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# MEDICAL ESSAYS

AND

# OBSERVATIONS.

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# CONTENTS.

Art. Pag.
I. *THE Meteorological Register,* 1
II. *An Account of the Diseases that were most frequent last Year in* Edinburgh, 26
III. *An Extract from the public Register of Burials in* Edinburgh, 29
IV. *The good Success of* opposite Caustics *and of a strong* alterative Mercurial *Medicine, by Dr* Edward Barry, *Physician at* Cork, *and* F. R. S. 29
V. *An Essay towards ascertaining the Doses of vomiting and purging Medicines, by Dr* Charles Balguy, *Physician at* Peterborough. 32
VI. *The bad Effects of Opium given too soon to stop the operation of* Emetics, *by Mr* John Steedman, *Surgeon at Kinross.* 35
VII. Pulvis Stypticus *recommended in uterine Hæmorrhagies, by Dr* Alexander Thomson, *Physician at* Montrose. 36
VIII. *Violent Effects of a* Mercurial Suffumigation, *by Mr* James Hill, *Surgeon in Dumfries.* 38
IX. *The Effects of a very small quantity of* Arsenic, *by Dr* Alexander Thomson, *Physician at* Montrose. 41
X. *Histories of* Gangrenes, *cured by the* Peruvian Bark, *by* several Hands. 42
XI. *The Description and Uses of the* Intestinum Duodenum, *by* Alexander Monro, *Professor of*

Art. Pag.
*of Anatomy in the University of* Edinburgh, *and* F. R. S. 57
XII. *Miscellaneous Remarks on the* Inteſtines, *by* the ſame. 65
XIII. *Obſervations concerning the* Placenta, *the two Cavities of the* Uterus, *and* Ruyſch's *Muſcles in* Fundo Uteri, *by Dr* Thomas Simſon, *Profeſſor of Medicine in the Univerſity of* St Andrew's. 79
XIV. *An Eſſay concerning the Motions of our* Eyes, *Part* II. *by Dr* William Porterfield, C. R. M. E. S. 103
XV. *A Wound with a hot Iron penetrating the* Pelvis, *by Dr* Andrew Williſon, *Phyſician at* Dundee. 236
XVI. *A Stone in the Bladder formed on a Needle*, *by Mr* Andrew Brown, *Surgeon in* Dalkeith. 238
XVII. *An* Aneuriſm, *by* Alexander Monro, *P. A.* 240
XVIII. *A white Swelling of the Knee*, *by* the ſame. 242
XIX. *Part of the Cartilage of the Joint of the Knee ſeparated and oſſified*, *by* the ſame. 244
XX. *An uncommon Tumor of the Knee*, *with Remarks on the* white Swellings *of the Joints*, *by Dr* Thomas Simſon, *P. M.* 246
XXI. *An Ulcer of the Leg*, *by* Alexander Monro, *P. A.* 251
XXII. *Remarks on the* Amputations *of the larger* Extremities, *by* the ſame. 257

XXIII. *An*

Art. Pag.
XXIII. *An Essay on* nervous Fevers, *by Dr* Ebenezer Gilchrist, *Physician at* Dumfries. 280
XXIV. *Remarks on the Cure of* Agues, *by Dr* Alexander Thomson, *Physician at* Montrose. 326
XXV. *Anomalous Shakings after an* Ague *cured by Dr* Andrew Willison, *Physician at* Dundee. 331
XXVI. *A* Mania *from a callous* Pia Mater, *by Dr* Edward Barry, *Physician at* Cork. 332
XXVII. *An* Epilepsy *from an uncommon Cause, by Dr* Thomas Short, *Physician at* Sheffield, *and* F. R. S. 334
XXVIII. *Of the Cure of an Ulcer of the* Lungs, by *Blood-letting, by* ——— 335
XXIX. *A collection of Matter in the* Liver *evacuated by Stool, by Mr* James Jamieson *Surgeon at* Kelso. 341
XXX. *An uncommon* Dropsy *from a steatomatous* Omentum, *by* Alexander Monro, *P. A.* 343
XXXI. *A total Obstruction of the* Valve *of the* Colon, *by Dr* Thomas Short, *Physician at* Sheffield. 353
XXXII. *A Child without an* Anus *or* Intestinum rectum, *by Mr* James Jamieson, *Surgeon at* Kelso. 354
XXXIII. *Coagulated Blood extravasated upon the* Uterus, *and the* Thickness *of the* Womb *in a laborious Birth, by Mr* John Paisley, *Surgeon in* Glasgow. 355

