviation from a perfect balance will be lefs in the Gothic arch, as before. In all cafes the aberration will be lefs when the altitude over the crown of the arch is lefs; becaufe, when quantities are in a given ratio, the lefs the quantities themfelves are, the lefs will be their difference. And therefore, when the height of the building above the fpring of the arch is given, the higher the arch the more it approaches to a perfect balance.

Hence we may obferve, that this arch was peculiarly adapted to the ftyle of thofe religious buildings, which were in fafhion in the middle ages, where the roof was to be raifed to an extraordinary height, and no great weight immediately incumbent on the point of the arch, or where one tier of arches was to be raifed over another. But when a very high building is to be erected upon a Gothic arch, the quantity of matter over the crown or vertex muft be very much lightened by windows or other perforations, as was the practice with the Gothic architects.

22. If an ellipfe be defcribed on the fpan of a femicircular arch, its ftrength, when in equilibrio, will be to that of a femicircular arch* reciprocally as the radii of curvature at the keyftones

* Hence the ftrength of an elliptic arch whofe leffer axis is perpendicular to the horizon, and of a portion of a circle of the fame fpan and altitude may be eafily compared. For the radius of the circle paffing through the points B E D is
Fig. 5. equal to $\frac{EK^2 + KD^2}{2\,EK}$, and the radius of the circle ofculating the ellipfe in D is equal to $\frac{KD^2}{EK}$; therefore the ftrength of the fegment of the circle is to the ftrength of the femi-elliptic arch as $2\,KD^2$ to $EK^2 + KD^2$, that is, always in a ratio of greater inequality. If to this we add, that the aberration from a perfect equilibrium in the femi-ellipfe, arifing from the horizontal termination of the building erected on it, is very great, but of no great confequence in any fegment of a circle which contains lefs than 120°, (fee Hutton's Effay on Bridges,) perhaps it may feem reafonable to conclude, that portions of circles are in all cafes preferable to femi-elliptic arches, or thofe curves of many centres, which of late have become fo fafhionable in the conftruction of bridges. In the celebrated bridge over the Sein, at Neuilly near Paris, confifting of five arches, each 120 feet opening, and rifing but 30 feet, the radius of curvature at the crown of the arch is 150 feet, and therefore is to the radius of a circular arch of the fame fpan, and rifing only to the fame height, as 2 to 1; and confequently the relative ftrength of thefe arches, without taking into account the enormous error from a true equilibrium in the former, is in the reciprocal ratio of thefe numbers. And the ftrength of a femi-ellipfe of the fame fpan and altitude is to the ftrength of the curve ufed in the bridge of Neuilly as 5 to 4.

The radius of curvature of a circle ofculating the vulgar cycloid at its vertex is equal to twice the diameter of the generating circle; and the radius of a fegment of a circle of the fame opening and height is equal to the fum of the fquares of half the circumference and diameter applied to twice the diameter; therefore a cycloid is weaker than a circular arch of the fame fpan and height in the ratio of 11 to 16 nearly. To this we may add, that the accurate conftruction of cycloidal arches, in ftone, is almoft impracticable, and therefore a confiderable deduction fhould be made from their ftrength, on account of their imperfect execution. For thefe reafons arches of this kind are not introduced into practice; though Drummond, in his travels, tells us, that thofe of the bridge of the Holy Trinity, over the Arno, at Florence, are cycloidal. And indeed what is related of that bridge is confiftent with theory, for on account of its extreme weaknefs, it is found neceffary to prevent waggons and other heavy carriages from paffing over it.

ſtones (by Art. 14.); that is, as the ſemidiameter of the ellipſe which is perpendicular to the horizon to the ſemidiameter of the circle, (Prop. 17. L. 5. Ham. Con.) or as the height of the ellipſe to the height of the ſemicircle; that is, if B *m* E D be an elliptic arch, its ſtrength will be to that of the ſemicircle B H D, as E K to K D; but the ſtrength of the ſemicircle is to the ſtrength of the Gothic arch B E D as $EK \times AK$ to CD^2; therefore, *ex æquo, &c.* the ſtrength of the elliptic arch is to the ſtrength of the Gothic arch of equal height and ſpan as $EK^2 \times AK$, or $AK^2 \times KD$ to $CD^2 \times KD$, or as AK^2 to CD^2, or in the duplicate ratio of the ſum of radius and the coſine of the Gothic arch to radius, that is, in a ratio of greater inequality. And ſince K D is conſtant, A K increaſes in a higher proportion than C D; therefore in the elliptic arch, the ſtrength increaſes in a higher proportion, as the altitude increaſes, than in the Gothic. Thus in an elliptic and Gothic arch in which the heights and ſpans are equal, and the ſubtenſes of their halves equal to the ſpan, the ſtrength of the elliptic will be to that of the Gothic as $22\frac{1}{2}$ to 10. And when the ſtrength of the Gothic arch is equal to that of a ſemicircle of equal ſpan, the ſtrength of an elliptic arch of equal height and ſpan exceeds it nearly in the ratio of 5 to 2 by Art. 17. Fig. 4. Fig. 2.

23. THE extradoſſo G R of an elliptic arch B E D may be thus inveſtigated: through any point N of the ellipſe draw the tangent N V meeting the axis K E produced in V; from K the centre let fall the perpendicular K A on the tangent; and draw K C the ſemiconjugate of K N Fig. 6.

K N which paſſes through the point of contact; and N T the ordinate to the axis: Alſo let $BK = c$, $KE = t$, $ET = x$, $KA = p$, and $KC = g$; then $KV = \frac{t^2}{t-x}$, (Pr. 48. L. 1. Ham. Con.); and $p = \frac{tc}{g}$ (Cor. 1. Pr. 31. L. 1. Ham. Con.) therefore $\frac{t^2}{t-x} : \frac{tc}{g} :: 1$ (rad.): Sine of the angle K V N or M N A, which is therefore equal to $\frac{c \times \overline{t-x}}{tg}$, and its cube equal to $\frac{c^3 \times \overline{t-x}^3}{t^3 g^3}$; alſo R, the radius of curvature at the point N is equal to $\frac{g^2}{p}$ (Cor. Pr. 18. L. 5. Ham. Con.) $= \frac{g^3}{tc}$; therefore R × Sin.^{3} of $MNA = \frac{c^2 \times \overline{t-x}^3}{t^4}$; and the height of the wall above the point N, which is reciprocally as R × ſin.^{3} M N A, will be directly as $\frac{t^4}{c^2 \times \overline{t-x}^3}$, which at the vertex E is $\frac{t}{c^2}$; and conſequently the height of the wall above N is to the height above E as $\frac{t^4}{c^2 \times \overline{t-x}^3}$ to $\frac{t}{c^2}$, or as t^3 to $\overline{t-x}^3$, that is, as EK^3 to NM^3; or as HK^3 to nM^3; therefore the heights of the wall above each point N of the ellipſe and the correſponding point n of the ſemicircle are equal. Conſequently when the altitude over the vertex of the arch is given, and the termination of the wall horizontal, there will be a leſs deviation from a true balance in the higher arch, that is, in the ellipſe; and the height over the point N is to a, the given height over E, as EK^3 to NM^3, therefore equal to $a \times \frac{EK^3}{NM^3}$; and the height over n equal to $a \times \frac{EK^3}{mM^3}$; and the difference equal to $a \times EK^3 \times \frac{mM^3 - NM^3}{NM^3 \times mM^3}$ which is greater than $a \times EK \times \frac{mM - NM}{NM \times mM}$, that is, greater than $a \times EK \times \frac{NM}{NM^2}$, which is greater than N M, the difference

difference of the ordinates, for the altitude can ſcarcely be conceived to be ſo ſmall as the difference of the ordinates; conſequently there will be a leſs deviation from a true balance in the elliptic arch than in the Gothic.

24. IT follows therefore, that an elliptic arch, whoſe tranſverſe axis is perpendicular to the horizon, is to be preferred to the Gothic arch for ſtrength; both becauſe its ſtrength exceeds that of a Gothic arch of equal ſpan and altitude, when both are in a ſtate of equilibration; and alſo becauſe a given altitude of building raiſed upon it and terminated by an horizontal line produces a leſs aberration from a perfect balance of the parts. It follows alſo that the Gothic arch, when carried up to a ſufficient height, is to be preferred to a ſemicircular arch for the ſame reaſons.

IT ſeems unneceſſary to compare the ſtrength of the piers requiſite to ſuſtain elliptic and Gothic arches with the ſtrength of the piers of ſemicircular arches, as, from the great elevation of their vertexes, they are entirely unfit to be applied in bridges, where alone piers are introduced.

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An Account of a DISEASE *which, until lately, proved fatal to a great Number of* INFANTS *in the* LYING-IN HOSPITAL *of* DUBLIN, *with Obſervations on its Cauſes and Prevention.*

By JOSEPH CLARKE, *M. D. Maſter of the Hoſpital above-mentioned, and M. R. I. A.*

LYING-IN HOSPITALS are inſtitutions of ſuch recent date, and ſo few in number, that hitherto we may conſider them as in a ſtate of infancy. Excepting ſome portion of the Hotel Dieu of Paris, which has been long allotted to the relief of poor pregnant women, I know of none that have exiſted above *forty years*, and very few can lay claim even to this antiquity. It can hardly appear unreaſonable, therefore, to ſuppoſe that imperfections ſtill exiſt in their management, which time and accurate compariſon may ſerve to detect; and although ſuch eſtabliſhments be at preſent confined to a few of the capital cities in Europe, it is pro- Read June 6, 1789.

bable their number will increafe as their good effects in fociety are experienced. It is hoped, therefore, that a few facts and obfervations, tending to point out a confiderable fource of error in an extenfive lying-in hofpital, may be deemed worthy of public notice; both prefent and future inftitutions of this nature may, perhaps, derive fome ufeful information from fuch enquiry.

SEVERAL years ago, in attempting to afcertain the nature of the difeafe which is the fubject of the following remarks, I found the doctrines contained in moft medical books of very little ufe: all the morbid caufes, commonly fuppofed to produce difeafes in infancy, appeared to me inadequate to an explanation of its phœnomena. Doubts of courfe arofe in my mind, fome of which have been already ftated to the public *. At length I was tempted to hazard a *conjecture*, which then appeared *probable*, and which fucceeding events *feem* to have confirmed. A fketch of the evidence is here, with deference, fubmitted to the candid confideration of phyficians, and of this Academy.

AT the conclufion of the year 1782, of feventeen thoufand fix hundred and fifty infants born alive in the Lying-in Hofpital of this city, two thoufand nine hundred and forty-four had died within the firft fortnight †, that is nearly every fixth child, or about feventeen in the hundred. This was obvioufly a large proportion

* See Obfervations on the Properties commonly attributed by medical writers to human Milk, &c. Vol. II. of the Tranfactions of this Academy.

† See abftract of regiftry at the end of this effay.

portion of deaths, as we ſhall prove more particularly hereafter. The diſeaſe which carried off moſt of theſe children, perhaps nineteen of twenty, was general convulſions, or what our nurſe-tenders have been long in the habit of calling the nine-day fits, as conſtantly occurring within the firſt nine days after birth. As this diſeaſe has hitherto yielded to no remedy, I have been always more engaged in attending to its prevention than cure. I am chiefly indebted for its hiſtory, therefore, to the united reports of ſeveral of our moſt experienced nurſe-tenders. I took down their remarks ſeparately, and from the whole collected what follows.

In general it has been obſerved that ſuch children as are diſpoſed to whine and cry much from their birth, and ſuch as are ſubject to heavy deep ſleeps, or ſtartings in their ſleep, are peculiarly apt to fall into convulſive affections. Twiſting of the upper extremities, while awake, without any evident cauſe; a livid circle about the lips, and ſudden changes of colour in the countenance, have now and then been thought to portend the *nine-day fits*. Screwing and gathering of the mouth into a purſe, accompanied at intervals with a particular kind of ſcreeching, well known to the experienced nurſe-tenders, are reckoned ſure, and by no means diſtant, forerunners. Sometimes previous to theſe ſymptoms, and ſometimes along with them, the infants are obſerved to be unuſually greedy for ſucking at the breaſt, or feeding by the ſpoon; laxatives given, in ſuch ſituations, ſeldom fail to operate freely, ſometimes bringing away greeniſh, ſlimy, or knotty ſtools; though not unfrequently they are of a natural yellow colour, as I myſelf have more than once ſeen.

Generally with one or more of thefe fymptoms preceding, but *fometimes* without any *warning whatever*, the infants are feized with violent irregular contractions and relaxations of their mufcular frame, but particularly of thofe of the extremities and face. Thefe convulfive motions recur at uncertain intervals, and produce various effects. In fome the agitation is very great; the mouth foams; the thumbs are rivetted into the palms of the hands; the jaws are locked from the commencement, fo as to prevent the actions of fucking and fwallowing; and any attempts to wet the mouth or fauces, or to adminifter medicines, feem to aggravate the fpafms very much; the face becomes turgid, and of a livid hue, as do moft other parts of the body. From this circumftance, and from the fhorter duration of the difeafe, when it occurs in this form, the nurfes reckon this a different fpecies, and call it the *black fits*. The conflict in fuch cafes lafts from about eight to thirty hours, and in fome very rare cafes to about forty hours, when the powers of nature fink exhaufted and overpowered, as it were, with their own exertions.

It much more frequently happens, however, that the fpafmodic contractions are not fo ftrong as above defcribed; that the extremities are rather twifted than convulfed; that the power of fucking, but more certainly of deglutition, is not loft 'till near death; that the mouth foams lefs; and that the paroxyfms, recurring at more diftant intervals, continue to harafs the patient from three to five days, and in fome rare inftances to feven and even nine. During all this period the face remains pale; and the body, from being perhaps very plump, is reduced to a moft miferable fpectre

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by emaciation and disease. This the nurses consider as a second species, and call it the *white fits*.

Both these supposed species, which may, perhaps, be more justly considered as varieties of the same disease, agree in *constantly* attacking within *nine* days from birth, and most frequently about the falling off of the umbilical chord. This is an event which generally takes place from the fourth to the sixth or seventh day. Diarrhœa is a constant concomitant of both species. Long and sad experience have found them also to be both equally fatal, insomuch, that the memory of the oldest person does not furnish an instance of one being cured.

In order to place my ideas of the cause of this fatal disease in the clearest point of view, I find it necessary to have recourse to extracts from a letter written by me in the year 1783 to the late Doctor Hutcheson, who was then consulting physician to the hospital in question.

In this letter, which was written after having seen some of the best regulated Lying-in Hospitals in London, I stated to Doctor Hutcheson,

That in an old hospital, which preceded the present, but instituted by and under the care of the same gentleman, and in a less airy part of Dublin, of three thousand seven hundred and forty-six children therein born, only two hundred and forty-

forty-one died within the firſt month *, which are in the proportion of one to fifteen and a half, or from ſix to ſeven in the hundred.

That during a period of five or ſix years in the Britiſh Lying-in Hoſpital, London, of three thouſand ſix hundred and eleven therein born, only one hundred and forty-ſix died within the firſt three weeks or month, which are as one to twenty-five, or four in the hundred.

That in the London Lying-in Hoſpital I was poſitively aſſured the death of an infant was a rare occurrence. It is there computed with ſome confidence (for I was told that no written account is kept) that the number of ſtill-born infants far exceeds the number of thoſe dying after birth. The proportion of ſtill-born we know to be about a twentieth part, or five in the hundred.

That near forty years ago, when the diſeaſes of children were leſs underſtood, and more eſpecially the ſalutary practice of inoculation, Doctor Short computed from ſome very extenſive regiſters, that London loſt thirty-nine per cent. under the age of *two* years —Edinburgh and Northampton thirty-four or thirty-five—Sheffield twenty-eight—country places from twenty to twenty-eight;—whereas in the Dublin hoſpital there was loſt a number equal to half of that loſt in many of theſe places, and nearly equal to the

* See the caſe of Mr. Moſſe, offered to the conſideration of the Iriſh Houſe of Commons in the year 1755.

the whole of that in ſome of them, in *two* weeks, or in about the fiftieth part of the ſame ſpace of time. From which, and ſome other conſiderations of leſs weight, I thought the uncommon mortality of children in the Dublin Lying-in Hoſpital ſatisfactorily proved.

I THEN ventured to hazard ſome conjectures concerning the cauſes of a mortality, by which ſo many uſeful lives were loſt to the ſtate.

1ſt, Foul air, or an impure atmoſphere,

2d, Neglect of keeping the children clean and dry,

3d, Irregularity in the manner of living of their mothers, more eſpecially in the abuſe of *ſpirituous* liquors,—were the cauſes which appeared to me the moſt probable, either ſeparately or perhaps combined; but I ſuſpected the firſt, viz. an impure or phlogiſticated atmoſphere, for contributing moſt powerfully to the general calamity. For,

FIRST. I remarked to him that public regiſters proved the mortality of children to increaſe proportionably with the ſize of towns; and that the larger towns are, the more numerous are the cauſes which have a tendency to taint their atmoſphere, and thereby render it leſs fit for the purpoſes of ſalutary reſpiration.

SECONDLY. That in private practice phyſicians in the city of Dublin did not find the mortality of infants in any degree ſo con-

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ſiderable as our regiſtry proved it to be in the Hoſpital, a proof that there was here ſome peculiar exciting cauſe of diſeaſe.

THIRDLY. That the difference between the mortality of the children in the old hoſpital and in the preſent one, when under the management of the ſame eminent character, Mr. Moſſe, afforded the ſtrongeſt evidence in favour of this conjecture. Such difference could not be ſuppoſed to ariſe from any different method of feeding or clothing them, or in the exhibition of medicines; to me it ſeemed to originate from a difference in the apartments and accommodations of the women. In the former, which was an old houſe, and never deſigned for an hoſpital, were one or two, or at moſt three beds in the ſame room, to each of which there muſt have been a door, and one or two, perhaps three windows; whereas in the latter were eight beds in the ſame room, and only one door properly ſpeaking *, with three windows in ſome, and two in others; whence it is evident that the ſupply of freſh air in each being nearly on an equality, it muſt be much ſooner corrupted by the reſpiration, lochial diſcharges, and other effluvia of eight women and as many children, than by thoſe of *two* or *three*.

FOURTHLY. I obſerved, in farther confirmation of this doctrine, that the Brownlow-ſtreet Hoſpital in London, which is very favourable

* There is indeed a ſecond door to each of our large wards, but as it opens into a ſmall ward, containing two beds, it is probable the air derived from ſuch communication is not very ſalubrious. The dimenſions of our large wards, in the front of the hoſpital, are 36 feet by 23, and 13 in height; in the rere 33½ by 23, and of equal height. The ſmall wards in front are 19 by 12½; and in rere, 18 by 13½.

favourable to the lives of infants, was an old building, which ſeemed not to have been originally deſigned for an hoſpital; in it there were but ſix beds in a room with one door, one ſmall and three large windows, with a ventilator to each of the latter; that their beds had curtains, but no canopies as in Dublin, and that the utmoſt cleanlineſs was in every reſpect obſerved. That in the City of London Hoſpital, which is an elegant modern building, there are but ſeven beds to a ward, with two large and four ſmall windows to each, one door with a large ventilator over it, the ceilings lofty and perforated by an air-pipe of ſeveral inches diameter, which paſſes out at ſome part of the roof. Here alſo the moſt ſcrupulous cleanlineſs is obſerved, and large ſupplies of clean linen given both for beds, women and infants; and here the death of an infant is a rare occurrence.

Lastly. I alledged it was by no means inconſiſtent with analogy or reaſon to ſuppoſe that the accumulated effluvia ariſing from the bodies of puerperal women and children in lying-in hoſpitals might acquire qualities peculiarly noxious to the delicate frame of infants. That in other hoſpitals and gaols, as the pernicious effects of accumulated human effluvia have been often experienced by robuſt adults, it is poſſible that degrees of contagion inferior to theſe may prove fatal to infants. I concluded with quoting the authority of Arbuthnot, who has obſerved "that "the air of cities is very unfriendly to infants and children; for "that as every animal is adapted by nature to the uſe of freſh and "free air, the tolerance of air replete with ſulphureous ſteams of "fuel and the perſpirable matter of animals (as that of cities) is "the effect of *habit* which young creatures have not yet acquired*;" and that if the air of cities be unfriendly, *a fortiori*, ſo muſt the air of hoſpitals in cities, and that in proportion to their want of ventilation.

To theſe reaſons I might have added, on the authority of Doctor Prieſtly, that healthy animals *almoſt always* die of *convulſions* on being put into air in which other animals have died, after breathing it as long as they could; and that moſt other kinds of air, noxious to animal life, produce ſimilar effects. See Experiments and Obſervations on different Kinds of Air, vol. i. page 71.

VIEWING the ſubject in this light, I propoſed a number of alterations intended for the more complete ventilation of the hoſpital, and for which I was principally indebted to Mr. White's excellent work on the management of lying-in women. My obſervations had the effect I wiſhed with Doctor Hutcheſon and the medical governors. Apertures of a conſiderable ſize were made in the ceilings of each ward, which have been ſince changed for air-pipes of ſix inches diameter. Three holes, of an inch diameter, were bored, in an oblique direction, through each window-frame at top. The upper part of the doors, opening into the gallery, were alſo perforated with a great number of holes. By theſe means a free and eaſy paſſage was given to the air through the wards at all times, and executed in ſuch a manner as to put it out of the power

* Eſſay concerning the Effects of Air on human Bodies.

power of nurſe-tenders or patients to controul. Since the above period alſo the number of beds in the large wards have been reduced to ſeven, and ſeveral changes made in their conſtruction, which render them more airy, and more eaſily kept clean. The conſequences have been favourable far beyond the expectation of every perſon concerned. The *nine-day* fits are become viſibly leſs frequent; and the abſtract of our regiſtry ſhews the fact at firſt view to the moſt inattentive obſerver. Of eight thouſand and thirty-three children born *ſince* the above *period*, only four hundred and nineteen have died in the hoſpital; that is nearly one in nineteen and a third, or from five to ſix in the hundred. Had the mortality of infants been in this proportion ſince the commencement of the Dublin hoſpital, the number of children dead would have been ſomewhat about thirteen hundred, inſtead of the preſent number, three thouſand three hundred and ſixty-three; or in other words, above two thouſand lives would have been ſaved to the community.

That this diminution of mortality is to be attributed to improvements in ventilation can admit, I think, of little doubt. No other new mode of management has been of late practiſed to account for it. No other remedies uſed than ſuch as have been tried a thouſand times unſucceſsfully. I know it has been objected, that it may be owing to their mothers now remaining a ſhorter ſpace of time in the hoſpital than formerly. In order to aſcertain whether this be a matter of fact, I have, for the laſt two years, had an entry made of the day on which each infant died; the number dead has been one hundred and fourteen, and they have died on the following days after their birth:

12th day,	11th,	10th,	9th,	8th,	7th,	6th,	5th,	4th,	3d,	2d,	1ſt.	Total.
1 died.	0.	3.	3.	5.	24.	37.	18.	6.	5.	10.	2.	114 died.

Hence it is obvious that the fatal days are the fifth, the ſeventh, but eſpecially the ſixth, and either of theſe are undoubtedly much within the average day of the diſcharge of our patients. Beſides, the early diſcharge of patients did not commence in any one year, as the leſſened mortality of infants did; it aroſe from a gradual increaſe in the number of poor demanding admiſſion; and I am happy to add, that ſome late very liberal donations, and a conſequent increaſe in the number of our beds, have put an end to the neceſſity of this diſagreeable expedient, adopted ſolely with a view of affording indiſcriminate relief.

It might naturally be ſuppoſed that an atmoſphere, which we have endeavoured to prove injurious to the health of infants, would alſo ſomewhat affect the chances of life in their mothers. The fact, however, certainly is, that on an average fewer women have died in child-bed in the Dublin hoſpital than in moſt other lying-in hoſpitals, (Compare the abſtract at the end of this eſſay with facts contained in the poſtſcript to Mr. White's treatiſe on the management of pregnant and puerperal women.) Here then a queſtion ariſes, why ſhould infants be ſo much more liable to injury from an impure atmoſphere than adults? Is it poſſible that mothers ſhall eſcape with impunity and their children periſh? This, I own, puzzled me extremely, and had almoſt made me doubt of what I conſidered a fact, ſupported by the ſtrongeſt probable evidence. By accident, however, in looking over a diſſertation on the food and diſcharges of the human body, by

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our celebrated countryman, the late Doctor Bryan Robinſon, I found ſome facts and obſervations which appear to me to go a great way towards an explanation.

In order to make theſe facts intelligible to perſons not very converſant in ſuch ſpeculations, I muſt premiſe, that Doctor Prieſtly has fully proved one great and indiſpenſable uſe of reſpiration to be to carry off or leſſen a certain quality in the blood, which is known by the name of phlogiſton. That this can only be done by pure air. That by the addition of phlogiſton to blood it acquires a deep black colour; and by its avolation, that blood returns to its natural *florid* hue.

Now Doctor Robinſon found by experiment *, that the weight of the heart, in reſpect to the weight of the body, is greater in children than in grown bodies, and that their quantity of blood is proportional to the weight of the heart. He found alſo, that the quantity of blood, which flows through the lungs in a given time, in proportion to the maſs of circulating fluids, is greater in children than in grown bodies; and that this proportion leſſens continually from the birth till bodies arrive at their growth. Hence he remarks, that as the blood of children paſſes oftener through the lungs, it is more fluid and of a brighter colour than the blood of grown perſons.

If this be a true picture of the conſtitution of infants, we muſt preſume that ſuch peculiarities are intended to anſwer ſome very

* See page 13, et ſeq.

very important purpofes in the œconomy of young animals; and that in proportion as the intention of *Nature* is in thefe refpects fruftrated, the effects will be more or lefs feverely felt. Would it be deemed a conjecture, exceeding the bounds of probability, to fufpect that the avolation of a very large quantity of phlogifton, and its due feparation from the mafs of blood by pure air, may be effentially neceffary to the growth of young animals; and that this may be one reafon why the impure air of cities has, in all ages, been particularly deftructive to their health?

With a view of reducing the *nine day fits* to its proper genus and fpecies in nofology, I have turned over the works of fome of our beft writers on this fubject. The only genus to which I think it can with any propriety be reduced, is that of eclampfia or convulfion des enfans of Sauvages. But although under this generic title he defcribes feventeen fpecies, there is not one of them to which it bears an exact refemblance. The eclampfia neophytorum of Vander Monde is widely different, as any one may eafily fee by cafting an eye over the hiftory of both. As moft of the fpecies enumerated by Sauvages are fymptomatic, and as he has diftinguifhed feveral of them from various kinds of deleterious fubftances taken into the fyftem; as eclampfia ab atropa, cicuta, &c. perhaps we might with equal propriety add eclampfia ab atmofphæra phlogifticata.

There is a fporadic difeafe in Minorca and fome other countries fo very like the nine day fits, in *fome particulars*, that it may be worth while here to collect, under one point of view, a few extracts concerning it. Nofologifts have given it the title of trifmus

trifmus nafcentium. " In hac urbe affli&tantur plurimi infantes, " adeo feroci convulfione mandibulæ inferioris, ut ea apprehenfi, " nullo poffint motu illam movere, et abhinc fuctus lactis impe- " ditur omnino. Tot interficit mala ifta convulfio, ac variolæ " aut morbilli. In hoc periculum incurrunt recenter nati ufque " ad *nonum* fuæ nativitatis diem, eoque tranfacto, omne difcrimen " *ceffare* docuit *femper* experientia." For thefe and fome other obfervations, from the writings of a Spanifh phyfician, we are indebted to my friend Doctor Cleghorne's valuable treatife on the difeafes of Minorca. After the hiftory of the difeafe, the doctor obferves that it is needlefs to add the remedies prefcribed by the Spanifh author, as he ingenuoufly confeffes the difeafe to be fo feldom curable, that in twenty years practife he had fcarce known fix to recover.

In Germany, Heifter, de maxillæ fpafmo, obferves " Quod fi " fponte, five e caufa interna, hic maxillæ fpafmus in infantibus, " ut fæpe vidi, contingit ut plurimum *moriuntur* et *vix ullum* fer- " vatum vidi; licet laudatiffima remedia nervina et antifpafmo- " dica internè atque externè quam folertiffimè adhibita fuerint."

Hofer, in the firft volume of the Acta Helvetica, has given a long account of a difeafe not infrequent in fome parts of Switzerland, which Sauvages and Cullen feem to think of the fame fpecies with the preceding, but which differs from them very materially in fome refpects. The title of his paper is, De tetano maxillæ inferioris in Infantibus. " Subjectum ifti obnoxium," fays he, " eft infans, qui inter tertiam et *duodecimam* ætatis diem " ætatis diem verfatur. Cura hujus morbi, quamvis valde lenta

" fit,

" ſit, attamen ſi infans quintam a morbi invaſione diem tranſe-
" gerit *certiſſime felix eſt*, ideoque dummodo tempus terere poſſu-
" mus, res in *ſalvo* poſita eſt." After giving an account of his method of cure, which conſiſts of a farrago of diſtilled waters, ſyrups and *inert* powders, as may be ſeen in Sauvages, he concludes, " hæc eſt methodus applicandorum medicamentorum, quâ
" ex tribus ægrotulis curæ meæ commiſſis plerumque *unus* gratiâ
" divinâ evaſit."

A LATE French author, Monſ. Fourcroy, in a treatiſe entitled Les Enfans elevées dans l'ordre de la Nature, remarks " Quand
" je ſuis arrivé en 1744 a St. Domingue, on ne pouvoit elever des
" negrillons dans la plaine du Cap Francois. Ils mouroient
" preſque tous, c'eſt a dire eviron quatre vingt ſur cent, d'une
" maladie appellée dans le pays *mal* de *machoire* ou *tetanos*, qui
" les emportoit dans les *neuf* premiers jours de leur naiſſance." This diſorder he informs us, when come on, is beyond the power of medicine, but that much may be done in the way of prevention.

FROM theſe obſervations it is evident,

THAT in certain parts of the world children are more ſubject to ſpaſmodic diſeaſes than others.

THAT theſe are more apt to come on within *nine* days after birth.

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THAT coming on within this period they are generally productive of the moſt fatal effects. And laſtly,

THAT their cauſes and cure are every where involved in obſcurity.

IN each of theſe particulars, there is a ſtriking analogy between the triſmus naſcentium or tetanus maxillæ inferioris and the *nine* day fits.

IT is farther worthy of obſervation, that the diſorders of adults, which are confined to particular diſtricts or tracts of country, more frequently ariſe from ſomething noxious infecting the atmoſphere of ſuch places than from any other cauſe; and however difficult it may be to apply this doctrine to the caſes in queſtion, it at leaſt affords ſome probable evidence towards the ſuppoſition, that they originate from ſomewhat ſimilar cauſes.

SUCH are the obſervations which reflection and ſome reading ſuggeſted to me on this ſubject, previous to the publication of the London Medical Tranſactions in the year 1785. In this very excellent work, however, I met with "An account of a ſingular "diſeaſe which prevailed among ſome poor children maintained "by the pariſh of St. James in Weſtminſter;" which appears to me to throw much light on this obſcure ſubject: I hope to be excuſed, therefore, for making ſome extracts from this valuable eſſay, for which the world is indebted to the accurate and learned Sir George Baker.

Sir George informs us, that on the 24th day of September, 1782, feventy-three children, viz. forty-fix girls and twenty-feven boys of different ages, from that of feven to fourteen years, were removed from Wimbleton to a large houfe near Golden-fquare. To this houfe thefe children came in good health, and continued fo for a fortnight; when on the 8th of October, a girl aged thirteen years was fuddenly feized with an excruciating pain in the region of the ftomach and in the back, which was foon followed by violent head-ach, *delirium* and *convulfions*. After a few days, another and another girl were attacked exactly in the fame manner; and towards the end of the month this difeafe had fo prevailed as very much to alarm all thofe to whom the care of thefe children had been committed. On the 29th day of October Sir George's advice was defired. He found *nine* of thefe poor girls and a female fervant in the *fame room* fuffering the various effects of a moft dreadful malady. Five were in the agonies of extreme pain, three were moft cruelly *convulfed*, and the other two were raving in a fit of delirium. None of them had any degree of fever, aud their complaints were always moft fevere immediately after fleep. The other inhabitants of this houfe had in general been healthy during the month of October, and it is remarkable that the difeafe above defcribed affected females only, and was confined to thofe who had flept together in a certain room on the fecond floor. The height of this room was a little more than eight feet, the length twenty, and the breadth fixteen: it contained ten beds, in which it was intended that eighteen girls, two in each bed, and a female fervant fingly fhould fleep; but Sir George difcovered that this being a favourite room on account of its warmth, was generally crouded at night

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by a much greater number than its complement: that as much ſpace as poſſible might be made for beds, the chimney had been ſtopped up with bricks, and it had been the conſtant cuſtom of the ſervant at night to keep the door ſhut and to cloſe the window ſhutters, that as little freſh air as poſſible might be admitted. On enquiry it appeared that three candles and a lamp of oil had been generally uſed during the night in this chamber, but they were hardly of any ſervice, giving a glimmering light and frequently almoſt extinguiſhed.

Sir George adviſed the chamber of the ſick to be evacuated without delay, the healthy to be ſeparated from the diſeaſed, the chimney to be opened, and whatever tended to exclude freſh air to be removed. Theſe directions were complied with, and the patients having been removed to a large apartment (where proper care was taken that freſh air might be admitted) paſſed a quiet night free from every ſymptom of the diſeaſe. However, the next morning, immediately on their awaking, they were all ſeized in the uſual manner, but it was very ſoon obſervable, that the paroxyſms returned leſs often and with leſs violence, and ſometimes without convulſions, and that during the intervals the delirium appeared gradually to abate.

From theſe and various other important facts which we cannot here recite, Sir George conjectures that the ſource of this extraordinary diſeaſe was *vitiated air*. To me his evidence appears ſufficient to afford conviction to every reaſonable mind, and if I am not miſtaken, it adds greatly to the probability of the opinion, which ſuppoſes that the nine day fits originated from a ſimilar ſource.

Upon the whole, from the evidence adduced, I hope the following inferences may not appear improbable.

1. That one effect of an impure atmosphere, on the human body, is to produce spasms and convulsions.

2. That all young creatures, and especially infants within *nine* days after birth, suffer most severely by such a noxious cause; and therefore

3. That in the *construction* of lying-in hospitals, and perhaps of all public buildings intended for the reception of children, lofty ceilings, large windows and moderate sized rooms should be especially attended to.

4. That in the *arrangement* of such edifices, no apartment should be completely filled with beds, if it can be conveniently avoided; and

5. That in their *management* attention is especially necessary to *cleanliness*, as well as to the *constant* and *uniform* admission of atmospheric air by *night* as well as by *day*; and

Lastly. That by pursuing such measures with care, diseases may be *prevented* which it has *hitherto* been found *difficult*, and sometimes *impossible*, to *cure*.

An Abstract of the Registry kept at the Lying-in Hospital in Dublin, from the 8th of December, 1757, (the Day it was first opened) to the 31st of December, 1788, each Year distinguished. By B. H. Register.

	Number of Patients admitted.	Went out not delivered.	Delivered in the Hospital.	Boys born.	Girls born.	Total Number of Children.	Women having Twins, and more.	Children dead.	Children still-born.	Women dead.
From 8th to 31st of December, 1757	55	–	55	30	25	55	–	6	3	1
1758	455	1	454	255	207	462	8	54	21	8
1759	413	7	406	228	192	420	13 (1 had 3)	95	22	5
1760	571	15	556	300	260	560	4	116	36	4
1761	537	16	521	283	249	532	11	104	29	9
Totals - -	26321	1075	25246	13505	12177	25682	432	3363	1349	282

Proportion of males and females born, about *nine* males to *eight* females.
——— children dying in the Hospital, as *one* to about *seven*.
——— children still-born, as *one* to about *nineteen*.
——— women having twins (and more) as *one* to about *fifty-eight*.
——— women dying in child-bed, as *one* to about *ninety*.
——— women having *three* (and *four*) children, as *one* to about *five thousand and fifty*.

An Abstract of the Registry kept at the Lying-in Hospital in Dublin, from the 8th of December, 1757, (the Day it was first opened) to the 31st of December, 1788, each Year distinguished. By B. H. Register.

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Year ending 31st of December, 1758	455	1	454	255	207	462	8	54	21	8
1759	413	7	406	228	192	420	13 (1 had 3)	95	22	5
1760	571	15	556	300	260	560	4	116	36	4
1761	537	16	521	283	249	532	11	104	29	9
1762	550	17	533	279	266	545	12	106	33	6
1763	519	31	488	274	224	498	10	94	29	9
1764	610	22	588	287	308	595	7	83	28	12
1765	559	26	533	288	251	539	6	94	25	6
1766	611	30	581	324	261	585	4	111	18	3
1767	695	31	664	373	301	674	10	125	29	11
1768	689	34	655	362	302	664	9	154	47	16
1769	675	33	642	350	301	651	9	152	38	8
1770	705	35	670	372	305	677	7	107	37	8
1771	724	29	695	370	341	711	16	102	44	5
1772	725	21	704	368	344	712	8	116	32	4
1773	727	33	694	367	344	711	17	136	31	13
1774	709	28	681	357	334	691	10	154	29	21
1775	752	24	728	364	378	742	14	122	27	5
1776	833	31	802	418	407	825	22 (1 had 3)	132	39	7
1777	872	37	835	452	395	847	12	145	35	7
1778	961	34	927	476	460	936	9	127	39	10
1779	1064	53	1011	550	476	1026	15	146	59	8
1780	967	48	919	499	441	940	21	115	41	5
1781	1079	52	1027	598	447	1045	18	121	38	6
1782	1021	31	990	549	458	1007	17	127	57	6
1783	1230	63	1167	632	553	1185	17 (1 had 3)	91	72	15
1784	1317	56	1261	643	641	1284	24	76	68	11
1785	1349	57	1292	711	609	1320	28 (1 had 3)	87	75	8
1786	1396	45	1351	716	656	1372	21	51	101	8
1787	1418	71	1347	705	670	1375	28	59	95	14
1788	1533	64	1469	725	771	1496	25 (1 had 4)	55	72	23
Totals - -	26321	1075	25246	13505	12177	25682	432	3363	1349	282

Proportion of males and females born, about *nine* males to *eight* females.
children dying in the Hospital, as *one* to about *seven*.
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women having *three* (and *four*) children, as *one* to about *five thousand and fifty*.

Q

DESCRIPTION *of a* STEAM ENGINE.

By JOHN COOKE, *Efq; M. R. I. A.*

STEAM is univerfally allowed to be the greateft moving power we have, and therefore if it were rendered manageable, and adapted to the occafions of art, it might be advantageoufly applied where water, wind, men or horfes, are now ufed. Read Feb. 7, 1789.

WATER is feldom convenient, wind is a feeble precarious agent, and mufcular force is very expenfive, and very limited; but fteam is free from each of thefe imperfections, and is fuperior to all in ftrength and duration.

IT has been already applied to work the reciprocating or lever engine, which is furprizingly effectual in pumping water, in beating iron, and in other operations which require diftinct and fucceffive impulfes.

BUT to make fteam anfwer the various purpofes of mechanics, it is neceffary that it fhould be capable of producing a *continu-*

ous and rotative motion, which itſelf is more extenſively uſeful than any other, and from which every modification of motion can be eaſily obtained.

THIS machine conſiſts of three principal parts, the wheel, repreſented fig. 1, the caſe, fig. 2, and the condenſer, which is the ſame with that uſed in Mr. Watts's engine, and requires no deſcription. The wheel, fig. 1, has a broad flat edge, *a b*, which is truly circular and ſmooth; at equal diſtances on this edge are placed eight folding clacks or valves, *b c d e f g h i*; theſe are attached to the wheel by ſmall moveable joints, which are ſo contrived that when ſhut they range exactly with the ſurface of the edge of the wheel, and are capable of opening half way, but no more: For inſtance, the valve *a n m b*, has a joint at *n m*, which will permit it to open through the arch *b k l*, until it arrives at the ſituation *l m*, but no farther. Theſe joints are ſo pliant that in the revolution of the wheel the valves will fall open when they deſcend near the level of the axis; and when they aſcend above it they will ſhut, by their own gravity.

THE caſe of this wheel is repreſented, fig. 2. The ſides of it are at ſuch a diſtance from each other as that the wheel will exactly fill up the aperture *a b*, and the caps *c* and *d* are ſo fitted that the edge of the wheel will come in cloſe contact with them. This caſe is ſo much deeper than the wheel, that the wheel, when fixed in it, leaves a ſemicircular vacancy, *e f g h*, below it, which is exactly filled up by the valves of the wheel when extended. *i* is the tube to admit the ſteam,

ſteam, and *k* is the pipe which leads to the condenſer and drains the machine, both which muſt be open, but in all other parts the caſe is ſteam tight. *a*, fig. 3, is a rod which ſhuts the valves as they approach it; and delivers them cloſed into the ſteam veſſel.

When the wheel is put into this caſe, and ſuſpended on its axis, fig. 3, the valves within the caſe will open and fill up the ſemicircular vacancy: When the ſteam paſſes up from the boiler through the tube it cannot eſcape unleſs through this ſemicircular vacancy, and as this is filled up by the extended valves it muſt force them forward in its paſſage to the condenſer, and conſequently turn the wheel round. The condenſer is worked by a crank in the axis, and a rod *b c* extending from it; this cauſes a conſtant vacuum in that part of the ſemicircular vacancy which lies between the cap *d* and the valve *e f*, on which the ſteam preſſes; by theſe means a power is added to the elaſticity of the ſteam equal to the preſſure of the atmoſphere, ſo that when the force of the ſteam is only equal to the preſſure of the atmoſphere, and the valves are ſix inches ſquare, the wheel will be forced round by a power equal to $531\frac{1}{4}$ lb. ſuſpended at its circumference.

After each valve has performed this operation, another ſucceeds it in the like circumſtances, and thus the wheel is turned round uninterruptedly by a cheap and ſimple contrivance.

A working model of this engine, without the condenſer, was exhibited to ſeveral members of the Royal Iriſh Academy.

Fig 1

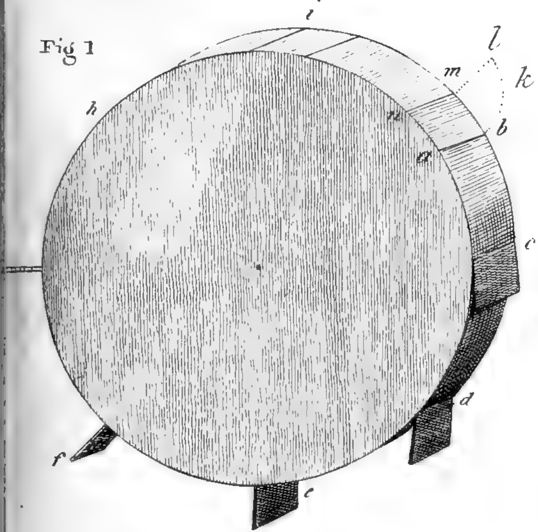


Fig 2

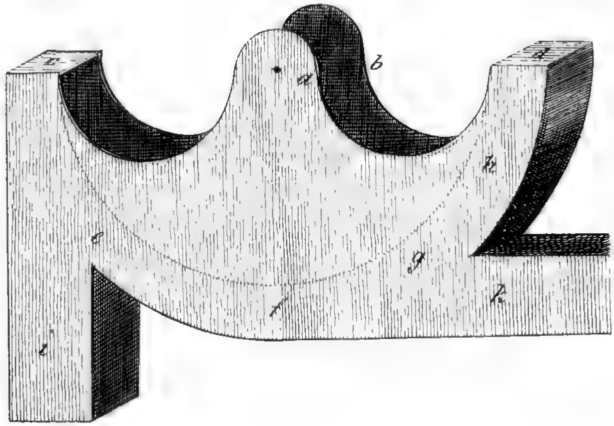
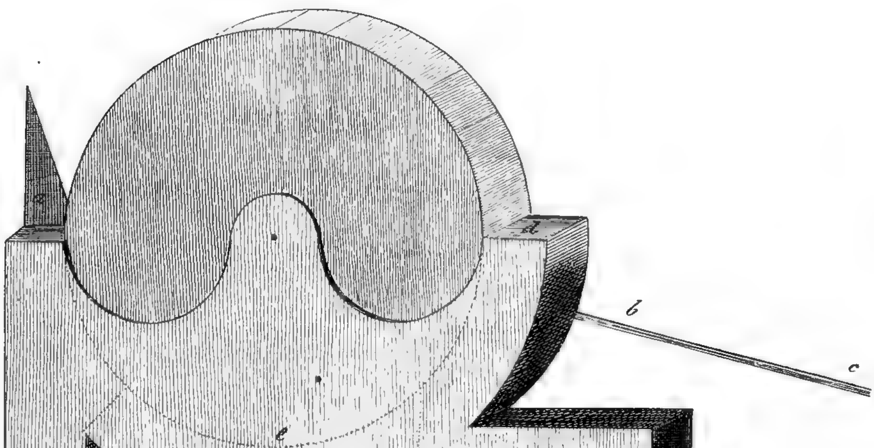
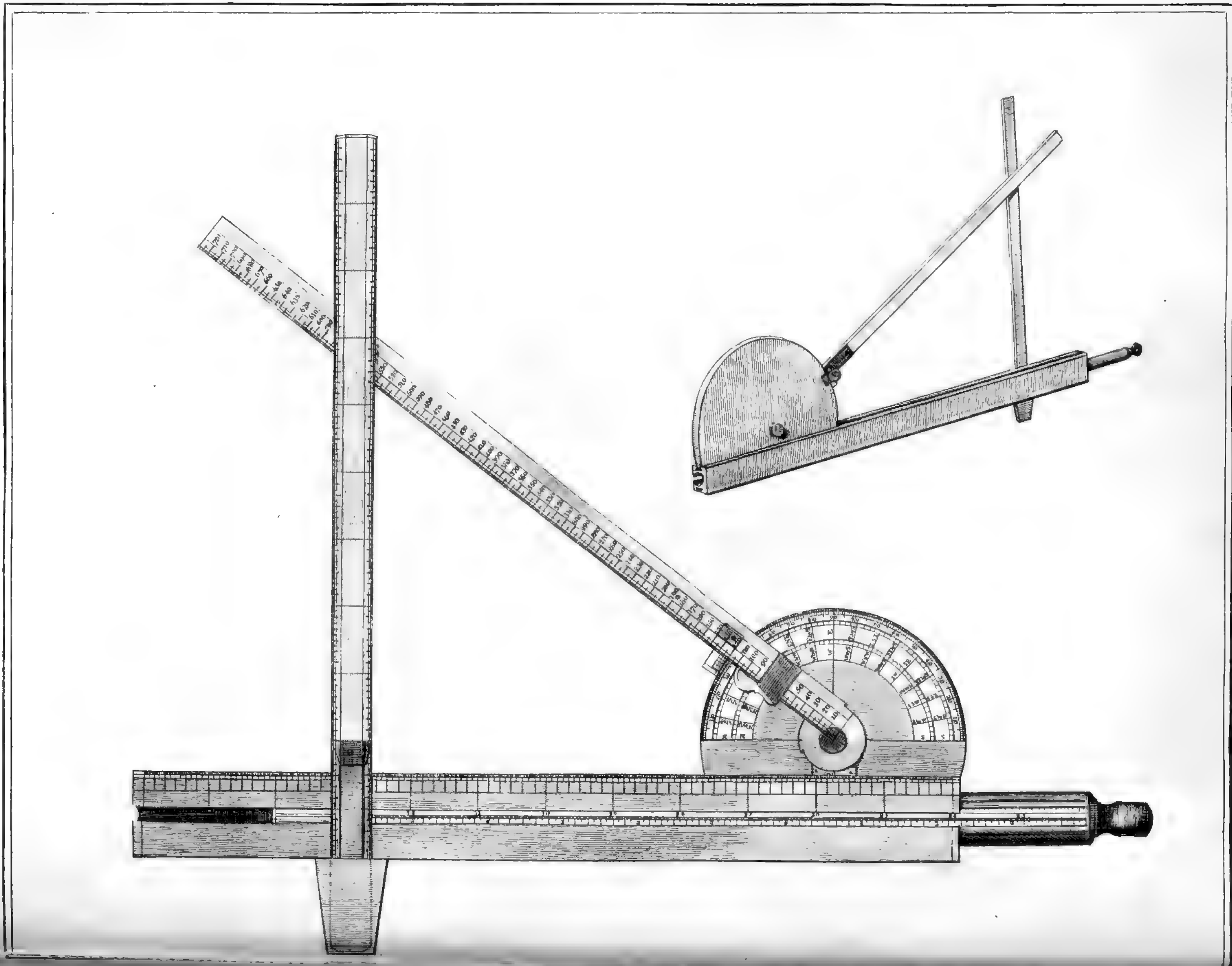


Fig 3



sculp



The USE *and* DESCRIPTION *of a* NEW-INVENTED INSTRUMENT *for* NAVIGATION, *by which every Case in plane, middle Latitude or Mercator's Sailing may be performed without Logarithms, Tables, or any numerical Calculations whatsoever.* By JOHN COOKE, *Esq; M. R. I. A.*

THE object of this instrument is to facilitate the art of navigation, by resolving mechanically every problem necessary in sailing. Read Nov. 7, 1789.

It is composed of a base, a perpendicular, a semicircle, and an index; which are so connected by slides, as to be capable of being moved into every necessary position. The semicircle contains the degrees of the circle, and also the points of the compass, for the purpose of laying off the course; the index, perpendicular and outside edge of the base represent the distance, departure and proper latitude respectively; they are equally graduated, and each division stands for a mile. There is also another line on the bafe, reprefenting the meridional parts; it is engraved on a cylinder contained within a groove in the bafe, and any part of this line neceffary may be eafily had by revolving the cylinder until it appears through the aperture prepared for that purpofe.

The method of working every cafe which occurs in navigation is to make the inftrument fimilar to that ideal triangle which is compofed of the difference of latitude, departure and diftance failed; or to that compofed of the meridional difference of latitude, difference of longitude, and enlarged diftance; or to that compofed of the difference of longitude, departure, and fine of the middle latitude, which is done by means of the data procured from the compafs, log-line and quadrant; whence it follows, from the nature of fimilar triangles, or from the relation which exifts between the fides, triangles, and the fines of their oppofite angles, that the parts of the inftrument become proportional to thefe which they reprefent, and will afcertain the length of the lines, or the extent of the angles fought, by its graduations.

To make this general rule more eafily underftood, it is here particularly applied to each of the cafes of navigation.

CASE I.

THE latitudes and longitudes of two places given, to find the direct course and distance between them.

REQUIRED the course and distance between
Lat. 50° 50′ N. Long. 19° 00′ W. and
Lat. 54° 30′ N. Long. 15° 30′ W.

THE rule on the principles of Mercator's sailing is,

MOVE the center of the semicircle perpendicularly over the meridional degree corresponding with the latitude departed from, 50° 50′ N. (by means of a square) then move the box until the edge of the perpendicular cuts the meridional parts of the latitude in 54° 30′ N. and move the index until it cuts the difference of longitude 3° 30′ on the perpendicular, and the index will mark the course 30° 10′ or N. N. E. $\frac{3}{4}$ E. nearly on the semicircle.

To find the distance sailed screw the index to this course, and move the center of the semicircle to the proper latitude sailed from, 50° 50′ N. and the edge of the perpendicular to the proper latitude arrived at, 54° 30′ N. and then the perpendicular will cut the distance 254$\frac{7}{10}$ miles on the index.

It is performed on the principles of middle latitude failing, thus:

Half the fum of both latitudes taken from 90° is the complement of the middle latitude 37° 20′, and the leffer longitude fubtracted from the greater leaves the difference of longitude 3° 30′, or 210 miles.

Move the center of the femicircle to the proper latitude departed from 50° 50′ N. and the edge of the index to the complement of the middle latitude 37° 20′ on the femicircle, then move the box until the edge of the perpendicular interfects the termination of the difference of longitude 210 miles on the index, which point of interfection will mark the departure 128 on the perpendicular, then move the edge of the perpendicular to the proper latitude arrived at, 54° 30′, and the index until it cuts the departure 128 on the perpendicular, then will the perpendicular mark the diftance on the index $254\frac{7}{10}$ miles, and the index will mark the courfe on the femicircle 30° 10′, or N. N. E. $\frac{3}{4}$ E. nearly.

CASE II.

Both latitudes and courfe given, to find the diftance and difference of longitude.

Lat. 50° 50′ N. Long. 19° 00′ W. failed from,
Lat. 54° 30′ N. arrived at; courfe N. 30° 10′ E.

By

By Mercator's ſailing.

Move the box and ſemicircle as in the former caſe to the meridional parts of the given latitudes, then ſet the index to the courſe, and it will mark the difference of longitude 3° 30′ on the perpendicular 15° 30′ W.

Then to find the diſtance move the perpendicular and ſemicircle to the proper latitudes given, and the perpendicular will cut the diſtance $254\frac{7}{10}$ miles on the index.

By middle Latitude ſailing.

Complement of the middle Lat. 37° 20′.

Move the ſemicircle and perpendicular to the latitudes given; and the index to the courſe, then the perpendicular will ſhow the departure 128 miles, and the index the diſtance $254\frac{7}{10}$ miles, at the point of interſection.

To find the difference of longitude ſet the index to the complement of the middle latitude on the ſemicircle, and move the box until the termination of the departure on the perpendicular meets the index, which will mark the difference of longitude thereon 210 miles, or 3° 30′.

CASE

CASE III.

Both latitudes and diſtance given, to find the courſe and difference of longitude.

Lat. 50° 50′ N. Long. 19° 00′ W. ſailed from,
Lat. 54° 30′ N. arrived at, diſtance ſailed $254\frac{7}{10}$ miles.

Move the perpendicular and ſemicircle to the proper latitudes given, and the index until the diſtance ſailed marked on it meets the perpendicular, then the index will mark the courſe N. 30° 10′ E. on the ſemicircle.

To find the difference of longitude, ſcrew faſt the index to this courſe, move perpendicular and ſemicircle to the meridional parts of the given latitudes, and the ſpace intercepted between the limb of the box and the index will ſhow the difference of longitude 3° 30′.

By middle Latitude ſailing.

Complement of middle Lat. 37° 20′.

Move the ſemicircle and perpendicular to the given latitudes, and the index until the diſtance ſailed marked on it cuts the perpendicular, then the perpendicular will ſhow the departure 128 miles, and the ſemicircle the courſe N. 30° 10′ E.

To

To find the difference of longitude, ſet the index to the complement of the middle latitude on the ſemicircle, and move the perpendicular until the termination of the departure on it cuts the index, then the point of interſection will mark the difference of longitude 210 miles on the index.

CASE IV.

Both latitudes and departure from the meridian given, to find the courſe, diſtance and difference of longitude.

Lat. 56° 40′ S. Long. 28° 55′ E. ſailed from.
Lat. 61° 20′ S. arrived at; departure 172 miles.

By Mercator's ſailing.

N. B. In ſailing through ſouthern latitudes, the end of the cylinder where the numbers begin muſt be turned towards the north, pointed out by the ſemicircle; and in ſailing through northern latitudes, it muſt be reverſed.

Rule.—Move the perpendicular and ſemicircle to the proper latitudes given, and lay the departure on the perpendicular, then move the index until it meets the point in which the departure terminates, the diſtance will be marked on the index 329, and the courſe S. 31° 35′ E. or S. S. E. $\frac{3}{4}$ E. nearly, on the ſemicircle.

To

To find the difference of longitude, move the perpendicular and ſemicircle to the meridional parts of the given latitudes, and the index will cut the difference of longitude on the perpendicular, 5° 35′.

By middle Latitude ſailing.

Complement of middle latitude 31° 00′.

Move the ſemicircle and perpendicular to the given latitudes, and the index to the termination of the departure marked on the perpendicular, the interſection will ſhow the diſtance 329 on the index, and the index the courſe on the ſemicircle, S. 31° 35′ E.

To find the difference of longitude, ſet the index to the complement of the middle latitude on the ſemicircle, and move the perpendicular until the departure marked on it cuts the index, and this point of interſection will mark the difference of longitude on the index 335 miles, or 5° 35′.

CASE V.

One latitude, courſe and diſtance given, to find the difference of latitude and difference of longitude.

Lat. 56° 40′ S. Long. 28° 55′ ſailed from.
Courſe S. 31° 35′ E.; diſtance ſailed 329 miles.

By

By Mercator's ſailing.

SET the ſemicircle to the proper latitude ſailed from, and the index to the courſe; mark the diſtance on it, and bring the perpendicular to the diſtance ſailed, which at the ſame time will mark the latitude arrived at on the line of proper latitude 61° 20′ S.; ſcrew the index to the courſe, and move the ſemicircle and perpendicular to the meridional parts of both latitudes, then the index will cut the difference of longitude on the perpendicular 5° 35′.

By middle Latitude ſailing.

FIND the latitude arrived at as above, and then find the difference of longitude as found by middle latitude ſailing in the preceding caſe.

CASE VI.

ONE latitude, courſe and departure given, to find the diſtance, difference of latitude, and difference of longitude.

Lat. 56° 40′ N. Long. 28° 55′ W.
Courſe N. 31° 35′ W.; departure 172.7 miles.

By Mercator's ſailing.

MOVE the ſemicircle to the proper latitude ſailed from, and the index to the courſe. Mark the departure on the perpendicular,

dicular, and move it until the termination thereof meets the index, then the point of interſection will ſhow the diſtance 329 on the index, and the perpendicular will ſhow the latitude arrived at 61° 20′ N. on the baſe.

THEN ſcrew faſt the index, and move the perpendicular and ſemicircle to the meridional parts correſponding with both latitudes, then the index will cut the difference of longitude, 5° 35′ on the perpendicular.

By middle Latitude ſailing.

FIND the latitude arrived at and the diſtance as above, and the difference of longitude as in caſe IV. by middle latitude ſailing.

CASE VII.

ONE latitude, diſtance ſailed and the departure given, to find the courſe, difference of latitude, and difference of longitude.

Lat. 48° 30 N. Long. 14° 40′ W. ſailed from.
Diſtance ſouth eaſterly 345 miles, and departure 200 miles.

MOVE the ſemicircle to the latitude departed from, mark the diſtance on the index and the departure on the perpendicular, move both until theſe points meet, then will the index ſhew the courſe S. 35° 26′ E. on the ſemicircle, and the perpendicular the latitude arrived at 43° 49′ on the baſe.

THE

The difference of longitude is found as in the preceding cafe.

By middle Latitude failing.

The courfe and latitude arrived at are found as above, and the difference of longitude as in cafe IV. by middle latitude failing.

CASE VIII.

One latitude, courfe and difference of longitude given, to find the diftance and difference of latitude.

Lat. 48° 30′ N. Long. 14° 40′ W. failed from.
Long. 9° 50′ arrived at; courfe S. 35° 26′ E.

By Mercator's failing.

Move the femicircle over the meridional parts of the latitude failed from, and the index to the courfe, and move the perpendicular until the difference of longitude marked on it meets the index, then the perpendicular will cut the meridional parts on the cylinder, which correfpond with the latitude arrived at, then bring the perpendicular and femicircle to the proper latitudes, and the diftance 345 will be marked on the index by the perpendicular.

N. B. This cafe cannot be performed by middle latitude failing; and in failing on a parallel, or when there is no difference of latitude made, the longitude cannot be had by Mercator's ſailing, which makes it neceſſary to ſubjoin the two following caſes.

CASE IX.

The difference of longitude between two places in one parallel of latitude given, to find the diſtance between them.

Lat. 49° 30′ N.
Difference of Long. 3° 30′ E.

RULE.

Set the index to the complement of the latitude, 40° 30′, on the ſemicircle, mark the difference of longitude in miles on the index, then move the perpendicular until it meets the termination of the difference of longitude on the index, and the part of the perpendicular intercepted between the limb of the box and the point of interſection will repreſent the diſtance 136.4 miles required.

CASE X.

The diſtance between two places in one parallel of latitude given, to find the difference of longitude between them.

Lat. of the given parallel 49° 30′ N.
Diſtance ſailed E. 136.4.

RULE.

R U L E.

SET the index to the complement of the latitude 40° 30′, and mark the diſtance ſailed on the perpendicular, and move it until it meets the index, then the point of interſection will ſhow the difference of longitude 210 miles, or 3° 30′ on the index.

IF it ſhould happen that the meridional parts of the given latitudes (36° and 44° N. lat.) are not contained on the ſame line of the cylinder, but begin upon one and terminate on another, then this difficulty is removed by ſliding the cylinder until the meridional degree of the latitude in 36° 00′ is brought near the extremity of the baſe, fixing the ſemicircle perpendicularly over it, and moving the edge of the perpendicular to the termination of the graduations on the cylinder, then turn the cylinder until the continuation of the line required appears, and place the beginning of it to the edge of the perpendicular where the former part of the line terminated, and move the perpendicular until it cuts the degree required, and the ſpace intercepted between it and the ſemicircle will be the meridional difference of latitude ſought.

The

The Method of working a Traverse.

Lat. 46° 48′ N. Long. 7° 47′ W. failed from,

Courses.		*Distances.*
S. S. W. ½ W.	- -	24 miles
S. b W.	- - -	36
S. ½ E.	- - -	40

required the latitude and longitude arrived at, together with the direct course and distance.

SET the semicircle to the proper latitude departed from 46° 48′, and the index to the course S. S. W. ½ W.; mark the distance 24 on the index, and bring the perpendicular to meet it, then the index will cut the departure 11.3 on the perpendicular, and the perpendicular will cut the latitude 46° 27′ N. on the base. For the next case, bring the semicircle to the latitude marked by the perpendicular 46° 27′, and lay down the course S. b W.; if it be towards the first meridian, move the last marked departure until it meets the index, and the limb of the box will mark the present departure; but if the course be from the first meridian, bring the last departure 11.3 to the limb of the box, and the index will mark the departure made good 18.3 on the perpendicular, and the latitude arrived at 45° 52′ will be marked on the base by the perpendicular; proceed in the same manner with all the cases of which the traverse consists, then the difference of latitude 11° 36′ will be intercepted between the latitude failed from 46° 48′ and the lati-

tude

tude arrived at 45° 12′ laſt marked by the perpendicular, and alſo the departure made good will be intercepted between that point on the perpendicular where the firſt departure commenced, and that where the laſt terminated, then the courſe S. 8° 30′ W. the diſtance 97, and the longitude arrived at 8° 8′ are found as in caſe IV.

The Methods of correcting Dead Reckoning.

CASE I.

If the courſe found by dead reckoning be leſs than three points or 33°.

Lat. by obſervation departed from - - 39° 18′ N.
Lat. by obſervation arrived at - - 37° 48′ N.
Lat. by dead reckoning arrived at - - 37° 31′ N.
Courſe by dead reckoning S. S. W. $\frac{3}{4}$ W.
Departure 64 weſterly.
Required the true difference of longitude?

RULE.

With the courſe S. S. W. $\frac{3}{4}$ W. and latitudes by obſervation 39° 18′ and 37° 48′, find the difference of longitude 1° 9′ by caſe II. of Mercator's or middle latitude ſailing.

CASE

CASE II.

If the courſe found by dead reckoning be more than three points or 33°, and leſs than five points or 56°.

Lat. by obſervation departed from - -	52° 40′ N.
Lat. by obſervation arrived at - -	54° 22′ N.
Lat. by account - - - - -	54° 04′

Departure weſterly 76 miles.

Required the true difference of longitude?

RULE.

With the latitude by obſervation 39° 18′, and latitude by account arrived at 37° 31′, and the departure 64, find the diſtance 113 by caſe IV. of Mercator's ſailing, and with this diſtance and latitudes by obſervation find a ſecond departure 47.7 by caſe III. of Mercator's ſailing, and with half the ſum of theſe departures 61.8 and the latitudes by obſervation, find the true difference of longitude 1° 43′ by caſe IV. of Mercator's ſailing.

CASE III.

If the courſe by dead reckoning be more than five points or 56°.

Lat. by obſervation departed from - -	38° 52′ N.
Lat. by obſervation arrived at - -	40° 18′ N.
Lat. by account - - - - -	40° — N.

Departure weſterly by account 112 miles.

Required the true difference of longitude?

RULE.

RULE.

With the latitude by obſervation departed from 38° 52′, and that by account arrived at 40° —′, and the departure 112, find the diſtance as above 113, and with this diſtance and the latitudes by obſervation, find the true difference of longitude 2° 10′ by Caſe III. of Mercator's ſailing.

The advantages expected to ariſe from the uſe of this inſtrument are, firſt, that no conſiderable miſtake can exiſt in the work, as the errors become obvious in proportion to their magnitude; whereas in calculations, miſplacing a ſingle figure will produce an error of the greateſt conſequence. Secondly, this method does not reſt on tables or logarithms, which abound in errors of the preſs even in the beſt editions, and are often the ſource of much confuſion. Thirdly, it will render the art of keeping a ſhip's reckoning eaſily learned, and eaſily practiſed. Many maſters of ſhips are perſons who have been raiſed to that office from the loweſt degree of ſeamen, who from want of capacity, or want of early opportunities, have acquired only a very ſuperficial knowledge of navigation; many who have been better educated diſqualify themſelves by their riotous and diſſolute lives for the practice of this intricate branch of ſcience. The accidents of the ſea, and other emergencies, frequently render it neceſſary to commit the care of a ſhip to an ignorant ſailor. In all which caſes the methods of calculating a ſhip's courſe now in uſe can not be adopted or relied on with ſafety, and therefore they generally have recourſe to the following dangerous alternatives:

In coaſting and in ſhort voyages they ſail by day, when they may ſee the land, ſhould they approach it, and *lie to* at night.

In longer voyages they get into the latitude of the port ſailed to by the aſſiſtance of their quadrants, and then ſail eaſt or weſt *(which they term running down their longitude)* until they *ſtumble* as it were on their object.

Every day's experience furniſhes inſtances of the melancholy effects of theſe contrivances; and ſhould this ſcheme on enquiry be found capable of aſſiſting thoſe who are ignorant of better methods, it is humbly expected that the Academy will give their advice and countenance, as far as may be neceſſary, to bring it to maturity, and render it uſeful to ſociety.

OBSERVATIONS *made on the* DISAPPEARANCE *and* REAPPEARANCE *of* SATURN's RING *in the Year* 1789, *with ſome Remarks on his diurnal Rotation. By the Rev.* H. USSHER, *D. D. M. R. I. A. and F. R. S.*

THIS year has been remarkably unfavourable in this country to aſtronomical obſervations in general, but more particularly ſo to thoſe propoſed to be made on the diſappearances and reappearances of Saturn's ring. Read December 5, 1789.

THE firſt diſappearance of the ring in the month of May could not from the ſtate of the weather be obſerved here at all.

AUGUST 21ſt. I obſerved Saturn with the greateſt power of the tranſit inſtrument; the ring itſelf was inviſible, but its ſhadow was viſible upon the body, though very faint.

August 26. Obferved him with the fame power, the ring ftill invifible.

August 27. Obferved him with a forty-inch double object-glafs, before he was near the meridian; ring ftill invifible, at leaft with that power, viz. 61 *, which power I afcertained accurately: rain came on before he paffed the meridian, and the weather continued rainy and cloudy till the 30th.

August 30. The ring was vifible, particularly on the eaft fide, with the forty-inch achromatic mentioned above: the fame alfo with the greateft power of the tranfit inftrument. By comparing the gradual increafe (as far as I can truft to my recollection) I imagine that the ring became vifible fome time in the night of the 28th; I mean to one ufing the fame inftruments that I did.

October 1. The ring was juft vifible on the eaftern fide only with the double achromatic, and likewife with the tranfit inftrument. Storm came on and continued to the 3d, on which night I got an obfervation, but the ring was not vifible with the tranfit inftrument; there was a heavy gale of wind.

Mr.

* It is much to be regretted that the generality of obfervers do not mention particularly that they have afcertained the power of their telefcopes by proper experiments, particularly in obfervations of this fort. I meafured mine by two different methods, viz. by the breadth of the emergent pencil, and alfo by the ufual method of a diftant circle and near parallel lines, the latter method gave 58, the other 61; this I believe beft.

Mr. Newenham of Cork informed me by letter that he ſaw it on the eaſtern ſide to the 5th, when at 10 H. 30′ he loſt ſight of it. This obſervation he made with a reflecting teleſcope of ſeven feet focus, and power 300.

The power I uſed was ſuperior to this; but the tranſit inſtrument being fixed, is badly ſuited to this kind of obſervation, as the planet paſſes the field with great rapidity, and with this inſtrument one cannot purſue it; which is a circumſtance eſſentially neceſſary to diſtinct viſion, as I formerly mentioned to this Academy in a paper publiſhed in the firſt volume of our Tranſactions.

The obſervations which I have made are hitherto ſo badly circumſtanced, that any deduction from them with reſpect to the tables, &c. would be trifling, until the next reappearance, which may either invalidate or fortify ſuch as have been mentioned.

It is, however, worthy of remark at preſent, that Saturn now diveſted of his ring appears exceedingly oblate; much more ſo than I could have ever ſuppoſed from common obſervation whilſt his ring was viſible: and as his rotation has not, that I know of, been yet determined by any ſpots viſible upon his ſurface, it occurred to me that if his equatorial and polar diameters were accurately meaſured, I could infer his time of rotation by means of the 19th Propoſition of Book III. of Newton's Principles, by a deduction from the formula which he there employed, to find the

the proportion of the polar and equatorial dimeters of Jupiter, by comparifon of the denfity and rotation of the earth with thofe of Jupiter.

Let T^2 = the fquare of the time of the fidereal rotation of the earth; and t^2 = the fquare of that of Saturn, the root of which is fought: let Δ = the earth's denfity, and δ that of Saturn; D = Saturn's greater diameter, and d his leffer one. Then from Sir Ifaac Newton's formula I deduce $t = \sqrt{\frac{d \times \Delta}{\delta} \frac{\times 1}{229} \frac{\times 1}{D-d}} \times 23'.56''$

The micrometers ordered for the Obfervatory being not yet arrived, I requefted a gentleman of known accuracy to take thefe meafures for me; he was fo obliging as to fend me a great number, agreeing remarkably together, from which I find Saturn's diameters, reduced to his mean diftance, 18'',12 and 15,855.

From hence, taking Sir Ifaac Newton's ratio of the earth's equatorial diameter and axis, and that of the earth and Saturn's denfities, as by him computed, the formula will give for Saturn's fidereal rotation 10 H. 12' ½.

It is a circumftance worth remarking, that the celebrated Huyghens, in his whimfical and ingenious work, intitled Cofmotheoros, has the following paffage: " Quam habeant dierum longi-
" tudimem (Saturnicolæ fciz.) certo cognofci nequit; fed ex
" comitis intimi diftantiâ ac periodo, exque eorum comparatione
" cum intimo Jovialium; verifimile fit non longiores effe dies
" illas

" illas quam ſint in Jove ſciz. 10 horarum aut paulo minus." We dwell with pleaſure upon the ſlighteſt conjectures of ſuch an enlightened mind.

By the ſame formula I computed the time of his rotation, taking the denſity of Saturn as deduced by Mr. de la Lande in the fourth volume of his Aſtronomy, by which it came out 12 H. 55′ $\frac{1}{3}$.

I computed it alſo, taking this laſt denſity and Mr. Bouguer's ratio of the earth's diameters (which has many advocates) and on this ſuppoſition it came out 14 H. 44′ $\frac{1}{2}$ nearly.

From the great modern improvement of teleſcopes it may reaſonably be expected that his time of rotation will ſoon be determined by actual obſervation; and from the two ſatellites lately diſcovered by the celebrated Mr. Herſchel, his denſity will ſoon be more accurately aſcertained.

And when the denſity of the Georgian planet ſhall have been determined by means of his ſatellites, diſcovered alſo by Mr. Herſchel, perhaps ſome correct law of the denſities may be had; which, if it be found to obtain accurately in thoſe planets which have ſatellites, may, perhaps reaſonably, be extended to thoſe which have none; and enable us to determine the quantity of matter and denſity of Venus, Mars and Mercury, without having recourſe to the decrement of the obliquity of the ecliptic, a matter ſo delicate in itſelf, that even at this day, there

there are ſome who doubt the fact, though ſupported by ſound theory and obſervation.

The following method of determining the quantity of matter in Mars, Venus and Mercury, or any other planet, is certainly true in theory.

The axis major of the earth's orbit as it revolves round the common center of gravity of earth and ſun $= 200000$. The quantity of matter in the earth we aſſume $= 1$, that of the ſun is then 352813, the parallax being allowed 8''.6. Now by Prop. 60, Book I. of Newton's Principles, as $\sqrt[3]{352814} : \sqrt[3]{352813}$ ſo is the axis major of the ellipſe round the common center of gravity, to the axis major of an ellipſe which would in the ſame periodical time be deſcribed round the ſun at reſt, and is therefore had by the analogy.

Venus's axis major (Coſſ. El. Aſt.) round the common center of gravity of the ſun and Venus $= 144662$ by obſervation. Now in the ſubſeſquiplicate ratio of the periodical times of the earth and Venus take the earth's diminiſhed axis major to a fourth quantity, and we have the axis major of the ellipſe which Venus would deſcribe round a fixed ſun; but we had it by obſervation round the common center of gravity.

Then let $\odot =$ the ſun's quantity of matter, $♀ =$ that of Venus, let $m =$ the axis major of her orbit round the common center of gravity, and $I =$ that deduced as above round the fixed ſun.

Then

Then $m : 1 :: \sqrt[3]{☉+♀} : \sqrt[3]{☉}$. And $m^3 : 1^3 :: ☉+♀ : ☉$. Therefore $m^3 - 1^3 : 1^3 :: ♀ : ☉$. And therefore $1^3 : m^3 - 1^3 :: ☉ : ♀$. Q. E. I. from whence the denſity is eaſily deduced. However I freely allow all the difficulties of reducing this to practice.

N. B. If we take the law of the denſities laid down by Mr. de la Lande, viz. as the roots of mean motion, the denſity of Saturn inſtead of ,10448, which is his number, will come out ,18351, much nearer to Newton's, which reduced to this ſcale would be ,16750. But Newton's quantities of matter were neceſſarily defective from applying the ſolar parallax 10",5 inſtead of 8",6.

Account of two PARHELIA *observed February 25th,* 1790. *By the Rev.* HENRY USSHER, *D. D. M. R. I. A. and F. R. S.*

ON Thurſday, February 25, at four in the afternoon, as I was on my way from Dublin to the Obſervatory, I remarked that the ſun's light was uncommonly pale; when getting a view of him clear of the houſes, I ſaw a corona and two parhelia; the parhelia were in the ſun's almacantar, and diſplayed the priſmatic colours, the red being next the ſun; their tails, parallel to the horizon, were about a degree and an half long; there was no luminous circle parallel to the horizon, as ſometimes happens. The corona in which the parhelia were formed was 22° 24' in ſemidiameter, as meaſured by an Hadley's ſextant; but it appeared to me that it was rather elliptical, the longer diameter tending to the pole of the dipping needle, as did manifeſtly that of an halo round the moon which ſucceeded this phenomenon, and which I have frequently remarked before.

Read March 5, 1790.

BUT

But what appears moſt ſingular in this phenomenon is, that in Dublin the corona was manifeſtly priſmatic, but in the clear air of the Obſervatory it was a vivid white. It continued viſible, though gradually decaying, for thirty-five minutes from the time I firſt ſaw it, and was at length obſcured by ragged clouds from the W. S. W. from which point the wind came, and where there appeared a vaſt accumulation of vapour. I could plainly ſee that theſe clouds were far below the ſeat of the parhelia; the whole was ſucceeded by a tempeſtuous night. The barometer had been riſing, but immediately began to fall to the amount of eight-tenths of an inch in the courſe of the night.

As to the elliptic figure of the corona, it is remarked by Opinus that in high latitudes they are more elliptical than in low ones.

The Population of Ireland as taken in the Year 1788.

COUNTIES	Number of houses in the year 1777	Number of houses, each paying: One hearth	Two or more hearths	Number of houses: Paupers, &c. exempted	Belonging to [illegible]	Total number of houses in 1788	Number of houses whereof population taken	Number of souls therein	Names of the officers, who took the population.	OBSERVATIONS.
Antrim	22154	2[illegible]519	3667	1066	3002	28234	—	—		½ taken from abstracts.
Armagh	14[illegible]	17[illegible]86	1363	722	2043	21511	1075	6983	Mr. Mich. Monteith, supervisor, and Messrs. Sam. Reed, John Irwin and J. Beauchamp, hearth-money collectors.	
Cavan	9521	1[illegible]129	[illegible]	[illegible]	2011	13[illegible]79	—	—		Nearly ¼ from abstracts.
Down	27[illegible]67	285[illegible]	31[illegible]	1234	2[illegible]20	35[illegible]2	—	—		
Donegal	13[illegible]43	13348	14[illegible]8	2[illegible]31	2528	17507	—	—		†
Fermanagh	[illegible]819	7749	[illegible]	2[illegible]3	1101	[illegible]83[illegible]	490	3131	Mr. Spence, hearth-money collector.	†
Londonderry	16457	19[illegible]34	1410	963	2415	24580	—	—		Nearly ½ from abstracts.
Monaghan	[illegible]	16451	889	997	1545	19,[illegible]18	994	6467	Messrs. William Spence and R. Rhumbold, hearth-money collectors	
Tyrone	17516	2[illegible]84[illegible]	2032	923	3121	27742	1386	8720	Mr. J. Richardson, supervisor, and N. Winchley and D. O'Brien, hearth-money collectors.	†
Carlow	5696	5534	821	308	18[illegible]6	8529	420	2452	Messrs. P. Walsh and P. Burchall, hearth-money collectors.	
Dublin	21236	[illegible]07	15654	5[illegible]3	1127	25[illegible]6	—	—		
Kildare	8325	[illegible]	[illegible]	33[illegible]	246	11272	—	—		Nearly ⅓ from abstracts.
Kilkenny	12680	134[illegible]6	1478	382	2035	17324	7[illegible]	5139	Thomas Wray, Esq; inspector-general, and P. Godfrey, hearth-money collector	
King's	9401	9[illegible]	1481	4[illegible]8	1814	12881	6[illegible]	3967	Messrs. P. Walsh and J. Mathews, hearth-money collectors.	
Longford	5489	5212	576	212	1922	8[illegible]12	4[illegible]	2348	Mr. Ch. Blunden, supervisor.	†
Louth	7741	7427	771	117	1[illegible]91	9926	—	—		†
Meath	15068	1[illegible]	216[illegible]	[illegible]4	31[illegible]	222[illegible]3	—	—		†
Queen's	11080	1[illegible]	1415	410	143[illegible]	1[illegible]977	—	—		†
Westmeath	12652	9656	1127	424	2037	13239	—	—		† A trifle from abstracts.
Wexford	12928	155[illegible]	2[illegible]03	766	2111	2[illegible]4[illegible]5	1022	6641	Mr. J. Saunders, supervisor, and R. Walsh and Edw. Howlin, hearth-money collectors.	
Wicklow	8[illegible]	10[illegible]46	1364	3[illegible]	2121	1[illegible]81	—	—		
Clare	12364	12[illegible]6	691	262	24[illegible]	[illegible]	715	4584	Tho. Wray, Esq. inspector-general, Mr. H. Bowen, supervisor, and G. Smith, hearth-money collector.	†
Cork	5[illegible]73	45135	8631	1944	7435	6617[illegible]	—	—		†
Kerry	11[illegible]73	12[illegible]	[illegible]	[illegible]	3228	17[illegible]12	901	5676	Mr. S. Frizell, supervisor, and Tul. McGillicuddy, hearth-money collector.	†
Limerick	2[illegible]232	19474	2584	790	312[illegible]	2[illegible]22	13[illegible]	7254	Mr. W. Percy, supervisor, and R. P. Davies, hearth-money collector.	†
Tipperary	1[illegible]951	21836	2736	771	3697	28673	986	6154	Mr. W. Percy, supervisor, and P. Walsh, hearth-money collector.	† A trifle taken from abstracts.
Waterford	[illegible]77	11136	2124	531	1400	15126	625	3917	Tho. Wray, Esq. inspector-general, and J. Bere, hearth-money collector.	
Galway	17[illegible]	17518	1438	357	4187	23490	1146	6409	Mr. J. Sullivan, supervisor, and Wm. Templeton and A. Lowe, hearth-money collectors.	
Leitrim	5[illegible]4[illegible]	[illegible]421	233	128	1219	7001	—	—		½ taken from abstracts.
Mayo	17229	18517	762	959	[illegible]322	25[illegible]	1253	7315	Mr. J. Hennon, supervisor, and J. Carr and J. Gildea, hearth-money collectors.	
Roscommon	8338	12[illegible]6	693	[illegible]	3908	15206	—	—		
Sligo	6152	7461	674	179	2496	10750	—	—		
Totals	448426 in 1777.	452756	6[illegible]225	18824	84679	621484 in 1788	14108	87895	Nearly 6¼ to each house.	

The number of houses returned in 1785 (the year before the new plan was formed) was	397644	53334	—	23344	474222	In 1785 the *new* houses were confounded with the others.
Increase of 1785 over 1777					25[illegible]96	
Increase of 1788 over 1785					147162	

Number of Houses included in the above return of 1788.—Athlone, [illegible]76—Belfast and suburbs, 26[illegible]1—Cork, 87[illegible]—Drogheda, 1731—Galway, 947—Limerick and suburbs, 45[illegible]6—Londonderry, 1642—Kilkenny, 2[illegible]9—District of the Metropolis, 14,327—Waterford, 4[illegible]97—Wexford, 1412—Newry, 1772—Sligo, 916—Youghall, 83[illegible]—Kinsale, 1079—Navan, 854.

The return made to the House of Commons of the same year, 1788, was but 614,457, which falls short of this return by 7027 houses. The cause of this variation is, that we were obliged to take the greater part of the returns from the abstracts, the survey-books not having come up. In a few instances the abstracts contained more than the survey-books, as the [illegible] included the [illegible]. Where [illegible] care has been taken to extract the waste houses in this return (except in the few instances noticed in the column of observations) where we were obliged [illegible] of [illegible] the abstracts. But [illegible] the whole return from the survey-books exceeds that from the abstracts by 7027 houses; the cause whereof is, that the [illegible] in their abstracts [illegible] several houses which they had entered in their survey-books (which are sworn to) and kept the money to themselves.

The method in which the population has been taken is as follows:—I have selected the most intelligent officers, and have requested them to subjoin to each house the number of souls inhabiting the same, but where they should not be able to obtain a clear and satisfactory account, to pass by such house without attempting to give any account whatever of the number of [illegible], rather than give one that should not be thoroughly depended upon. Having the books before me, I first find out the number of widows and paupers in the county; secondly, the number of new houses; thirdly, [illegible] houses with more hearths than one to each; fourthly, the number of houses with a single hearth to each, which pay duty; and when I have sufficient materials I cause a proportionate number of each [illegible] to be extracted, till I have the twentieth part of the houses in the county. Thus in the county of Wexford there are 2111 paupers and widows, so I take the first [illegible] houses [illegible] by persons of that description which appear on the face of the books, with the number of souls annexed to each, 126 being as nearly the one twentieth part of 2111 as I could go without a fraction; then I find the number of new houses to be 766, so I take the number of souls annexed to the first 38 new houses; then I find the number of houses having [illegible] hearths [illegible] 2[illegible]3, so I take the number of souls annexed to the first 1[illegible]3 houses in that description; lastly, I find the number of houses having but one hearth each and paying duty to be 155[illegible], so I take the number of souls annexed to the first 775 of such houses, and I find the account to stand thus:

COUNTY OF WEXFORD.

	Total Number.	Number of houses whereof the population has been taken.	Number of souls therein	
Poor	2111	126	549	About 5¹ to each house.
New	766	38	164	About 4¼ to each.
Double hearths	2[illegible]	1[illegible]3	919	Nearly 9 to each.
Single ditto	155[illegible]	775	5[illegible]3	About 6½ to each.
Totals	2[illegible]448	1022	6641	About 6½ to each house. N. B. This is higher than the general average. Wexford is one of the most industrious counties in the kingdom.