XXXIV. Books

Art. Pag.
XXXIV. Books *omitted in the former Volumes of this Collection.* 361
XXXV. *An Account of the most remarkable* Improvements *and* Discoveries *in Physic made or proposed since the beginning of the Year* 1734. 363
XXXVI. *A list of* Medical Books *published since the beginning of the Year* 1734. 400
XXXVII. Books *proposed, and other medical News.* 408

MEDICAL

# MEDICAL ESSAYS AND OBSERVATIONS.

## ARTICLE I.

### *The Meteorological Register.*

FOR underſtanding the following Obſervations, and comparing them with others, it is neceſſary to know the form and ſituation of the inſtruments with which they were made, which are deſcribed in Art. II. of Vol. I.

## *JUNE* 1734.

| D. | Hour. | Baro. In. | Baro. D. | Ther. In. | Ther. D. | Ther. I. | Hyg. D. | Wind. Dir. | Wind. For. | Weather. | Rain In. D. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 9 a m | 29 | 7 | 12 | 8 | 3 | 0 | N E | 0 | cloudy | 0,988 |
|  | 5 p m | 29 | 7 | 13 | 1 | 2 | 1 | E | 1 | cloudy |  |
| 2 | 9 a m | 29 | 7 | 13 | 4 | 2 | 2 | S E | 0 | rain | 0,084 |
|  | 6 p m | 29 | 7 | 12 | 6 | 2 | 7 | E by S | 0 | rain |  |
| 3 | 9 a m | 29 | 7 | 13 | 5 | 2 | 0 | S W | 1 | fair | 0,135 |
|  | 5 p m | 29 | 7 | 14 | 0 | 1 | 5 | W | 1 | fair |  |
| 4 | 9 a m | 29 | 8 | 13 | 6 | 1 | 6 | W | 0 | fair | 0,075 |
|  | 6 p m | 29 | 8 | 14 | 9 | 1 | 4 | W | 1 | fair |  |
| 5 | 8 a m | 29 | 8 | 12 | 7 | 2 | 0 | N | 0 | fair | 0,056 |
|  | 7 p m | 29 | 9 | 13 | 6 | 1 | 6 | N E | 0 | cloudy |  |
| 6 | 9 a m | 29 | 9 | 12 | 4 | 1 | 8 | E | 2 | cloudy |  |
|  | 7 p m | 29 | 8 | 12 | 5 | 1 | 5 | E | 2 | cloudy |  |
| 7 | 9 a m | 29 | 8 | 11 | 9 | 1 | 4 | N E | 2 | fair | 0,044 |
|  | 5 p m | 29 | 8 | 12 | 9 | 1 | 3 | E | 1 | fair |  |
| 8 | 9 a m | 29 | 9 | 12 | 3 | 1 | 5 | E | 2 | fair |  |
|  | 7 p m | 29 | 9 | 12 | 1 | 1 | 5 | E | 2 | fair |  |
| 9 | 9 a m | 29 | 9 | 13 | 1 | 1 | 4 | E | 1 | fair |  |
|  | 9 p m | 29 | 9 | 12 | 6 | 1 | 4 | E | 1 | cloudy |  |
| 10 | 9 a m | 30 | 0 | 13 | 2 | 1 | 4 | E | 2 | fair |  |
|  | 5 p m | 30 | 0 | 13 | 1 | 1 | 4 | E | 2 | cloudy |  |
| 11 | 9 a m | 30 | 0 | 13 | 0 | 1 | 5 | S E | 0 | cloudy |  |
|  | 5 p m | 30 | 0 | 14 | 1 | 1 | 6 | S E | 0 | fair |  |
| 12 | 9 a m | 30 | 0 | 14 | 8 | 1 | 5 | N W | 1 | fair |  |
|  | 5 p m | 30 | 0 | 15 | 7 | 1 | 3 | N W | 1 | fair |  |
| 13 | 9 a m | 29 | 9 | 14 | 5 | 1 | 7 | E | 0 | fair |  |
|  | 7 p m | 29 | 9 | 14 | 8 | 1 | 5 | E | 1 | fair |  |
| 14 | 9 a m | 29 | 9 | 13 | 2 | 2 | 2 | N E | 3 | fair |  |
|  | 5 p m | 29 | 8 | 13 | 2 | 1 | 9 | N E | 2 | fair |  |
| 15 | 9 a m | 29 | 7 | 12 | 7 | 2 | 7 | N E | 2 | cloudy |  |
|  | 8 p m | 29 | 7 | 12 | 8 | 2 | 8 | N E | 2 | fair |  |
| 16 | 9 a m | 29 | 7 | 13 | 7 | 1 | 6 | N E | 2 | fair |  |
|  | 5 p m | 29 | 8 | 13 | 6 | 1 | 7 | N E | 2 | cloudy |  |
|  |  |  |  |  |  |  |  |  |  |  | 1,382 |