In the other counties where I had sufficient materials I proceeded in the same manner; but where I have not been able to give the population of the twentieth part of the houses of each description, I have given a smaller number of houses of each sort. The foregoing statement of the county of Wexford proves that to neglect the taking a due proportion of each would lead to false conclusions.

The number of houses returned in the year 1788 is certainly far short of the truth. In the thirteen counties marked with a † I have some grounds whereon I have computed the number of houses omitted, to wit, the check-surveys made by supervisors of certain parishes in each walk, and returned upon oath. Whether the hearth-money collectors have been more or less fraudulent in the other parts of their walks than in the parishes thus surveyed, must be a matter of accident; but as these parishes were chosen by myself, without any previous knowledge that there were more or less frauds committed in them than in other places, the probability is that the frauds committed in them have been pretty much upon an equality with those committed in other parts of the same walks collected by the same officers. If the frauds committed through the thirteen counties be equal to those committed in the parishes so surveyed therein, then the number of houses omitted in the said thirteen counties amounts to about 24,8[illegible]; and if any of the supervisors have been guilty of collusion or negligence, the number of houses omitted is probably greater than 24,8[illegible].

To this number must be added the houses omitted in several of the other counties. The frauds committed in some of these appear as great as in the other thirteen counties. My opinion, formed on a variety of circumstances, is that they amount to 20,000. But I can only give my opinion.

The waste houses, or those returned as such, are not included. Many of them, probably some thousands of them, were inhabited. The inhabitants frequently shut their doors, and hide from the collector. He frequently returns houses as waste which have paid the duty.

In the houses the population whereof has been taken, no barrack, hospital, school-house or public building is included.

Whether the other houses in the kingdom contain in proportion an equal number of souls with the 14,108 houses whereof the population has been taken, must be a matter of conjecture. If they do, and if the number of houses amounts to 650,000 (which I believe to be short of the truth) then the number of souls in the kingdom must be computed at above 4,040,000.

An ESSAY *towards ascertaining the* POPULATION *of* IRELAND. *In a Letter to the Right Honourable the Earl of* CHARLEMONT, *President of the Royal Irish Academy.*

By GERVASE PARKER BUSHE, *Esq;* *M.R.I.A.*

MY LORD,

I BEG leave to submit to the Royal Irish Academy a paper compiled in the year 1789 from the survey books of 1788. Imperfect as it is, it may throw some new lights upon the population and resources of Ireland; and the communication of it to so respectable a body may lead to the obtaining farther iuformation upon the subject, under their auspices. It was my intention to have added the increased number of houses returned in 1789; but after having examined some of the books, and after a detection of the frauds committed by several of the hearth-money collectors in that year, I find that should I undergo the labour of totting up the number of houses they have returned, I should still be left to computation and conjecture. Several increases

Read June 5, 1790.

increaſes indeed over the year 1788 have appeared in the ſurvey books of 1789. I have obſerved in the paper that the number of houſes ſuppoſed to be omitted in the thirteen counties marked with a † amounts to 24,800; I have now reaſon to conclude that it amounts to conſiderably more.

The number ſuppoſed to be omitted in 1788 was,		The increaſed number returned in 1789 is,	
In the county Longford -	1000	2143	
In Kerry - - -	1,500	2441	
In Clare - - -	2500	2482	
In Limerick - -	1600	2090	
In Cork - - -	8000	7460	in about *half* the county.
In Tipperary - -	1000	833	in about one *fifth* part of the county.
In Donegal - - -	2500	2304	in about one *fifth* part of the county.

It would be uſeleſs to give an accurate account of returns which I know to be deficient; my utmoſt labour could lead me to nothing more than a tolerable computation. I am perfectly convinced that the houſes ſuppreſſed in 1788 in theſe thirteen counties, which I computed at 24,800, do amount to 40,000; and I have now ſome grounds on which I can compute the number omitted in the remaining nineteen counties. I believe that when I eſtimated them at 20,000, I did not exaggerate. It may eaſily be conceived that there may be ſome official reaſons which

which may make it imprudent to publiſh the particulars, or to divulge to the officers the exact quantum of my expectations from each of them; but I have no objection to ſhewing to any member of the Royal Iriſh Academy the principal grounds of my computations, the returns from which the accounts of population have been taken, the inſtructions to the officers under which theſe returns have been made, and to let them judge of the impartiality and care with which the computations have been formed.

In the paper which I have encloſed I have obſerved that if the number of houſes amounts to 650,000, and if they contain an equal number of ſouls to each with thoſe whereof the population has been returned, then the whole number of inhabitants muſt be computed at above 4,040,000. That we may ſafely add 30,000 houſes to the computed number of 650,000, I have no doubt whatever; but as to the number of ſouls which ſhould be allowed to each houſe, I can by no means ſpeak with equal confidence. Since the compilation of the paper I have received ſome farther returns.

Counties.		No. of Houfes.	No. of Souls.	Perfons from whom I received the Returns.
City of Dublin, taken in Spring	Merrion-fquare -	33	417	P. Hackett, hearth-money collector.
	Sackville-ftreet -	59	656	T. Vaughan, hearth-money collector.
Taken in Autumn	Kevin's and Audoen's Parifhes -	723	7910	Hackett and Nugent, hearth-money collectors.
Dublin county - - -		1808	10329	H. Haughton, hearth-money collector.
Cork city - - - -		1566	14193	J. Croker, Efq; infpector of hearth-money.
Cork county, about Fermoy -		1139	6835	W. Boyce, hearth-money collector.
Ditto, about Baltimore - -		100	521	Counfellor Grace.
Waterford - - - - -		180	1135	L. Walfh, hearth-money collector.
Kildare - - - - -		189	1069	Tho. Thompfon, hearth-money collector.
Ditto - - - - - -		1264	7033	Mr. R. Barker, hearth-money collector.
Londonderry - - -		621	3148	Mr. D. Downing, fupervifor of hearth-money.
Donegal - - -		554	4072	Wm. Campbell, hearth-money collector.
Tyrone - - - - -		235	1508	From a return made to the Hon. Thomas Knox many years ago.
		8471	58796	

In thefe returns the proportions adhered to in the paper are *not* obferved. The population of Merrion-fquare and Sackville-ftreet, and of the many other parts of Dublin, is fictitious. The fame perfons are counted over again in the country. And whether the fouls contained in the Barracks, College, fchools and public buildings of Dublin (which have not been counted) may make up for the number which migrates during the fummer, is a matter a matter which I have not at present ſufficient materials to determine. Upon the whole, theſe returns of population exceed the returns in the paper; but in my opinion we ſhould be nearer the truth by forming our conjectures upon the paper alone, than by adding theſe to them.

To acquire complete knowledge upon the ſubject requires more aſſiſtance than I can expect from thoſe officers who are concerned in the collection of hearth-money. The office of a tax-gatherer is not popular, and he has not the ſame facility of acquiring information which the gentlemen of the country or the perſons employed by them would have, if ſome of them ſhould think proper to aſſiſt in the enquiry, and to return liſts of the number of perſons contained in the houſes of their reſpective neighbourhoods. To the hearth-money collector the people frequently refuſe an anſwer. The wealthy, or their ſervants, often think the queſtion impertinent, and the poor often ſuſpect that it is aſked with ſome bad deſign. From careleſſneſs, and from ſuſpicion, ſome of the inhabitants are frequently forgotten in the enumeration. I cannot ſay that in any place where I have checked the returns by a partial return made by other officers, I have found the numbers to have been exaggerated. In a country where the local population varies ſo much in the different parts of the ſame county, it ſeems to me impoſſible to arrive at any thing like accuracy, unleſs we ſhall be able to obtain returns from different parts of the ſixteen counties where the population has not yet been taken. Should they fall ſhort of the returns I have received, perhaps the population of the kingdom may ſtill be eſtimated at above four millions. The inhabitants of 30,000 houfes may certainly be added to the number conjectured in the paper, and together with the inhabitants of the public buildings, they would make up for confiderable deficiencies.

I COULD have wifhed to have marked the proportionate progrefs of building and of population in the different parts of the kingdom, and to have traced the effects of the linen manufacture, and of encouraged or neglected agriculture; but after much examination I find myfelf more capable of warning others againft falfe conclufions, than of leading them to true ones. The increafes which appear on the face of the paper are no evidence whatever of the proportionate increafe of each county; they depend principally on the accident of having had lefs faithful officers in a particular diftrict in 1777, or on being more fortunate in 1788. The reader who fhall conclude that the county of Monaghan, where the houfes appear to have almoft been doubled in number fince 1777, has made a greater progrefs in population than the county of Limerick has made in the fame period, will draw his conclufion from very uncertain premifes.

ACCORDING to Sir William Petty, the houfes amounted in 1672 to but 200,000, of which 160,000 had no fixed hearth, 24,000 had but one chimney, and 14,000 had more chimnies than one, and the people were but 1,100,000. If we can believe that under a variety of difadvantages which are now removed, Ireland did in an hundred and fixteen years more than treble her population, our expectations of future increafe may be great indeed. But I cannot give implicit credit to the returns of the number

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of houſes in the time of Sir William Petty. When I reflect that in 1786, when ſworn officers had been appointed to collect the duty, and after the frauds of ſeveral of them had been detected and puniſhed, there were houſes ſuppreſſed to the number of near two hundred thouſand, can I ſuppoſe that the liſts formed in 1672, under leſs effectual laws, and a more imperfect method of calculation, could have been free from fraud and error? In many reſpects, however, Sir William Petty's tract contains true and valuable information; and I believe that he was a writer to be much relied upon as to any matter which he *could* know. I can readily believe that the houſes of Ireland, though probably more numerous, were to the full as wretched as Sir William Petty repreſents them. The perſons who then collected hearth-money could have had no temptation to return houſes as having *no* fixed hearths which *had* fixed hearths. An houſe without a fixed hearth was not exempted from duty, but by the 17th and 18th of Charles II. it was to be charged as having *two* hearths. It ſhould ſeem that the legiſlators of that day were ſenſible of the wretchedneſs of their fellow-ſubjects, and thought double taxation an admirable receipt for curing poverty. Theſe 160,000 houſes, the common abodes of the Iriſh peaſantry, are deſcribed by Sir William Petty as not worth five ſhillings each building. Compare Sir William Petty's account with the encloſed paper, and we may contemplate with pleaſure the progreſs of Iriſh proſperity. Sir William Petty's computation of $5\frac{1}{2}$ to each houſe, I ſuppoſe to have been founded on ſome enquiry, though he does not ſtate the grounds on which he formed it. Ireland was at that day a country of paſturage, and the cauſes which appear to me to account for a greater number of ſouls being generally found in the houſes of Ireland at preſent, did not exiſt in the time of Sir William Petty. Timber was probably more plenty; the wretched habitations, worth but five ſhillings each, could not hold two families; and another houſe, ſuch as the peaſant was accuſtomed to, was eaſily built. Probably he had neither a male nor a female ſervant, as is uſual with many of the peaſants in the tillage counties; nor an apprentice, as is cuſtomary in the North.

If the actual population of Ireland ſhould be found greater in proportion to the number of the houſes than that of other countries, perhaps the following cauſes may account for the fact. The great body of the people who inhabit 450,756 houſes of one hearth each, paying the duty (as they are returned in the paper), but who probably inhabit 500,000 houſes, appear to me to have generally arrived at that ſtate of induſtry wherein a man becomes valuable, and the price of his labour is on the increaſe; but not at that ſtate of proſperity which would enable them to procure as comfortable habitations as may be ſeen in other countries. They are almoſt all married, and there are few inſtances of their not having children. They generally marry young; and potatoes being their general food, they are under no apprehenſions of being unable to ſupport their children; perhaps too for children there is no food ſo good. Theſe children are their wealth; the father, therefore, is in no hurry to part with his ſon till he marries, and will ſtay with him no longer. Even after marriage the ſon or the daughter frequently lives with the parents.

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The houſes are built, not by the landlord, but by the tenant; and to build an houſe for his family is not always an eaſy matter to an Iriſh peaſant: The ſcarcity of timber and of watling is an additional impediment. In the tillage counties (and tillage is becoming very general) if the peaſant arrives at better circumſtances, it is after a courſe of painful induſtry; and money ſo acquired is not readily parted with. He is more deſirous of taking more land than of improving his houſe. He lives in the ſame ſort of habitation that he has been uſed to, and that his poorer neighbours inhabit; but he hires labourers and ſervants who live in his houſe; and increaſes the number of his houſehold without increaſing their accommodations. However I may contradict the opinions of others as to the nature of the Iriſh peaſantry, I will venture to ſtate what I have obſerved in a county where tillage has taken root. I think that no peaſantry can be more induſtrious; and the continued frugality of thoſe who have grown rich is perfectly aſtoniſhing. The ſhortneſs of tenures is another cauſe that the peaſantry are unwilling to build houſes in ſeveral parts of the kingdom, and of courſe of families not ſeparating early. I am happy to obſerve that in my own memory the peaſantry have grown more intelligent, more ſturdy, and have more confidence in the laws than they uſed to have. But I am informed, and I cannot diſbelieve, that in thoſe parts where the lands are let to middle-men, the peaſant wiſhes ſtill to conceal his ſubſtance, and eſpecially that he is unwilling to build a good houſe, which would only increaſe the rent of his farm, of which he generally has but a ſhort tenure.

Upon the whole, I believe that more people are crowded into one houſe in Ireland than in other countries, for two reaſons; firſt, that many cannot afford to build; and ſecondly, becauſe thoſe who can, have many motives to prevent them. Notwithſtanding this the new buildings in an increaſing country cannot be inconſiderable. We ſee by one of the columns in the paper that they amount to 18,824 in one year; and I believe that column to approach nearer to the truth than any other in the paper. In order to know the real annual increaſe of houſes, we ſhould deduct from that number the houſes which have gone to decay; and there is ſo much fraud and error in the accounts of them, that I can give nothing more than a looſe conjecture. I believe they may amount to about nine thouſand. Perhaps there may be another cauſe for the number of people being ſo great in proportion to the number of houſes; in my opinion the being without poor rates has that effect. When the labourer dies, his houſe is often broke up, and his family is divided amongſt the neighbouring peaſantry. Thoſe who are able to work hire with them as ſervants; and even thoſe who are not ſo, are readily taken by their neighbours, both from kindneſs, and from the hope of future aſſiſtance from their labours. Even in the houſes inhabited by widows and paupers I do not find the population to run very low, but it is very unequal. In one houſe I frequently find but one poor widow; in the next the remains of two, three or four families living together. In the newly inhabited houſes the population runs low; the cauſe is that many of them are not *fully* inhabited, which ſinks their general average; ſometimes there is but one ſervant in the houſe. In the houſes of two hearths and upwards, inhabited by the gentry,

gentry, and by wealthy farmers and manufacturers, the population uniformly runs the higheſt.

I have the honour to be,

With the greateſt reſpect,

My Lord,

Your Lordſhip's moſt humble ſervant,

G. P. BUSHE.

July 5th, 1790.

LETTRE *de Monſ.* POUGET *à Monſ.* KIRWAN, *F.R.S.* *&* *M.R.I.A. ſur les* CONDENSATIONS *produites par* L'ALLIAGE *de* L'ALKOOL *avec* L'EAU.

A Montpellier le 12 Mai, 1783.

MR. CHAPTAL mon ami qui m'a procuré, Monſieur, le plaiſir de lire la ſeconde partie de votre excellent mémoire ſur les peſanteurs ſpécifiques des ſubſtances ſalines, m'a dit qu'il vous avoit parlé du travail que j'ai fait ſur celle de l'eſprit de vin, et que vous deſiriez de le connoître; je m'empreſſe à vous en préſenter un extrait, et je ſouhaite vivement que vous y trouviez quelque choſe d' intéreſſant. Lû le 6 Juin, 1789.

Frappé de l'imperfection de tous les Aréomêtres qu'on employe communément dans l'uſage du commerce pour déterminer les degrés de ſpirituoſité des eaux-de-vie, j'ai voulu chercher les moyens d'en conſtruire un meilleur, et c'eſt uniquement dans cette vue d'utilité pratique que je commençai mes recherches. Mais en m'occupant à reconnoître les peſanteurs ſpécifiques de ces fluides, et les proportions des augmentations de denſité produites par l'alliage de l'alkool avec l'eau, je conçus l'eſpoir de parvenir à trouver la loi générale de ces pénétrations ou abſorptions d' une liqueur par l'autre, et je me fis en conſéquence le plan d' un très grand travail dabord ſur les liqueurs ſpiritueuſes, enſuite ſur les acides et toutes les ſubſtances ſalines en général; ſur les métaux et enfin tous les corps qui acquiérent dans leurs alliages mutuels une augmentation de denſité; perſuadé qu'il doit y avoir à cet égard une loi générale dans la nature qui tient à celle de l' attraction et dont la découverte pourroit donner de grandes connoiſſances ſur les affinités chimiques. Mais des circonſtances particulieres m' ayant empeché alors d' entreprendre ce travail et même d'achever celui que j'avois commencé ſur les Eaux-de-vie, je n' ai pas pu m'en occuper depuis plus de dix ans, et j'attendois une occaſion favorable pour le reprendre. Je me félicite aujourdhui de ne l' avoir pas fait, puiſque vous travaillez ſur cet objet intéreſſant et que vous avéz déjà fait de très grands pas dans cette carriere. Vous la parcourrez ſurement avec plus de ſuccès que je n'aurois pu le faire, et je ſuis perſuadé que ſi vous continuez à vous en occuper, nous vous devrons la découverte d' une loi de la nature encore inconnue. Agréez je vous prie l' hommage des eſſais que j'ai tentés pour parvenir à connoître un des faits qui peuvent y conduire.

Dans l' éxamen de la pesanteur spécifique des Eaux-de-vie ou des mêlanges d' alkool et d'eau, j'ai été dabord arrêté par une très grande difficulté, celle de déterminer les termes fixes et de décider ce qu'on doit prendre pour alkool pur. Je considere cette liqueur comme une mixte dont l'eau est un des principes constituans, ensorte qu'on ne peut la lui enlever entièrement sans le détruire et en faire une substance différente, comme l' Ether.

La volatilité de l' alkool plus grande que celle de l' eau, ou sa plus grande affinité avec le feu, donne le moyen de séparer une partie de l'eau surabondante. Mais il n'y a pas de procédé, du moins je n'en connois pas, par lequel on soit assuré d'enlever exactement toute cette eau surabondante, en laissant l'eau principe, de maniere que l'alkool soit aussi déflegmé qu'il est possible sans avoir souffert d'altération et de changement dans ses propriétés. J'ai cru ne devoir pas m'occuper longtems de cette recherche et me suis contenté d' examiner les alliages d'eau et d'alkool quelconque dont la pesanteur spécifique seroit dans un rapport déterminé avec celle de l'eau distillée, et pour cela prenant de l'esprit de vin foible du commerce, très pur : c'est à dire, qui n'étoit point altéré par le mélange des substances étrangères, et aussi débarassé qu'il étoit possible de l' huile du vin (qui y est souvent en assez grande quantité pour être reconnue par le gout Empireumatique :) je l' ai rectifié par plusieurs distillations successives au bain marie, et il a été réduit à peu près, au 15ieme de la premiere quantité. La pesanteur spécifique de cet alkool étoit à celle de l' eau comme 0.8199, à 1, à la température de 15 degrés du thermomêtre de mercure de Reaumur.

Vous avez toujours fait vos alliages d' eau et d'acide en mêlant enfemble des poids déterminés de ces liqueurs. J'ai fuivi une méthode différente. Et confidérant la pefanteur fpécifique feulement comme un moyen de reconnoître l'augmentation de denfité, ou la diminution de volume, j'ai cru parvenir plus directement au but que je me propofois, en déterminant les quantités de liqueur qui devoient former l'alliage, non par rapport à leurs poids, mais par rapport à leurs volumes, et j'ai compofé dix mélanges contenant le premier 9 mefures d'alkool et 1 d'eau; le fecond 8 mefures d'alkool, 2 d'eau; et ainfi de fuite jufques au dernier qui ne contenoit qu' une mefure d' alkool et 9 d'eau. Mais comme les mefures actuelles font toujours incertaines, j'ai employé la balance pour déterminer les pefanteurs fpécifiques reconnues, ainfi 10,000 grains d' eau et 8199 d'alkool formoient un mélange à parties égales en volume.

Après que tous ces mélanges ont été bien faits et parfaitement refroidis (car j'ai reconnu, ainfi que vous l'avez fait par rapport aux acides, qu'il faut un tems affez long pour l'alliage complet, furtout lorfque l'eau eft en grande proportion) la pefanteur fpécifique de ces liqueurs a été reconnue au moyen de la balance hydroftatique. J'ai employé auffi un autre inftrument qui me paroît encore plus propre à donner des réfultats exacts, principalement lorfqu'on ne veut avoir que des rapports et qu'on ne cherche pas à reconnoître la pefanteur réelle d'un volume déterminé de la liqueur. C'eft l'Aréomètre de Farenheit. Vous favez qu'il a une tige très courte, fur laquelle il n'y a qu'un feul point marqué et furmonté d'un petit godet deftiné à recevoir les poids qu'on y place pour faire caler l'aréomètre

toujours

toujours jusques au même point, ensorte qu'il mesure toujours aussi des volumes de liqueur égaux, dont le poids est celui de l'aréomêtre plus ceux ajoutés dans le godet. C'est donc une vraie balance hydrostatique; mais avec laquelle on a l'avantage de pouvoir peser des volumes de liqueur aussi grands que l'on veut, et par conséquent de diminuer les erreurs sans risquer de fatiguer le fléau d' une balance par un poids excessif, et de la rendre moins sensible. Il est vrai qu'il faut prendre quelques précautions dans l'usage de cet instrument, comme de lui donner la même température qu' aux fluides qu'on veut peser, surtout s'il est de métal; d'employer des vases assez grands pour qu'il soit suffisamment éloigné des bords, d' empêcher que dans les balancemens le godet ne touche à la surface de la liqueur, dont l'attraction produit alors un effet sensible, ce qui pourroit indiquer un moyen de mesurer la force de l'attraction des fluides avec différens corps. Mais comme vous êtes bien plus versé que moi dans l'art des expériences, je crois inutile d' entrer dans beaucoup de détails à cet égard. J'observerai seulement qu'il est essentiel de construire cet aréomêtre de manière qu'on ne soit pas obligé d'ajouter beaucoup de poids dans le godet pour le faire caler, parcequ'alors il ne peut pas conserver son équilibre, et il arrive quelquefois qu'il se renverse. Il faut donc, lorsqu'on veut examiner des liqueurs dont la pesanteur spécifique differe beaucoup, trouver les moyens de lester plus ou moins l'aréomêtre sans changer son volume. Celui dont je me servois, et dont le volume étoit d'environ trois pouces cubes, étoit construit en cuivre, et j'avois fait faire plusieurs poids en forme d'olives qu'on pouvoit visser au bas de l'instrument et substituer l'un à l'autre. Leurs volumes étoient parfaitement égaux et leurs pesanteurs différentes.

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Il faudroit pour les acides en conſtruire un de verre, dont la tige ſeroit un tube mince et ouvert, terminé par un petit entonnoir, par lequel on verſeroit le mercure néceſſaire pour leſter l'inſtrument à chaque expérience, de maniere qu'on ne fût obligé de placer enſuite dans cet entonnoir ſervant de godet que de très petits poids pour faire plonger cet aréomètre juſqu' à la ligne marquée avec un diamant ſur le tube.

J'en ai peut-être trop dit ſur cet objet; mais comme il paroît par votre lettre à Mr. Chaptal que vous deſirez de connoître les moyens que j'ai employés pour determiner les peſanteurs ſpécifiques, j'ai cru devoir décrire les inſtruments dont je me ſuis ſervi.

Connoissant les peſanteurs ſpécifiques réelles, ou comme vous dites phyſiques, des mélanges d'alkool et d'eau, prenant une moyenne entre un très grand nombre d'obſervations toutes faites à la même température de 15 degrés, et les comparant aux peſanteurs ſpécifiques mathématiques calculées directement par la ſimple regle d'alliage, on peut en conclure l'augmentation de denſité ou la diminution de volume produite dans la maſſe totale par la pénétration mutuelle des fluides. Car nommant A la peſanteur ſpécifique phyſique, B la peſanteur ſpécifique mathématique, n le nombre des meſures qui compoſent la maſſe totale, $n-x$ celui auquel il eſt réduit par l'effet de la pénétration, il eſt évident, puiſque cette augmentation de denſité ne diminue pas le poids de la maſſe totale, que $nB=\overline{n-x}\times A$. donc $x=\frac{A-B}{A}\times n$, ou faiſant $n=1, \frac{A-B}{A}$. qui exprime les diminu-

tions

tions du volume total ou les quantités de fluide abſorbé pendant l'alliage.

La table ſuivante contient les réſultats de mes expériences, ou les diminutions du volume total ſuppoſé $= 1$ de chacun de mes mêlanges calculées ſuivant cette formule. Je les ai calculées de même d'après les expériences faites par Mr. Briſſon, et qu'il publia dans le tems même où je m' occupois de ce travail. Mémoires de l'Académie des Sciences de Paris Vol. de 1769.

Suivant mes expériences.		
Nombre de meſures		Diminutions du volume total $= 1$.
d'Eau.	d'Alkool.	
1	9	0.0109
2	8	0.0187
3	7	0.0242
4	6	0.0268
5	5	0.0288
6	4	0.0266
7	3	0.0207
8	2	0.0123
9	1	0.0044

Suivant

Suivant les Expériences de Mr. Briffon.		
Nombre de mefures d'Eau.	Nombre de mefures d'Alkool.	Diminution du volume total = 1.
1	15	0.0063
2	14	0.0115
3	13	0.0157
4	12	0.0189
5	11	0.0215
6	10	0.0235
7	9	0.0251
8	8	0.0257
9	7	0.0243
10	6	0.0217
11	5	0.0189
12	4	0.0144
13	3	0.0099
14	2	0.0057
15	1	0.0021

Les diminutions totales ſont moins grandes ſuivant les expériences de Mr. Briſſon, parceque l'alkool qu'il a employé étoit moins déflegmé que le mien.

Vous voyez d' après ces tables que les nombres qui expriment les diminutions de volume ſuivent une marche réguliere. Les plus grands répondent au mêlange de parties égales, et les autres vont en décroiſſant depuis ce terme juſqu' aux deux extrêmes de la progreſſion. Il paroît donc certain qu'elles ſont déterminées par une loi générale. J'ai tâché de la découvrir et de concilier différentes hypothéſes avec ces obſervations; je me ſuis enfin arrêté à celle que je vais vous expoſer.

Considerant l'alkool comme diſſous par l'eau qui en a abſorbé et retient une partie dans ſes pores (Sans attacher d'ailleurs un ſens bien ſtricte à ce mot de *diſſolution*), La quantité abſorbée doit être en raiſon de celles du diſſolvant et du corps diſſous, et chaque meſure d'eau retiendra des quantités d'alkool proportionelles au nombre de meſures de cette liqueur qui feront entrées dans le mêlange. Ainſi, par exemple, dans un alliage formé de 9 meſures d'alkool et une d'eau, cette meſure d'eau abſorbera une quantité d'alkool qui ſera comme 9: et dans un autre mêlange de 8 meſures d'alkool avec 2 d'eau, chaque meſure d'eau abſorbera une quantité qui ſera comme 8; mais qu'il faut multiplier par 2, nombre de meſures d'eau, pour avoir la diminution totale du volume. Par conſéquent ces diminutions de volume de chaque mêlange feront entre elles en raiſon compoſée

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des nombres de mefures d'alkool et d'eau qui le forment, et dans la table de mes expériences comme, 1×9, 2×8, 3×7, 4×6, 5×5, &c. Dans celle des expériences de Mr. Briffon comme 15×1, 14×2, 13×3, 12×4, &c. et en général prenant pour conftante la diminution du volume à parties égales, le nommant c, nommant auffi n le nombre total de mefures, x celui des mefures d'alkool d'un mélange quelconque, et z l'augmentation de denfité ou diminution de volume de ce mélange, on aura, $c : z :: \frac{n}{2} \times \frac{n}{2} : \overline{n - x} \times x$. et $z = \frac{4c}{n^2} \times \overline{nx - x^2}$. ou faifant $n = 1$, $4cx - 4cx^2$. J'ai calculé d'après cette formule les augmentations de denfité de tous les mélanges des tables ci-deffus, en prenant dans chacune le terme qui répond à l'alliage à parties égales pour conftant. La table fuivante contient les réfultats de ces calculs.

Suivant

Suivant mes Expériences.			
Nombre de mesures d'Eau.	Nombre de mesures d'Alkool.	Diminutions réelles ou physiques du volume total.	Diminutions calculées.
1	9	0.0109	0.0103
2	8	0.0187	0.0184
3	7	0.0242	0.0242
4	6	0.0268	0.0276
5	5	0.0288	
6	4	0.0266	0.0276
7	3	0.0207	0.0242
8	2	0.0123	0.0184
9	1	0.0044	0.0103

Suivant les Expériences de Mr. Briſſon.			
Nombre de meſures d'Eau.	Nombre de meſures d'Alkool.	Diminutions réelles ou phyſiques.	Diminutions calculées.
1	15	0.0063	0.0060
2	14	0.0115	0.0112
3	13	0.0157	0.0157
4	12	0.0189	0.0193
5	11	0.0215	0.0221
6	10	0.0235	0.0241
7	9	0.0251	0.0253
8	8	0.0257	
9	7	0.0243	0.0253
10	6	0.0217	0.0241
11	5	0.0189	0.0221
12	4	0.0144	0.0193
13	3	0.0099	0.0157
14	2	0.0057	0.0112
15	1	0.0021	0.0060

Les augmentations de denſité ou diminutions de volume, calculées d'après ma formule, ſont aſſez exactement d'accord non ſeulement avec mes expériences, mais encore avec celles de Mr. Briſſon pour tous les mêlanges où l'alkool eſt en plus grande quantité que l'eau, et on peut en conclure, ce me ſemble, que la théorie que je viens de donner eſt vraie à cet égard. Mais il n'en eſt pas de même pour les alliages où l'eau eſt en plus grande proportion. Leurs augmentations de denſité réelles ſont beaucoup moindres que le calcul ne l'indique, et les différences deviennent d'autant plus conſidérables que la quantité d'eau eſt plus grande. Par l'hypotheſe et ſuivant la formule $z=\frac{4c}{n^2}\times\overline{nx-x^2}$, le plus grand des termes doit être celui qui répond au mêlange de parties égales, et les inférieurs doivent être égaux aux ſupérieurs chacun à chacun. Cependant cela n'eſt point; et les termes inférieurs de la table décroiſſent ſuivant l'expérience beaucoup plus rapidement que les ſupérieurs. Si on diviſe les termes par les nombres correſpondans, qui expriment les quantités d'alkool contenues dans chaque mêlange, les quotiens indiqueront combien chaque meſure d'alkool a perdu de ſon volume dans l'alliage, et ces nombres devroient par l'hypotheſe former une progreſſion arithmétique croiſſante, puiſque le nombre de meſures d'eau augmente dans cette proportion. Elle eſt en effet aſſez réguliere depuis le premier terme juſqu' à celui de mêlange de parties égales. Mais enſuite les différences deviennent ſucceſſivement plus petites et même enfin négatives. Il paroît donc que lorſque la quantité d'eau eſt plus grande que celle de l'alkool, la loi de cet alliage et de l'abſorption eſt

troublée,

troublée; qu'il y a ici une caufe qui agit en fens contraire de la premiere et s'oppofe à fon effet. On a en Phifique plufieurs exemples de phénomènes femblables, où deux caufes l'une pofitive l'autre négative agiffent en même tems, comme Mr. de Luc l'a dévelopé par rapport à la dilatation et à la condenfation de l'eau. Je penfe que la caufe négative eft dans ce cas-ci l'attraction refpective des parties de l'eau, qui tend à les faire demeurer raffemblées, et s'oppofe par conféquent à leur réunion avec une autre fubftance. Elle eft vaincue par l'effet de l'affinité plus forte de l'alkool, lorfque la maffe de cette derniere liqueur eft plus grande que celle de l'eau, ou au moins égale. Mais quand le volume d'eau devient très grand par rapport à celui d'alkool, alors cette caufe négative, qui n'eft plus fuffifamment contrebalancée, commence à agir et à diminuer fucceffivement les effets de l'attraction de ces deux liqueurs. Je vous prie de confidérer, Monfieur, que je ne préfente ceci que comme une conjecture, mais qui me paroît affez vraifemblable et confirmée par quelques expériences. Je n'en citerai qu'une bien connue de tous ceux qui font le commerce des eaux-de vie. C'eft l'extrême difficulté qu'on trouve à mêler parfaitement une petite quantité d'efprit de vin à celles qui font trop foibles. Le mélange ne fe fait jamais bien complettement à moins qu'on n'agite et qu'on ne roule les barriques: et j'ai reconnu quelquefois, même après plufieurs jours, que l'efprit de vin ne s'étoit pas également répandu dans toute la maffe. Mais il ne fuffit pas de connoître la caufe générale des différences d'augmentation de denfité entre les alliages qui contiennent plus ou moins d'alkool que d'eau; il faudroit encore déterminer la maniere dont cette caufe agit, et en découvrir la loi.

C'eft

C'eſt ce dont je me ſuis occupé. Je croyois même l'avoir trouvée, lorſque je fus obligé d'abandonner ce travail, avant que d'avoir pu faire les expériences néceſſaires pour décider la queſtion. Ainſi cette partie de mes eſſais eſt trop incomplete pour que je puiſſe la mettre ſous vos yeux. Mais je crois pouvoir poſer en principe que dans tous les mélanges d'alkool et d'eau, où l'alkool forme au moins la moitié de la maſſe totale, les augmentations de denſité, ou ce qui eſt la même choſe, les diminutions de volume ſont entre elles, comme les produits des nombres qui expriment les proportions d'alkool et d'eau, qui forment le mélange et peuvent être repréſentées par la formule $z=\frac{4cnx-4cx^2}{n^2}$.

J'OBSERVERAI que cela me ſuffiſoit pour remplir l'objet que je me propoſois d'abord; parce qu'il n'y a pas dans le commerce d'eau-de-vie moins ſpiritueuſe que mon mélange à parties égales. Ainſi on peut au moyen de cette formule déterminer les degrés de ſpirituoſité de toutes les eaux-de-vie et eſprits de vin du commerce; ou en d'autres termes trouver quel eſt le nombre de parties d'eau et de mon alkool dont on peut les ſuppoſer compoſées et pour cela faiſant,

Le nombre de meſures de la maſſe entiere, ou le volume total - - - - $=1$.

Celui des meſures d'alkool contenues dans un mélange quelconque - - - $=x$

La

La diminution du volume du mêlange à parties égales reconnue par l'expérience - - $=c$

La diminution du volume du mêlange contenant x mesures d'alkool est par l'hypothese - - $=4cx-4cx^2$

La pesanteur spécifique de l'eau - - $=a$
celle de l'alkool - - - - - $=b$
celle du mêlange inconnu - - $=y$

On aura, puisque l'augmentation de densité ne change rien au poids de la masse, $\overline{1-x}\times a+bx=\overline{1-4cx+4cx^2}\times y$.

Equation de laquelle on peut conclure la valeur de x, ou la proportion d'alkool, si on connoît la pesanteur spécifique du mêlange; ou déterminer cette pesanteur spécifique, valeur d'y, si on connoît les proportions d'alkool et d'eau.

$$x=0.5-\frac{a-b}{8cy}+\sqrt[2]{\frac{a-y}{4cy}+\left(\frac{a-b}{8cy}-0.5\right)^2}$$

$$y=\frac{a-ax+bx}{1-4cx+4cx^2}$$

et faisant $a=1$, $b=0.8199$, $c=0.0288$.

$$x=0.5-\frac{0.1801}{0.2304y}+\sqrt[2]{\frac{1-y}{0.1152y}+\left(\frac{0.1801}{0.2304y}-0.5\right)}$$

$$y=\frac{1-0.1801x}{1-0.1152x+0.1152x^2}.$$

Je

Je vais ajouter ici un exemple de la maniere dont on peut employer ces formules. Ayant pesé un esprit de vin foible, nommé ici dans le commerce *trois-cinq*, et deux especes d'eau-de-vie, leurs pesanteurs spécifiques étoient à la température de 15 degrés. *Trois-cinq*, 0.8585—*Premiere eau-de-vie*, 0.9275—*Seconde eau-de-vie*, 0.9295. Substituant ces nombres successivement dans la formule de la valeur de x, on en conclut la quantité de mon alkool que chacune de ces trois liqueurs contient: Sçavoir: Le *trois-cinq*, 0.854—*La premiere eau-de-vie*, 0.549—*La seconde eau-de-vie*, 0.539.

Ayant ensuite formé deux mêlanges l'un composé de parties égales de *trois-cinq*, et de *premiere eau-de-vie;* l'autre aussi de parties égales de *trois-cinq*, et de la *seconde eau-de-vie*, les quantités d'alkool, que chacun de ces mêlanges contenoit, étoient pour le premier mêlange $\frac{0.854+0.549}{2}=0.7015$. Second mêlange $\frac{0.854+0.539}{2}=0.6965$.

Et substituant ces nombres dans la formule de la valeur de y, on a les pesanteurs spécifiques de ces alliages, qui sont peu différentes de celles que je leur ai reconnu à la balance hydrostatique, et l'une et l'autre s'éloignent beaucoup des pesanteurs calculeés par la simple regle d'alliage.

	Pesanteurs spécifiques calculées d'après ma formule.	Pesanteurs spécifiques réelles ou physiques.	Pesanteurs spécifiques calculées par la regle d'alliage.
Premier mélange. Parties égales de *trois-cinq* et de la *premiere eau-de-vie* -	0.8952	0.8951	0.8930
Second mêlange. Parties égales de *trois-cinq* et de *la seconde eau-de-vie* - -	0.8964	0.8970	0.8940

Je me proposois de faire un grand nombre d'expériences de ce genre, d'examiner les pesanteurs spécifiques de toutes les especes d'eaux-de-vie du commerce, de les mêler à différentes doses entre elles, avec les esprits de vin et avec l'eau, afin de me bien assurer si ces formules donnoient toujours les pesanteurs spécifiques avec une exactitude suffisante pour la pratique lorsque les mêlanges ne contiennent pas plus de moitié d'eau. Enfin j'avois commencé des tables d'alliage pour déterminer les doses des mêlanges qu'on est dans l'usage de faire des eaux-de-vie trop foibles avec d'autres plus spiritueuses, des esprits de vin avec l'eau, &c. Mais ce travail n'est point terminé.

Si vous pensez, Monsieur, que la route que je suivois peut conduire au but que je me proposois pour parvenir a la connoissance des pesanteurs spécifiques de ces liqueurs, et que vous desiriez d'éclaircir d'éclaircir entièrement la queſtion, je crois que vous y parviendrez aiſément ſoit en faiſant de nouvelles expériences ſur l'alkool, ſoit en vous occupant des acides. Il me ſemble même qu'il ſera plus facile de réuſſir par ce moyen, les différences des peſanteurs ſpécifiques ſont plus grandes, les mélanges moins ſujets à s' altérer par l'évaporation, et l'affinité avec l'eau bien plus marquée. J'ai lieu de ſoupçonner que les augmentations de denſité ſuivront la même loi. J'ai trouvé des rapports aſſez juſtes en réduiſant quelques uns des termes des tables contenues dans votre mémoire à la même forme que les miennes; mais pour s' en aſſurer parfaitement, il faudroit faire une ſuite d'expériences particulièrement deſtinées à cet objet, déterminer les doſes par le volume, et ſurtout compoſer un aſſez grand nombre de mélanges où l'eau fût en moindre quantite que l'acide, afin de connoître la loi de la pénétration dans ce cas là qui eſt le plus ſimple, enſuite augmenter ſucceſſivement la doſe de l'eau pour s' aſſurer ſi cette loi eſt troublée, et dans quel rapport. Si vous découvriez que les acides ſont abſorbés par l'eau ſuivant le même rapport que l'alkool, il reſteroit à comparer les différentes diminutions de volume qu' éprouvent ces différentes liqueurs, et chercher ſi cela a quelque rapport avec leurs degrés d'affinité. Je vois par exemple que votre huile de vitriol étant mêlée avec l'eau à peu près à parties égales, il y a une diminution de 0.085 de la maſſe totale, par conſéquent environ trois fois plus grande que celle de l'alkool. Ainſi l'eau en retient dans ſes pores trois fois plus, quoique cet acide ſoit plus denſe que l'alkool dans une proportion plus que triple. D'où on pourroit peut-être conclure que l'affinité de l'eau avec cet acide eſt ſix fois plus forte que celle avec l'alkool, puis qu'elle lui fait abſorber ſix fois plus de matière. Mais je ſens parfaitement qu'il faut avoir recueilli bien plus d'obſervations pour être en droit de tirer de pareilles conſequences. Je deſire ſeulement que cet objet vous paroiſſe mériter votre attention, et ſi vous feſiez quelques expériences à cet egard, je vous prierois de vouloir bien me les faire connoître.

Il me reſte à vous parler des dilatations des mélanges d'alkool et d'eau, dont il faut connoitre les effets pour déterminer les peſanteurs ſpécifiques, à toutes les températures. Ce n'eſt pas la partie de la queſtion qui préſente le moins de difficultés; et je n'ai pas fait tout ce qu'il faut pour les réſoudre. Je croyois que l'augmentation totale du volume produite par la dilatation à une température quelconque, étoit la ſomme des dilatations de l'eau et de l'alkool qui compoſent le mélange, moins celle de la partie d'alkool abſorbée par l'eau. Mais j'ai vu par mes expériences et par celles de Mr. de Luc que cela n'eſt point exact. En ſuppoſant même cette théorie, il n'eſt pas bien facile de déterminer d'une manière générale les dilatations de tous ces alliages, puiſque celles de l'eau et de l'alkool, non ſeulement ne ſont pas égales, mais encore ne ſont pas ſemblables, et les degrés de leurs échelles ne ſont pas proportionels. Le changement de temperature qui fait parcourir au thermomêtre d'eſprit de vin, la moitié de ſon échelle depuis le terme de la glace juſqu'à celui de l'eau bouillante, ne fait parcourir à celui d'eau que les $\frac{26}{80}$ de la ſienne. Il ſuit delà que les mélanges d'alkool et d'eau ne doivent être ni également ni ſemblablement dilatables; et qu'il faudroit déterminer pour chacun d'eux, non ſeulement ſa dilatation abſolue (c'eſt-à-dire la quantité dont ſon volume augmente

à une

a une température donnée), mais encore ſon échelle particulière de dilatation comparée à celles de l'alkoöl et de l'eau.

Malgre cette difficulté, ayant reconnu que les dilatations des eaux-de-vie, dont la peſanteur ſpécifique ne differe pas de plus d'un centième, pouvoient être conſidérées ſans erreur ſenſible comme ſemblables et proportionelles, j'ai oſé eſpérer de parvenir à conſtruire un aréomêtre avec lequel il feroit poſſible de reconnoître les degrés de ſpirituoſité de toutes les eaux-de-vie à toutes les températures et dont l'exactitude feroit ſuffiſante pour la pratique: Je vais vous expoſer auſſi briévement que je le pourrai mes idées à cet égard.

Comme il faut toujours employer le thermomêtre en même tems que le peſe-liqueur, j'ai voulu réunir ces deux inſtrumens et même leurs échelles; en ſorte que celle du thermomêtre ſervît de graduation à l'aréomêtre.

J'ai obſervé qu'une eau-de-vie, dont la peſanteur ſpécifique eſt 0.9275 à la temperature de 15 degrés, ſe trouvoit reduite à 0.9185 à la température de 30 degrés: en conſéquence ſoit fait un areomêtre de verre de manière qu'il ſe plonge dans cette eau-de-vie, à la température de 15 degrés, juſqu'à un point marqué au milieu de ſa tige, enſuite le plaçant dans une autre eau-de-vie dont la peſanteur ſpécifique eſt 0.9185, ou ce qui eſt la même choſe, dans la même échauffée au 30^{ieme} dègré et marquant le point d'enforçement; ſi on diviſe l'intervalle compris entre ces deux points en 15 parties égales, chacun de ces degrés marquera une diminution de peſanteur ſpécifique de 0.0006, 0.0006, ou ce qui est la même chose, la dilatation produite par un degré de chaleur. Faisant ensuite un thermomêtre avec la même eau-de-vie colorée, construit de façon que la distance entre les degrés 15 et 30 soit égale à celle qui se trouve sur la tige de l'aréomêtre entre les deux points qu'on y a marqués, et y plaçant ce thermomêtre ensorte que ces deux points correspondent aux degrés 15 et 30, il est évident que cet aréomêtre s' enforcera toujours dans l'eau-de-vie, qui a servi à le construire, quelle que soit sa température, jusqu' au point qui sera marqué alors par son thermomêtre, pourvu que la liqueur qui est contenue ait le même degré de chaleur que celle dans laquelle nage l'instrument. Il servira donc à faire connoître si l'eau-de-vie dans laquelle on le plonge a le même degré de spirituosité que celle dont la pesanteur spécifique est 0.9275 a la température de 15 degrés. Mais puisque la liqueur du thermomêtre s'éleve toujours au niveau dans cette eau-de-vie, il en résulte que dans une autre plus spiritueuse et par conséquent plus légère, le point marqué par ce thermomêtre sera constamment au-dessous de ce niveau et s' élevera au contraire au-dessus dans celle qui contiendra plus d'eau. Le nombre de degrés compris entre le point d'immersion et celui de thermomêtre déterminera les différences positives ou négatives de pesanteurs spécifiques qui seront 0.0006 multiplié par ce nombre de degrés, de maniere que l'échelle de l'aréomêtre est en quelque sorte attachée au thermomêtre qui s' éleve ou s' abaisse suivant le changement de température. Il est vrai qu'il y auroit une correction à faire dans la supposition que les dilatations sont proportionnelles. Il s' ensuit que les distances entre les points d'immersion dans

deux

deux liqueurs échauffées à différens degrés ne feront pas toujours rigoureufement égales et devoient être d'autant plus grandes que le degrè de chaleur feroit plus fort. Mais ces différences font fi petites, qu'il me femble qu'on peut les négliger dans la pratique; d'autant plus que cet inftrument ne feroit particulièrement défigné qu'à reconnoître les liqueurs qui fe rapprochent beaucoup de celle qu'on auroit chofie pour terme fixe. Je m'étois furtout occupé à le rendre propre à l'ufage des diftillateurs d'eau-de-vie, auxquels il feroit, ce me femble, fort utile. Les premiers produits de cette diftillation font les plus fpiritueux, et ils deviennent fucceffivement plus aqueux dans le cours de cette opération. Il faut que ceux, qui la conduifent, reconnoiffent le moment où le mélange de ces différens produits a formé l'eau-de-vie au degré de fpirituofité qu'on defire, et cela n'eft pas toujours facile à déterminer. On le verroit aifément en plaçant cet aréomètre dans le vafe, qui fert de récipient ou Baffiot: au commencement de la diftillation il feroit enforcé dans l'eau de vie bien au-deffous du point marqué par le thermomêtre. Il s' éleveroit par degrés, et le moment où la liqueur de ce thermomêtre paroîtroit à la furface, feroit celui où il faudroit arrêter et (fuivant lè langage de cet art) couper la diftillation. Il me paroît qu'on pourroit faire auffi de pareils aréomêtres pour ceux qui s' occupent de la rectification des acides lors qu'on aura determiné leur pefanteur fpécifique et leur dilatation. Enfin il faudroit joindre à ces inftrumens des tables qui indiqueroient les différens degrés de fpirituofité ou de concentration et les proportions des alliages.

Les détails ſur la conſtruction et la forme de ces aréomêtres ne ſeroient pas intéreſſants. Je terminerai donc ici cette lettre en vous priant d'excuſer ſon exceſſive longueur. J'ai l'honneur d'être avec l'eſtime la plus reſpectueuſe,

Monſieur,

Votre très humble et très

Obéiſſant ſerviteur,

POUGET.

POLITE LITERATURE.

Thoughts on the HISTORY *of* ALPHABETIC WRITING. *By* MICHAEL KEARNEY, D.D. M.R.I.A. *and of the Etruſcan Academy of Cortona.*

THE origin of the art of expreſſing the conceptions of the mind by viſible, permanent ſigns, hath at all times been an object of curioſity. Cicero adduces the invention of alphabetic writing as a proof of the cæleſtial nature of the human ſoul. Many who have ſpeculated on the ſubject, overpowered by a view of the utility of the art, and the comprehenſive ſagacity which the formation of it implies, have aſcribed its introduction to the immediate inſpiration of the Deity. The Heathens, in this opinion, conformed to their uſual practice of aſſigning a divine author for every uſeful and extraordinary invention. But Chriſtian writers have alſo held the divine infuſion of this art, though there are different opinions concerning the time of the com- Read Dec. 12, 1789

 munication;

munication; ſome ſuppoſing it made to Adam; while others conſider the promulgation of the Moſaic law as its æra. Without producing the poſitive arguments that may be urged againſt this opinion, we may obſerve, that it is unneceſſary to have recourſe to it: for however inadequate the energy of any individual human intellect may ſeem to the completion of alphabetic writing in its preſent form; yet a ſucceſſion of inventive powers might have raiſed it from the rudeſt beginning to the moſt aſtoniſhing excellence. This appears to be now the prevalent opinion: yet though admirable ingenuity and erudition have been exerted in detailing the gradual expanſion of the art from its naſcent rudiments, ſeveral chaſms in the progreſſion ſeem to be ſtill unfilled.

To attempt the ſupply of *this* deficiency, and to give a connected hiſtory of alphabetic writing, is the object of the paper now ſubmitted to the conſideration of the Academy.

THAT the propriety of the links, which I ſhall venture to ſupply, may be more clearly ſeen, it is unavoidable that I ſhould trace the progreſs of writing as far as Warburton and others have deſcribed it. Alphabetic letters denote *ſounds;* it is however probable, that the firſt ſtep was a delineation of the forms of *things;* the conception, not the words expreſſing it, was what offered itſelf for communication, and it readily preſented a naturally reſembling, permanent mark. Here it may be obſerved, that moſt inſtituted ſigns may be traced to others ſuggeſted by nature; thus articulate words marking ideas by compact were probably

probably formed from the cries by which nature directs us to exprefs our feelings. Hiftory confirms this account of the origin of the art; as picture-writing has been found in ufe among many nations fomewhat raifed above the favage ftate, and not yet arrived at refinement. Picture-writing was improved into a more artificial mode, and gradually paffed through the feveral hieroglyphic forms enumerated by Warburton with erudition and accuracy. He has followed the fucceffive fhades of hieroglyphic denotation, beginning with the rude effays of picture-writing, and advancing through analogy, emblem, &c. to the ufe of figns by inftitution or the arbitrary characters of the Chinefe. He juftly obferved that the earlieft fpecies of arbitrary characters, of which thofe ufed by the Chinefe feem to be a juft example, were probably derived from the hieroglyphic forms, and of this refemblance the Chinefe letters ftill retain fome faint veftiges. The likenefs continually declining by the carelefs delineation of current ufe, their affociation with the *things* fignified, which they now ceafed to exhibit to the eye, became weaker, while their connection with founds, their brother figns, grew ftronger; hence at length they feemed, and in fact became, folely reprefentative of them. Here Warburton ended. Monfieur Goguet, "De l'origine des "loix, des arts, &c." advanced farther, and conjectured, that the next ftep in the feries was made by the introduction of marks denoting fyllables, or the more compound elements of words. This mode of writing ftill prevails among the Ethiopians and fome Indian nations; and, according to Kœmpfer, in Japan. It is probable that fyllabical denotation fucceeded to verbal, as Monfieur Goguet has conjectured; but of this tranfition he has

affigned

aſſigned no reaſon. It ſeems to me, that it may be accounted for in this manner: we cannot ſuppoſe that writing was improved even to this degree of excellence before language was in ſome meaſure cultivated: but one of the firſt artifices, that occurs in the refinement of language, is compoſition or the union of diſtinct words in the formation of new terms, which the encreaſed wants and enlarged ideas of men in the progreſs of ſociety would require. To expreſs ſuch compound ſounds, the marks of the ſeveral conſtituent ſounds would be joined together. It was ſoon obſerved, that many of theſe ingredient ſounds were the ſame with others non-ſignificant, that has occurred as parts of uncompounded words, that is, as ſyllables: hence was eaſily ſuggeſted the artifice of denoting ſuch ſyllabic parts by diſtinct marks; the combination and varied arrangement of which would repreſent the numerous words of language compendiouſly, without the multitude of ſigns which verbal notation required. To this ſyllabic alphabet, it appears to me, that one entirely compoſed of conſonants ſucceeded. The mind being now accuſtomed to analyſis, the reſolution of words into ſyllables would in time be followed by that of ſyllables into their component elements. But the variety of ſyllabic ſounds chiefly ariſing from organic articulations or conſonants; and the number of vowels or ſimple breathings being few; men would be contented to give marks to the firſt, leaving the others to be furniſhed by the reader. This conjecture becomes more probable, if, according to the opinion of Lord Monboddo, ſyllables in the primæval languages contained, each only one conſonant. It is alſo ſupported by, and accounts for the nature of the Hebrew and ſome other oriental alphabets,

which

which have no characters denoting vowels, the Masoretic punctuation being novel. In all the western alphabets both consonants and vowels have letters appropriated to them; because the art of writing was not imported from Asia into Europe, until the want of vowel marks had been found to be inconvenient. The addition of such marks brought this wonderful art to its present state of perfection.

BRIEF STRICTURES *on* CERTAIN OBSERVATIONS *of* LORD MONBODDO *respecting the* GREEK TENSES.

By ARTHUR BROWNE, *LL. D. Fellow of Trinity College, Dublin, Representative in Parliament for that University, and M. R. I. A.*

PREFATORY REMARKS.

1. IN philological disquisitions nothing is more difficult than to express our meaning so precisely as to avoid all danger of being mistaken. It becomes necessary, therefore, to premise and specify the sense which, in the following essay, is annexed to certain terms often vaguely used. Read Feb. 13, 1790.

2. Aorists, or indefinites, are sometimes so called because they are used for many tenses indifferently, pasts, presents, and futures. Sometimes, because they do not mark any precise point of time when an action happened, but only express that it did happen. Sometimes, because the verb when used in these tenses doth not *express* whether the action signified be perfected or im-

 perfect.

perfect. In this laſt ſenſe only is the word applied in the following treatiſe, as by definite is meant the tenſe in which the verb expreſſes the perfection of the action. The reaſon of adopting theſe definitions will appear in the ſequel.

3. It will be obſerved that I ſay, "do not *expreſs* perfection "or imperfection," for it may be implied and yet the tenſe be indefinite. Thus Mr. Harris, in his Hermes, truly calls *I wrote*, and *I write*, indefinites; although the man who wrote has written, that is, the action is perfected; and the man who writes is writing, that is, the action is imperfect: but the perfection and imperfection, though it may be implied, not being expreſſed, not being brought into view (to do which the auxiliary verb is neceſſary), nor intended to be ſo, ſuch tenſes are properly called indefinites. They may be called, if we pleaſe, verbal indefinites; but it is of theſe we ſpeak; and if they be not indefinites, there can be no other in the ſenſe which, as it has been obſerved, is applied to the word in this treatiſe, and I think in common uſe, and by moſt philological writers.

4. To illuſtrate this definition ſtill further by example. When Dr. Louth ſays, that *I do*, and *I did*, often expreſs the preſent imperfect and preterimperfect, I muſt beg leave, with great deference, to differ from him. They are indefinite. "He loves "not plays, as thou *doſt*, Anthony," does not directly bring under our conſideration or view Anthony's continuing, or not continuing to love plays (though it may by implication), but merely his love for them, the affection merely, without calling our attention

to

to its perfection or imperfection. *I do love*, *thou doſt love*, are exactly ſynonimous to *I love*, *thou loveſt* *.

5. It muſt alſo be premiſed that in the two laſt ſenſes, of the three mentioned in the ſecond ſection, the word indefinite will ſometimes have directly contrary effects; if it marks not the perfection or imperfection of the action, it will oftentimes refer to a preciſe point of time, and the converſe. When I ſay, *I wrote a letter*, I muſt have been ſpeaking of ſome particular time when I did ſo; but when I ſay, *I have written to him often* †, I only expreſs that I have written at ſome times antecedent to the preſent, but *at what times* does not appear. Thus the tenſes uſually called definites frequently refer to the paſt time indefinitely.

* Suppoſe the queſtion, *do you ride the ſame horſe you uſed to do?* and obſerve how very different the meaning of the anſwer, *I* do *ride him*, is from that of the anſwer, *I* am *riding him*.

† Note, *I wrote to him often*, is not intelligible without referring to ſome preciſe point of time, e. g. *when I was in France.* Why then does Dr. Beattie, in his late excellent work, (the Elements of Moral Science) ſay, *I wrote*, is indefinite, becauſe it refers to no particular part of paſt time? No, it is indefinite becauſe the verb in that tenſe does not define whether the action be completed or imperfect. And why does he ſay, *I have written*, is definite, in reſpect of time? for it refers to no particular time at which the event happened. Put this further Example. A. ſays to B. "I wiſh you would write to that man." B. anſwers, "I have written to him." The ſenſe is complete. The expreſſion is not ſuppoſed to refer to any particular time, and does not neceſſarily elicit any further query. But if B. anſwers, *I wrote to him*, he is of courſe ſuppoſed to have in his mind a reference to ſome particular time, and it naturally calls upon A. to aſk *when*. Is it not clear then that, *I wrote*, refers to ſome particular time, and cannot have been called *indefinite*, as Dr. Beattie ſuppoſes it is, from its not doing ſo? Take another example. "I have gone to plays," does not relate to any particular period. "I went to plays," muſt mean at ſome preciſe time, e. g. *laſt winter, or when I was in France.* The author of the article *Tems*, in the Encyclopedie, had ſome ideas of this kind, when he inſiſts that the *preterit abſolu* of the French has all the characters of an indefinite.

The author therefore of the article Aorift in the Nouvelle Encyclopedie fhould not have laughed fo unmercifully at Mr. Demandre for giving the name of *definites* to tenfes which marked the paft time *indefinitely*; " an odd reafon," fays the article, " and Mr. " Demandre fhould have underftood himfelf before he began to " write." Mr. Demandre's ideas were probably clearer than the critic's; but he leaves room for the criticifm, by referring the word definite to time. If he had, with me, intended the word to mark the perfection or completion of action, the feeming contradiction would have vanifhed.

6. To illuftrate this ftill further, *I write*, in one way of ufing the word, may be a definite, to wit, " I am a writer or author;" this is definite, for it expreffes imperfection, to wit, " that I con- " tinue to be fo;" but it immediately ceafes to refer to any precife point of time, though it does to the prefent epocha in general. On the other hand, *I write*, the indefinite, meaning the mechanical act of writing, applies to this prefent individual moment, without expreffing extended time.

7. So in other examples given by Dr. Beattie, of what he calls prefent indefinites, (and what I would call prefent definites). " God is good," " two and two are four." Thefe propofitions muft ever continue to be true. The time of the exiftence of thefe truths never can be perfectly paft; therefore they do not refer to any particular time or part of time.

8. In the meaning which I have affigned to indefinites Mr. Harris agrees with me, though in a different mode of expreffion.

By

By an indefinite tenſe he means a non-extended time; by a definite, an extended time. Now the verb, expreſſing the action, when uſed in its extended time or tenſe, expreſſes the perfection or imperfection of that action. When uſed in a non-extended time or tenſe, it expreſſes neither. I diſtinguiſh the tenſes by this attribute or conſequence: He by their intrinſic nature: It comes to the ſame thing. But we both differ from Dr. Beattie, who means by *indefinites*, tenſes which do not refer to any particular time; by *definites* thoſe which do. Conſequently he calls, *I write*, a definite, whereas Mr. Harris calls it, with me, an indefinite. For the ſame reaſon, according to his ſyſtem, I think he ought to have called *I wrote*, a definite, at leaſt in many inſtances; becauſe very often, to be intelligible, it muſt refer to a particular time, as appears from the preceding note. *I have written to him*, is intelligible, without referring to any preciſe paſt time, becauſe all it means to expreſs, is *before the preſent time*, but *I wrote* is not intelligible unleſs ſo referred: And therefore I cannot agree to his application of the word, which refers it to a particular part of time, becauſe, as it appears to me, it would ſometimes make the tenſes uſually called aoriſts, to be called definites. For example, in the Greek language, the ſecond aoriſt generally refers to a particular time, and therefore, in propriety of ſpeech, ſhould change its name, and be called a definite: And beſides, the ſame tenſes might be definite or indefinite according to the words or ſentences that followed them; all which, though it would make no material difference in mental reaſoning, would occaſion confuſion, by deviating from the common uſe of words.

9. FROM

9. FROM the preceding Remarks it will by this time appear, why I have adopted the definitions of the words definite and indefinite, which are laſt mentioned in the ſecond ſection; and why thoſe names are by me referred to time through the medium of action, rather than to time immediately: becauſe the latter method makes the ſame tenſes definite or indefinite, according to accident; the former keeps them invariably diſtinct. The latter, if conſiſtently followed, muſt occaſion deviation from common uſe in naming the tenſes: The former will produce a conformity to it. It will, however, appear hereafter, that the application of the word to time immediately, does not affect the following theory *.

10. IF it be objected that many verbs do not expreſs *action*, let the word *event* be ſubſtituted in the following pages, as it may without affecting the argument; e. g. perfect, or imperfect event, and the objection is anſwered.

* Vide note at the end of this eſſay.

BRIEF

BRIEF STRICTURES, &c.

THE three great objects in the acquisition of languages are the knowledge of grammar, of words and their signification, and of idiom or phrase. Without accurate grammatical knowledge the two last will form a very imperfect linguist; and in the province of grammar the doctrine of tenses or times is evidently one of the most important. For how is the meaning of the author or speaker to be distinctly and definitely known, without knowing precisely the time of which he speaks, and to which the action is referred? Such enquiries, therefore, though less respected than formerly, perhaps because rendered less necessary by the immense labours of the two last centuries, have a certain utility, and have within the present age obtained the attention, and employed the industry, of three celebrated philosophers, and most ingenious investigators of universal grammar, Dr. Clark, Mr. Harris, and Lord Monboddo. Mr. Harris has given us the most philosophic and universal division of time, from whence he argues that there are in nature twelve tenses or times.

Three Indefinites.

Past.	Present.	Future.
I wrote.	I write.	I shall write.

Three past Definites.

Inceptive.	Imperfect.	Perfect.
I was beginning to write.	I was writing.	I had written.

Three

Three present Definites.

Inceptive.	Imperfect.	Perfect.
I am going to write.	I am writing.	I have written.

Three future Definites.

Inceptive.	Imperfect.	Perfect.
I ſhall be beginning, or going to write.	I ſhall be writing.	I ſhall have written.

THIS philoſophic diviſion, as Lord Monboddo truly obſerves, has never taken place in any language with which we are acquainted; but it ſeems univerſally acknowledged that the Greek in this, as well as in many other reſpects, may challenge a preference to moſt if not all languages that have ever been formed. In the Greek active and middle voices there are eight tenſes, and in the paſſive nine, which, if you ſtrike off Mr. Harris's inceptive tenſes, whoſe utility I do not well ſee *, comes very near to his philoſophic number, nor do they much differ from his univerſal tenſes in ſignification. This merit, however, of the Greek language, Lord Monboddo has endeavoured, though without avowing ſuch intention, to depreciate, by boldly ſtriking off two of theſe tenſes, and by roundly aſſerting, and calling to his aid, the aſſertions of ſome ancient grammarians, that the ſecond future and the ſecond aoriſt mean nothing different from the firſt

* Take Mr. Harris's example. " I do not well ſee how they can be called tenſes of the verb *to write;* they are tenſes of the verb *to go*, or *to begin*, joined with the infinitive *to write.* I do not clearly apprehend how one compound tenſe can be made out of two verbs, unleſs one of them be a mere auxiliary. *Scripturus ero* may be a tenſe; but will it be ſaid that θησειν γαρ ἐμελλεν, Lib. 2d, Iliad, line 39, is a tenſe of any verb? If it be, of what verb? It may be a tenſe of philoſophic grammar, but not of the grammar of any particular language."

firſt tenſes of the ſame name, and are only old obſolete preſents and preterimperfects, preſerved after the verb was moderniſed, merely to vary and enrich the ſound of the language †.

It may well be doubted whether varying ſounds without varying the ſenſe can ever be an improvement of language; but no man will deny, that if thoſe various words have diſtinct meanings, not only the variety of ſounds will remain the ſame, but the language will be more definite, accurate, and perfect; and the various turns and actions of the mind or body will be more clearly and certainly expreſſed. To try whether the Greek language may not be reſcued from this imputation, for ſuch it may be conſidered, of having diſtinct ſounds without diverſity of meaning, is the object of the preſent eſſay, which is not ſo much propoſed in the light of a ſyſtem as of an enquiry.

In purſuing this enquiry, it muſt be premiſed that we are not to expect to meet with any rule that can be adopted with reſpect to the meaning of the Greek tenſes, to which numerous exceptions will not occur.

In truth the Greek writers uſe them all in common practice ſo promiſcuouſly, that it ſeems hard to ſay to what philoſophic time they have not applied every tenſe of grammarians

 upon

† If that was the caſe, it would have been incumbent on Lord Monboddo to have ſhewn how they came to be uſed for futures and aoriſts, and not, as they naturally would, for other expreſſions of the new preſent and preterimperfect.

upon ſome occaſion or other*. But that does not prevent an enquiry, whether they were not meant originally to expreſs diſtinct and ſpecific times, and whether they are not more frequently uſed according to thoſe original meanings than to any other.

To begin with the aoriſts. That the aoriſts are often uſed without diſcrimination as mere paſt indefinites, cannot be denied; but this is true alſo of the preterimperfect, and of the preterpluperfect. They are frequently uſed as paſt indefinites, but no man would venture to ſay, that the two laſt are therefore not diſtinct tenſes, having proper and original meanings of a definite nature, as I think has alſo the firſt aoriſt. The queſtion is not what takes place in practice. This may ariſe from abuſe, from negligence; for we may be ſure that the beſt Greek writers, as well as the beſt Engliſh, are ſometimes negligent; or from the neceſſity which the poets felt of accommodating the length and ſize

* Προσδεχεσθε, uſed as a future. Æſchines cont. Ctes, ſec. 8th. Μετεγνωσαν, ἐγενοντο, uſed as preterpluperfects. Dem. de corona, ſec. 50. In the 11th book of Homer, line 296, ἐβεβήκει, uſed as an indefinite, though a preterpluperfect. Every one knows that aoriſts are often uſed as preſents and futures. Theſe inſtances, out of ten thouſand, happen at this moment to occur to me; but they teem in every page of every Greek author; nor is it poſſible to miſs them. It appears to me, however, and it may well afford matter for ſurpriſe, that the more ancient authors, and particularly Homer, are more nice and diſcriminating in the uſe of the tenſes than the more modern; and that Dr. Clark could not have found any book that would have ſupported his analogy of the tenſes nearly ſo well as his and the world's favourite author. Languages certainly advance gradually to perfection; but perhaps in ſimpler times there is, if I may be allowed the expreſſion, leſs luxury and wantonneſs in the uſe of language as of every thing elſe; or perhaps the uſes of life do not require ſuch various applications of it.

ſize of words to the metre and ruthmos. But the queſtion is, whether the aoriſts are not in ſtrict propriety diſtinct, as to ſignification, and were not meant originally to be ſo, and even whether one of them may not have ſignifications incommunicable to the other.

I THINK then it cannot eſcape our obſervation that the firſt aoriſt has much more frequently a definite meaning than the ſecond. The ſecond appears to me, in nine inſtances out of ten, to be uſed indefinitely. *He went*, *he roſe*, *he ſpoke*, and all ſimilar phraſes, are uſually expreſſed in Greek in the ſecond aoriſt, ἐϐη, ἀνεϛη, ἐφη. There is no difficulty in finding inſtances to the contrary; as in Tenth book of Homer, line 556, Neſtor, ſpeaking of the horſes of Rheſus, ſays, I have never ſeen, nor have I obſerved them before, ȣπω ἴδον ȣδ' ἐνόησα. So Æſchines cont. Ctes, ſaying that even the man who had received no public money ſhould yet render an account, viz. that he had not received, and therefore not expended, makes him ſay, οὔτ' ἐλαϐον, οὔτ' ἀνηλωσα*. In theſe paſſages Ἴδον

 and

* And yet perhaps, without too much refinement, theſe might be tranſlated, one definitely, the other indefinitely, at leaſt in words if not in ſenſe. I neither *ſaw*, nor *have* I obſerved. I neither *received*, nor *have* I expended, [*Vide ſec.* 3, *prefatory remarks.*] Such diſtinctions are arbitrary, and therefore perhaps not generally noticed, yet they are not uſeleſs, nor without beauty. Dr. Gregory, in a treatiſe on moods, in the laſt Edinburgh tranſactions, takes notice of this very contraſt, and obſerves " that there is a preciſion and beauty in this uſe of the ſimple, contraſted with " the compound paſt tenſe, (e. g. the Lord *gave* and the Lord *hath taken away)* not to " be attained in Latin, which would ſay Dominus dedit, and Dominus abſtulit; but " attainable in Greek, by uſing the aoriſt for *gave*, and the preterperfect for *hath* " *taken away*." No, not according to my idea; but by uſing the ſecond aoriſt for *gave*, and the firſt for *hath taken away*. The contraſt of the ſecond aoriſts bearing ſuch poſſible conſtruction appears in every book and every line; but Dr. Gregory has given no example of the contraſt he mentions, as I remember. Perhaps, after all, the

and ἔλαβον appear to have a definite signification, but such instances are comparatively few. The first aorist, on the contrary, is most frequently used as a past definite; and indeed so frequently, that grammarians observe it is oftner introduced to denote the past perfect time than their preterperfect tense itself. Have we not reason then to suppose that its proper meaning is of a definite nature, and that it is not *properly* an aorist? Sanctius seems to have been of this opinion when he calls the second only by the name of aorist. And if it can be shewn that such a tense was actually wanting in the Greek language, to express the time of an action which is past and perfect, will not the truth of the position be strongly confirmed? In shewing this, in fact it will require more pains to distinguish the first aorist from the preterperfect than from the second.

Having then distinguished the first from the second aorist, by arguing that the first is not *properly* an aorist, and that where they seem to be used in the same sense, either such a contrast as Dr. Gregory alludes to is intended, or it arises from necessity in defective verbs; I proceed to shew that such a definite, as I conceive the first aorist to be, was wanting in the Greek language, and is not supplied by the preterperfect.

The tenses of vulgar and philosophic Grammar frequently differ, or, in other words, the times which common grammarians sup-

the only instances in which the Greeks ever use the second aorists thus, without apparent distinction from the first, are where they are the second aorists of defective verbs, which have no first aorists, at least in common use fuppofed to be denoted by the written tenfes of a language, and according to which they therefore denominated thofe tenfes, are not the real times they were intended to exprefs. Thus Dr. Clark has proved, in the Greek language, that the tenfe ufually called by them the preter or paft perfect, does not properly exprefs fuch a time, but a different one, which ought to ftamp it with a different name*; that the time it was intended to exprefs is complex, including the confideration both of the paft and of the prefent; and implying, that the action has been done, and ftill continues to be done. Αμφιϐεϐηκας; thou haft protected, and doft ftill protect. Τεθαυμακας; thou haft admired, and doft ftill admire. This opinion is confirmed by the authorities of many other of the moft able linguifts, particularly Lord Monboddo and Mr. Huntingford.

This then being in ftrictnefs of fpeech the true fignification of the preter tenfe, ufually, though improperly, called the preterperfect, it is plain that we muft feek in the Greek language fome other tenfe, to exprefs the time of the performance of an action which was perfected at a time paft, and has ceafed to continue; whofe conclufion was antecedent to

* The appellation he gives it is the prefent perfect. In this appellation I muft beg permiffion not to concur with him, becaufe whatever is perfected muft be paft. Non meus hic fermo. Scaliger had heard the name, which did not originate with Clark, and laughed at it for the fame reafon, for whatever action or paffion is ftill continuing muft be imperfect. Some name is wanting to exprefs this compound of the paft and prefent.

to the preſent time, and which bears no other relation to the preſent, and whoſe time therefore is the real preterperfect. Accordingly Dr. Clark has aſſigned to this office the tenſe uſually called the preterpluperfect; but ſurely without authority, for that tenſe expreſſes ſomething more. The tenſe we want is only to denote that the action was paſt and perfected at a time antecedent to the preſent. But the preterpluperfect is always uſed to ſignify that the action was paſt and perfected at a time antecedent alſo to a paſt time, i. e. antecedent to ſome given period or epocha paſt, to which we look back. *He had done it, fecerat*, muſt mean that he had done it before ſome certain time or event paſt, of which we have been ſpeaking, and is ſomething very different from ſaying, *he* HAS *done it*. Grammarians were therefore in this inſtance right in calling it the preterpluperfect, as implying ſomething more than the mere paſt perfect. This tenſe then not anſwering our purpoſe, it ſeems to me that we muſt have recourſe to the firſt aoriſt, and that the original intention of the firſt aoriſt was to expreſs the real preterperfect time of philoſophic grammar.

To confirm this by examples would be an endleſs taſk; the only method of proof that can be adopted, is that of referring the reader to the general uſe of this tenſe when it is not an indefinite. When it is uſed as a paſt definite it can have but two meanings; either that of an action entirely paſt, or of an action which commenced at a time paſt, and is ſtill continuing. The latter meaning has been appropriated by the writers abovementioned to the preteritum perfectum. It follows then of courſe that

that the former belongs to the firſt aoriſt. Exceptions * to the general rule cannot be allowed to ſubvert it, for if ſo the multitude of exceptions † to Dr. Clark's and his followers eſtabliſhed interpretation of the preterperfect muſt overthrow his ſyſtem. The one ſeems equally ſtrong with the other.

Have I explained my argument clearly? it is, that a peculiar ſignification having been proved in the preterperfect, vulgarly ſo called, viz. that of a *continuing* action, and there being a philoſophic time belonging to an action quite paſt, and not now continued, there ought in vulgar grammar to be a tenſe expreſſing ſuch a time, and in Greek no tenſe can be found applicable to it but the firſt aoriſt, which undoubtedly often expreſſes a paſt definite of ſome kind or other, and therefore, without any force or violence, naturally falls into that place. When it expreſſes a paſt definite, it muſt mean either an action quite paſt, or partly paſt, but ſtill continuing. In the latter meaning it would be confounded with the preterperfect, from which danger Dr. Clark has reſcued it for me. It remains therefore clear that the former muſt be its meaning ‡

The

* In the two firſt words of Xenophon's Memorables, is an exception, ΠΟΛΛΑΚΙΣ ἰθαύμασα, where the ſurpriſe of Xenophon certainly had not ceaſed. But ſuch exceptions are few.

† That ſuch exceptions are numerous may eaſily be ſeen, by caſting the eye over the firſt twenty pages of Demoſthenes de Corona, where the preterperfect tenſe is very frequently uſed, not in Dr. Clark's ſenſe.

‡ It will be ſaid here that I have been employed in diſtinguiſhing the firſt aoriſt from the preterperfect, and not from the ſecond; and it will be aſked, how does it appear

The argument is ſtill further confirmed by Lord Monboddo's extenſion of Dr. Clark's theory of the preterperfect.

In the total inefficacy of the preterperfect of grammarians to expreſs the real paſt perfect time, Lord Monboddo agrees with me, though not in the mode of remedying it. He draws a ſtill more accurate line about the preterperfect than Dr. Clark, and endeavours to ſhew that in every inſtance that tenſe is compound, and relates to the preſent time. The apparent exceptions to Dr. Clark's idea of its expreſſing a continuing action, he removes, by inſiſting that in thoſe caſes it is made uſe of becauſe the effects and conſequences are continued to the preſent time; and that whenever it is uſed, we ſhall find, either that the action or its effects are continued to the preſent time. ' There are actions, ſays he, ' which end in energy, and produce no work that re- ' mains after them. What ſhall we ſay of ſuch actions? cannot ' we ſay, we have danced a dance, taken a walk, &c. and how ' can ſuch actions be ſaid in any ſenſe to be preſent? My anſwer ' is, that the conſequences of ſuch actions, reſpecting the ſpeaker ' or ſome other perſon or thing, are preſent, and what theſe ' conſequences are, appear from the tenor of the diſcourſe; I ' have taken a walk, and am *much the better for it.* I have danced ' one dance, *and am inclined to dance no more.* So in Demoſ- ' thenes's oration againſt Ariſtocrates, whom he accuſes of tranſ- ' greſſing a decree, when he conſiders the tranſgreſſion of the ' decree

appear that the ſecond alſo may not expreſs this paſt and perfect action? The anſwer is, that we ſet out with ſhewing that it failed in expreſſing the *perfection* of the action. The preterperfect does not expreſs that it is *totally paſt.* The ſecond aoriſt does not expreſs that it is *perfected.* The firſt aoriſt alone expreſſes both.

'decree as prefent by its effects and confequences, he ufes the 'preterperfect, παραβεβηκε, he has tranfgreffed. If he confiders the 'tranfgreffion fimply as paft, he ufes the aorift παρεβη.'

Having thus confined the meaning of the preterperfect to a compound fenfe, which always has a connection with the prefent, Lord Monboddo naturally proceeds to fupply tenfes for the paft, i. e. for that paft which has no connection with the prefent. This purpofe he confiders as anfwered by the aorifts, ufed indefinitely, and without diftinction—e. g. *he fpoke*, *he faid.* But he forgets that thefe tenfes, in the meaning by him affigned to them, fignify only the paft indefinite, and that he has not pointed out to us any tenfe, which may exprefs the paft perfect. Either therefore he muft affert that there is no paft perfect in nature, unconnected with the prefent, (which would contradict his previous divifion of the paft into perfect, imperfect, and indefinite) or he muft allow that he has omitted to fhew any correfpondent tenfe in the Grecian grammar. Indefinites only exprefs that the action is paft, but fay nothing about its perfection; whereas we want a tenfe to exprefs both that it is paft and perfect *.

But to prove further, that indefinites will not fuffice, as Lord Monboddo feems to fuppofe they will, to denote that *paft* which excludes the prefent, we muft obferve that the fpeaking of an action

* If thefe expreffions offend, viz. of tenfes or times expreffing the perfection or imperfection of action, let it be remarked that they are only ufed for the fake of brevity, and that the reader is always fuppofed to fubftitute my original definitions of definite and indefinite tenfes, viz. thofe in which *the verb expreffes* the perfection or imperfection of action; or we may with Dr. Beattie fpeak of the tenfes as perfected or imperfect *with refpect to action.*

merely as paſt, i. e. indefinitely, does not exclude the preſent: it may be in part paſt, yet ſtill continuing and we may be ſpeaking of that portion which is paſt. *He ſpoke well,* does not imply that he may not be ſpeaking ſtill: It may only mean that he ſpoke well during that part of the ſpeech which the relator happened to hear. But when we ſay, *he has been a good ſpeaker*, we exclude the preſent, and evidently expreſs that he is no longer ſo. How would indefinites anſwer this purpoſe? Lord Monboddo himſelf takes notice of this diſtinction in Latin. " The Latins, ſays he," " have a mode of " ſometimes uſing their preterperfect, in a ſenſe which poſitively " excludes the idea of the preſent time."

" Fuit Ilium, Fuimus Troes." VIRGIL.

" Vivite felices, memores & vivite noſtri,
" Sive *erimus*, ſeu nos fata *fuiſſe* volent."

TIBULLUS.

HE obſerves at the ſame time that the Latin language was probably derived from the Greek before the Greek had arrived at its greateſt improvements, and before it had been enriched by a greater variety of tenſes, and therefore has no aoriſts.

IF this be true, do not his obſervations make it probable, that after the ſeparation of the Latin language, the Greeks had furniſhed theirs with the firſt aoriſt, for the very purpoſe of expreſſing, by a ſeparate tenſe, this excluſion of the preſent time, as they furniſhed it with the ſecond aoriſt to expreſs the mere pure indefinite?

BUT

But allowing Lord Monboddo's extenſion of Dr. Clark's theory to be too refined, as many have ſuppoſed it to be, and that the exceptions to Dr. Clark's explication of the preterperfect are as numerous as they ſeem to be, and that it often means an action entirely paſt and diſcontinued, even in its conſequences; give me leave to hazard a conjecture, that even in this caſe it is diſtinguiſhable from the firſt aoriſt, and that this tenſe has ſtill a peculiar meaning of its own. "The difference between the preter-"perfect, and the aoriſts," ſays Mr. Huntingford, "is that which we "underſtand when we ſay, I have written γεγραφα, and I wrote "έγραψα. It is ſo when this aoriſt is uſed indefinitely, and in ſuch "caſe the diſtinction equally applies to the ſecond. But we are "now ſpeaking of caſes where it is uſed definitely, where they "both ſignify, *I have written*, and in ſuch caſe ſome grammarians, "as is obſerved in the Port Royal Grammar, have conceived the "difference to be, that the firſt aoriſt denotes a time *very lately paſt*, "the preterperfect, *one long ſince*." It is with much deference that I propoſe an opinion directly oppoſite. Let us ſee, by a few examples out of many which have occurred to me, whether there be not ſome ground for this opinion, having firſt ſtated clearly what the opinion is *.

 In

* The author of the article *Tems*, in the Encyclopedie, obſerves that ſuch diſtinctions are poſſible, and therefore ought to be noticed in treating of univerſal grammar, though he does not know whether they ever have taken place in the grammar of any particular language. I think they have in French as well as Greek, in their *preterit abſolu* and *preterit indefini*. Pere Harduin ſays ſuch diſtinctions are arbitrary. Be it ſo; their exiſtence is not thereby diſproved. The diſtinction above-mentioned between the aoriſts, (viz. that of the contraſt obſerved by Dr. Gregory between the ſimple and compound paſt,) is certainly arbitrary, depending on expreſſion in words, not on the nature of things; yet, though we can from thence account for the diſtinction having eſcaped general notice, we are not thereby authoriſed to deny its exiſtence.

In the Latin and English languages we have no diverſity of tenſes, or of ſingle words, to expreſs whether a paſt action has been done lately, or a long time ſince. It is only from the tone of the ſpeaker, from the circumſtances of the event, or from the context, that we can find out the difference. I *have* done it, *feci*, does not tell us whether it was done this inſtant, or in the commencement of the ſpeaker's life, fifty years ago. But if the agent enters in haſte and perturbation, and ſays, I have *done* it; from his looks and accents, and the circumſtances of the time, we collect that he has done it the inſtant before: it is the preſent perfect. If, on the other hand, he ſays, *I have done ſuch things in my youth*, we know that there is a conſiderable interval between the doing of them and the preſent æra. In the paſſive, the difference is manifeſted in words, *it is done*, *it has been done*, but not ſo in the active. To ſupply this defect, which the Engliſh and Latin languages labour under, in their *active* voice, in not diſtinguiſhing, by different ſounds or words, the difference between what was lately perfected and what ſome time ago, and in the *paſſive*, in not making this diſtinction but by the help of the auxiliary verbs, the Greeks ſeem to me to have invented their firſt aoriſt, and to have intended by it to indicate the latter, as the preterperfect did the former.

The opinion of Theodorus Gaza, as quoted by Lord Monboddo, with reſpect to the meaning of the preterperfect, in ſome meaſure coincides with this theory, which is alſo ſtrengthened by the obſervation made in the eſſay upon the origin of languages, that the preterperfect was called παρακειμενος, as being a time near to the preſent. But without relying upon this argument, I ſhall proceed

proceed to produce some examples tending to shew how far this supposition corresponds with experience.

When Archimede rushes out of the bath, after making his celebrated discovery, he cries out ἑυρηκα, because he had just at that moment found out and solved the difficulty. But when Nestor speaks of ancient days and ancient heroes with whom he had been conversant, he speaks in the aorist, ἀρείοσιν ἤεπερ ὑμῖν Ανδρασιν ὡμίλησα. 1 Lib. Homer, line 260 and 261.