## *JUNE* 1734.

| D. | Hours | Baro. In | D. | Ther. In | D. | Hyg. I. | D. | Wind. Dir. | For. | Weather. | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | 1,382 |
| 17 | 9 a m | 29 | 9 | 12 | 9 | 1 | 9 | N E | 2 | cloudy | 0,035 |
| | 8 p m | 29 | 9 | 4 | 1 | 1 | 7 | N E | 2 | fair | |
| 18 | 9 a m | 30 | 0 | 15 | 0 | 1 | 8 | S E | [illegible] | fair | |
| | 6 p m | 30 | 0 | 16 | 5 | 1 | 3 | S | 1 | fair | |
| 19 | 8 a m | 30 | 0 | 16 | 8 | 1 | 4 | S | 0 | fair | |
| | 6 p m | 29 | 9 | 17 | 4 | 1 | 3 | S E | 2 | fair | |
| 20 | 9 a m | 29 | 8 | 15 | 2 | 1 | 4 | S E | 1 | fair | |
| | 7 p m | 29 | 8 | 15 | 6 | 1 | 2 | S E | 1 | fair | |
| 21 | 9 a m | 29 | 8 | 14 | 3 | 2 | 0 | N E | 2 | fair | |
| | 7 p m | 29 | 7 | 13 | 7 | 2 | 5 | N E | 2 | fog | |
| 22 | 9 a m | 29 | 7 | 14 | 8 | 3 | 1 | N W | 1 | cloudy | |
| | 8 p m | 29 | 7 | 14 | 6 | 2 | 0 | S W | 0 | fair | |
| 23 | 9 a m | 29 | 7 | 13 | 6 | 3 | 0 | N E | 2 | fog | 0,455 |
| | 8 p m | 29 | 7 | 12 | 7 | 3 | 2 | N E | 2 | fog | |
| 24 | 9 a m | 29 | 7 | 12 | 2 | 3 | 7 | N E | 3 | cloudy | 0,234 |
| | 6 p m | 29 | 7 | 12 | 1 | 3 | 3 | N E | 1 | cloudy | |
| 25 | 9 a m | 29 | 6 | 13 | 1 | 3 | 0 | E | 0 | cloudy | 0,035 |
| | 6 p m | 29 | 6 | 13 | 9 | 2 | 6 | E | 0 | fair | |
| 26 | 9 a m | 29 | 6 | 13 | 9 | 2 | 3 | W | 1 | fair | |
| | 8 p m | 29 | 7 | 14 | 7 | 1 | 7 | W | 1 | fair | |
| 27 | 9 a m | 29 | 8 | 14 | 4 | 1 | 8 | W | 1 | fair | |
| | 8 p m | 29 | 8 | 15 | 0 | 1 | 5 | W | 1 | fair | |
| 28 | 9 a m | 29 | 8 | 14 | 6 | 1 | 4 | W | 2 | fair | 0,044 |
| | 7 p m | 29 | 9 | 14 | 8 | 1 | 3 | W | 2 | fair | |
| 29 | 9 a m | 29 | 9 | 14 | 7 | 1 | 5 | W | 2 | fair | |
| | 6 p m | 29 | 9 | 15 | 6 | 1 | 2 | W | 1 | fair | |
| 30 | 9 a m | 29 | 7 | 14 | 3 | 1 | 7 | S | 1 | cloudy | 0,025 |
| | 7 p m | 29 | 5 | 14 | 4 | 1 | 8 | S | 1 | cloudy | |
| H. at a med. | | 29 | 8 | 13 | 8 | 1 | 8 | | | Total depth | 2,210 |
| Gr. height | | 30 | 0 | 17 | 4 | 3 | 7 | | | | |
| L. height | | 29 | 5 | 10 | 1 | 1 | 2 | | | | |