When Demosthenes supposes the question τεθνηκε Φιλιππος; it follows plainly that if the fact had been so, and any person had come in suddenly to announce it, he would have said τεθνηκε. But when Chryses, in the first Iliad, line 40, alludes to actions by him formerly and frequently performed, he uses the aorist, *if I have ever crowned your altars or burnt victims*, ἐρέψα and ἐκηα.

When Æschines concludes his oration, with calling Heaven to witness as to his own efforts in the progress of it, he uses the preterperfect, because those efforts had just then been made, βεβοηθηκα και ειρηκα. So Demosthenes, in multiplied allusions to the calumnies just before thrown out against him by Æschines, uses the preterperfect, βεβλασφημηκε περι ἐμυ. Nor perhaps is it an objection that Demosthenes, in the same oration, speaking to the Athenians of his own life and administration (much of which had long since elapsed), says βεβιωκα and πεπολιτευμαι; because as he was *still* continuing to live among them, and *still* to administer their public affairs, he spoke of matters not entirely past.

So Demosthenes, in the 36th section of the same oration for the crown, says,

Στεφανωσάντων

Στεφανωσάντων τοίνυν ὑμῶν ἐμὲ ἐπὶ τέλεις τότε, και γράψαντος Αριςονικε τας αυτας συλλαβας ἅς περ οὑτοσὶ Κτησιφῶν νῦν γέγραφε.

The crowns to which he alludes were given formerly, τοτε; the participle of the firſt aoriſt is uſed, Στεφανωσάντων; Cteſiphon's is comparatively recent, νυν, therefore he uſes the preterperfect γεγραφε.

So in the Œdipus Coloneus of Sophocles, the Chorus, ſpeaking of the death of Œdipus, which has juſt happened, ſays βεβηκεν. More examples would be tireſome; theſe may be ſufficient to draw the attention of the reader to ſimilar paſſages.

It muſt be acknowledged that in the very next page a contrary inſtance occurs. The Chorus aſks again, Finiit Vitam? ἔπραξεν ουν; and the anſwer is ἐπραξεν, in the aoriſt *. But then it muſt be remarked, that the preterperfect active of the verb πραττω is rarely uſed; which circumſtance, or ſome defect in the verb, may poſſibly account for this and other inſtances, of recurring to the aoriſt, inſtead of the proper tenſe, the preterperfect †. Or if this method of accounting for them ſhould not be admitted, I muſt ſtill contend that we are to be governed by the great tide of practice and majority of authorities, and are not to yield

* See another memorable inſtance againſt me (unleſs the firſt aoriſts there are to be conſtrued as preſents) in 3d book Iliad, line 367 and 368. We muſt meet them fairly.

† The ſame reaſons ſometimes occaſion a recurrence even to the ſecond aoriſt, (viz. where not only the preterperfect, but the firſt aoriſt alſo, are wanting, or ſeldom uſed) in order to expreſs an event which has juſt happened, e. g. ἦλον. 5th Book of Homer's Iliad, line 127; but if theſe exceptions to my ſyſtem ſeldom or never occur but in defective verbs, it is not thereby impeached.

yield immediately to ſome contrary inſtances, which may be owing to inattention or poetic licenſe; and I muſt again recall the reader's attention to this truth, that if ſuch weight be given to contrary inſtances, neither the interpretation given by Dr. Clark, and all the eminent Greek ſcholars of this century, of the preterperfect, nor indeed any ſyſtem calculated to reduce the Greek tenſes to ſome certain and clear analogy, can be maintained. Nothing is more common than a primogenial and a vulgar uſe of words, of tenſes, moods, &c. It is the caſe, as Dr. Clark has ſhewn, with reſpect to the preterperfect. It is the caſe with reſpect to the middle voice; its primogenial uſe, as is univerſally acknowledged, is to mark a reflected action, like a reflex verb; yet how often is it uſed without any ſuch poſſible meaning? Why may not, in the ſame manner, a primogenial ſignification and uſe be granted to the firſt aoriſt, though contrary inſtances occur? The weight and number of inſtances is to determine *.

The probability that the Greek language might entertain a diſtinct tenſe to denote what had lately or what had long ſince happened, is augmented by our knowledge that they had a tenſe to expreſs what was ſoon to come. The nation that uſed a *paulo poſt future* might have a *paulo antepreterit*. Some circumſtances reſpecting the French language may tend alſo to illuſtrate and

* In aſſuming this criterion I am ſupported by Caſaubon, Voſſius, and Henry Stephens. Notwithſtanding numberleſs oppoſite inſtances, they advanced an opinion that the firſt aoriſt denoted a time leſs remote than the preterperfect did; this being an opinion directly contrary to mine, I might be thought guilty of great preſumption, if Henry Stephens, from whom the opinion originated, had not latterly expreſſed great doubts of its truth.

and to confirm the conjecture. They have two preterperfects, one of which is in fact an aorist. All their grammarians say that this last is never used if the time be not entirely past; for instance, "il a été heureux cette semaine, ce mois ci, cette annee." "He was happy this week, this month, this year;" not "il *fut* heureux," though that expression would be applied to the happiness of the last week, "il *fut* heureux la semaine passée." So they observe it never is used to express an action done the day we speak in, but one done always at some small distance of time; for instance, they do not say, to express the happiness of this morning, "Je fus heureux ce matin," but "J'ai ete heureux ce matin." "The speaker would not say, "I eat a chicken this morning," but "I have eaten," "J'ai mangé un poulet ce matin." This is mentioned only to shew that distinctions of this nature are not chimerical, but do exist in languages.

But whether this conjecture be well-founded or not, if the former positions be admitted, as I think they must, that the second aorist is rarely used definitely; that the first is so frequently used in that sense, of a past definite, as to occur oftner than the preterperfect itself; and that this last tense having assigned to it a specific and appropriate meaning, there naturally remains for the first aorist a proper and peculiar signification belonging to it in strictness of speech, though not always so applied in common use; I say, if these positions be admitted, there will remain a considerable distinction between the two aorists.

There is still, I apprehend, another perceptible distinction in the use of these tenses. If an action be spoken of which has been often done, I think it is observable that the Greeks generally use the

first

firſt aoriſt. Thus, in the beginning of the Cyropædia, where Xenophon reflects how many democracies have been * reduced, and how many oligarchies ſubverted, caſes frequent in human affairs, he uſes the firſt aoriſt; ſo in the beginning of the Memorables, Xenophon expreſſes his frequent ſurpriſe at the errors reſpecting Socrates, *I have often wondered*, in the firſt aoriſt; ſo in the example above-mentioned, Neſtor, wiſhing to expreſs that he often converſed with men much ſuperior to thoſe of the preſent day, ſays ὡμίλησα; ſo in ſimiles where the compariſon is made with ſomething frequently occurring, the firſt aoriſt is generally uſed. E. g. the ſimile uſed by Demoſthenes, de Cor. ſec. 57. "as if we ‡ ſhould blame the mariner who has prepared, &c. &c." is expreſſed throughout in this tenſe. We may obſerve that in theſe and ſimilar caſes, we ſpeak of no particular time at which the action happened, but only of an action which has often paſt, without determining any preciſe time. The ſecond aoriſt, therefore, which generally refers to a particular time, is, I believe, ſeldom if ever uſed to denote a frequentative; although Lord Monboddo has aſſigned this office to it, as well as to the firſt,

 from

* Εννοια ποθ' ἡμιν ἐγένετο, ὅσαι δημοκρατίαι κατελύθησαν ὑπὸ τῶν ἄλλως πως βυλομένων πολιτεύεσθαι μᾶλλον ἢ ἐν δημοκρατίᾳ, ὅσαι τ' αἶ μοναρχίαι, ὅσαι τε ὀλιγαρχίαι ἀνῄρηνται [a] ἤδη ὑπὸ δήμων. καὶ ὅσοι τυραννεῖν ἐπιχειρήσαντες, οἱ μὲν αὐτῶν και ταχὺ πάμπαν κατελύθησαν.

[a] Here a preterperfect is introduced among the firſt aoriſts, perhaps on account of the vicinity of the word ηδη, which brings the view to the preſent time, ſignifying Jam, abhinc—before the preſent day.

‡ Ωσπερ ἂν ἔι τις ναύκληρον πάντ' ἐπὶ σωτηρίᾳ πράξαντα, καὶ πᾶσι κατασκευάσαντα τὸ πλοιον, ἀφ ὧν ὑπελάμβανε σωθήσεσθαι, εἶτα χειμωνι χρησάμενον, καὶ πονησάντων αὐτῷ τῶν σκευῶν, ἢ καὶ συντριβέντων ὅλως, τῆς ναυαγίας αἰτιῷτο.

from which latter alone, however, he draws his examples. In like manner the French expreſs cumulatives by one of their preterperfects only, viz. the compound; (the other, as I conceive, correſponds with the ſecond aoriſt of the Greek,) and ſay, "J'ai "parlé de vous trente fois en ma vie."

It is eaſy to produce apparent examples to oppoſe this theory. E. g. * in the ſimile in the third book of Homer, line 23, it will be ſaid the aoriſts are uſed indifferently, in an inſtance ſimilar to thoſe mentioned above. I deny it; ἐχαρη is not uſed there as a paſt tenſe at all; it is uſed for the preſent, as this aoriſt frequently is, and the line ought to be tranſlated thus:

"As a lion *rejoices* who *has* fallen upon a large prey."

If it be doubted, take Virgil's tranſlation:

"Impaſtus ſtabula alta leo ceu ſæpe peragrans
"*Conſpexit* capream, aut ſurgentem in cornua cervum,
"*Gaudet* hians immane——"

Æneid. 10. 723.

In the ſame manner, in the famous ſimile at the end of the 8th book of Homer, Ὡς δ' ὅτ' ἐν ϰρανῷ ἄςρα, various tenſes are uſed in deſcribing a beautiful night-piece, ſuch as has been often beheld; but they are all uſed for the preſent tenſe, and Dr. Clark tranſlates them accordingly. My poſition only is, that when on ſuch

* Ὡςε λέων ἐχάρη μεγάλῳ ἐπὶ σώματι κύρσας.

ſuch occaſions a *paſt*, but indefinite time, is to be expreſſed, the firſt aoriſt is generally uſed*.

The diſtinction that I mean to impreſs (I repeat it again) is this, that the ſecond aoriſt is the only proper indefinite; that when it is introduced as a definite†, it errs from its primogenial uſe; that it is even doubtful whether it ever be a definite, at leaſt verbally‡ or expreſsly, though it may by implication—That, on the contrary, the primogenial uſe of the firſt aoriſt is to expreſs a paſt definite, and of a peculiar nature, viz. an action totally paſt, and not now continued, wherein it differs from the preterperfect; that the ſecond aoriſt, even when it is uſed definitely, if it ever be ſo, only ſhews that the *action* was perfected at a time paſt, without determining the time when it was done, whether a long or a short time ſince; whereas the firſt aoriſt, when uſed in its ſtrict

* If definite and indefinite therefore be referred to time, the firſt aoriſt is indefinite, the ſecond definite. If to action (in which manner it has been applied through this treatiſe) the converſe takes place.—*Vide Prefatory Remarks, Section* 5.

† Quere, whether it ever is properly a definite, for I ſcarcely remember an inſtance where it may not be conſtrued indefinitely; for inſtance, where ἴδον and ἔλαβον are conſtrued, *I have ſeen*, and *I have taken*, it would be as eaſy to tranſlate them, *I ſaw*, and *I took*. But this is not the caſe with the firſt aoriſt, which it is very often impoſſible to tranſlate indefinitely; for inſtance, how could Neſtor's expreſſion above-mentioned, ὡμίλησα, be conſtrued, but, I *have* been converſant, or I *was* converſant, among ſuperior men. Conſtrue it, I *converſed*, and obſerve how the paſſage will ſound.

‡ By a verbal definite I mean that where the perfection or imperfection of the action is actually expreſſed, by means of the auxiliary verbs *to have* or *to be*. Perhaps this note, as well as many preceding it, are unneceſſary; but the abſtract nature of the ſubject muſt apologiſe for poſſibly too much anxiety to be explicit.

and proper ſignification, implies that it was done a conſiderable time ſince, and the preterperfect, when uſed with the ſame ſtrictneſs, implies it was done very lately, or even continues to be done yet; that the ſecond aoriſt is probably never uſed definitely, but from neceſſity, viz. in defective verbs, which have no firſt aoriſt; that the firſt aoriſt is alſo uſed as a cumulative or word of multitude, to expreſs an action which has been often done, and in other ſenſes (which will occur to the attentive reader) ſeldom or never communicated to the ſecond aoriſt; that whenever theſe two aoriſts ſeem to be uſed indifferently as paſt definites, there is a beautiful contraſt intended, analogous to that which Dr. Gregory obſerves in our language, and which he might have obſerved in the French, between the ſimple and compound preterperfect; and that this is evident, becauſe the firſt aoriſt often cannot be conſtrued but by the help of the auxiliary verb, as in the inſtance of ὡμίλησα, (1 Iliad, line 260.) while the ſecond always can be couſtrued without any ſuch aid; and that all theſe circumſtances together make a wide diſcrimination between theſe two tenſes, which Lord Monboddo conceived to have no difference of meaning.

Let us proceed now very briefly to conſider the two future tenſes of the Greek language, on which a very few obſervations will ſuffice.

Dr. Clark diſtinguiſhes between the two futures, by calling the firſt the imperfect, and the ſecond the perfect, which are thus by him enunciated, *cœnabo*, I ſhall be ſupping; *cœnavero*, I ſhall have

have ſupped *. Lord Monboddo, though he does not perfectly accede to his tranſlation of the firſt future, which he ſays is an indefinite, does to that of the ſecond, and thereby allows there is a diſtinction in Latin, but denies it in Greek. " The Greeks," ſays he, " have no one word to expreſs the future perfect or " imperfect; their ſecond future does not do it; both futures are " merely indefinite; if they wanted to ſay, *I ſhall write*, they could " expreſs it by one word, the future of γραφω; but if they wiſhed " to ſay, *I ſhall be writing*, they muſt uſe a circumlocution γραφων " ἐσομαι, and if they wiſhed to ſay, *I ſhall have written*, they muſt " ſay γεγραφως ἐσομαι. I challenge (ſays he) any perſon to ſhew that " *cœnabo* ever means, *I ſhall be ſupping*, in any author." I ſhall not diſpute with him about his meaning of the firſt future, though I do not accede to it; let it be indefinite or imperfect, *I ſhall ſup*, or *I ſhall be ſupping*: if the ſecond future be a perfect, and a definite, and ſignifies, *I ſhall have ſupped*, or *I ſhall have written*, the diſtinction will be ſufficient †. But before I endeavour to ſhew that it does, let me take notice of the poſition that the future definite, that is, the perfection or imperfection of the future, cannot be expreſſed by one word without circumlocution. Now the fact is that the future perfect is very frequently

* Τυψω, I ſhall be beating. Τυπω, I ſhall have beat.

† To make good his challenge, Lord Monboddo ſhould have defied us to ſhew that the ſecond future ever has the laſt-mentioned ſignification; and if I ſhew that it has, the futures are thereby diſtinguiſhed, and his triumph over Dr. Clark is incomplete.

quently indeed expreſſed by one word, in the infinitive mood; for inſtance, the prayer of Agamemnon, in the ſecond book of Homer *, line 412.

> Ζεῦ κύδιϛε, μέγιϛε, κελαινεφὲς, αἰθέρι ναίων,
> Μὴ πρὶν ἐπ' ἠέλιον δῦναι, και ἐπὶ κνέφας ἐλθειν,
> Πρίν με κατὰ πρηνὲς βαλέειν Πριάμοιο μέλαθρον
> Αἰθαλόεν, πρῆσαι δὲ πυρὸς δηΐοιο θύρετρα.
> Ἑκτόρεον δὲ χιτῶνα περὶ ϛήθεσσι δαΐξαι,
> Χαλκῷ ῥωγαλέον· πολέες δ' ἀμφ' αὐτὸν ἑταῖροι
> Πρηνέες ἐν κονίησιν ὀδὰξ λαζοίατο γαῖαν †.

In the five laſt lines of which paſſage, all the verbs, which are ſingle words, ſignify the future perfect, and are ſo tranſlated by Dr. Clark. In truth, there is no difficulty in finding inſtances, except in the indicative mood; and really one would imagine that it was that mood only that Lord Monboddo took into view. Now it muſt be conſidered that the future perfect of the indicative, *I ſhall have ſtricken*, *I ſhall have ſupped*, can ſcarcely offer itſelf to uſe but in dialogue or in oration. And this may be the reaſon why it is ſeldom obſerved in that

* Let not the ſun ſet, *before that I ſhall have levelled* to the ground the flaming palace of Priam, &c. &c. Ne prius ſol occidat quam dejecero, &c. &c. ſays Dr. Clark.

† See other inſtances in the Iliad of Homer, 17th book, line 32; 3d book, line 55; 2d book, line 355; 23d book, line 45, and in book 22, line 509, a firſt future is uſed in this perfect ſenſe. Demoſthenes does uſe the expreſſion, γεγραφως εσομαι, but not of neceſſity.

that ſenſe, becauſe, though it may occur in common converſation, it can ſeldom be met with in the perpetuum Carmen, or perpetua Hiſtoria; and when it does occur in dramatic writers it is expreſſed in various ways; e. g. by the participle and genitive caſe, ἐς τοσουτον ἐλπιδων ἐμȣ βεϐωτος, cum eo ſpei pervenero, Œdip. Tyrann. line 781; but ſcarcely ever by that circumlocution which Lord Monboddo deſcribes. But the future perfect that moſt generally occurs, is that which is preceded by and depends upon ſome adverb or conjunction, e. g. WHEN *he ſhall have done it*, AFTER *he ſhall have done it*, and therefore throws itſelf into the ſubjunctive mood; and in that mood there is no difficulty in finding inſtances in abundance of the ſecond future being uſed as a future perfect; e. g. * WHEN *you ſhall have come to the end of life*, χωταν εἰς τελος τȣ ζῆν ἀφικη, the verb is the ſecond future conjunctive mood. Sophoc. Œdip. Colon. line 1526. So Jupiter, in the firſt book of the Iliad, ſays to Juno, οτε κεν τοι ααπ]ȣς χειρας ἐφειω, which I apprehend is to be tranſlated, *when I ſhall have laid my irriſiſtible hands on you*. Ηυπερ γαρ πολεμον γε φυγη, 22. Iliad, line 486. φυγη, tranſlated by Clark, and I think truly, *effugerit*,

* If it be ſaid there is no ſecond future in the ſubjunctive mood, that ſeems to me to be only quibbling, for the ſecond future and the ſecond aoriſt being the ſame in that mood, I have as good a right to call it by one name as the other; or if it be inſiſted that it is the ſecond aoriſt uſed for a future, the reaſoning will be the ſame, ſince whenever it is ſo uſed it is to all intents and purpoſes a future, and whatever is applicable to future is then applicable to it. I am not ignorant that Mr. Dawes, in his Miſcellanea Critica, has totally denied the exiſtence of the ſecond future in any mood whatſoever, of the active and middle voices of the Greek language. With reſpect to the ſubjunctive mood, I hope the preceding obſervation is an anſwer. With reſpect to the indicative, I proceed upon the common hypotheſis of grammarians, and eſpecially of thoſe whoſe opinions I have undertaken to examine.

effugerit, not *effugiat*. The following is an inftance of a future fenfe expreffed in a fingle word even in the indicative, παθων ἐγνως αν ὁιαπερ φρονεις. Paffus cognoviffes, ea quæ agis. Œdip. Tyr. line 411.

These inftances may fuffice to lead the reader to attend to others; they may be found in abundance. Thus much in defence of Doctor Clark's explanation of the fecond future. Nor can I acquiefce implicitly in Lord Monboddo's tranflating the firft always indefinitely. When Antigone fays, in the Œdipus Coloneus, *whether exiled in foreign lands or wandering over the feas*, δυσοισον ἑξομεν τροφαν, I do not fee why we have not as good a right to tranflate it with Dr. Clark, *we fhall be leading a miferable life*, which is the future imperfect, as with Lord Monboddo, *we fhall lead*, which is indefinite. The former, though it may found inelegantly, appears to me the true tranflation.

Having thus fubmitted the few obfervations which occurred to me on thefe tenfes, let me not be fufpected of entertaining the leaft inclination to detract from the reputation of Lord Monboddo, as a profound and accurate linguift. I agree with Mr. Huntingford in thinking that his work on the Origin of Language, is a work of the greateft penetration, erudition, and tafte; and the Greek fcholar is particularly indebted to him for the chapters relative to the Greek language; but I truft we may be indulged, without offence, in thinking that his fentence on the aorifts and futures was hafty and unfounded; and if a favourite hypothefis fhould have mifled its author,

and

and ſhut his eyes to invincible contrary examples, ſome hints may poſſibly be offered by this diſcuſſion to the minds of others, which may tend to elucidate and aſcertain the analogy of the Greek tenſes. To fix their meaning accurately muſt be undoubtedly conſidered as an object of much moment, while the knowledge of the true ſenſe and meaning of the Greek authors is held in eſtimation by the world; and that it ever muſt be ſo, is evident from the conſideration that the New Teſtament itſelf is written in that language, in the interpretation of which the application of a tenſe may be of conſiderable importance. It is hoped, therefore, that any endeavour to elucidate the ſubject will meet with indulgence; and if it be allowed that the preterperfect applies to an action commenced at a time paſt, but ſtill preſent in itſelf or in its conſequences; the ſecond aoriſt in its primogenial uſe to the time paſt only and indefinitely, *i. e.* without expreſſing the perfection or imperfection of the action; and the firſt aoriſt to the time paſt, excluſively of the preſent, and denoting the perfection of that paſt action (as the preterimperfect does its imperfection), every difficulty is removed, the parts all harmoniſe, and the meaning of the other tenſes being already agreed upon and acknowledged, every one will have its own proper and peculiar primogenial ſignification.

N. B. If the foregoing application of the word *indefinite* to action, and not to time, ſhould ſeem to any one unnatural, let it be applied to time, and then I ſay the preterperfect and firſt aoriſt are *definites*, becauſe they reſpectively define and mark the time of the event to have been *lately*, or *long ſince* paſt. The ſecond aoriſt is indefinite, becauſe it does not mark, define, or indicate, whether the time of the event has lapſed *lately*, or *long ago*, but leaves it totally uncertain. This mode of referring them to time, though not the one uſually received, is the only one which can conſiſtently juſtify us in applying theſe names, according to common uſe.

EVIL EFFECTS *of* POLYTHEISM *on the* MORALS *of the* HEATHENS. *By a Young Gentleman, an Under-graduate in the Univerſity of Dublin. Communicated by the Reverend* JOHN KEARNEY, *D.D. S.F.T.C.D. and M.R.I.A.*

THE divine will being the criterion of moral rectitude* to man, his conceptions of the divine nature, upon which depend his Read Feb. 13, 1790.

* To this it may be objected, that thus the ſummit of moral rectitude in the Deity will differ not only in degree, but in very eſſence, from that which is moral rectitude in imperfect agents. I anſwer—No. Perfect goodneſs therefore reſides in the Deity, becauſe he wills and ever unerringly conſults the aggregate of happineſs in his creation. His moral creatures are the leſs imperfect in goodneſs the more uniformly they co-operate in promoting the ſame end. But it is neceſſary that ſuch a moral agent as man ſhould have ſome other immediate ſtandard for directing his actions than their tendency to advance the general good of the univerſe; ſince his limited faculties muſt continually expoſe him to erroneous judgments, from the impoſſibility of taking in at one view all the dependencies of cauſes and effects—all that chain of conſequences, which unites the moſt diſſimilar events and the moſt diſtant periods. Hence the neceſſity of ſeeking the immediate criterion of right and wrong in the will of that Being who cannot err; and hence too the neceſſity that his will ſhould be *revealed.*

his conceptions of the divine will, muſt materially influence his moral conduct. And hence we may draw this general concluſion, that falſe notions of the Deity muſt ever produce more or leſs immorality in our lives. This concluſion we ſhall proceed to confirm, by a particular conſideration of the moral effects which the popular theology of the heathens was calculated to produce. And if all thoſe effects were not actually produced, we muſt look for their prevention to the operation of other cauſes. Nor does it concern our preſent ſubject to conſider what were the opinions of ſome more enlightened philoſophers about the divine being and attributes; for the opinions of philoſophers have ſcarcely any influence upon the practice of the multitude.

And here *polytheiſm* firſt offers itſelf to our view, as the fundamental doctrine of every pagan ſyſtem; and polytheiſm is a doctrine which ſtrikes at the very ground-work of all morality. With the *unity* of God, the immutability and permanence of moral rectitude is immediately connected. Where is the permanence or immutability of virtue, if the divine will be not immutable and permanent? And how can the divine will be immutable or permanent, if there be a *plurality* of Gods? Admit a plurality of wills, and a contrariety is poſſible: but if there be a contrariety, not only the uniformity of virtue is deſtroyed, but the ſame action may become at once virtuous and vicious, as that action may to one divinity be agreeable which is diſpleaſing to another. The impure ſenſualiſt, the violent oppreſſor, the fraudulent, the revengeful—will find ſome tutelary gods to ſanction their moſt criminal exceſſes—gods to whom their moſt criminal exceſſes will be but grateful ſacrifices; while the man of ſtricteſt virtue muſt

ſee

ſee himſelf expoſed to the reſentment of each deified vice. Thus polytheiſm, in deſtroying the ſtability of virtue, deſtroys her very eſſence.

The ſame thing may alſo be inferred by examining one of the ſources from which the ſyſtem of polytheiſm has originated. And here it is neceſſary to combat an aſſertion of Mr. Hume's, that contains one of thoſe ſecret attacks on revealed religion, which are no further dangerous than as they come from an inſidious foe. In his "Natural Hiſtory of Religion" he aſſerts and advances ſpecious arguments to prove that "poly-"theiſm was the primary religion of man." But his aſſertion and his arguments are founded on this ſuppoſition, that the Moſaic account of man's original production is falſe—that the origin of all human ſociety is not to be traced back to a ſingle pair. The truth of this ſuppoſition we ſhall not try upon the ground of revelation, but examine it merely by the light of reaſon. It involves one of theſe two hypotheſes—either that there has been a ſeries of generated beings actually infinite, or that the Deity at firſt *created a ſociety of men.* The former is one of thoſe old atheiſtical hypotheſes, which if at all difficult to be refuted, is only ſo on account of its abſurdity and inherent contradictions, But the conſideration of that is foreign from our preſent purpoſe. As to the latter hypotheſis, if it be received as a maxim that in the inanimate world Nature does nothing in vain, muſt it not be equally true that in the animate world the operations of the God of Nature are none of them in vain? And would it not have been operating in vain to have produced a multitude of human beings, by the immediate interpoſition of

 his

his *creative* power, when the formation of a firſt pair would have been ſufficient to effect the ſame end? Secondary cauſes, I allow, and what are called the eſtabliſhed laws of Nature, are but more remote or more regular exertions of the divine omnipotence. But experience and reaſon teſtify that the divine omnipotence will thus regularly operate by thoſe ſecondary cauſes, and according to the eſtabliſhed laws of Nature, except where thoſe regular operations are inadequate. If men were formerly *created*, who might as well have been produced by *generation*, why is not the immediate fiat of the Deity ſtill ſimilarly interpoſed? And if not interpoſed now, becauſe it need not, why ſhould we imagine that it has been at any former period, when its interpoſition was unneceſſary? Thus then the inutility of the ſuppoſition, and its repugnance to analogy are ſufficient to make us reject it, and conclude that the whole human race are deſcendants of but one man and one woman.

But there is a ſtill ſtronger argument in ſupport of the ſame concluſion. To maintain the contrary hypotheſis is to maintain that the power of the Deity in man's creation was exerted in a manner not only uſeleſs, but poſitively hurtful. Human ſociety, at the ſame time that it affords us means of ſupplying our wants with facility, encreaſes their number. In the catalogue of our neceſſities, by far the greater part will be found to originate in our connection with others. That connection ſupplies the neceſſities which it creates. But how does it ſupply them? By the reciprocation of advantages acquired in the gradual advancement of ſociety. By the commutation of good offices between the wealthy, the ſkilful, the experienced, and the powerful;

which

which intereſt directs, and progreſſive civilization enables them to interchange. What then muſt be the conſequence of a *ſociety* of *created* men? of men equally needing aſſiſtance in all things; and from that very equality of wants, equally incapable of imparting it in any:—the neceſſities of each much greater and more numerous than if he were the only individual of his ſpecies, and his ability to ſupply them infinitely leſs. Is it neceſſary to add that the darkneſs of the proſpect is heightened, when in ſuch an aſſembly of human beings we look in vain for thoſe ties of kindred, affinity, gratitude, and previous acquaintance:—for all that now cements the materials of ſociety, and ſecures its advantages? The conſequences that would enſue are ſuch as the leaſt acute muſt perceive, and the leaſt candid muſt acknowledge to be fatal.

Reason, therefore, as well as revelation, authorizes us to affirm that there has been a firſt man; and the immediate inference from this truth directly contradicts Mr. Hume's aſſertion, that *polytheiſm* made part of man's primary religious faith. The firſt man ſtarts into exiſtence at the word of almighty power. What is the firſt thought which muſt obtrude itſelf on his mind? Is it not this enquiry—"whence am I?" And muſt not his unbiaſſed, unſophiſticated reaſon aſcribe ſuch a great effect to the operation of a mighty cauſe? He views the wonderful ſymmetry of his frame, and concludes that the ſame cauſe is a wiſe intelligence. He exults in his young exiſtence, and acknowledges that cauſe to be as good as he is intelligent and powerful. In himſelf, in his faculties, and his exiſtence, he he reads—he is forced to read the evident characters of a Being, mighty, bountiful and wise; and this Being is his God. Whether there be other beings equally wise, powerful or good, it concerns not him to enquire. Even if there be, they are no Gods to him. His Creator he naturally regards as his sole divinity; and clothes him with those attributes alone which awe, gratitude and admiration are calculated to suggest. But this simplicity in the object of religious worship will naturally cease, as the number of worshippers is augmented. When men are multiplied on the earth, their corruptions will be multiplied;—the variety of human dispositions, circumstances and fortunes will be encreased;—and the image of the Deity, viewed through all these mediums, will be tinged with a variety of colours. Different men will form different conceptions of the divine nature; and each different conception will constitute a distinct divinity. And here the vices and evil propensities of mankind will operate most powerfully. The god of the voluptuous, will be a god of sensuality—the god of the dishonest, will be a god of fraud—the god of the indolent, will be a god of selfish inactivity—of the turbulent, a god of war and violence. Each earthly corruption will by degrees extend its influence to the heavens; and each corrupt deity in return will patronize, extend and perpetuate those vices from which he has derived his origin.

I HAVE thus as briefly as possible endeavoured to prove that the general tendency of polytheism is inimical to good morals. I should now proceed to examine the various particular evils which

which it occaſions, eſpecially that one of diſſolving the bond of univerſal benevolence; and the other numerous tenets of pagan theology would next come to be conſidered. But theſe open too wide a field to be entered on at preſent. If the ſubject be reſumed at any future period, it will be neceſſary to refute ſome more opinions, advanced by a writer on whom I have already had occaſion to animadvert.

ANTIQUITIES.

ACCOUNT *of a* SINGULAR CUSTOM *at* METELIN, *with ſome* CONJECTURES *on the* ANTIQUITY *of its* ORIGIN. *By the Right Honourable* JAMES, *Earl of* CHARLEMONT, *Preſident R. I. A.*

THOUGH the extreme beauty and amenity of the Grecian iſlands, eſpecially thoſe on the Aſiatick ſide of the Egean ſea, may render it difficult to make a choice among them, yet, if I were deſired to declare a preference, I ſhould probably fix on Metelin, the ancient Leſbos.—This enchanting iſland, proud of the birth of Alceus and of Sappho, ſtill retains thoſe charms which gave riſe and inſpiration to their poetry; and though its groves no more reſound with their ſacred ſtrains, the cauſe that inſpired them ſtill ſeems to exiſt, and love ſtill lingers in his favourite retreat.— Read Dec. 19, 1789.

Spirat adhuc Amor
Vivuntque commiſſi Calores
Æoliæ Fidibus Puellæ!

 NATURE

Nature here reigns triumphant, and by ſhewing what ſhe can perform alone and unaſſiſted, teaches us to deſpiſe the weak efforts of her inadequate mimick.—The mountains, whoſe rugged tops exhibit a pleaſing interſperſion of rocks and of pine groves, have their green ſides, for many miles along the coaſt, covered with olives, whoſe leſs agreeable verdure is corrected, embelliſhed, and brightened by a lively mixture of bays and of laurels aſpiring to the height of foreſt trees, of myrtles, pomegranates, and of arbutes, rich at once in bloſſom and in berry, of mulberries growing wild, and laden with fruit, and of every other tree

> Of nobleſt kind for ſight, ſmell, taſte!

While the luxuriant vine, climbing wild and unreſtrained even to their topmoſt branches, adorns and enriches them with its vivid green, and with its cluſtering fruit.—Winter is here unknown.—The climate forbids it.—The verdure is perpetual, and the frequency of evergreens gives to December the colour of June.—The parching heat of ſummer is never felt.—The thick ſhade of trees, and thouſands of cryſtal ſprings, which every where ariſe, and form themſelves into unnumbered rivulets, joined to the refreſhing ſea breeze, the conſtant companion and corrective of noontide heat, qualify the burning air, and render the year a never ending May—

> ——————Airs, vernal airs!
> Breathing the ſmell of field and grove, attune
> The trembling leaves, while univerſal Pan,
> Knit with the Graces, and the hours in dance,
> Leads on the eternal ſpring!—

No

No wonder then if the inhabitants, the better to enjoy thefe various beauties, fhould conftruct their houfes in the following peculiar manner:—Each houfe is a fquare tower neatly built of hewn ftone, fo high as to overtop the trees, and to command a view of the fea and neighbouring iflands.—The lower ftories are granaries and ftore-houfes, and the habitable apartments are all at the top, to which you afcend by a ftone ftair, built, for the moft part, on the outfide and furrounding the tower, fo that from the apartment the trees are overlooked, and the whole country is feen, while the habitations themfelves, which are very numerous, peering above the groves, add life and variety to the enchanting profpect, and give an air of human population to thefe woodlands, which might otherwife be fuppofed the region of Dryads, of Naiads, and of Satyrs.

But the charms of this delightful fpot have fo far tranfported my imagination that I have almoft forgotten the fubject of which, in this effay, I meant to treat, and which is no other than a remarkable and fingular cuftom of this ifland, peculiar, I believe, to itfelf, and, as far as I know, never yet detailed by any traveller.

The women here feem to have arrogated to themfelves the department and privileges of the men.—Contrary to the ufage of all other countries, the eldeft daughter here inherits, and the fons, like daughters every where elfe, are portioned off with fmall dowers, or, which is ftill worfe, turned out, pennylefs, to feek their fortune.—If a man has two daughters, the eldeft, at her marriage, is entitled to all her mother's poffeffions, which are by far

far the greater part of the family eſtate, as the mother, keeping up her prerogative, never parts with the power over any portion of what ſhe has brought into the family, until ſhe is forced into it by the marriage of her daughter, and the father alſo is compelled to ruin himſelf by adding whatever he may have ſcraped together by his induſtry.—The ſecond daughter inherits nothing, and is condemned to perpetual celibacy.—She is ſtyled a Calogria, which ſignifies properly a religious woman or nun, and is in effect menial ſervant to her ſiſter, being employed by her in any office ſhe may think fit to impoſe, frequently ſerving her as waiting maid, as cook, and often in employments ſtill more degrading.—She wears a habit peculiar to her ſituation which ſhe can never change, a ſort of monaſtick dreſs, coarſe, and of dark brown. One advantage however ſhe enjoys over her ſiſter, that whereas the elder, before marriage, is never allowed to go abroad, or to ſee any man, her neareſt relations only excepted, the Calogria, except when employed in domeſtick toil, is in this reſpect at perfect liberty.—But when the ſiſter is married the ſituation of the poor Calogria becomes deſperate indeed, and is rendered ſtill more humiliating by the compariſon between her condition and that of her happy miſtreſs. The married ſiſter enjoys every ſort of liberty—the whole family fortune is her's, and ſhe ſpends it as ſhe pleaſes—her huſband is her obſequious ſervant—her father and mother are dependent upon her—ſhe dreſſes in the moſt magnificent manner, covered all over, according to the faſhion of the iſland, with pearls and with pieces of gold, which are commonly ſequins *; thus continually

* This ſpecies of finery, which prevails through many of the iſlands, is never worn in Metelin but when full dreſs is deemed neceſſary.

tinually carrying about her the enviable marks of affluence and ſuperiority, while the wretched Calogria follows her as a ſervant, arrayed in ſimple homeſpun brown, and without the moſt diſtant hope of ever changing her condition. Such a diſparity may ſeem intolerable, but * what will not cuſtom reconcile? Neither are the misfortunes of the family yet at an end—The father and mother, with what little is left them, contrive by their induſtry to accumulate a ſecond little fortune, and this, if they ſhould have a third daughter, they are obliged to give to her upon her marriage, and the fourth, if there ſhould be one, becomes her Calogria; and ſo on through all the daughters alternately. Whenever the daughter is marriageable ſhe can by cuſtom compel the father to procure her a huſband, and the mother, ſuch is the power of habit, is fooliſh enough to join in teazing him into an immediate compliance, though its conſequences muſt be equally fatal and ruinous to both of them. From hence it happens that nothing is more common than to ſee the old father and mother reduced to the utmoſt indigence, and even begging about the ſtreets, while their unnatural daughters are in affluence; and we ourſelves have frequently been ſhewn the eldeſt daughter parading it through the town in the greateſt ſplendour, while her mother and ſiſter followed her as ſervants, and made a melancholy part of her attendant train.

The

* To what indeed cannot uſage reconcile us? Perhaps if it were the general cuſtom of the world that in all families children ſhould ſhare alike, we ſhould be as much ſurpriſed at the ſingularity of any particular country where the rights of primogeniture prevailed, as we now are at the Metelinean cuſtom, and ſhould pity the comparative indigence of the ſecond brother as we do the ſituation of the miſerable Calogria.

The ſons, as ſoon as they are of an age to gain a livelihood, are turned out of the family, ſometimes with a ſmall preſent or portion, but more frequently without any thing to ſupport them; and thus reduced, they either endeavour to live by their labour, or, which is more uſual, go on board ſome trading veſſel as ſailors or as ſervants, remaining abroad till they have got together ſome competency, and then return home to marry and to be henpecked. Some few there are who, taking advantage of the Turkiſh law *, break through this whimſical cuſtom, who marry their Calogrias, and retain to themſelves a competent proviſion; but theſe are accounted men of a ſingular and even criminal diſpoſition, and are hated and deſpiſed as conformiſts to Turkiſh manners, and deſerters of their native cuſtoms; ſo that we may ſuppoſe they are few indeed who have the boldneſs to depart from the manners of their country, to adopt the cuſtoms of their deteſted maſters, and to brave the contempt, the deriſion, and the hatred of their neighbours and fellow-citizens.

Of all theſe extraordinary particulars I was informed by the French conſul, a man of ſenſe and of indiſputable veracity, who had reſided in this iſland for ſeveral years, and who ſolemnly aſſured me that every circumſtance was true; but indeed our own obſervation

* It may be aſked, how it happens that the Turks do not exert their ſovereign and abſolute power entirely to aboliſh a cuſtom ſo contradictory to the ſpirit and practice of their laws? But this is eaſily anſwered—In all their conqueſts the Turks, either through mercy or through indolence, have left the natives in poſſeſſion of their own cuſtoms, contenting themſelves with their court of final appeal, and with a ſort of cenſorial power, which they exerciſe with much harſhneſs, to their own great emolument, and to the oppreſſion of their ſubjects by arbitrary fines.

obſervation left us without the leaſt room for doubt, and the ſingular appearance and deportment of the ladies fully evinced the truth of our friend's relation. In walking through the town it is eaſy to perceive, from the whimſical manners of the female paſſengers, that the women, according to the vulgar phraſe, *wear the breeches*. They frequently ſtopped us in the ſtreets, examined our dreſs, interrogated us with a bold and manly air*, laughed at our foreign garb and appearance, and ſhewed ſo little attention to that decent modeſty, which is, or ought to be, the true characteriſtick of the ſex, that there is every reaſon to ſuppoſe they would, in ſpite of their haughtineſs, be the kindeſt ladies upon earth, if they were not ſtrictly watched by the Turks, who are here very numerous, and would be ready to puniſh any tranſgreſſion of their ungallant laws with arbitrary fines. But nature and native manners will often baffle the efforts even of tyranny. In all their cuſtoms theſe manly ladies ſeem to have changed ſexes with the men.—The woman rides aſtride—the man ſits ſideways upon the horſe.—Nay I have been aſſured that the huſband's diſtinguiſhing appellation is his wife's family name.—The women have town and country houſes, in the management of which the huſband never dares interfere.—Their gardens, their ſervants, are all their own; and the huſband, from every circumſtance of his behaviour, appears to be no other than his wife's firſt domeſtick, perpetually bound to her ſervice, and ſlave to her caprice. Hence it is that a tradition obtains in the country, that this iſland was

* In the nineteenth Epiſtle of the firſt book, Horace applies an epithet to Sappho which might with great aptneſs be given to her preſent countrywomen:

"Temperat Archilochi Muſam pede maſcula Sappho."

formerly inhabited by Amazons, a tradition however founded upon no ancient hiſtory that I know of. Sappho indeed, the moſt renowned female that this iſland has ever produced, is ſaid to have had manly inclinations, in which, as Lucian informs us, ſhe did but conform with the ſingular manners of her countrywomen; but I do not find that the mode in which ſhe choſe to ſhew theſe inclinations is imitated by the preſent female inhabitants, who ſeem perfectly content with the dear prerogative of abſolute ſway, without endeavouring in any other particular to change the courſe of nature; yet will this circumſtance ſerve to ſhew that the women of Leſbos had always ſomething peculiar, and even peculiarly maſculine, in their manners and propenſities. But be this as it may, it is certain that no country whatſoever can afford a more perfect idea of an Amazonian commonwealth, or better ſerve to render probable thoſe ancient relations which our manners would induce us to eſteem incredible, than this iſland of Metelin. Theſe lordly ladies are, for the moſt part, very handſome in ſpite of their dreſs, which is ſingular and diſadvantageous. Down to the girdle, which, as in the old Grecian garb, is raiſed far above what we uſually call the waiſt, they wear nothing but a ſhift of thin and tranſparent gauze, red, green or brown, through which every thing is viſible, their breaſts only excepted, which they cover with a ſort of handkerchief; and this, as we were informed, the Turks have obliged them to wear, while they look upon it as an incumbrance, and as no inconſiderable portion of Turkiſh tyranny. Long ſleeves of the ſame thin material perfectly ſhew their arms even to the ſhoulder. Their principal ornaments are chains of pearl, to which they hang ſmall pieces of gold coin. Their eyes are large and fine, and the nofe, which we term Grecian, ufually prevails among them, as it does indeed among the women of all thefe iflands. Their complexions are naturally fine, but they fpoil them by paint, of which they make abundant ufe, and they disfigure their pretty faces by fhaving the hinder part of the eyebrow, and replacing it with a ftrait line of hair, neatly applied with fome fort of gum, the brow being thus continued in a ftrait and narrow line till it joins the hair on each fide of their face. They are well made, of the middle fize, and, for the moft part, plump; but they are diftinguifhed by nothing fo much and fo univerfally as by a haughty, difdainful and fupercilious air, with which they feem to look down upon all mankind as creatures of an inferior nature, born for their fervice, and doomed to be their flaves; neither does this peculiarity of countenance in any degree diminifh their natural beauty, but rather adds to it that fort of bewitching attraction, which the French call *piquant.*

The peculiar fingularity of the cuftom above-mentioned induced me carefully to examine whether any fimilar ufage had any where exifted in ancient times, and, after much fearch, I at length found in Herodotus an account of a cuftom among the Lycians, which bears a ftriking refemblance to this of Metelin, and is, I believe, the only inftance in all antiquity* at all refembling

* *The only inftance in all antiquity.*] I have faid in the text that I knew of no inftance in ancient manners refembling the cuftom of the Metelineans, that of the Lycians only excepted. We know however that the Egyptian laws, though there be nothing in them which can induce us to fuppofe that any fimilar ufage exifted in Egypt, gave to the women of that extraordinary country certain peculiar and fingular privileges. Diodorus Siculus, Lib. 1. p. 31, after having

resembling the peculiar privileges of the ladies of that island.— " The Lycians, says he, lib. 1. p. 82, make use of the Cretan " and

having informed us that in Egypt brothers were permitted to intermarry with their sisters, in imitation of Isis whose marriage with her brother Osiris had been productive of such signal advantage, &c. proceeds thus —Διὰ δὲ ταύτας τὰς αἰτίας, " For these reasons," (that is to say from the veneration of the Egyptians for Isis, and from their sense of the benefits which had accrued from her excellent administration after the death of her husband) " It has become a custom that " more power and honour should be allotted to the queen than to the king, " and that among private people in their matrimonial contracts, command is " given to the wife over the husband, the husbands therein promising to obey " their wives in all things."

Herodotus also, in his enumeration of the many customs in which the Egyptians differ from the rest of mankind, mentions some particulars respecting their women which seem to indicate a marked superiority in that sex over the other. Among the Egyptians, says he, Lib. ii. p. 119, the women transact matters of trade and retailing, but the men, remaining in their houses, weave. And, after some other whimsical and ridiculous usages, he adds in the next page that there is no obligation on sons to provide for their parents if they should be unwilling, but that, even though against their will, daughters are obliged to make such provision. Which last circumstance seems to imply some peculiar advantages in the women respecting their patrimony, as it would seem unreasonable that the weaker sex should be particularly burthened unless the laws had furnished them with sufficient ability.

This however is merely conjecture, and the Lycian custom still remains the only instance in point. And indeed, notwithstanding the authorities above cited, there is, in my opinion, much reason to doubt that the women in Egypt in reality possessed any considerable degree of superiority. In every country where a plurality of wives is allowed, the fair sex must always be more or less degraded, and that Polygamy was permitted among the Egyptians we know from Diodorus Siculus, who tells us, Lib. 1. p. 91, that in Egypt the priests were confined to one wife, but that all others might marry as many as they pleased.— Γαμουσι δὲ παρ' Αἰγυπτίοις οἱ μεν Ιερεις μιὰν, τῶν δ' αλλων ὅσας ἂν εκαςτος προαιρῆται—A custom which appears to me wholly inconsistent with the law enforcing obedience in husbands, "and Carian laws; one custom however they have peculiar to "themselves, and in which they agree with no other race of "men. They call themselves by the names of their mothers and "not of their fathers. If any one should ask his neighbour who "he is, he describes himself by his mother, and recites his maternal genealogy; and, if a gentlewoman should marry a slave, "the children by that marriage shall be accounted noble; but, if "a citizen, or even the first among them, should take to wife a "foreign woman, or a concubine, the children by that marriage "are degraded or ignoble.—Νομοισι δε τα μεν Κρητικοισι, τα δε Καριοισι "Χρεονται, &c." * Plutarch also makes mention of this usage in his

husbands, since the situation of the man, who had married many wives, and was bound to obey them all, is too whimsical ever to have existed in any country whatsoever. We must therefore suppose that this law, which had been originally enacted in honour of Isis, and in favour of the sex to which she belonged, had, through lapse of time, and the contradictory manners of the people, become obsolete and inoperative. Vide Sophocles. Oedip. Col. page 282 Edit. Stephani, with the note by Camerarius, page 191.

There is likewise some reason to suppose that in Attica, before Cecrops, as the best means of civilizing that hitherto savage country, had introduced the practice of marriage, the women enjoyed the privilege of voting in the public assemblies, and even that children were named after their mothers. While the connexion between the sexes was casual and unrestrained, who the real father was could scarcely be known with any degree of certainty, and the child therefore took the name of the only parent that could be ascertained.

See for this, and for much other curious information respecting the privileges of women in various ages and countries, Millars distinction of ranks in society, page 32 to page 37.

* Vide Plutarch, as quoted in the text, for several fabulous accounts of the story of Bellerophon, one of which contains a circumstance something to our purpose

his Treatiſe upon the Virtues of Women.—Tom. ii, p. 248.—Edit. Par.—And aſſigns as its cauſe, that, when Bellerophon, in revenge for the ingratitude of the people of Xanthus, a principal city of Lycia, who returned him no thanks for having deſtroyed a boar that laid wâſte their country, had by his prayers drawn down a curſe of barrenneſs upon the whole region, he was at length induced, by the interceſſion of the women, to deprecate the wrath of his patron Neptune, and to reſtore fruitfulneſs to the ſoil.—" From hence," continues he, " it was a law among the " people of Xanthus to be called after the names of their mothers, " and not of their fathers.—δια και Νομος ην τοις Ξανθιοις, μη Πατροθεν αλλ' απο Μητρων Χρηματιζειν.

We find too, in a fragment of Nicolaus Damaſcenus, an account of this ſame cuſtom, which is ſtill more to our purpoſe.—Λύκιοι τὰς γυναῖκας μαλλον ἤ τοὺς ἄνδρας τιμῶσιν, και καλοῦνται μητροθεν. Τας τε κληρονομίας ταις Θυγατρασι λειπουσιν, οὐ τοῖς υἱοῖς.—Vide Excerpta ex Collectaneis, &c. p. 517.——

" The Lycians honour their women more than their men, and " are named after their mothers.—*They leave the inheritances to* " *their daughters, not to their ſons.*

In

purpoſe, viz.: That this hero not only freed the Lycians from an invaſion of pyrates, but from the *Amazons* alſo, whom he drove out of their country. So that there may be ſome reaſon to ſuppoſe that the Lycian women, by an intercourſe with the Amazons, who had, it ſhould ſeem, dwelt among them, were already previouſly prepared for the introduction of thoſe cuſtoms, which were finally eſtabliſhed in conſequence of their patriotick merit in deprecating the wrath of Bellerophon, and in averting its fatal conſequences.

In consequence of the striking resemblance between this custom and that of Metelin, I used my best endeavours to find out some authority for the peopling this island from Lycia, but could trace no such origin. The inhabitants of Lesbos were Æolians, and, let us go back ever so far, we can only find that the island had, in times of the highest antiquity, been colonized by the Pelasgi. However, if we may suppose that this usage subsisted among the Lycians so late as in the times of Nicolaus Damascenus and of Plutarch, that is to say in the reign of Augustus, and in those of Trajan and of Adrian; and, since from the silence of ancient writers upon this point, and more especially from the negative authority of Herodotus, who expressly says that in this instance the Lycians agree with no other race of men, we have some reason to believe that it may not be of very ancient standing in Lesbos, there is no impossibility in the supposition that in the latter ages some colony may have passed over from Lycia into Metelin, and may have there established this extraordinary custom, a fact of which the deficiency of historians in those times may have left us ignorant.

With this flimsy conjecture I was compelled to content myself, and, though by no means satisfied, had well nigh given up the point, when, happening one day to turn over Diodorus Siculus for some other purpose, I fortunately, but accidentally, met with a passage which had hitherto escaped all my painful researches, and which I read with a degree of pleasure only to be conceived by my antiquarian brethren. It seems that those Pelasgi, who under their leader Xanthus, the son of Triopas king of Argos, first

first inhabited Lesbos, had, before their settling in that island, dwelt for some time in a certain part of Lycia which they had conquered, and may of consequence be supposed to have brought from thence the usage in question. The passage is as follows, Tom. I. Lib. v. page 396. Edit Wesselingii.——

——περι τȣ Λεσβȣ νῦν λεγειν επιχειρησόμεν.——Ταυτην γαρ την νῆσον το παλαιον ὤκηκασι πλείω γένη, πολλῶν μεταναστασεων ἐν αὐτῇ γενομενων. Ερημȣ γαρ ȣ̓́σης αὐτῆς πρώτους πελασγȣς καταςχειν αυτην τοιῷ δὲ τινι τροπῷ. Ξανθος ὁ Τριοπȣ των εξ Αργȣς πελασγων βασιλευων, και καταςχων μερος τι τας Λυκιας χωρας, το μεν πρῶτον εν αυτῃ κατοικων, εβασιλευε των συνακολȣθησαντων Πελασγων. υςτερον δε παρωθεις εις τον Λεσβον ȣσαν Ερημον, την μεν χωραν τοις λαοις εμερισε, την δὲ νησον απο των κατοικȣντων αυτην Πελασγιαν ωνομασε το πρωτον καλȣμενην Ισσαν. υςτερον δε γενεαις επτα γενομενȣ τȣ κατα Δευκαλιωνα κατακλαυσμȣ, &c.

Let us now endeavour to speak of Lesbos.—Many nations anciently inhabited this island, repeated emigrations having been made into it. When yet a desart the Pelasgi first occupied it, and in this manner—Xanthus the son of Triopas, ruling over the Pelasgi who came from Argos, inhabited a certain part of Lycia which he had subdued, and governed the Pelasgi who had gone thither with him. Afterwards passing into Lesbos, then a desart, he divided the country among his people, and he named the island from its inhabitants Pelasgia, which had before been called Issa. But after seven ages the deluge of Deucalion happening, &c. &c.

He then proceeds to relate how (many of theſe firſt inhabitants having periſhed) the iſland was in a courſe of ages peopled by various colonies from different nations.

Thus have we traced a poſſible, though very remote ſource of the uſage in queſtion. The diſtance of time, to be ſure, is great; for Triopas, according to Blair, was king of Argos in the year 1553 before Chriſt, or, according to Diodorus, ſtill much earlier, as that hiſtorian places the colonizing Leſbos by the Pelaſgi under the ſon of this prince ſeven ages previous to the flood of Deucalion, which event took place in the year 1503 before the Chriſtian Era. But in the eye and eſtimation of a true antiquarian what are thirty or forty centuries? To ſpeak however ſeriouſly, though I be far from having the boldneſs to deſire that implicit credit ſhould be given to an origin ſo remote as almoſt to tranſgreſs the bounds of hiſtory, conſcious as I am that ſuch a ſpeculation may be liable to ridicule, and aware of ſome objections not eaſy to be anſwered, the coincidence will, I believe, notwithſtanding, be allowed to be curious and very remarkable. The well-known pertinacious adherence to ancient manners among the eaſtern nations may in ſome meaſure excuſe our credulity, and we may ſtill add to our authority by ſuppoſing that this ſame Xanthus may probably have given his name to the Lycian city of that denomination, and conſequently muſt have inhabited that very part of Lycia where, according to Plutarch, the cuſtom is ſuppoſed more immediately to have flouriſhed. It would indeed be whimſically curious if we could allow ourſelves to imagine that a ſingular cuſtom at this day ſubſiſting could be traced back to an origin ſo very remote, and ſhould have taken its riſe in a period when the world was yet in its infancy; or that the relations of Diodorus and of Plutarch, which, conſidering the times of which they treat, might, with much appearance of reaſon, be deemed fabulous, ſhould be corroborated, and, as it were, authenticated by a cuſtom at this day ſubſiſting.

I SHOULD not perhaps have hazarded giving to the public a relation of ſo extraordinary a nature, had the facts, as I had ſuppoſed when upon the ſpot my notes were taken, reſted ſolely upon my teſtimony, but, having lately found with much ſatiſfaction that this ſingular cuſtom is mentioned, though by no means detailed either in its circumſtances or moral conſequences, in the Letters concerning Greece, written from Conſtantinople by Monſieur de Guys, under the title of "Voyage Litteraire." I am the rather emboldened to venture the above detail of an uſage, the exiſtence of which has been thus authenticated, though, from want of ſufficient information, it has been ſlightly and ſuperficially treated; neither can I help expreſſing my ſurpriſe that a matter ſo ſingular in the hiſtory of mankind ſhould have been deficient in point of evidence, or ſhould have eſcaped the notice of ſuch travellers as have ſpent much time in theſe iſlands, and conſequently have had every opportunity of accurate inveſtigation.—Monſ. de Guys, in the preſent inſtance, did not moſt certainly poſſeſs this advantage, not having, as he tells us, been able to verify the fact: "Comme dans le cours de ſes voyages il n'a fait "qu' aborder a cette iſle, et n'y ayant pas fait de Sejour." He had however been informed that in the iſle of Metelin, "Toutes "les proprietés et tous les immeubles appartiennent aux filles, et a

"la

" la fille aineè, ce qui emporte l'exhereditation des Garçons."—I was affured, continues he, by the firft inhabitant whom I queftioned, that the fact was literally true, that the cuftom was of very ancient date, and that the males had willingly confented, " A tout ceder a leurs fœurs pour leur procurer de meilleurs eta-" bliffements. Ils pourroit, ajoutoit il, s'ils vouloient, reclamer " la Loi turque, qui admet tous les Enfans au Partage des Biens " paternels ou maternels, mais ceux qui voudroient ainfi fe fouf-" traire a la Loi du Pais, feroient deshonorés."—*Voyage Litteraire*, Tom. i. p. 398.

Monf. de Guys next proceeds to account for the rife of the cuftom, which he fuppofes to have been very ancient in the ifland; and this he does in a manner which appears to me rather unfatisfactory, by a detail, extracted for the moft part from Diodorus Siculus, lib. xii. of the bloody quarrels between the Lefbians and the Athenians, and of the many revolutions to which the ifland had been fubject, concluding with the following remark:—" Or " comme je vois dâns toutes ces revolutions les femmes epargneès, " ne furent elles pas d'intelligence avec les nouveaux habitants " pour affurer dans leur familles la proprietè des poffeffions, et fe " les attribuer á elles feules? Defquelles eurent etè reconnues " maitreffes des Biens qui leur etoient auparavant communs avec " leurs maris, l'ufage en fit peutetre enfuite une loi qui ne " peut etre plus favorable pour elles.

Where Monf. du Guys has found, that in all thefe revolutions *the women were fpared*, I know not, but, be this as it may, there are unfortunately few countries in Greece, or indeed I fear in the

the world, where the women might not have had, at ſome period or other, a ſimilar opportunity for encroachment; neither does it appear very probable that they ſhould have made ſuch extraordinary terms with the conquering Athenians, ſince we know, from the experience of all ages, that conquerors are not apt to accept of terms from the women of the vanquiſhed. It is indeed difficult to believe that the ladies would have hazarded, or even thought on, ſo ſtrange a propoſition, unleſs we ſhould ſuppoſe ſome pre-exiſting peculiarity in their manners, and in the ancient cuſtoms of their country, which might induce them to propoſe ſuch wonderful terms, and encourage them with the hope that, if obtained, their men would comply; and this ſuppoſition will bring us back to the early eſtabliſhment of the Lycian uſage, which, having poſſibly grown into diſuſe in a long courſe of ages, may have been renewed upon this occaſion, when the ladies found themſelves ſtrong enough, with the aſſiſtance of the conquerors, to reaſſume that ancient ſuperiority, which, though for a time they might have been compelled to yield, it is highly probable they would never forget; and indeed there is ſome reaſon to ſuppoſe, from the ſilence of hiſtorians, that the cuſtom may not have been always permanent in the iſland, but may have occaſionally ceaſed, and been renewed from time to time.

OBSERVATIONS *on the* DESCRIPTION *of the* THEATRE *of* SAGUNTUM, *as given by* EMANUEL MARTI, *Dean of Alicant, in a Letter addressed to* D. ANTONIO FELIX ZONDADARIO. *By the Right Honourable* WILLIAM CONYNGHAM, *Treasurer to the Royal Irish Academy.*

THE commentators upon Pollux, Vitruvius and other ancient writers on the form of the Grecian and Roman theatres have differed so much in their explanation of the rules laid down for their construction, that the best commentary upon them would seem to be the accurate measurement and plan of some one existing theatre: And as the description of that of Saguntum given by Emanuel Marti, the Dean of Alicant, and published amongst his letters at the beginning of this century has been inserted in the work of Monfaucon, and quoted in the last Tour through Spain by Don Antonio Ponz, as the most accurate and satisfactory account Read Dec. 19, 1789.

account given to the public of any ancient theatre, it may not be an unacceptable prefent to the Academy to lay before them the plan and fection of that theatre, taken with all the accuracy that the ruins would permit in the year 1784, together with fome obfervations on the defcription of it given by Dean Marti in the 9th letter of his 4th book, which is as follows:

" THE theatre of Saguntum is placed in a moft convenient and " healthy fituation, for it opens toward the north and rifing fun, " hanging over a pleafant valley through which runs a river, " with a view of the fea to the eaft. It is defended from the " fouth and weftern winds by the intervention of the mountain " which encircles it; fo that it only admits the falutary north and " eaft, fecured from the other noxious blafts. Such is the advice " of Vitruvius in the conftruction of theatres.

" THE circuit of the femicircle is five hundred and fixty-four " of our palms*, each palm being three-fourths of a Roman " foot: the diameter, by a line drawn from each angle, is three " hundred and thirty palms: the height of the theatre, from the " orcheftra to the fumma cavea, one hundred and thirty-three " palms and an half, but to the top of the remaining wall one " hundred and forty-four palms and an half: the diameter of " the orcheftra is ninety-fix palms; from which place we will " begin the defcription as from the centre. In that moft diftin- " gifhed place fat the prætor in the fuggeftum, the remains of " which

* The Valencian palm is nine inches; and thirty-fix Valencian inches are equal to thirty-three inches Englifh.

" which appear in the middle of the orcheſtra at the podium;
" then the veſtals, prieſts, ambaſſadors, ſenators. That the view
" of the ſtage (pulpitum) might not be intercepted, it was inge-
" niouſly contrived that the pavement of the orcheſtra ſhould riſe
" gently from the ſuggeſtum to the loweſt bench of the knights,
" the pavement being lowered and cut circularly in the nature of
" a belt for placing and fixing the ſeats, leaving a little higher
" ſpace between the rows of ſeats to facilitate the going out and
" coming in. From the level of the orcheſtra the ſeats of the
" knights begin, being fourteen rows as appropriated to them by
" the Roſcian and Julian laws of the theatre. At the ſeventh
" row two vomitoria afford entrances, which row is therefore
" broader than the others, that the knights, who were nume-
" rous, might not be preſſed by the narrowneſs of the place,
" but diſperſe themſelves with eaſe into their ſeats. This thea-
" tre is founded on a very hard rock, whoſe reſiſtance defeated
" the ſkill and labour of the workmen: on that account the
" knights had only two doors, which not being ſufficient, ſtair
" caſes were added, one on each ſide, in an open place below,
" whoſe loweſt ſteps go under an arch in the very proſcenium.
" The upper bench of the equeſtrian order is encompaſſed by a
" precinctio; by which name the ancients called a bench twice as
" high and broad as the others, which as it were girded and em-
" braced the reſt. They were inſerted in this manner, that the
" difference of orders, namely, ſenatorial, equeſtrian and plebeian,
" might be apparent, and that there might be no intercourſe be-
" tween them. Twelve benches of the people follow on a higher
" and more remote ſituation, where plebeians ſat; this they call
" the

" the ſumma cavea. To theſe places they poured in by many " paſſages, to which acceſs was had through interior vaults and " corridors. There was alſo an upper portico, which anſwered " two purpoſes: the one, that the people might have a place of " ſhelter in caſe any ſudden ſtorm or ſhower ſhould come on " during the plays; the other, that the theatre below might be " preſerved from the rain and filth. This portico has eight doors " before, and as many behind, and oppoſite. Seven ſtair caſes " afford an aſcent to theſe doors, beginning at the loweſt bench " of the knights next to the orcheſtra. The ſtair caſes however " are not interrupted and winding as in many amphitheatres, but " in a ſtrait and continued courſe; ſo that they make long divi-" ſions like wedges, and form an agreeable coup d'œil to the " ſpectators from top to bottom. Theſe ſtairs were the ways " between the cunei for aſcending, for as the benches were higher " than a common ſtep, and could not be eaſily climbed, the ſtairs " were ingeniouſly contrived ſo as to make a third ſtep between " every two benches, except where the precinctios intervened, " and in theſe there are four. The breadth of theſe ſtairs is three " palms and a half, and the heighth one palm a digit and a " half; which dimenſion is doubled by that of the ſeats. Theſe " ſtairs were contrived for this purpoſe, that the people in the " cunei might eaſily get out whichever way they ſhould turn, " and likewiſe that thoſe who could not get ſeats might ſee the " performance ſtanding. There is a difference between the inter-" nal and external doors; thoſe within are ſquare and broad, and " thoſe without arched and narrower. The upper portico is twelve " palms and three-fourths in height, and fifteen and a quarter

" broad,

" broad, that the people coming in or going out ſhould not ſuffer " from the narrowneſs of the portico. This portico does not ex- " tend to the angles of the theatre, but ſtops and is cut off on " each ſide by an interval of thirty-five palms, which was filled " by four benches differing from the cavea below only in this, " that the upper bench of the people was broader than the reſt, " and formed a ſort of ſmall precinctio or area diſtinguiſhing the " upper from the lower order. Certain concealed ſtairs lead from " thoſe benches to the priſons (of which one ſtill remains), where " yokes or iron rings are fixed to the walls for binding the " malefactors. I muſt obſerve ſtill farther that the portico is " broken in the middle, an interval being left between of twenty- " two palms, on each ſide of which there are four ſteps, extend- " ing ſeven palms and a half. Some faint remains of a baſe give " room to conjecture that a ſtatue was placed in the middle of " theſe, which the uniformity and finiſhing of the building re- " quired to mark the centre of the ſemicircle. The ſides of the " baſe are ſix palms and three-fourths broad. In the higheſt bench " of the cavea, on each angle of the theatre, appear ſix arched " windows, three on each ſide. Over the portico there are four " benches, the upper bench being broader than any even of the " precinctios: certain ſmall ſtairs furniſh an aſcent from the four " ſeats above mentioned to theſe upper benches, as well on each " ſide of the theatre as in the middle. The entrance to theſe " ſeats is by detached ſtairs projecting behind the portico and con- " tiguous to the mountain, leading to arched door-ways in the " higheſt part of the wall, of which only one remains. In the " back part of this wall there are likewiſe quadrangular ſtones, ten

 " palms

" palms and a half diſtant from one another, of a ſquare figure,
" two palms and a half each way; for the explanation of which it
" is neceſſary that you ſhould know that it was formerly the
" cuſtom to ſtretch ſails over the theatres, as well as the amphi-
" theatres, to keep off the heat and ſun. Theſe ſails were tied to
" upright poles, ropes being ſtretched acroſs to prevent their
" floating about by their looſeneſs; the poles were paſſed through
" circular holes in the upper ſtones, and received in theſe pro-
" jecting ſtones, in the middle of which little grooves are hollowed
" to prevent the poles from ſlipping and ſhaking. The wall that
" roſe above theſe benches is deſtroyed by the ravages of time,
" and in the remains there is neither coping or cornice. The
" ſteps of the ſeats are higher than preſcribed by the rules of
" architecture, for they are two palms one fourth, far different
" from what Vitruvius directs: the breadth correſponds exactly
" to his rules, being three palms one-fourth. You may be ſur-
" prized at ſuch a breadth, but nothing could be of greater con-
" venience to the audience, that thoſe who ſat in the upper
" bench might not ſuffer from the contraction of their legs, or
" hurt thoſe below by kicking them; perhaps alſo there might be
" room to paſs behind if any went out or came in late. The
" heighth of the precinctio is double, according to the rule, being
" four palms three-fourths; and alſo the breadth, being ſix palms
" one-fourth. An entry is open to theſe ſeats by many doors, which
" are vulgarly called vomitoria, from which the people entering
" in crouds proceed in to the ſeats as if vomited forth. Two
" porticos lead to theſe approaches: the upper one through the
" open air, as explained before, the other below, creeping through
" the

" the bowels of the mountain like a burrow, and receiving the
" light from the doors. Perhaps you would rather call this a
" vault than a portico: it is nine palms three-fourths in breadth,
" and twelve in heighth. At each extremity of the theatre there
" are many remains of buildings that have yielded to the in-
" juries of time, but which ſufficiently teſtify the greatneſs of
" the work; in theſe various vaults are ſeen, part in ruins, part
" entire, which ſuſtained the covering of the ſcena.

" The various benches of this theatre (allowing a palm and
" a half to each perſon, and omitting the ſtairs for aſcending and
" deſcending) contained ſeven thouſand four hundred and twenty-
" ſix perſons: beſides which we muſt add thoſe who ſat in the
" upper bench above the portico, and brought ſeats, or leaned
" againſt the wall, amounting, as I judge, to about one thouſand,
" and thoſe of the more diſtinguiſhed order in the orcheſtra, the
" compaſs of which ſeems to allow for ſix hundred ſeats. Theſe
" altogether make nine thouſand and twenty-ſix.

" In the front of the theatre lie the proſcenium, pulpitum
" and ſcena. The proſcenium is that ſpace extended before the
" ſcena, whereon was erected the pulpitum on which the actors
" appeared.

" Nothing remains of our pulpitum except the foundation of
" the wall diſtant from the orcheſtra about twelve palms, which
" wall, according to rule, ought to be only five feet or ſix palms
" two-thirds high, that thoſe who were in the orcheſtra might ſee

" the geſtures of the actors: the pulpitum then was lower than
" the ſcena, which is obſerved in our theatre. That ſpace which
" extended oppoſite the theatre, between the two wings, was
" called the ſcena; the length of which ought to be double the
" diameter of the orcheſtra, as directed by the ancients. That
" part is almoſt deſtroyed in our theatre, except the advancing
" wall which ſeparated the ſcena from the pulpitum and ſtretched
" to the angle of the theatre. From the orcheſtra to the ſcena is
" twenty-eight palms and an half, of which twelve were allotted
" to the proſcenium, and the reſt to the pulpitum; the breadth
" therefore of the latter was ſixteen palms and a half, which ſpace
" ſeems ſufficiently convenient for the performances. In the
" middle of this wall, oppoſite the centre of the orcheſtra, a plain
" cemicircle appears, from whoſe circuit a convex wall was raiſed
" and vaulted in the form of a ſhell, called the valvæ regiæ
" from their ornament and extent: each of the doors on either
" ſide were of the ſame form but leſs, and theſe they called
" hoſpitalia becauſe deſtined to the introduction of ſtrangers on
" the ſcena; ſome remains of theſe appear, particularly of that
" on the left, whoſe circle can be traced. That on the right has
" periſhed entirely, except ſome ſigns of the lateral wall at the
" angle of the entrance."

Such is the deſcription given of this theatre by a very learned man, who was reſident for many years at Valencia, within four leagues of Morviedro, the modern name of Saguntum.

The

The Dean feems to have fallen into the common error of thofe who adopt a fyftem. Being perfuaded that this theatre was a Roman work, he previoufly determined what ought to be the difpofition of every part, as defcribed by Vitruvius; and adapting all his obfervations to thofe rules, he faw nothing but what correfponded with his fyftem.

As the plans now fubmitted to the Academy were taken at a time when there was no opportunity of confulting any ancient writer on the fubject, and the meafurements made from what was actually feen to exift, thefe obfervations are at leaft free from that error.

This theatre is fituated on the north-eaft declination of the hill on which the renowned citadel of Saguntum is fituated, where "*Eminet excelfo confurgens colle Saguntos*," and within one hundred and twenty yards of the wall. Advantage is taken of the ground for the difpofition of the feats: the feveral approaches from the town and entrances to the feats are contrived with great ingenuity, and fo difpofed as to afford a convenient admiffion, without incurring great expenfe in levelling and working the rock upon which it is founded *.

It is built entirely of limeftone in even courfes of from feven to eight inches thick without any mixture of bricks. No ornament

* Fundamentorum autem fi in montibus fuerit facilior erit ratio. *Vitr. Lib.* 5, *Cap.* 3.

ment of architecture whatever appears to have been made ufe of on the outfide, nor in the part of the theatre allotted to the fpectators, though probably the whole on the infide was covered with cut ftone, if not with marble, and the fcena ornamented with columns, of which however no remains appear.

As to the ftructure, it feems in fome meafure to correfpond with the rules laid down by Vitruvius for Roman theatres, as I find
Plate I. that by infcribing the four equilateral triangles * A C B, G H I, M K L, D F E, in a circle whofe diameter F I is equal to the diameter of the orcheftra, in the triangle A C B the fide A B, which is parallel to the fcena, determines the front of it: A line F I drawn through the centre and parallel to A B feparates the pulpitum from the orcheftra †: The points K ‡ and L determine the entrances *b b* to the firft clafs of benches, and the length of the fcena § S M R is double the diameter of the orcheftra, according to

* Ipfius autem theatri conformatio fic eft facienda, uti quam magna futura eft perimetros imi, centro medio collocato, circumagatur linea rotundationis, in eaque quatuor fcribantur trigona paribus lateribus, et intervallis, quæ extremam lineam circinationis tangant. *Vitr. Lib.* 5. *Cap.* 6.

† Ex his trigonis cujus latus fuerit proximum fcenæ, ea regione, qua præcidit curvaturam circinationis, ibi finiatur fcenæ frons, et ab eo loco per centrum parallelos linea ducatur, quæ disjungat profcenii pulpitum, et orcheftræ regionem. *Vitr. ibid.*

‡ Cunei fpectaculorum in theatro ita dividuntur, ut anguli trigonorum, qui currunt circum curvaturam circinationis, dirigant afcenfus fcalafque inter cuneos ad primam præcinctionem. *Vitr. ibid.*

§ Scenæ longitudo ad orcheftræ diametron duplex fieri debet. *Vitr. Lib.* 5. *Cap.* 7.

to the rule of that writer; a continuation of A B determines the angles of the outward and circular buildings N O: a tangent to the circle drawn parallel to this line fixes the hypofcenium R S, and the principal entrances anfwer to the vertices F, I of the two triangles whofe bafes cut the fcena at right angles.

Dean Marti fays the diameter of the orcheftra is ninety-fix palms, or fixty-fix feet Englifh; but in this he muft include the firft bench, the orcheftra being only fifty-eight, and the bench four feet broad: he is miftaken greatly in the meafurement acrofs the whole from angle to angle N O, by him ftated to be three hundred and thirty palms, or two hundred and twenty-fix feet and eleven-twelfths Englifh; which fhould have been given as the extent of the fquare part of the building P Q. The diameter N O from angle to angle is two hundred and eighty-three feet, and confequently from the conftruction of the theatre the exterior femicircle from N to O, including the platform that communicated to the corridor leading to the benches of the people, was four hundred and fifty-three feet fix inches*, that is, a diameter and a half and twice the fourth part of a diameter of the orcheftra. Where the dean found a femicircle of five hundred and fixty-four palms, or three hundred and eighty-feven feet and an half Englifh, I cannot conjecture. The height † that he affigns to the fumma cavea is one hundred and thirty-three palms and an half,

* Lipfus. Cap. 8 de Amphitheatro. Theatrum non jufti hemicycli forma fed amplius diametri quarta parte fuit.

† See the fection, plate 3.

half, or ninety-two feet eleven-twelfths, and to the top of the upper wall one hundred and forty-four palms and an half, or one hundred feet and an half. This varies very confiderably from my meafurement, which is only feventy-one feet; but part of the upper wall is deftroyed. I conclude therefore that the dean miftakes when he takes his meafurement from the orcheftra, and that he really took it from the bottom of the front buildings, to which it anfwers.

As to what he afferts of the remains of the fuggeftum in the middle of the orcheftra, I could not perceive them, but if there were any they were probably thofe of the θυμελη or tribunal*.

He is evidently miftaken when he afferts that there are fourteen benches appropriated to the knights; this is a ftriking circumftance in which the conftruction of this theatre difagrees with the rules for the Roman theatre. Thefe benches are here divided into two diftinct claffes: in the firft divifion there are feven benches, to which an entrance is afforded immediately in ftrait corridors from the eaft and weft fides, *a* 2, *a* 2. The upper bench of thefe, as the dean obferves, is wider than the reft, being four feet

* Εν ορχηστρα και ἡ θυμελη, ειτε Βημα τι, ουσα ει τε Βωμος. *Pollux, Lib.* 4. *Cap.* 19.

Sueton. in Claudio. Cap. 21. "Ludos dedicationis Pompejani Theatri, quod ambuftum reftituerat, e tribunali pofito in orcheftra commifit, cum prius apud fuperiores ædes fupplicaffet, perque mediam caveam fedentibus ac filentibus cunctis defcendiffet."

feet broad, but a precinctio of three feet high feparates this clafs of benches from the next fix, which have their diftinct communications by the corridors, *a* 3, *a* 4, and through the vomitoria, *c* 1, *c* 2, *c* 3, *c* 4, *c* 5, *c* 6, to the fixth bench. A double precinctio feparates this clafs of benches from the next immediately above, which feem to be appropriated to the great body of the people, and which confift of ten benches only, and not of twelve as defcribed by Dean Marti.

THESE benches as well as thofe of the other orders are nearly two feet fix inches in breadth, and one foot fix inches in height, except that next the precinctio, which is a little higher than the reft, and the uppermoft, which is only one foot fix inches in breadth.

I OBSERVED a particularity which is not taken notice of by the the Dean. On the top of the principal precinctio there are feveral grooves cut in the ftone, of about one foot fix inches broad, placed two and two, at two feet afunder, and alternately between the vomitoria. This fingularity furprized me; they do not appear large enough to fupport the bafes for ftatues, and had they been intended for other ornaments, or for fupporting rails, they would probably have been placed at leffer intervals. Might they not then be deftined to receive the vafes, mentioned by Vitruvius*, as neceffary for increafing the found of the voice,

* Si non erit ampla magnitudine theatrum, media altitudinis tranfverfa regio defignetur. *Lib.* 5. *Cap.* 5.

and which he directs to be placed in the middle of the height, which here exactly correfponds, the top of the precinctio being thirty-two feet nine inches from the orcheftra, the probable half of the heighth when the outfide wall was entire.

It is to be obferved however that Vitruvius directs thirteen cells at twelve equal diftances, but here there are only nine, and it appeared to me that the intervals between the grooves were unequal. The fkilful in thefe fubjects will decide whether by means of this inequality of the diftances the nine might not have anfwered the fame purpofes as the twelve at equal diftances according to Vitruvius.

Dean Marti fays there are feven ftairs communicating from the orcheftra upwards: this I could not perceive, as the benches are fo ruined it is not eafy to difcover where there were and where there were not ftairs. It is not unreafonable to fuppofe that there were ftairs leading down from each of the vomitoria, and as thefe were alternate in the two upper claffes of benches, (except where the loggia interfered in the centre and occafioned two doors inftead of one,) we may conjecture that there were feven ftair cafes leading from the great corridor to the benches of the people; and fix, alternis itineribus as expreffed by Vitruvius, defcending from an equal number of vomitoria, and communicating to the fecond clafs of benches; though I have doubts about the two next, the centre ones, as they feem to have been obftructed by the principal precinctio, and not cut through it as the others are: there

there might, however, have been a communication by four ſteps*.

THERE is one window on each ſide, *b*, *b*, next the ſcena, which gives light to rooms under the ſumma cavea, and two doors, *g*, *h*, which lead to theſe and other detached rooms. Here it may be proper to obſerve the contrivance of the architect in managing the ſeveral approaches on the ſide of the hill, which are ſupported by walls and entrances made to the theatre, to communicate in the moſt convenient manner poſſible†. Theſe entrances are uniformily contrived to give admiſſion on the level of the uppermoſt bench of the ſeveral claſſes, with ſtairs deſcending to the precinctio, which ſeparates each claſs from that immediately adjoining and below: a convincing proof that the two firſt claſſes of benches were not intended for one and the ſame order of citizens. The principal entrances, *a* 1, *a* 1, are by arches of thirty feet high and anſwering the vertices, F I, of two of the equilateral triangles, whoſe baſes are at right angles to the ſcena. This ſeems to be the approach to the platform K H C E D, of four feet broad and one foot high, next and adjoining the orcheſtra. About thirty feet diſtant from this principal entrance and higher up the hill two other arches, *a* 2, *a* 2, about five feet lower than the others, lead by

Plate II.

Plate I. and II.

* See Plate I. in which the ſtairs are marked by dotted lines to anſwer to the ſeveral vomitoria.

† Aditus complures et ſpatioſos oportet diſponere, nec conjunctos ſuperiores inferioribus, ſed ex omnibus locis perpetuos, et directos ſine inverſuris faciendos, uti cum populus dimittitur de ſpectaculis, ne comprimatur, ſed habeat ex omnibus locis exitus ſeparatos ſine impeditione. *Vitruvius Lib.* 5. *Cap.* 3.

by a corridor of ten feet wide through the vomitoria, *b*, *b*, directly to the feventh bench of the firft clafs next above thofe adjoining the orcheftra.

Plate I. These two main entrances on each fide have communications with each other of ten feet wide, at thirty-eight feet fix inches diftant from the outfide of the theatre. On the eaft fide there is a double communication between thefe corridors, which appears to have been occafioned by the form of the hill. The approach to the upper arch being eafier was probably more frequented, and thofe crofs corridors gave admiffion, the firft to the apartments behind the fcena, the fecond to the orcheftra. The entrance to the fecond clafs of benches on the weft fide is by a corridor of fix feet, *a* 3, at right angles to and communicating from the principal corridors below, aud leading to five doors or vomitoria, *c* 1, *c* 2, *c* 3, *c* 4, *c* 5. The declivity of the hill on the eaft fide prevents a fimilar communication to the feveral doors or vomitoria on that fide, and there the corridor, *a* 4, leads to a flight of ftairs coming out like cellar fteps from under the third bench of the next clafs of benches above at *c* 6. But there are two entrances from without higher up the hill, *a* 5, *a* 6, communicating to the corridor, *a* 3, *a* 6, and communicating to the firft five vomitoria, *c* 1, &c.

The uppermoft bench of this clafs is four feet wide, and immediately above this the principal precinctio is four feet fix inches in breadth and four feet in height, feparating the benches of the main body of the citizens, to which a regular approach is afforded

by

by a portico of ten feet ſix inches in width, communicating, as the Dean obſerves, by doors, *a* 7, *a* 7, from without, and oppoſite to thoſe which give admiſſion to the upper benches of this claſs. There is no entrance to this upper claſs of ſeats but by the principal and upper portico, or rather corridor, except one, *f*, which communicates by a narrow paſſage, *a* 8, from the weſt ſide under the corridor: all the reſt are by regular doors from the corridor. This entrance ſeems to be made for the convenience of the people coming from the weſtward, as the declivity of the hill prevented a communication with this corridor. It is brought under the corridor, and communicates at the ſixth bench from the top at *f*. This corridor ſerves no other purpoſe but for entrance being only ten feet ſix inches wide, and has eight doors, thoſe on each ſide the centre being cloſe to the wall of the middle loggia. It is nine feet three inches in heighth, upon the top of which is placed the ſumma cavea, conſiſting of four benches: the two centre ones of the ſame width of thoſe below, the loweſt four feet, and the uppermoſt contiguous to the wall five feet ſix inches; to which uppermoſt bench ſmall ſtairs *e*, *e*, *e*, communicate from the outſide of the theatre. So that it is evident that each ſeparate order of citizens gained admiſſion at the top of thoſe benches appropriated to their order, and that, beſides the places adjoining the orcheſtra, there were four ſeparate claſſes of benches, being one more than appears to have been uſual in the conſtruction of Roman theatres, which conſiſted only of ima, media, and ſumma cavea*.

ANOTHER

* The diviſion of this theatre ſeems to correſpond to the Βυλητικοι and Εφηϐικοι of the Greeks. *See Pollux.*

Another circumſtance diſtinguiſhing this from the Roman theatres is that the orcheſtra is not divided in cunei, but ſhews two rows of places in its circumference of extraordinary breadth, and which being only one foot in heighth and too low for ſitting were probably appropriated to the chairs of the magiſtrates and other diſtinguiſhed perſons.

As a proof of the juſtneſs of the Dean's remark on the projecting ſtones at the top of the theatre, there is a hole at three feet ſix inches from the centre and nearer the ſcena, of ſix inches ſquare and four feet ſix inches in depth, which appears as if intended for the purpoſe of fixing a pole to ſupport the awning. What he ſays of a priſon ſeems to ariſe from cavities, *m*, *m*, in the building, which were neceſſary to ſave expenſe: and as to his concealed ſtairs, he miſtakes the vomitorium on the eaſt ſide to the ſecond claſs of benches for theſe ſtairs. The loggias in the centre and the ſides do not extend ſo far as laid down by him: the breadth of that in the centre is only ſixteen feet; a groove of about three inches broad is cut in the ſtone in front of this, with a ſmall one on each ſide. Similar loggias appear in the theatre of Herculaneum, but I could not aſcertain the entrances of either, poſſibly they might be on the ſides communicating from the ſumma cavea.