## JULY 1734.

| D. | Hour. | Baro. In | Baro. D | Ther. In | Ther. D. | Hyg. I. | Hyg. D. | Wind. Dir. | Wind. For. | Weather. | Rain. In D. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 9 a m | 29 | 6 | 13 | 3 | 1 | 8 | W | 3 | fair | |
| | 7 p m | 29 | 6 | 14 | 2 | 1 | 5 | W | 3 | fair | |
| 2 | 9 a m | 29 | 6 | 13 | 3 | 1 | 6 | W | 3 | fair | |
| | 8 p m | 29 | 7 | 13 | 5 | 1 | 7 | N W | 2 | fair | |
| 3 | 9 a m | 29 | 9 | 14 | 6 | | 5 | N | 1 | fair — | 0,050 |
| | 6 p m | 30 | 0 | 14 | 5 | 1 | 5 | N E | 1 | fair | |
| 4 | 9 a m | 30 | 1 | 14 | 4 | 1 | 4 | N E | 2 | fair | |
| | 7 p m | 30 | 1 | 14 | 7 | 1 | 3 | N | 1 | fair | |
| 5 | 9 a m | 30 | 1 | 15 | 4 | 1 | 3 | N | 0 | fair | |
| | 7 p m | 30 | 1 | 15 | 4 | 1 | 3 | N W | 1 | fair | |
| 6 | 9 a m | 30 | 1 | 14 | 6 | | 5 | W | 1 | fair | |
| | 8 p m | 30 | 1 | 14 | 8 | 1 | 3 | W | 1 | fair | |
| 7 | 9 a m | 30 | 0 | 14 | 5 | 1 | 8 | E | 1 | cloudy | |
| | 5 p m | 30 | 0 | 14 | 6 | 1 | 4 | E | 1 | fair | |
| 8 | 9 a m | 30 | 1 | 14 | 6 | 1 | 7 | E | 1 | fair — | 0,114 |
| | 7 p m | 30 | 1 | 15 | 0 | 1 | 4 | E | 1 | fair | |
| 9 | 9 a m | 30 | 0 | 15 | 6 | 1 | 6 | S E | 1 | fair — | 0,056 |
| | 5 p m | 30 | 0 | 15 | 3 | 1 | 5 | S E | 1 | fair | |
| 10 | 9 a m | 30 | 0 | 15 | 3 | 1 | 8 | N W | 1 | cloudy | |
| | 5 p m | 30 | 0 | 16 | 4 | 1 | 5 | N W | 1 | fair | |
| 11 | 9 a m | 29 | 9 | 15 | 5 | 1 | 4 | W | 2 | cloudy | |
| | 6 p m | 29 | 9 | 15 | 4 | | 4 | S W | 2 | cloudy | |
| 12 | 9 a m | 29 | 7 | 15 | 3 | 1 | 4 | W | 2 | fair | |
| | 7 p m | 29 | 6 | 14 | 7 | 1 | 5 | S W | 1 | rain | |
| 13 | 9 a m | 29 | 4 | 14 | 6 | 1 | 7 | S W | 2 | fair — | 0,074 |
| | 7 p m | 29 | 4 | 15 | 5 | 1 | 3 | S W | 2 | fair | |
| 14 | 9 a m | 29 | 3 | 14 | 9 | | 4 | W | 2 | cloudy | |
| | 6 p m | 29 | 3 | 13 | 1 | 2 | 0 | N | 2 | rain | |
| 15 | 9 a m | 29 | 6 | 12 | 6 | 1 | 9 | N W | 1 | cloudy | |
| | 7 p m | 29 | 7 | 13 | 6 | 1 | 3 | N W | 1 | cloudy | |
| 16 | 9 a m | 29 | 5 | 14 | 6 | 1 | 8 | S W | 2 | cloudy | |
| | 6 p m | 29 | 5 | 14 | 8 | 1 | 4 | W | 2 | cloudy | |
| | | | | | | | | | | | 0,294 |