The conſtruction and ſituation of this theatre bear a ſtriking reſemblance to that of Athens, which Pauſanias tells us communicated to the Acropolis by a cave. That citadel towered upon a hill extended nearly eaſt and weſt like this of Saguntum: to the north,

north, with a little inclination to the eaſt, was ſituated the theatre of Bacchus, which appears by the meaſurement of Le Roi to have been nearly of the ſame dimenſions with this: and Monſieur de Choiſeuil, in the Voyage Pittoreſque de la Grece, informs us that the theatre at Sparta was ſituated in the ſame manner, and almoſt all the theatres that he met with in Greece.

The diviſion of the benches in this theatre, totally different from the theatres of Pompey, Marcellus and Pola, correſponds with that of the Grecians, as well as with thoſe of Taormina and Syracuſe: and though the original conſtruction of it, which is evidently upon the plan laid down by Vitruvius as characteriſtical of the Roman theatres, (that is, of being determined by the diſpoſition of four equilateral triangles, and differing from the Grecian theatres, determined, according to him, by three ſquares inſcribed in a circle whoſe diameter is equal to that of the orcheſtra) would ſeem to refer the building of this theatre of Saguntum to the æra of the Roman ſettlements in Spain; yet there are not wanting reaſons for doubting the validity of this argument, ſince the plans that have been lately publiſhed of the theatres in Sicily convince us that this rule was not peculiar to the Roman theatres. It may, therefore, be thought perhaps of a date much earlier, and of Grecian ſtructure, as it cannot well be ſuppoſed that a Grecian colony, powerful as the Saguntines, who reſiſted the whole ſtrength of the Carthaginians during a ſiege of eight months, would be leſs inclined to public amuſements, and leſs ſplendid in their preparations for them, than the numberleſs

colonies

colonies ſettled in Sicily, in each of which a theatre ſeems to have been a neceſſary building, not only for their amuſements but for every purpoſe of the public aſſemblies of the people.

Deſcription of the Plates.

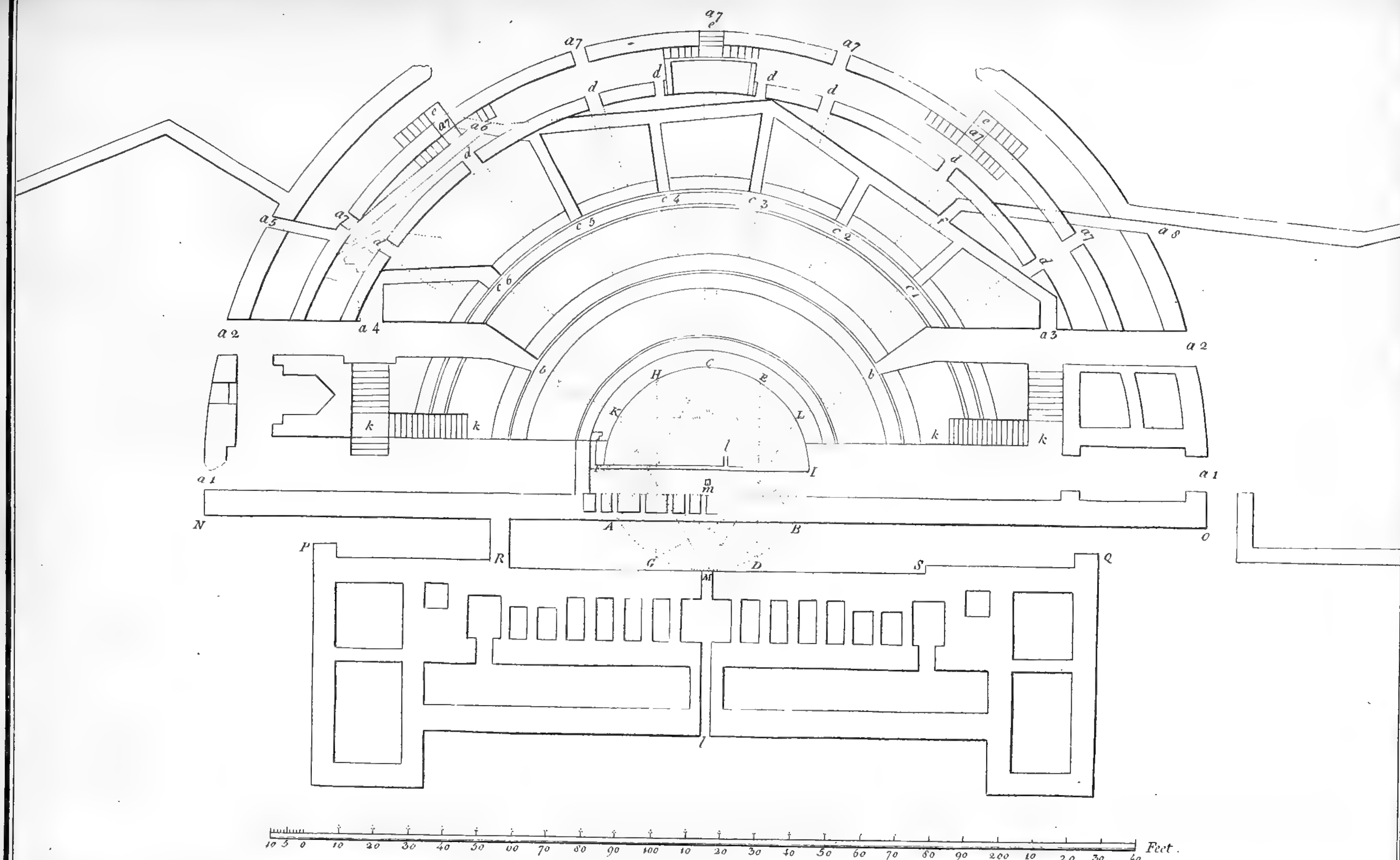
Plate the Firſt ſhews the general diſpoſition of the foundation walls and different entrances to the theatre. The ſemicircular lines mark the ſituation of the precinctios, ſeparating the different claſſes of the people. The dotted lines ſhew the probable poſition of the ſtairs leading down from the ſeveral vomitoria; but the benches are ſo much ruined at preſent that the exact ſituation cannot be aſcertained.

Plate the Second repreſents the walls of the ſcena, proſcenium, pulpitum and orcheſtra, with the three ſemicircles viſibly to be traced on the ſcena. It ſhews the diſpoſition of the benches in the ſeveral claſſes on one ſide, and the great corridor leading to the third claſs of benches on the other. The part not ſhaded diſtinguiſhes that part of the platform communicating to the principal corridor which is cut in the natural rock.

Plate the Third ſhews the ſection of the theatre.

Plate

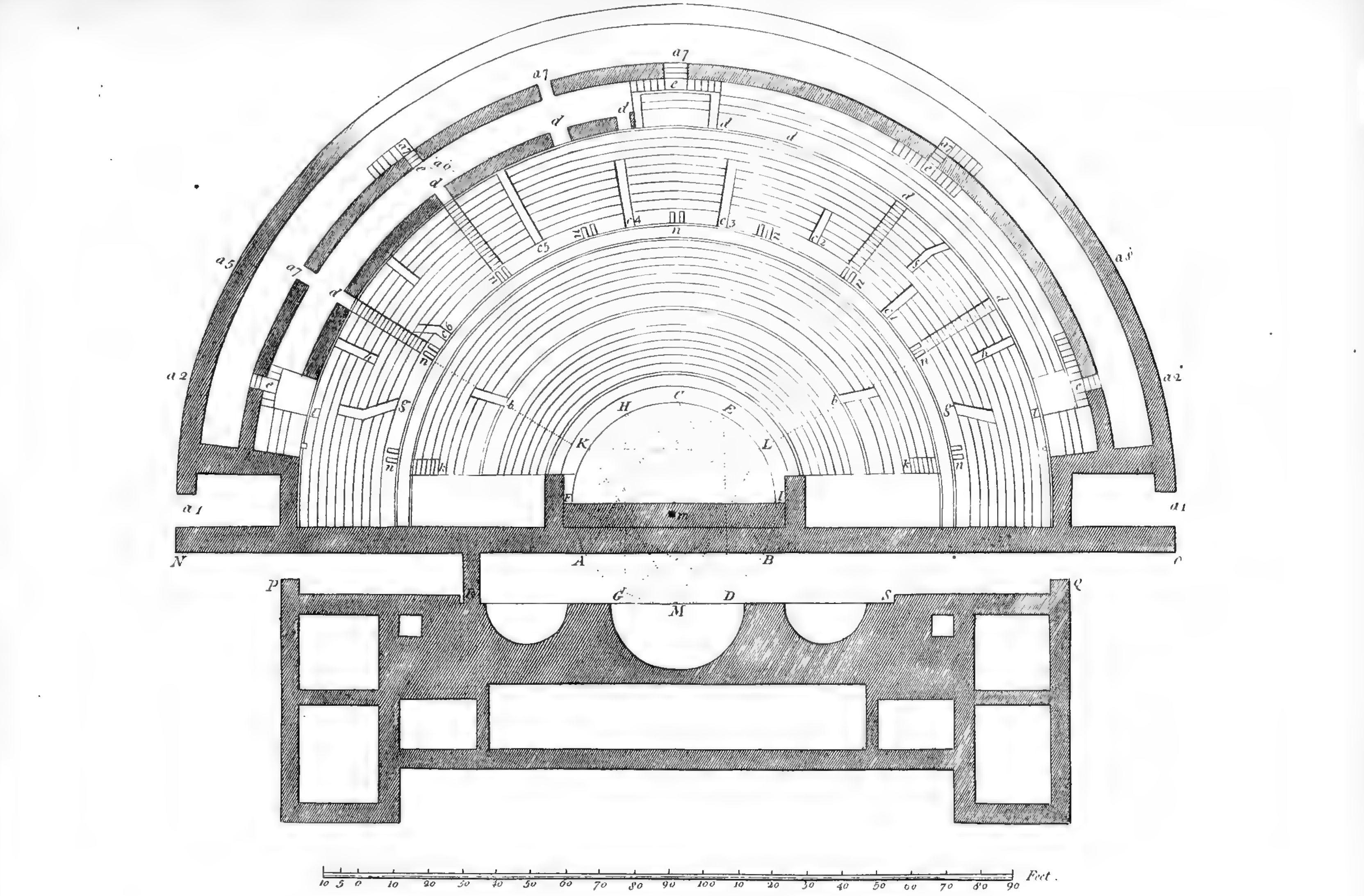
Plate 1



Chaigneau Del.

G. Byrne Sculp

Plate 2



Chaigneau Del.

G. Byrne Sculp.

Plate 3.

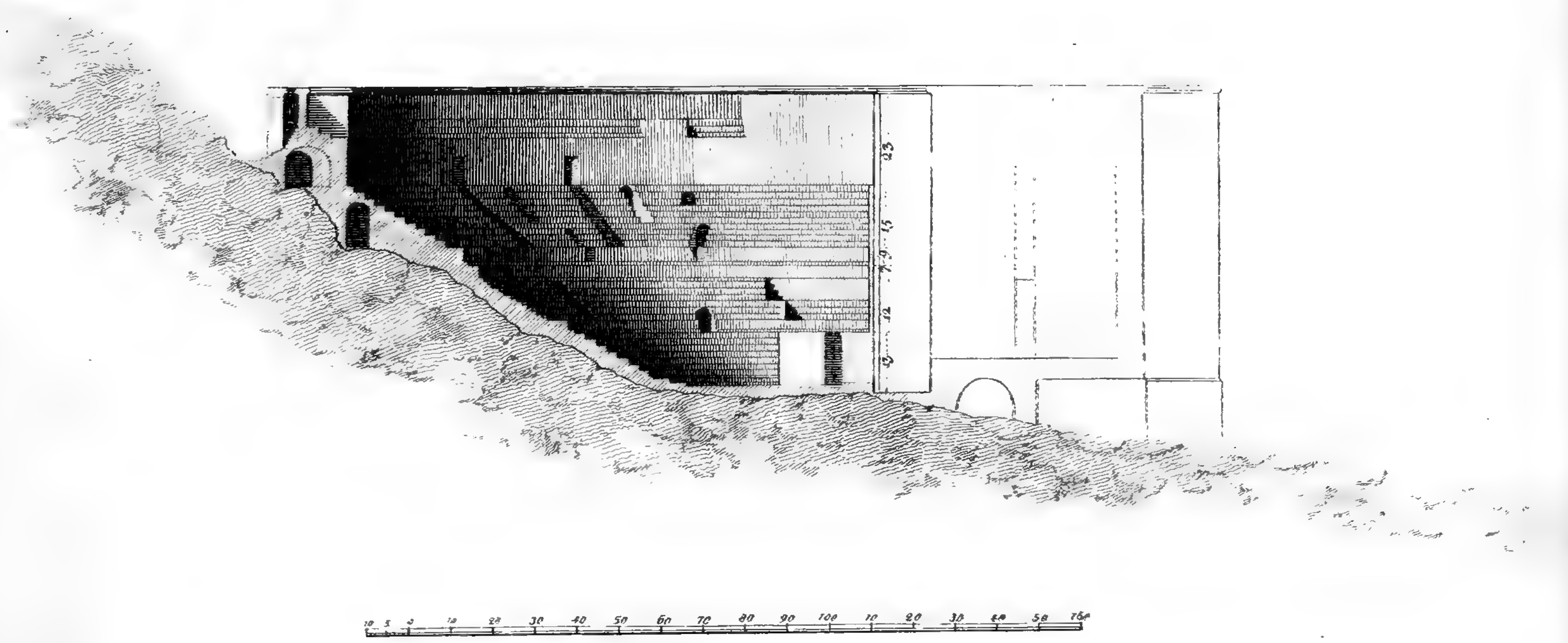
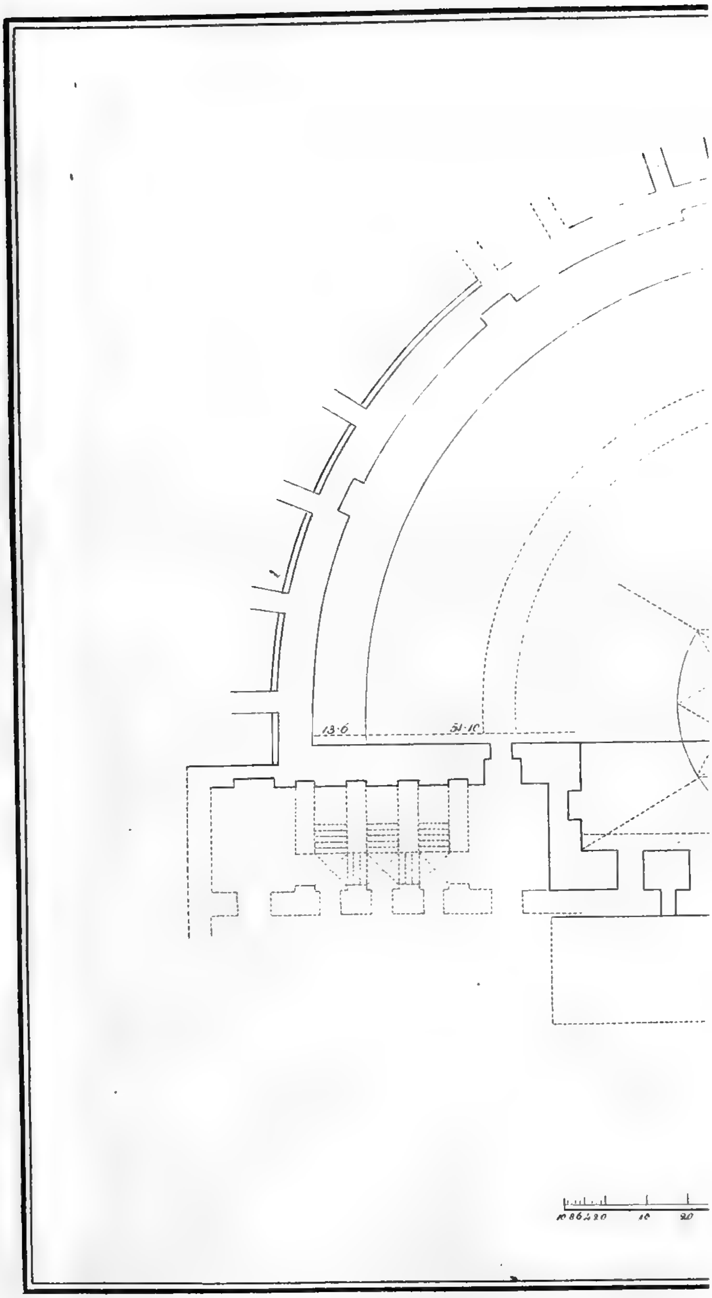


Plate. 4. To face Page 42. Ant:

F. Basire Sculp.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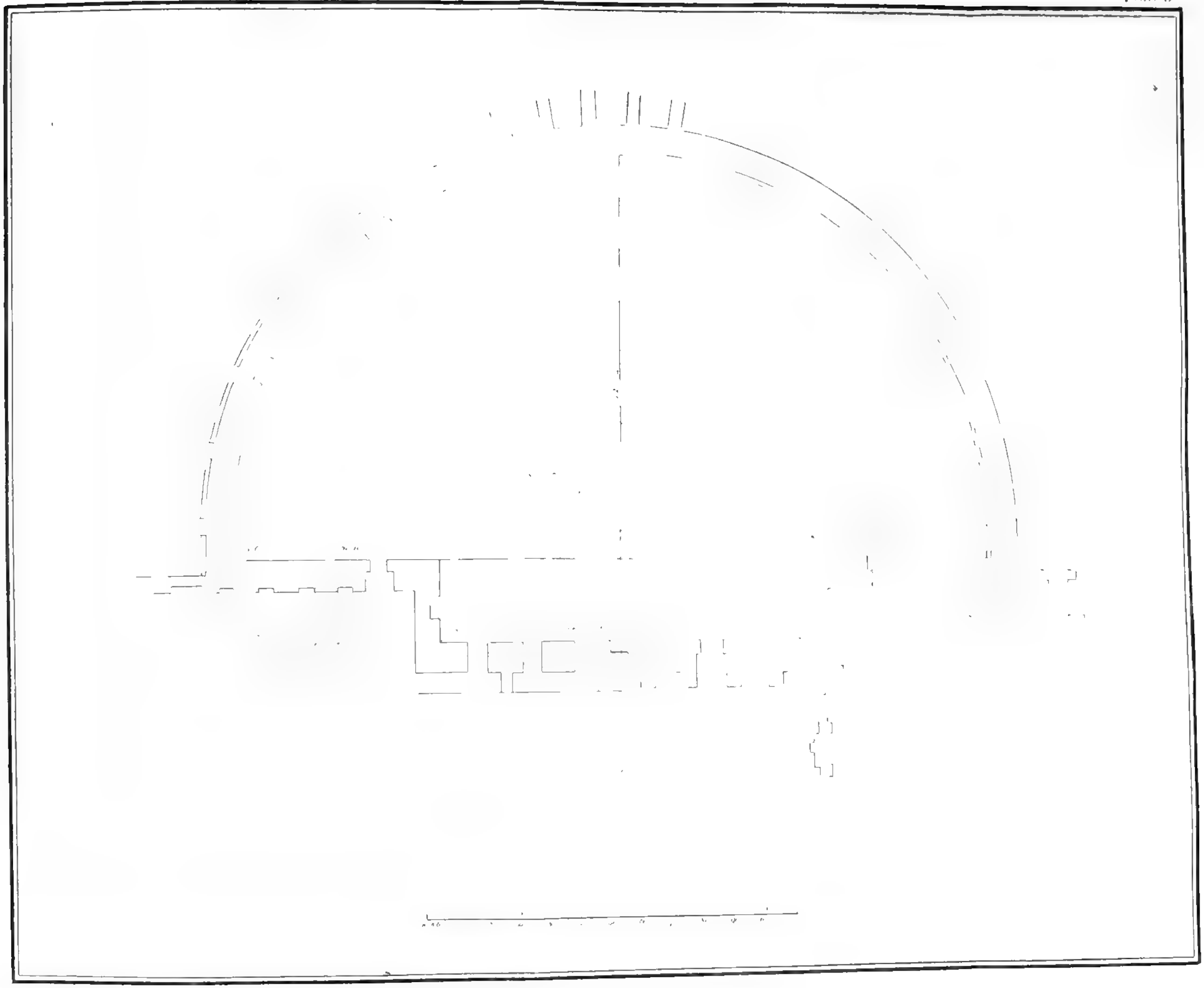


Plate the Fourth gives a view of the theatre as exiſting at preſent.

References.

a 1. *a* 1. Principal entrances to the orcheſtra and ſeats round it.

a 2. *a* 2. Entrances to the firſt claſs of benches by the vomitoria *b b*.

a 3. Entrance to the ſmall corridor of ſix feet wide leading to the ſecond claſs of benches by the vomitoria *c* 1. *c* 2. *c* 3. *c* 4. *c* 5.

a 4. Entrance to the ſecond claſs of benches by the vomitorium *c* 6.

a 5. *a* 6. Entrances to the ſmall corridor as at *a* 3.

a 7. *a* 7. Entrances to the principal corridor of ten feet ſix inches wide communicating to the third claſs of benches by the doors *d d d*.

a 8. Entrance to the third claſs of benches by the vomitorium *f*.

b b. Vomitoria leading to the firſt claſs of benches.

c 1. *c* 2. *c* 3. *c* 4. *c* 5. *c* 6. Vomitoria leading to the ſecond claſs of benches.

d d d. Doors opening from the principal corridor to the third clafs of benches.

e e e. Stairs leading up to the fumma cavea over the principal corridor.

f. Vomitorium leading to the fixth bench from the top of the third clafs.

g g. Communication to rooms from the fecond clafs of benches.

h h. Communication to rooms from the third clafs of benches.

i i. Small windows to light rooms.

k k. Stairs leading up from the level of the profcenium to the fourth bench from the top of the fecond clafs.

l l l. The fewer communicating from under the orcheftra to the outfide of the building.

m. A hole fix inches fquare and four feet fix inches deep.

n n n. Grooves cut in the ftone of the principal precinctio.

I HAVE

I HAVE already had the honour of laying before the Academy the inſcription which I diſcovered on the theatre, and which the learned profeſſor D. Franciſco Perez Bayer, the king's librarian, to whom I communicated it, ſuppoſes to be Rabbinical Hebrew, and not older than the thirteenth century. It is cut upon a window-ſtool in a room at the eaſt end of the ſcena; but upon the moſt ſtrict examination it appears to me to have been built up with the walls of that part of the building which, in its ſtructure, is ſimilar to the circular part, being in regular courſes of hammered limeſtone. The window-ſtool itſelf is only three inches thick, and has the appearance of baked clay, being of a yellowiſh red colour, of the conſiſtence of hard freeſtone, and very like the fragments of ſome alto relievos built up in the wall of a tower, improperly on that very account ſuppoſed to be, and called The Tower of Hercules. Fac ſimiles in yeſo of this inſcription, and ſeveral in the Celtiberic character, taken off at Saguntum, are now in the poſſeſſion of the Academy, copies and deſcriptions of which I take this opportunity of laying before them, together with all the other remains of inſcriptions in ancient Spaniſh characters which I was able to collect, and which have not hitherto been publiſhed.

THE firſt ſix were communicated to me by D. F. Perez Bayer, with the following obſervations:

No. I. This inſcription ſtood ſome time in the ancient Caſtulo, the country of Himilce the wife of Hannibal, now called Caldona or Cazlona, and depopulated: It lies between the city of Baeza and the town of Linares in Andaluſia.—In the year 1782 I was at this place, and fearched with the greateft care, but in vain, for this infcription. I was informed by the people of Linares that this ftone, with other large ones that had infcriptions on them, were converted into mill-ftones for the ufe of the mills on the river Guadalimar.

No. 2. In the hermitage of N. S. del Cid, in the Termino of Iglefuela, on the corner of the gate or portico of the fquare at entering on the right hand.

No. 3. In Alcala del Rio, three leagues from Seville, fuppofed by fome to have been Ilipa, by others Offet or Julia Conftantia, on the houfe of D. Matthias Felix Peraza in the fquare. I copied it accurately on the 15th October 1782.

No. 4. In the hermitage of N. S. del Cid, in the Termino of Iglefuela, on the left lintel of the door of a building called The Tenada, contiguous to the church.

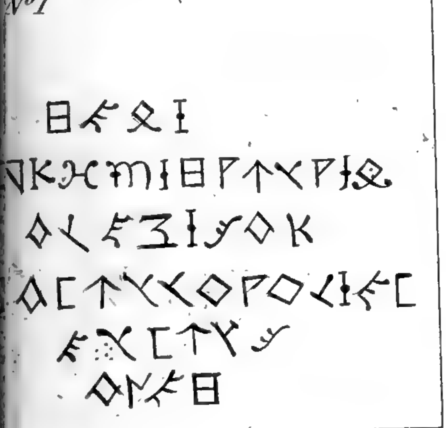
No. 5. On the door of the old houfe of the fame hermitage.

No. 6. In Polpis, a village of the territory of Alcala de Gibert in the kingdom of Valencia, on the lands of Jofeph Vincent Puig.

The next fix are the infcriptions copied at Morviedro in the month of April 1784, of which the Academy are in poffeffion of fac fimiles.

No.

No 1



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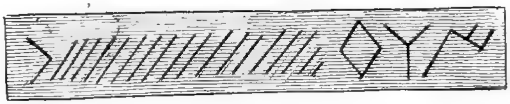

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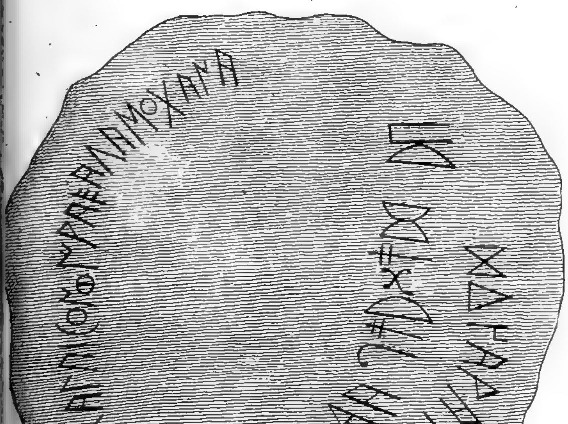

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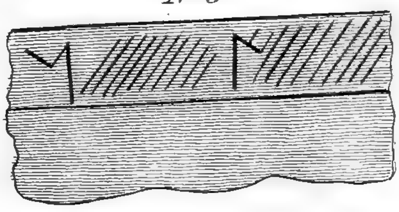

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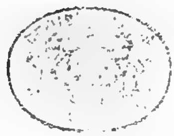

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Nº 12
Nº 15
Nº 16
Scale of Inches

No. 7. One foot eight inches in length by eight inches and a half in depth, upon limeftone.—It is on a pier of the cloyfter in the convent of the Trinitarios Calcados.

No. 8. One foot feven inches by eight inches, on limeftone. In the houfe of Michael Cambra Calle Ramor.

No. 9. One foot fix inches by eight inches, on marble. In the houfe of widow Michaele Serbera Calle Real.

No. 10. The fituation of this not noted.

No. 11. One foot eight inches by ten inches. In the wall within the citadel at the angle on the right hand below the tower of Hercules.

No. 12. One foot eight inches by one foot, on a gritftone. In the wall of the Ermita de Sangre de Chrifto.

No. 13. The infcription on a window-ftool of a room at the eaft end of the fcena in the theatre of Saguntum.

No. 14. An infcription fent to the Conde Lumiares by D. Leonardo Soler, canon of Orihuela, as exifting on the fection of a column in the citadel of Santa Pola; but the Count, who went to fee it, thinks that it is only the veins of the marble and ftrokes of the pick that have been taken for characters.

No.

No. 15. Two feet by one foot one inch and an half. This is a fragment, and was taken out of one of the old Moorifh gates of the town, pulled down in the year 1780.

No. 16. Four feet by eight inches. In the wall of the Ermita.

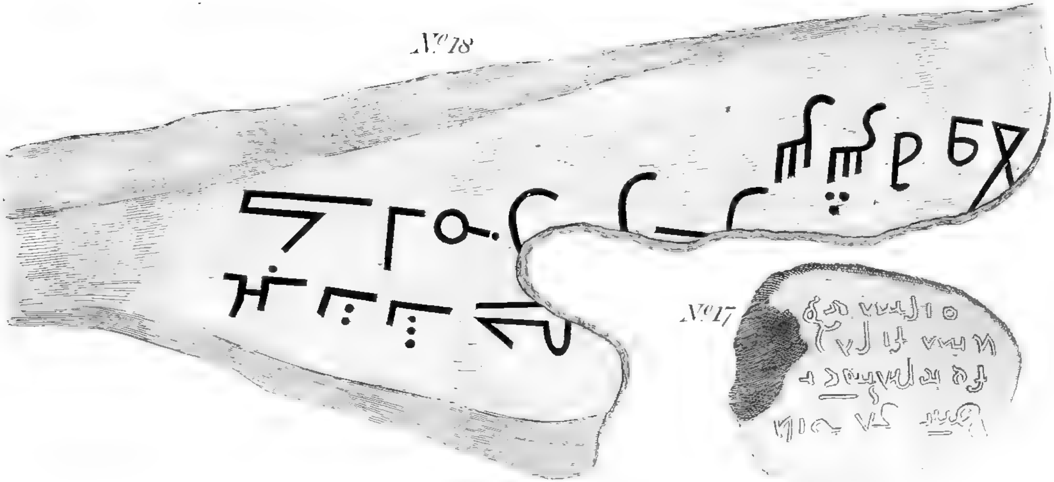
No. 17. The letters of this infcription are relieved, and the whole has the appearance of a petrified feal. It was found on the fea fide near Morviedro, of the fize and fhape of the engraving.

No. 18. This infcription is cut on a bone, and was found in the year 1761 on digging the foundation of the new cathedral at Lerida.—It was copied with the greateft care by D. Jofeph Corrador, and fent to Mr. Conyngham by D. Jayme Pafqual, canon of Bellpuiz de las Avellanas near Balaguer.

No.

N° 18

N° 17



LETTER *to* JOSEPH C. WALKER, *Esq*; *M.R.I.A.* *&c. from the Right Honourable* W. CONYNGHAM, *Treasurer to the Royal Irish Academy*; *being an* APPENDIX *to his* MEMOIRE *on the* THEATRE *of* SAGUNTUM.

DUBLIN, FEBRUARY 4th, 1790.

SIR,

SINCE I had the honour of laying before the Academy the Plans of the Theatre of Saguntum, I have seen that of the Theatre at Athens in the second volume of Mr. Stewart's Designs, and, as he has unsuccessfully attempted to ascertain the construction according to the rules of Vitruvius for Grecian Theatres, by inscribing three squares in a circle a tangent to whose diameter determines the front of the Scena, I beg to add to my former plans that of the Athenian Theatre, adapted to the principle of inscribing four triangles as represented in the plan of Saguntum; and so exactly does it correspond with the latter, as far as dependance may be had on the plan of Mr. Stewart, so remarkably coincident is the distribution and even measurement of every part,

part, that it might be ſuppoſed that the plan of the Theatre of Athens was adopted at Saguntum.

The relative meaſurements are as follow:

	Feet.	Inches.
Semi-diameter of the interior circle of the Theatre		
of Athens - - - - -	35	—
of Saguntum - - - - - -	29	—
Extent of ſcena at Athens - - -	117	1
at Saguntum - - - - -	118	—
Return of Scena at Athens - - -	25	1
at Saguntum - - - -	21	4
Upper portico including inſide wall at Athens - -	13	6
at Saguntum -	14	6
Width of benches of people at Athens - - -	29	—
at Saguntum - -	23	3
Principal Precinctio at Athens - - -	8	6
at Saguntum - - - -	8	6
Next diviſion of benches at Athens - - -	14	10
at Saguntum - - -	14	9
Total ſemi-diameter at Athens - - - -	124	4
at Saguntum - - -	122	7

SOME

Some difference appears by a larger ſpace being allotted to the benches of the people in the Theatre of Athens, and being conſtructed on a larger circle it makes a *ſcena receſſior*, but by the three ſemi-circles inſcribed on the ſcena at Saguntum this muſt have appeared conſiderably larger than that of the former.—The general diſpoſition is however evidently the ſame, and appearing to be inconſiſtent with the rules laid down by Vitruvius for the conſtruction and arrangement of the Greek theatres, it ſeems to be an argument in favour of thoſe travellers who ſuppoſe this to be the theatre built by Herodes Atticus, and not the ancient theatre of Bacchus.

I have the honour to be,

SIR,

Your moſt obedient,

And very humble Servant,

WILL. CONYNGHAM.

Joſ. C. Walker, Eſq;

LETTER *from Mr.* WILLIAM BEAUFORD, *A. B.* *to the Rev.* GEORGE GRAYDON, *LL.B. Secretary to the Committee of Antiquities, Royal Irish Academy.*

S I R,

THE account which Ptolemy the Egyptian geographer has given of the Britiſh iſlands in general, and of Ireland in particular, has been much controverted, reſpecting its authenticity; ſome aſſerting that the names of the places and people mentioned by that ancient writer have not the leaſt foundation in truth, and that no people under ſuch denominations ever exiſted in this iſland; whilſt others on the contrary contend, that the fourteen tribes given in his tables, comprehend all the nations at that time inhabiting Ireland. Thus circumſtanced, the ſubject may be thought not altogether unworthy of farther conſideration, when we ſhall find perhaps neither of the above aſſertions perfectly juſt. Read Jan. 16, 1790.

PTOLEMY compoſed his ſyſtem of geography from the MSS. in the Alexandrian library, and principally from the works of Marinus Tyrius, a navigator under the Romans, who, we may reaſonably preſume, collected every diſcovery made by the Romans on this ſubject, eſpecially that relative to the Britiſh iſlands,

from

from the accounts taken about the year 79 by order of Agricola, who was the firſt that directed a Roman fleet under the command of Demetrius to circumnavigate Great Britain and Ireland. (Euſeb. Præp. Ev. l. v. c. 17.) From the diſcoveries made by this fleet, and the information obtained from merchants trading to theſe iſlands, the Romans obtained the firſt circumſtantial account of Ireland. But from theſe accounts there is reaſon to imagine, that theſe merchants knew no more of its internal ſtate, than thoſe to whom Cæſar applied, did of that of Britain. (Comment. 24. 19.) They had indeed traded to the coaſts, landed their wares, and perhaps enquired and obtained the name of the diſtrict or people with whom they traded; but they made no further diſcoveries, the internal parts of the country were in a manner unknown to them.

The names therefore mentioned by Ptolemy, relate only to a few maritime diſtricts. But theſe diſtricts, both in reſpect to their names and ſituation, are far more accurately deſcribed than is generally imagined. The latitudes and longitudes are indeed wrong, probably ariſing from the inaccurate inſtruments in thoſe times uſed to take obſervations. The longitude is taken from Ferro, one of the Canary Iſlands. But we will paſs by the geographical and aſtronomical errors of our author, and proceed to conſider the topographical part of his work, which commences thus:

ΙΟΥΕΡΝΊΑΣ ΝΉΣΟΥ ΒΡΕΤΑΝΝΙΚΗ͂Σ ΘΈΣΙΣ.

Ireland thus denominated by Ptolemy Ιουερνια, and by Strabo Ιερνε, is perfectly agreeable to the indigenous name given to it by the natives from the remoteſt periods, who always diſtinguiſhed their

their country by the denomination of *Eirean*, *Erin* or *Ibh Eirean*, pronounced like *Cw Erin* or *Jov Erin*, whence the Greeks could exprefs the found no otherwife in their language than Ιερνε or Ιουερνια, nor the Romans than by *Hibernia*. Even the Welfh call it *Yverdon*, and alfo the Anglo-Saxons, according to Alfred in Orforius, *Ighernia*. The Danes were the firft who gave this ifland the appellation of *Irland* or *Iraland*, or the land of the *Ira*; but which like the others is derived from the indigenous *Ibh Erin*.

Hence we may obferve the care the ancients took to obtain the true names of the countries they difcovered; and Ptolemy, in the method he has taken in defcribing the coaft, feems to have followed the real tract of the Roman fleet, which having circumnavigated the northern coafts of Britain, fell in with the weftern ifles, which are thus defcribed, "Demetrius ait, infularum "quæ Britaniæ adjacent, effe multas defertas—quam pauci ad-"modum incolerent, fed qui Britannis facri omnes erant, et ab "omni direptione injuriâque tuti," (Eufeb. Præp. Ev. l. 5. c. 17, et Plutarch de Oracul. defect. T. 2. p. 419.) From thence failing for Ireland, the firft land they could make would be the northern parts of the county of Donegal about the north cape, called by Ptolemy Βορειον ακρον μοιρας, whence they proceeded eaftward, till they came to Fair Head in the county of Antrim, the utmoft ftretch of the northern coaft eaft, then returned and coafted along the weftern fhores of the ifland, thence along the fouthern, then returning by the eaftern again to Fair Head, croffed the channel and ran down the weftern coafts of Britain to the fouth, and finifhed their circumnavigation of both the ifles.

Ptolomy

Ptolomy thus deſcribes the northern coaſts:

Αρκτικῆς πλευρᾶς περιγραφῆ, ἧς ὑπέρκειται Ὠκεανός Ὑπερβόρειος. (Palat. addit.) ὁ ἄυτος καλεῖται πεπηγώς Ὠκέανος, και Κρονιος, ἢ νεκρος.

	Lon.	Lat.		Lon.	Lat.
Βόρειον ἄκρον μοίρας -	11.00	61.00	Ἀργίτα ποτ. ἐκβολαι -	14.30	61.30
Ὀυεννίκνιον ἄκρον - -	12.50	61.20	Ῥοβογδιον ἄκρον - -	16.20	91.30
Ὀυιδουα ποτ. ἐκβολαι -	13.00	61.00			

Παροικοῦσι δε τὴν πλευρὰν, ἀπὸ μεν δυσμῶν Ὀυεννέκνιοι. Ἔιτα εφεξῆς και πρὸς ἀνατολὰς Ῥοβόγδιοι.

Βόρειον ἄκρον, or the northern promontory, a name which it bears to this day, being north cape in the county of Donegal. Ware makes it Telen Head, a degree more ſouth, which is not at all probable.

Ὀυεννικνιον ἄκρον ſeems to be the *Bhen nigh nion* of the Iriſh, pronounced nearly Vennicnion, being the moſt northern headland of Innis Owen, in the county of Donegal, which ſtill among the natives retains its ancient name.

Ὀυιδουα ποτ. ἐκβολαι ſeems to be Lough Foyle, the *Loch Faibhail* of the Iriſh.

Αργιτα

Ἀργίτα ποτ. ἐκβολαι, or mouth of the White River, moſt probably the *Abhan Bán* of the Iriſh, the preſent River Ban in the county Antrim. Ware makes this Lough Foyle; and Camden Lough Swilly, though Ptolomy places it the moſt eaſtern river on the northern coaſt.

Ῥοβογδιον ἄκρον, as this cape, according to Ptolomy, is the moſt eaſtern point on the northern coaſt, it was probably Fair Head in the county of Antrim, the *Riabhachdagh* or fair cape of the Iriſh. Ware places it in Innis Owen, contrary to the aſſertion of Ptolemy.

Παροικοῦσι δε την, &c. that is, on the weſt of this (that is the coaſt) dwell the Venicnii. The Romans do not appear to have obtained the name of the inhabitants of this diſtrict, but to have called them from the neighbouring cape. They were the ancient inhabitants of the northern parts of the county of Donegal.

Ἔιτα εφέξῆς και προς, &c. that is, the others on the eaſt were the Robogdii. Here likewiſe the Romans did not obtain the real name of the inhabitants of the northern parts of the county of Antrim, but alſo denominated them from their principal cape, *Robogdii*, or the inhabitants of Fair Head.

PTOLOMY having deſcribed the northern coaſt, proceeds to the weſtern, being the next, moſt probably, diſcovered by the Romans.

Δυτικος

Δυτικῆς πλευρᾶς περιγραφὴ, ἡ παράκειται δυτικὸς ὠκεανός.

	Lon.	Lat.		Lon.	Lat.
Μετὰ τὸ βόρειον ἄκρον ὅ ἐστιν - - -	11.00	61.00	Αὔσοβα ποτ. ἐκβολαί -	10.30	59.30
Ῥαουιε. ποτ. ἐκβολαι -	11.20	61.00	Σηνόν ποτ. ἐκβολαί -	9.30	9.30
Νάγνατα πόλις ἐπίσημος	11.15	60.15	Δοὺρ ποτ. ἐκβολαί - -	9.40	58.40
Λιβνίου ποτ. ἐκβολαί -	10.30	60.00	Ἰέρνου ποτ. ἐκβολαί -	8.00	58.00
			Νότιον ἄκρον - - -	7.40	57.45

Ῥάουιου ποτ. ἐκβολαί. Though this river is placed by Ptolemy in the ſame latitude as the Vennicnium Cape, yet it has a more weſtern longitude ; and was probably the *Abhuin Reabhlangadh* of the Iriſh, the *Samarius* of Cambrenſis, and *Trowis* of Spencer and Camden ; the preſent mouth of the river Ern ; which by the Iriſh was frequently denominated the bounding or leaping river, from the waterfal at Ballyſhannon.

Ναγνατα πόλις επισημος, that is, the famous city of the Nagnatæ. It is not certain what city this was, no ſuch town being mentioned by any of the Iriſh writers. By its latitude and longitude it ought to be ſituated ſomewhere in the barony of Carbery and county of Sligo. It might be *Cnoc na teagh* or *Druimcliff*, which though at preſent only a deſolated village, is ſaid to have been in former times a large town, and in in ſubſequent periods, ſoon after the arrival of St. Patrick, an epiſcopal ſee.

Λιβόιου ποτ. ἐκβολαί, that is, the mouth of the river *Liboji* and the *Libnii* or *Limnii* of Pal. From the latitude and longitude this river ſeems to be the *Cluidh*, *Cluidhean* or *Cluibhagh* of the Iriſh, the preſent Clew Bay in the county of Mayo. Cambrenſis calls it *Slichneium*, and with Baxter the bay of Sligo. Richard of Cirenceſter calls it *Labius*, and Camden will have it the River Liffey; though Ptolemy is abſolutely deſcribing the weſtern coaſts.

Αὐσόβα ποτ. ἐκβολαί. Ware and Baxter make this river Lough Corrib in the county of Galway, from whence proceeds the River Glavia. It ſeems to be the *Abhſidhe* or *Abhanſidhe* of the Iriſh, the preſent bay of Galway.

Σηνου ποτ. ἐκβολαί, latitude 9° 30′ in moſt copies, but there ſeems to be an error in the table, the latitude ought certainly to be 59° 30′, and by Oroſius called *Sena*. It was certainly the *Seanan* of the Iriſh, the preſent mouth of the Shannon, as given by Baxter and Richard of Cirenceſter.

Δουρ ποτ. ἐκβολαί. Ware and Richard of Cirenceſter make this river the bay of Dingle, and Camden that of Tralee. It was probably the former, but the old Iriſh name is loſt. *Dur* in old Iriſh and Welſh is water.

Ιερνου ποτ. ἐκβολαί. This is called by Richard of Cirencester *Iberus*, and by Ware is supposed to be Kenmare river; it was evidently *Abhuin Ibh Earnan* of the Irish, the present Bear Haven.

Νότιον ἄκρον or southern promontory. The Romans do not appear to have obtained the Irish name of this cape, but have denominated it from its situation. It was most probably the *Ben Meisaimh Beallen* and *Mallobhaghagh* of the Irish, the present Mizen Head. Ware calls it *Willen Head*, and Camden *Bear Head.*

Of the people inhabiting the western coast, Ptolemy thus speaks, beginning where he set out on the north west.

Παροικοῦσι δε τὴν πλευρὰν μετα τοῦς Ουεννικνίους, Ἐρδίνοι
Ὑφ᾽ ὅυς Ναγνᾶται
Ἐῖτα Αυτεροί
Ἐῖτα Γαγγανοί
Ὑφ᾽ ὅυς Ουελαβοροι.

Ἐρδίνοι, in Edit. Pal. they are called ὁι και Ἐρπεδιτανοι. Ware thinks they were the ancient inhabitants of the county of Fermanagh about Lough Erne. Richard calls them *Hardini.* They were the northern *Ernai* or *Eir Gall* of the Irish, and the ancient inhabitants of *Eir Dunnagh* or *Dal Eirnagh*, and called in the Ulster annals *Dalnarians*, comprehending the counties of Donegal, Fermanagh, Leitrim and Roscommon. By the old

old Irish poets, Keating, and other Irish Historians, they are denominated *Tuatha de Danann*, *Fir Domnann*, *Fir Galeon*, *Nemed* and *Fir Bolga*; or rather they were several tribes, under these names, of the Fir Bolga. Some learned men have imagined that they were of Gothic and Germanic origin, who settled in this island prior to the Christian æra. If so, they might not improbably be some of the tribes of the *Balcæ*, the ancient inhabitants of the southern coasts of the Baltic (Mela) extending from Jutland in the west to the river Niemen or Nemed on the east, comprehending a number of tribes under various denominations, as *Vast Ernai*, the *Basturnæ* of Pliny; *Ira*, the *Hirri* of Pliny; the *Gyllen* of upper and lower Saxony, who also settled in Norway and Sweden under the several denominations of *Dief Gyllen*, *Fingyllen*, *Oestergyllen* and *Westergyllen*, and the *Dumna* in the Danish Isles. Wherefore the Ερδινοι of Ptolemy and *Ernai* of the Irish might be descendants from the *Vast Ernai* and *Ira*, and who were denominated also *Nemethones* or *Nemedones*, from being settled on that river; and were the *Nemed* of the Irish. The *Gyllen* in general were the *Fir Galeon* of the Irish. The *Dieff Gyllen* were the *Dubh Gals* of the Irish; the *Fingyllen* the Fin Gals of the Irish. The *Oestergyllen* the *Oir Gals* and the *Westergyllen* the *Eir Gals* of the Irish. The *Dumna* or Danes, were the *Fir Domnann* of the Irish. (For this compare Keating and other Irish historians with Pom. Mela, Pliny, Strabo, Snorro, Torfeus and other northerns.) These Gothic nations arrived in different periods at this island, that is from three hundred years prior to the Christian æra, to the close of the tenth century.

Ναγνᾶται. The Nagnatæ, as observed before, were the ancient inhabitants of the barony of Carbury in the county of Sligo.

Ἀυτεροι. The Auteri, are supposed by Ware, to be the ancient inhabitants of the counties of Galway and Roscommon. They were evidently the ancient inhabitants of the Irish *Ibh Errus*, the present barony of *Erris* in the county of Mayo.

Γαγγανοί. Ware thinks them the ancient inhabitants of the southern parts of the county of Galway. The Romans do not seem to have obtained the real name of these people, but, as in other instances, to have denominated them from their principal headland, in Irish *Cean Gibbne*, pronounced Kan Ganné, that is Dog's Head, still retaining its ancient name. They were therefore the ancient inhabitants of Conmacnemarra in that county.

Ὀυελιβοροι. Edit. of Pal. has οικαι Ἐλλιβροι. Ware thinks them the inhabitants of the northern parts of Kerry; but the Romans here also denominated them from their headland, in Irish *Beal Ibh Eiragh.* They were the inhabitants near Kerry head. In the neighbourhood of which Orosius places the

the *Lucanos*, the ancient inhabitants of Lixnaw in the barony of Clanmorris in the county of Kerry.

The Irifh writers alfo place here the *Degadii*, whom they call the fouthern Ernai. They are fuppofed to be a colony from Spain of the tribe or nation of the *Iberii*. They are called in the poem of the battle of Sliabhmis and in other Irifh poems *Dias Dhana* and *Sluagh adhbhal Eafbaine*; and in the Ulfter annals equally with the northern *Ernai Dalnarians*. If therefore they were a colony from Spain, they moft probably arrived in periods fubfequent to Ptolemy, and fometime in the eighth century. For the battle of Sliabhmis, according to the Ulfter annals, was fought in the year 775. And being denominated *Dias Dhana*, fouthern foreigners or Danes, in oppofition to *Tuatha de Danann* or northern foreigners or Danes in the fame poem, and equally Dalnarians, they were moft probably *Vandals*, who fled from the Saracens on their arrival in Spain. In this battle, faid by Keating and others to be the firft fought between the Tuatha de Danann and the Milefians from Spain, were flain O'Niall and Mc. Donnal, both chiefs of the northern Ernai; and alfo the monumental ftones, faid in the above poem to have been erected over the graves of the noble warriors, are ftill remaining on Mount *Cahirconree*, one of the Sliabhmis mountains in the county of Kerry.—(Smith's Kerry, Ulfter annals.) But to return, Ptolemy having defcribed the weftern coafts, doubles Notium Prom. and defcribes the fouthern coafts on the Vergivian ocean.

Τῆς ἐφεξῆ μεσημβρινῆς πλευρᾶς περιγραφή, ἣ παρακειτι ωκεανος Ουεργινιος (Palat. Ουεργιονιος.)

	Lon.	Lat.		Lon.	Lat.
Μετὰ τὸ νοτιον ἄκρον ὅ εστιν - - -	7°.40	57.45	Βιργου ποτ. εκβολαι -	12.30	57.30
Δαβρῶνα ποτ. εκβολαι	11.15	57.00	Ιερον ἀκρον - - -	14.00	57.30

Δαβρῶνα. This river Camden thinks is the river Lee or Cork river, which is alſo the opinion of Cambrenſis, who calls it Saveranus. But it was moſt probably the *Dubh Riannagh* and *Abham mor* of the Iriſh; now Blackwater or Youghal river. Dubh Riannagh is pronounced Duvronna, nearly.

Βριγον, &c. Edit. Pal. Βαργον. This river appears to be the *Abhan Barragh* or *Berbhagh* of the Iriſh, and the mouth of the preſent Barrow, at Waterford Haven, agreeable to Ware and moſt other writers.

Ιερὸν ἄκρον. The Romans called this the Sacred Promontory, probably from ſome religious worſhip performed on it. It was called in Iriſh *Cean Grian Cradh*, now Greenore Point, and by Cambrenſis denominated *Montem S. Dominici.*

Of

Of the inhabitants of this coaſt, Ptolomy obſerves,

Παροικοῦσι δὲ τὴν πλευρὰν μετα τους Ουελλαβορους Ουτερνοι.
Ὑπὲρ οὓς Ουοδιαι.
Και ανατολικώτεροι, Βριγωντες.

Ουτερνοι Pal. Ιουερνιοι. Ware thinks them the inhabitants of Deſmond. They being denominated *Iberni*, *Iberi*, and *Juërni*, were moſt probably the *Ibh Earagh* of the Iriſh, the ancient inhabitants about Bear Haven, and the ſouthern parts of the county of Kerry, and a part of the ſouthern Ernai.

Ουοδιαι. Ware makes theſe the ancient inhabitants of the counties of Cork, Limerick and Tipperary. The ancient name of this diſtrict is loſt; they were probably the inhabitants of Corcaluighe, containing the ſouthern parts of the preſent county of Cork.

Βριγαντες. Ware makes theſe the inhabitants of the counties of Carlow, Kilkenny, and the Queen's County. But no ſuch name is mentioned by the Iriſh. The Romans therefore probably denominated them from their neighbouring river *Brigus* or *Bargus*, if they did not miſtake *Brigus* for Brigantes, a nation in Britain. They ſeem to be the ancient inhabitants of the county of Waterford, called by the Iriſh *Hy Breoghan*, and the inhabitants *Breaghnach* or *Breoghnach*, that is *Breathnach* or Britons. Admitting therefore that they extended farther inland, they might be the *aboriginals* from Britain, before the

arrival

arrival of the *Ernai*, *Heremonii*, and other Gothic tribes. But of this there is no certainty. However that the *Breoghnach* were Britons, is in ſome meaſure evinced from the mountains near which they dwelt in the county of Waterford, being denominated *Cummeragh* or Welſh Mountains to this day.

Of the eaſtern coaſts Ptolomy thus ſpeaks

Ανατολικῆς πλευρᾶς περιγραφῆ, ἡ παράκειται Ωκεανός καλουμενος Ιουερνιος.

	Lon.	Lat.		Lon.	Lat.
Μετὰ το ιερον ἄκρον ὃ εστι - - - -	14.00	57.30	*Βουουινδα ποτ. εκβολαι* -	14.49	59.40
			Ισαμνιον ακρόν - -	15.00	60.00
Μοδονου ποταμοῦ εκβολαι - - - -	13.40	58.40	*Ουινδέριος ποτ. εκβολαι* -	15.00	60.15
Μανάπια πόλις - -	13.30	58.40	*Λογια ποτ. εκβολαι* -	15.20	60.40
Οβοκα ποτ. εκβολαι -	13.12	59.00	*Μεθ᾽ ἃς το Ροβογδιον ακάκρον* - - - -		
Ἔβλανα πολις - -	14.00	59.30			

Μοδονου, &c. Ware ſuppoſes this river to be the preſent Slany, and harbour of Wexford; it was therefore the *Abhuin Maidoc* of the Iriſh.

Μαναπια πολις. Neither Camden, Ware, Baxter, or other modern writers, have been able to aſcertain the true ſituation of this city, placing it at Waterford, Wexford or Wicklow; but having

ing the ſame latitude, and only 10′ difference of longitude with Modonus, it was moſt probably ſomewhere on that river. Ptolomy has not obtained the real Iriſh name, which was *Lough Garman*, which the Danes of the 9th century tranſlated into *Waesfiord*, now Wexford; and improperly called the city of the Manapii, which was *Choille Mantann* or *Lough Garchon*, now Wicklow.

Οβοκα ποτ εκβολαι. This river ſtill retains its ancient name, being called in Iriſh *Oboca* or *Ovoca*, and is the preſent river of Arklow.

Ἐβλανα πολις. This city has been ſuppoſed by Richard of Cirenceſter, Camden, Ware and others, to be Dublin, from the idea that *Eblana* is the ſame as *Dubhleana*. Ptolomy by his latitudes places it ¾ of the diſtance between the rivers Οβοκα and Βουουινδα, that is the Arklow and Boyne rivers, which is the real ſituation of Ruſh harbour. An harbour in former ages much frequented by foreign traders. Ptolemy indeed doth not mention the city by its real name, but only calls it the city of the *Blanii*. It poſſibly may be Dublin, though I am rather inclined to think it was near Ruſh, perhaps Luſk, which is ſaid to be an ancient city, and erected into a biſhopric ſoon after St. Patrick, and anciently denominated *Luſcán*, in Latin *Luſcanum*. As to Dublin, the word is probably not of Iriſh but Teutonic origin, for the Norwegians and Danes who ſettled in this iſland called it *Diflin* and *Divelin*, from whom the Iriſh denominated it *Dubhlean*, and the Welch *Dinas Dulin*, but

 the

the indigenous name in Irifh was always as at this day *Blacli* or *Ballicleath* and *Balliathcliath.* It was therefore moft probably founded by fome of the northern nations, but in what period uncertain.

Βουουινδα ποτ. εκβολαι. This river is evidently the *Abban Boand* or *Abbuin Bouind* of the Irifh, the prefent river *Boyne*, agreeable to the affertions of Ware, Camden and others.

Ισαμνιον ακρον. Thought by Ware, Camden and Baxter, to be St. John's Point in the county of Down; the Irifh name is now loft.

Ουινδεριος ποτ. εκβολαι. Thought by Ware and Baxter to be the bay of Carrickfergus, but was rather the *Aban Daragh* or *Loch Cuan* of the Irifh, the prefent bay of Strangford.

Λογια ποτ. εκβολαι. Thought by Ware to be Lough Neagh and Ban river, and by Baxter Lough Foyle; but Ptolomy is defcribing the eaftern, not the northern coafts. It moft probably was the *Loch Guis* or *Bella Fearfad* of the Irifh, the prefent bay of Carrickfergus.

PTOLOMY now fpeaks of the people inhabiting the eaftern coafts, in doing which he returns, beginning at the north and proceeds to the fouth.

Παροικουσι

Παροικουσι δε την πλευραν ταυτην μετα τους Ροβογδιους.

Δαρνιοι	Ειτα Καυκοι
Ὑφ' ὅυς Ουολουντιοι·	Ὑφ ὅυς Μαναπιοι
Ειτα Βλανιοι	Ειτα Κοριονδοι υπερ τους Βριγαντας.

Δαρνιοι, Edit. Pal. Δαρινοι. Ware thinks theſe were the ancient inhabitants of the counties of Londonderry, Antrim and Tyrone. They were probably the ancient inhabitants of the Iriſh *Dairanii*, *Dairecalgaic*, *Dal Rieta*, *Dal na Ruidhe* and *Dalriada*, the ſouthern parts of the county of Antrim. Richard of Cirenceſter places here the *Damnii*, who are thought to be of Germanic or Gothic origin, though immediately from Britain. They might be deſcended from the Saxon tribes *Reudigni* and *Dumna.* As we find a tribe by the Iriſh called *Rudrici* ſettled in the counties of Armagh and Monaghan.

Ουολουντιοι. Theſe ſeem to be the ancient inhabitants of *Ulagh*, *Uladh* or *Ulidia*, the preſent county of Down, agreeable to Ware and others; they are alſo thought to be of Gothic origin, and ſettled here under *Ulagh*, a Goth or Norwegian, ſometime before the Chriſtian æra. (Vide Harris's Down, Keating, &c.)

Βλανιοι. Edit. Pal. Εβλανοι. Thought by Ware and Baxter to be the ancient inhabitants of the counties of Meath and Dublin. They were the inhabitants of the Iriſh *Almeanna* or *Almain*, containing the maritime coaſts from the Boyne to the Liffey

 (vide

(vide book of Lecan.) This diſtrict was alſo called *Acibh Leanna*, from whence not improbable the Βλανιοι of Ptolemy and Εβλανοι of Pal. There is ſome appearance of theſe alſo being of Gothic origin; perhaps deſcended from the *Almanni*, a tribe of the *Hermionii* or Germans (vide Pliny and Tacitus), and who were denominated by the Iriſh *Heremonii* and *Eireamhoin* (vide Keating, O'Conor).

Καυκοι. Theſe are thought by Ware to be the ancient inhabitants of the counties of Kildare and Wicklow. They were the inhabitants of *Atha Cuacdhagh* or *Atacdii*, the maritime parts of the county of Dublin ſouth of the Liffey (vide book of Lecan.) Theſe too are thought by Ware to be of Germanic origin, and deſcendants from the *Chaucii*, a tribe of the Hermionii, *Hecrmanii*, *Garmanii* or Germans (vide Ortelii Theſaur Geogr. in Chaucis, et Pliny.)

Μαιαπιοι. Ware thinks theſe the ancient inhabitants of the counties of Wexford; they ſeem to be thoſe of *Acibh Mantann* or *Ibh Garchon*, a maritime diſtrict near Wicklow. Camden and Ware think them the deſcendants of the Belgic Menapii.

Ειτα Κοριονδοι υπερ τους Βριναυτας. Theſe are thought by Ware to be the ancient inhabitants of the counties of Cork, Limerick and Tipperary, but Ptolemy is ſpeaking of the eaſtern coaſts, not the ſouthern or midland parts. But what led Ware into this miſtake was probably reading Ptolemy. *After the Coriondii above the Brigantes are the inland cities*, inſtead of, *Afterwards the*

the Coriondii above the Brigantes. The inland cities are. They were most probably the Carimandii of *Ibh German*, a maritime district near Wexford. They were also denominated *Martinii* and *Moragh*, and were probably Britons, for the mountains on the west of the county are denominated in Irish *Sliabh Breaghnach* or British mountains to this day.

THE Irish called this county, and also all the south of Leinster, *Lagean*, and the inhabitants *Ligmanii*, who are said also to be *Heremonii* and *Gallians* or *Goillens*. They might therefore not improbably be of Germanic origin, as the *Lygii* or *Lygmanii* were a tribe of the *Hermionii* or *Gyllen*, in Germany. (Book of Lecan, Keating, O'Connor, Pliny, Tacitus, Cluverius, &c.)

PTOLEMY having thus finished his description of the maritime coasts, treats of the internal parts as far as then known; but which knowledge extended only to a few cities or the residence of chiefs.

Πολεις δε εισι μεσογειοι.

	Lon.	Lat.		Lon.	Lat.
Ριγια - - - -	13.00	60.20	Ετερα Ριγια - - -	11.00	59.30
Ραιβα - - - -	12.00	58.54	Δουνον - - - -	12.20	58.45
Λαβερος - - -	13.00	59.51	Ιουρνις - - - -	11.00	58.10
Μακολικον - - -	11.30	58.40			

Ριγια. This city is ſuppoſed by Mercator to be Limerick, and by Camden near Logh Ree. By its latitude and longitude in Ptolemy's tables, it appears to be *Eamania* or *Eamhan*, ſituated near Armagh, and at preſent called *Rath n'Eavan*, or the fort of Navan, whoſe remains conſiſt of a circular intrenchment of conſiderable extent.

Ραιβα. The latitude of this city is in ſome copies 56° 54′, but as no part of the iſland according to Ptolemy's tables, is in ſo low a latitude, it is evidently wrong, and other copies which have it 58° 54′ ſeem to be right. It is juſtly by Ware and Camden ſuppoſed to be *Rebon*, ſituated on the Barrow near Athy. There are ſtill remaining a large rath, and the ruins of a caſtle, built by Michael Lord of *Reban* in the reign of king John.

Λαβερος. This city is ſuppoſed by Ware to be *Cenanus* or *Canenus*, the preſent Kells in the county of Meath. It is evidently the celebrated *Tara* or *Teambra*, called alſo *Labhradh*. On the hill of Tara ſtill remains ſome circular raths and other remains of antiquity.

Μακολικον. Mercator and Camden will have this city to be *Malc* or *Milick* on the Shannon, but no ſuch name exiſts. Baxter thinks it Kilkenny. By its ſituation according to Ptolemy, it was moſt probably the *Cancora* of the Iriſh, the capital and reſidence of the chiefs of Thomond, ſituated near Killaloe, where there ſtill remains a large rath or intrenchment.

Ετερα

Ετερα Ριγια or *another Regia.* Camden thinks this ſituated in Lough Derg in the county of Donegal, where St. Patrick's purgatory was; and Camden thinks it Athenry in the county of Galway; but from the ſituation in Ptolemy's tables it ſeems to be the *Croghan* of the Iriſh, the ancient capital of Conaught, ſituated between Boyle and Elphin in the county of Roſcommom. There are ſtill remaining a rath and an ancient cemetery, called by the natives *Rioligh na Righ.*

Δουνον. From Ptolemy's tables, this city and Ραιβα were not far from each other; yet Camden will have it to be Downpatrick in the county of Down; whilſt Ware very juſtly thinks it *Dunnamaes* in the Queen's county, the ancient ſeat of the chieftains of Leix, ſituated on an iſolated rock, where ſtill remains the ruins of a caſtle built by Lord Pembroke in 1216. In reſpect to *Dunum* and *Reban* there ſeems an error in Ptolemy's tables, that is, the latitude and longitude of Reban is given to Dunum, and vice verſa.

Ιουρνις Edit. Pal. Ιερνις. According to Richard of Cirenceſter there were two *Ibernia*, one he places on the eaſt ſide of the Shannon, the other on the Blackwater, the port of *Inſovenach.* The former being an inland city was moſt probably the Ιουρνις of Ptolemy, and thought by Whitaker to be *Cahir* near Bruff in the county of Limerick, where there ſtill remains ſome raths, cromlecs and other monuments of antiquity.

PTOLOMY

Ptolomy having thus finiſhed his deſcription of both the maritime and inland parts of Ireland, ſpeaks of the iſlands which ſurround it. Among which he includes ſeveral of the weſtern iſles of Scotland under the name of Εβουρα. But I ſhall only treat of thoſe which more properly appertain to this iſland, as

	Lon.	Lat.		Lon.	Lat.
Ρικινα - - - -	15.40	62.00	Εδρου ἔρημος - - -	15.00	57.30
Μονα Νῆσος - -	15.00	57.40	Λιμνου ἔρημος - -	15.00	59.00

Ρικινα. This iſland is evidently the iſle of *Rachlin* or *Raghlin* the *Ricnea* of Pliny on the coaſt of Antrim.

Μον Νῆσος. This is evidently the iſle of Angleſea in Wales, but by Ptolemy brought too near Ireland, who calls alſo the iſle of Man Μοναυιδα or Μοναρινα according to ſome MSS.

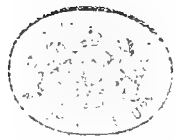
Ειδρου Edit. Pal. Ὀδρου. This is called by Pliny *Andros*, and by Ware is juſtly ſuppoſed to be *Beg Eri* on the coaſt of Wexford.

Λιμνου. Ware juſtly ſuppoſes this to be *Lambey* on the coaſt of the county of Dublin.

Thus I have endeavoured to explain Ptolemy, in reſpect to his geography of this kingdom. If theſe comments on one of the


ANCIENT
IRELAND
Of the 1st Century
According
to
Ptolomy
OKEANOS
UPERBOREIOS
DUTIKOS
OKEANOS
OKEANOS
IOUERNIOS
OKEANOS OUERGINIOS
ALOUIONOS
ROBOGDOI
DARNOI
ERDINOI
NAGNATOI
AUTEROI
OUOLOUNIOI
GANGANOI
ELANOI
OUELIBOROI
KAUKOI
MANAPIOI
KORIONDOI
OUTERNOI
OUODIAI
BRIGANTES
Nagnata
Rigia
Labero
Rigia etera
Eblana
Makolikon
Raiba
Ioumis


ANCIENT
IRELAND
Of the 5th Century
According to
the
Natives
ALBAN
MUIR
MUIRGEAG
ANIOR
Emania
ULAD
RUDRICI
Tara
Croghan
GALLAN
ERNAI
IBERI
IBREOGHAN
CARIMANDI
Garman
Cahir
Mullohhoghagh



the oldeſt geographers extant, who have treated on this country, ſhould be thought to merit the attention of the Royal Iriſh Academy, you will do me the honour to lay them before that learned body.

THE Iriſh names which I have collated with thoſe of Ptolemy, are either taken from Iriſh writers, or from the voice of tradition. I have alſo added two maps, one drawn by Ptolemy's tables on the nautical projection; the other according to the moderns, on the ſame projection; and the names taken either from Iriſh authors, or from the names of places ſtill remaining. In theſe maps Ptolemy's names are given in the original language, but in modern characters, by which means, may be ſeen more eaſily by inſpection, to what degree of accuracy that author attained.

DURING this inveſtigation, I have alſo mentioned the opinion of the learned reſpecting the origin of the tribes, but have only ſtated them as opinions and conjectures. To confute or ſupport them would render this letter much too long; I ſhall therefore conclude with aſſuring you, that

I am, Sir,
Your faithful, much obliged,
And moſt obedient, humble ſervant,
W. BEAUFORD.

Athy,
Jan. 2, 1790.

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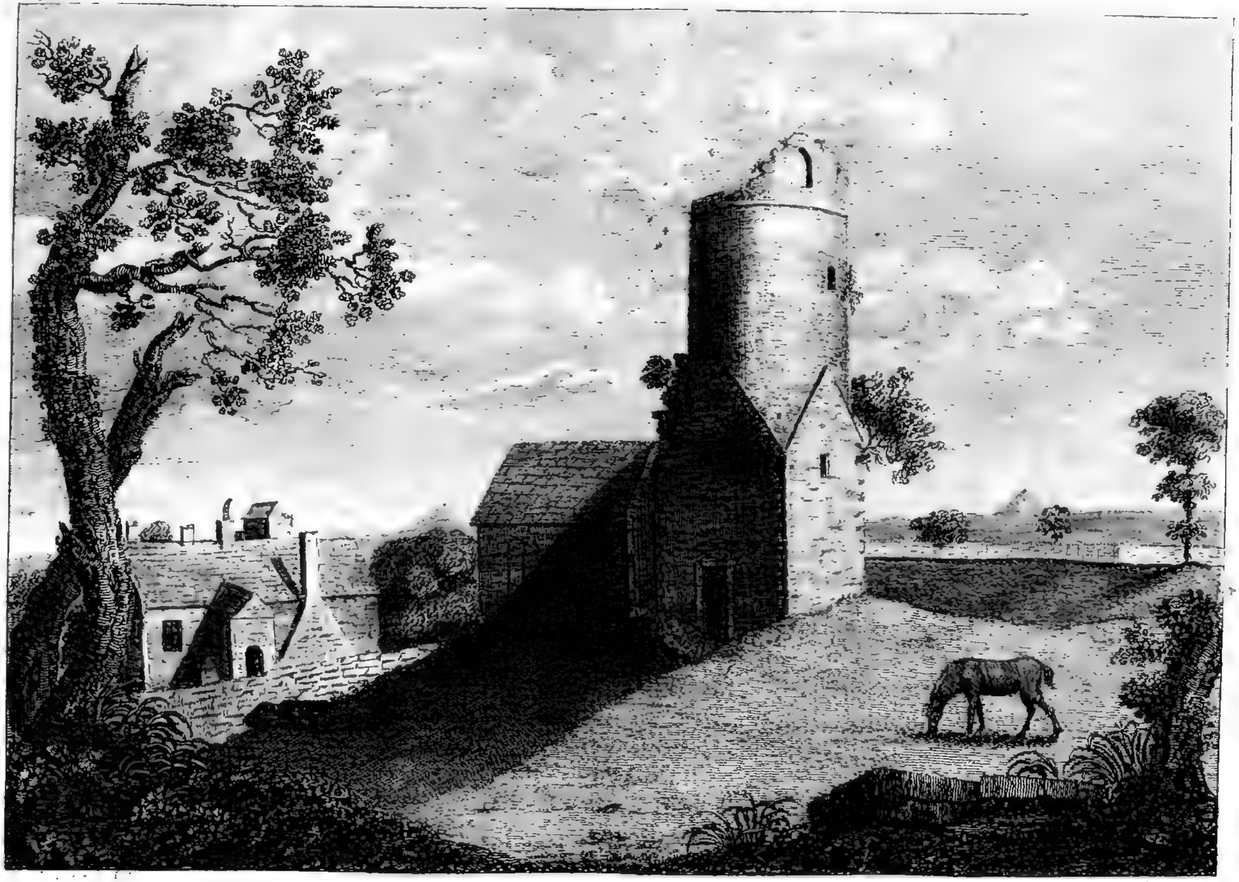

KILLOSSY.

A MEMOIR *respecting the* ANTIQUITIES *of the* CHURCH *of* KILLOSSY, *in the County of* KILDARE; *with some* CONJECTURES *on the* ORIGIN *of the* ANCIENT IRISH CHURCHES. *By Mr.* WILLIAM BEAUFORD, *A. B.*

THE parish church of Killossy, in the barony of Naas and county of Kildare, for the singular construction of its steeple, merits the attention of the curious in antiquities. The present church is modern, but stands on the foundation of the ancient, which it equals in every dimension, except the height of the roof, which originally was constructed of stone, as in the ancient churches of Glendaloch and Cashel, as is apparent from some remains of it still visible at the tower. The tower which joins the church is round, and founded on a square base, nearly half of the present height, each side terminating in a pediment or triangle of equal altitude, from one of which proceeded the stone roof of the old church. Read Jan. 20, 1790.

This is probably an unique, being the only one of the kind, I believe, yet discovered in this kingdom, if not in the British isles. The tower of the ivy church at Glendaloch, and the old church of Little Saxum near Bury in England, are those which have the greatest resemblance to this of Killossy; but the square base only of the tower at Glendaloch joins the church, and the whole tower at Saxum is round from the foundation*. In the island of Zante is, or was till lately, a Greek church exactly on the same construction with this; also several others in various parts of the Levant †. The insulated round towers, and the small old churches discovered in Ireland, with stone roofs and circular arches, as those of Cashel and Glendaloch, and those in England called Saxon, as well as those in Spain named Massorabic, bear a great resemblance to each other, and are apparently of the same origin ‡; being all, most probably, constructed on the model of the Grecian architecture, but not in that noble style so conspicuous in the monuments of ancient Greece and Rome, but in the degenerate style of the latter Greek empire, from the time of Arcadius at the beginning of the fifth, to the close of the tenth century.

The various Gothic and Vandalic tribes, which seated themselves in the west of Europe, and on the ruins of the Greek and Roman

* Antiquarian Rep.

† Plan and View of Zante, by a French Engineer.—*Paris Edit.* See also Drummond's Travels, in which are views and plans of a number of curious buildings in the east similar to those under consideration, and round towers resembling those in Ireland, several of which have been converted into minarets by the Turks.

‡ Grose's Antiquities, vol. 1. Antiq. Rep. Swinburn's Travels in Spain.

Roman empires, were undoubtedly ignorant of the art of conſtructing edifices of lime and ſtone; it muſt therefore have been from the inhabitants of the ſouthern parts of Europe that they obtained that knowledge, and the Grecian and Roman ſtyles of architecture of the middle ages became diffuſed through the weſt of Europe prior to the ninth century; as well from the zeal of the Greek and Roman miſſionaries, as from the ſpirit of pilgrimages to the holy land.

As early as the middle of the ſeventh century the Greek architecture was introduced among the Anglo Saxons by Greeks, who followed Theodorus, Archbiſhop of Canterbury. By the intereſt of Theodorus theſe Grecians eſtabliſhed a ſchool or academy at *Creekelade*, or *Greekeſlade*, in Wiltſhire, and erected that church in the Grecian architecture about the year 670*, reſembling which the church of Hexham was founded by Wilfred anno 674†, and the church of Weremouth by Benedict in 675‡. However it doth not appear that the Grecian and Roman architecture were common in Britain prior to the eſtabliſhment of the academies at Cordova and Otranto. For about the year 759 an academy was eſtabliſhed at Cordova by the Saracenic Prince Abdoulrahman, for the ſtudy of agriculture, geometry, aſtronomy, architecture and phyſic, by profeſſors brought from Greece, Conſtantinople, Egypt and the Eaſt; which academy, during the ninth and tenth centuries,

* Groſs's Antiq. vol. 1. Godwin's Eng. Biſhops, page 54, 558, 560.

† Godwin's Eng. Biſhops, p. 560.

‡ Groſs's Antiq. vol. 1.

centuries, was much frequented by the ſtudious of all nations and religions in the Weſt *.

Also, between the years 843 and 882, Charles the Bald eſtabliſhed a ſimilar academy at or near Otranto in Italy, and ſupplied it with learned men from Greece, principally prieſts †. In this ſeminary the Anglo Sexons maintained and educated a number of their youths, at the deſire of Alfred the great, and through the intereſt of Plegmund his tutor with Pope Marianus ‡.

But the Iriſh in thoſe periods, not being under the dominion of the Roman pontiffs, would not be very deſirous of ſtudying either at Rome or in Italy, but chiefly applied to the Maſſorabic and Gothic Chriſtians, and reſorted to the academy of Cordova. A circumſtance which has cauſed the inhabitants of this iſland to look up to Spain with veneration §, and pilgrimages from hence to that kingdom were frequent down to the fifteenth century ||.

From theſe academics the Grecian and Roman architecture of the middles ages became diffuſed throughout the weſt of Europe, and divided into various branches among the ſeveral nations, by means

* Abulfud Amal. Swinburn's Travels in Spain. Voltaire.

† Swinburn' Travels through Sicily, vol. 1.

‡ Godwin's Eng. Biſhops, p. 60.

§ Keating, Chronicum Scotorum, and other I. MSS. ſpeak of theſe journies, and the connexion of the Iriſh Princes with Spain, though placed in too early periods.

|| Smith's Hiſt. of Waterford.

means of the clergy and other learned perſons, who brought home with them certain foreign artificers, compoſed of Greeks and Italians, and Greeks and Spaniards formed into fraternities of architects, and who in ſubſequent periods joining thereunto French, Germans and Flemings, obtained papal bulls from their encouragement and particular privileges, became corporate bodies, and ranged from one nation to another, under the denomination of Free Maſons*. To the firſt of theſe fraternities we are indebted for the Roman, Saxon and Iriſh architecture of the middle ages, and to the latter for the various ſpecies of the Gothic, to the cloſe of the fifteenth century.

The ſeveral branches of the Grecian and Roman ſtyle during the middle ages, though they preſerved a general likeneſs of their original, differed in ſome reſpect from each other, according to the genius, taſte and manners of the people among whom they were cultivated. The Roman, Saracenic, Maſſorabic, Saxon and Iriſh, had ſome peculiar traits which ſpecified the nation in which they were erected.

The Saxon churches were generally rectangular edifices, terminating in a ſemicircle at the eaſtern end, and their roofs low, ſcarcely viſible above the cornice. The ſteeples, when they had any, were generally ſquare, and placed either at the weſt end or ſides; though ſome, not common, were round. And they had generally under them a cript or under croft, for the preſervation of

* Wren's Parentalia.

of ſacred reliques, &c. after the manners of the Romans and Italians*.

The Iriſh alſo had cripts to their ſtone churches, but theſe cripts were not under but upper crofts, ſituated in the roofs between the circular ſtone ceiling and the ſtone pediment roof; as in the churches of Glendaloch and Caſhel, agreeable to the cuſtom of the Maſſorabic Chriſtians, who ſtill uſe ſuch apartments as choirs†. On this account the roofs of the Iriſh churches were raiſed remarkably high, and gave them a different appearance from thoſe of the Saxons. Another diſtinguiſhing feature between the Saxon and the Iriſh architecture, was the inſulated round tower of the latter, which the Anglo Saxons do not appear to have uſed, any more than the Italians, at leaſt very ſparingly; but numbers are to be found near the old Greek churches in the Eaſt, which were by the Moſlems adopted as minarets to their temples, and watch towers to their fortreſs; as appears from ſeveral ſtill remaining on the Ebro, and in other parts of Spain‡.

Though it has been obſerved that the Greek architecture had been introduced into Britain about the middle of the ſeventh century, yet ſtone buildings were by no means common till towards the cloſe of the ninth; for St. Paul's Church, London, continued of wood to the year 900, when Biſhop Theodred rebuilt it of ſtone

* Groſe's Antiq. vol. 1.

† Swinburn's Travels in Spain.

‡ Ibid.

ſtone with an under croft after the Roman architecture, called ſince St. Faith's Church, which church was alſo again rebuilt in the Gothic ſtyle in 1087*.

THE Iriſh do not appear to have adopted this ſpecies of architecture, or to have built in lime and ſtone, prior to the ninth century. The firſt edifice of this kind was moſt probably the oratory of Malachy, about 850; towards the cloſe of which century the churches of Glendaloch were erected and ornamented in the taſte of the northern nations†. Soon after Cormac's chapel in 906‡; the monaſtery of Monaincha about 1000§; and the round tower of Kenith, in the county of Cork, in 1015||. From this period, ſtone-roofed criptic churches and round towers became common in this iſland, and continued the reigning taſte for eccleſiaſtical buildings until the introduction of the Norman and Gothic architecture in the beginning of the twelfth century, when the ſtyle was diſcontinued.

HAVING thus, in as conciſe a manner as poſſible, given ſome conjectures relative to the origin of the ancient Iriſh churches, and the times of their erection, it will be neceſſary to con-

 ſider

* Godwin's Eng. Biſhops.

† Ledwiche's Iriſh Antiquities.

‡ Harris's Iriſh Biſhops.

§ Ledwiche's Iriſh Antiq.

|| Smith's Hiſtory of Cork, vol. 2.

ſider the period in which the church under conſideration was founded.

If we give credit to legendary accounts, St. Patrick founded an abbey here for his nephew St. Auxil, who died in 454, and who gave name to the church, that is, *Kill-auxaile*, *Kill-aſaille*, or *Ceall-uſall*, afterwards *Kill-uſſi* and *Killoſſy**. But leaving ſuch relations to the biographers of Saints, it may not be unneceſſary to obſerve, that the diſtrict in which this church ſtands was anciently denominated *Magh Laiſagh*, ſituated in Magh Libhiadh, the old name of the N. E. parts of the county of Kildare; and part of the demeſne lands of the O'Kelly's, chieftains of the country, who in the eighth century are ſaid to have founded a monaſtery here; the church and other buildings whereof, according to the faſhion of the country, were moſt probably erected of wood, and denominated from the diſtrict *Ceallmagh Laiſagh* or *Cillmoilaiſſy*, and from thence *Killmoloſſy* or *Killoſſy*, or the *Church of Magh Laiſſy*†. This monaſtery in 833 was burnt to the ground by the the Danes‡, and being rebuilt, was a ſecond time with Cillcullen or Killcullen deſtroyed in 984 by the Danes, under the command of Ambroſe ſon of Godfrey, when one thouſand perſons were taken priſoners, and the intire country deſtroyed§. After this period

* Arch. Monaſt. ex M'Geog. p. 658.

† Pſalter of Caſhel, Keating.

‡ Ann. 4 Maſters.

§ Ibid.

period the church was rebuilt of lime and ſtone, in the then new mode of architecture, of which the tower ſtill remains a monument, as in the annexed view.

The ancient wooden churches, and other edifices of the Iriſh, being eaſily deſtroyed by fire, were conſtantly expoſed to the depredations of the Danes and other roving plunderers. There were no other means of ſaving the ſacred reliques, veſtments, &c. of the churches, and the wealth of the inhabitants, than by hiding them in ſubterraneous caves. The method therefore of building churches entirely of ſtone, with upper crofts, was a great improvement, as it gave a place of ſecurity to the goods of the inhabitants, as well as to the ſacred utenſils; for the churches being entirely of ſtone could not be eaſily burnt; and the entrances into the upper crofts, being only by narrow newel ſtairs, or by ladders through ſtone trap doors, they could not be plundered without pulling down the building, which in thoſe deſultory expeditions they had ſeldom time to do.

On the arrival of the Engliſh, the diſtrict of Magh Laiſſy in Naaſe O'Felim, in the country of M'Caëlan or Kelly, with its church, was granted to Maurice Fitz Gerald by Henry the Second*, who erected a caſtle near the church, ſome remains of which are ſtill viſible. From this period Killoſſy became

 a pariſh

* Ware. Sir John Davis.

a parifh church, which during the conteft between the Irifh and Englifh was laid in ruins, and continued in that ftate for feveral years; but it has lately been repaired for divine fervice, by a grant from the board of Firft-fruits.

On the weft end of the church, under a rifing ground, are a number of fubterraneous artificial caves, in a dry fandy foil, with pediment roofs, and communicating with each other by fmall apertures. One of thefe caves, near the church, had its fides compofed of ftone, and covered with flat ftones, in which was found part of a quern and the bones of fome fowls. The other caves have no other wall or covering than the native earth. Thefe caves, with others of a fimilar nature found in feveral parts of Ireland, were the granaries or magazines of the ancient inhabitants, in which they depofited their corn and provifions, and into which they alfo retreated in time of danger. In the Brehon Laws they are mentioned under the name of *Log*, and by thofe laws a fine was inflicted on any perfon who ftole any provifions or goods out of them*. Thefe granaries are thus defcribed by a Danifh captain of the ninth century in the Iflandic annals: " Leifr going a pyrating " towards the weft, infefted Ireland with his arms. Here he found " large fubterraneous caves, whofe entrances were difmal and " dark, but on proceeding he faw the glittering of the weapons, " which the foldiers within held in their hands. Leifr killed the men, " and brought the fwords away, together with a great quantity of " other

* Afdad lan log legad creice, viz. Afda lan log legaide creachadh.

" other riches; whence he was afterwards called *Hiorleifr*, or " *Leifr* of the Swords *." These caves were used as granaries long after the arrival of the English †; and numbers of various constructions have been from time to time discovered in several parts of the kingdom. Those at Killossy seem to have belonged to the ancient monastery, and were within its inclosure.

* Antiq. Celto Scandic, p. 14, ex Landnamaboc.

† Cambrensis, l. 2, c. 1.