## JULY 1734.

| D. | Hour. | Baro. In | D. | Ther. In | D. | Hyg. I. | D. | Wind. Dir. | For. | Weather. | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | 0,294 |
| 17 | 9 a m | 29 | 5 | 13 | 9 | 1 | 6 | W | 1 | fair | |
| | 5 p m | 29 | [illegible] | 13 | 7 | 1 | 6 | E | 1 | cloudy | |
| 18 | 9 a m | 29 | 8 | 12 | 6 | 1 | 6 | E | 2 | fair — | 0,030 |
| | 6 p m | 29 | 9 | 13 | 7 | 1 | 3 | E | 2 | fair | |
| 19 | 9 a m | 29 | 9 | 14 | 3 | 1 | 4 | W | 2 | fair | |
| | 5 p m | 29 | 8 | 15 | 3 | 1 | 2 | S W | 2 | cloudy | |
| 20 | 9 a m | 29 | 6 | 13 | 7 | 1 | 5 | W | 3 | cloudy — | 0,053 |
| | 6 p m | 29 | 7 | 14 | 0 | 1 | 4 | W | 3 | cloudy | |
| 21 | 9 a m | 29 | 7 | 14 | 4 | 1 | 6 | N W | 2 | cloudy | |
| | 6 p m | 29 | 7 | 14 | 6 | 1 | 6 | N W | 2 | cloudy | |
| 22 | 9 a m | 29 | 9 | 14 | 7 | 1 | 8 | W | 2 | fair | |
| | 7 p m | 29 | 9 | 15 | 6 | 1 | 6 | N W | 2 | fair | |
| 23 | 9 a m | 29 | 9 | 15 | 3 | 1 | 5 | N W | 2 | fair | |
| | 7 p m | 29 | 9 | 16 | 5 | 1 | 5 | W | 2 | fair | |
| 24 | 9 a m | 29 | 8 | 16 | 0 | 1 | 9 | N by W | 1 | fog | |
| | 8 p m | 29 | 8 | 15 | 3 | 1 | 6 | N W | 1 | cloudy | |
| 25 | 9 a m | 29 | 8 | 15 | 6 | 1 | 6 | W | 1 | fair — | 0,190 |
| | 5 p m | 29 | 8 | 15 | 0 | 1 | 7 | W | 1 | fair | |
| 26 | 9 a m | 29 | 5 | 15 | 1 | 2 | 6 | S E | 1 | hazy — | 0,020 |
| | 6 p m | 29 | 5 | 15 | 0 | 1 | 9 | S E | 2 | rain | |
| 27 | 9 a m | 29 | 4 | 14 | 3 | 2 | 4 | E | 2 | fog | |
| | 6 p m | 29 | 3 | 15 | 0 | 2 | 1 | E | 1 | cloudy | |
| 28 | 9 a m | 29 | 3 | 14 | 1 | 3 | 0 | E | 1 | rain | |
| | 6 p m | 29 | 4 | 13 | 5 | 3 | 5 | E | 1 | fog | |
| 29 | 9 a m | 29 | 5 | 13 | 7 | 3 | 8 | E | 1 | fog — | 0,076 |
| | 8 p m | 29 | 6 | 14 | 6 | 2 | 9 | E | c | cloudy | |
| 30 | 9 a m | 29 | 7 | 14 | 3 | 2 | 7 | E | 2 | fair | |
| | 3 p m | 29 | 8 | 14 | 1 | 2 | 5 | E | 1 | fair | |
| 31 | 9 a m | 29 | 8 | 13 | 6 | 2 | 7 | E | 1 | cloudy — | 0,046 |
| | 5 p m | 29 | 8 | 14 | 2 | 2 | 1 | E | 1 | cloudy | |
| H. at a med. | | 29 | 7 | 14 | 1 | 1 | 7 | | | Total depth | 0,709 |
| Gr. height | | 30 | 1 | 16 | 5 | 3 | 8 | | | | |
| L. height | | 29 | 3 | 12 | 6 | 1 | 2 | | | | |

## *AUGUST* 1734.

| D. | Hour. | Baro. In. | D. | Ther. In. | D. | Hyg. I. | D. | Wind. Dir. | For. | Weather. | Rain. In D. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 9 a m | 29 | 7 | 13 | 7 | 2 | 6 | W | 2 | cloudy | |
| | 5 p m | 29 | 8 | 14 | 3 | 2 | 7 | E | 2 | rain | |
| 2 | 9 a m | 30 | 0 | 12 | 6 | 1 | 9 | W by N | 2 | fair — | 0,045 |
| | 6 p m | 30 | 0 | 14 | 2 | 1 | 5 | N W | 2 | fair | |
| 3 | 9 a m | 30 | 0 | 13 | 0 | 1 | 8 | N W | 2 | fair — | 0,090 |
| | 5 p m | 30 | 1 | 14 | 7 | 1 | 5 | N E | 2 | fair | |
| 4 | 9 a m | 30 | 0 | 14 | 0 | 1 | 7 | N W | 2 | fair — | 0,180 |
| | 5 p m | 29 | 9 | 15 | 6 | 1 | 5 | W by N | 2 | fair | |
| 5 | 9 a m | 29 | 9 | 14 | 4 | 2 | 3 | N W | 2 | hazy | |
| | 7 p m | 29 | 8 | 14 | 5 | 2 | 2 | W | 2 | cloudy | |
| 6 | 9 a m | 29 | 8 | 14 | 9 | 1 | 6 | W | 1 | hazy — | 0,260 |
| | 7 p m | 29 | 9 | 14 | 6 | 1 | 6 | W | 1 | hazy | |
| 7 | 9 a m | 29 | 7 | 14 | 5 | 1 | 7 | S W | 2 | cloudy | |
| | 7 p m | 29 | 7 | 14 | 4 | 1 | 9 | W | 2 | cloudy | |
| 8 | 9 a m | 29 | 7 | 13 | 5 | 1 | 7 | W | 1 | fair | |
| | 7 p m | 29 | 7 | 14 | 3 | 1 | 4 | W | 1 | fair | |
| 9 | 9 a m | 29 | 6 | 13 | 8 | 2 | 1 | E | 1 | hazy — | 0,340 |
| | 7 p m | 29 | 5 | 14 | 7 | 2 | 6 | E | 1 | fog | |
| 10 | 9 a m | 29 | 2 | 14 | 4 | 3 | 0 | E | 1 | hazy —— | 0,115 |
| | 7 p m | [illegible]9 | 0 | 12 | 7 | 4 | 9 | N | 1 | rain | |
| 11 | 9 a m | 29 | 1 | 12 | 5 | 3 | 0 | W | 2 | rain | |
| | 6 p m | 29 | 2 | 12 | 9 | 2 | 5 | W | 2 | rain | |
| 12 | 9 a m | 29 | 6 | 13 | 5 | 2 | 2 | W | 2 | fair | |
| | 7 p m | 29 | 7 | 13 | 6 | 1 | 8 | W | 1 | clear | |
| 13 | 9 a m | 29 | 7 | 13 | 3 | 2 | 0 | W | 1 | fair | |
| | 7 p m | 29 | 6 | 13 | 8 | 1 | 7 | S W | 1 | fair | |
| 14 | 9 a m | 29 | 7 | 13 | 7 | 1 | 8 | S W | 1 | fair | |
| | 7 [illegible] m | 29 | 6 | 14 | 0 | 1 | 6 | S S W | 1 | cloudy | |
| 15 | 9 a m | 29 | 6 | 13 | 9 | 2 | 0 | S W | 1 | cloudy — | 0,090 |
| | 7 p m | 29 | 6 | 14 | 3 | 1 | 5 | W | 1 | cloudy | |
| 16 | 9 a m | 29 | 6 | 13 | 7 | 1 | 8 | W | 2 | fair | |
| | 7 p m | 29 | 6 | 14 | 0 | 1 | 6 | W | 1 | fair | |
| | | | | | | | | | | | 1,120 |

## AUGUST 1734.

| D | Hour. | Baro. In | D. | Ther. In | D | Hyg. I. | D, | Wind. Dir. | For. | Weather. | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | 1,120 |
| 17 | 9 a m | 29 | 7 | 13 | 5 | 2 | 0 | N E | 2 | fair | |
| | 7 p m | 29 | 7 | 13 | 7 | 1 | 8 | N E | 1 | fair —— | 0,050 |
| 18 | 9 a m | 29 | 9 | 13 | 5 | 1 | 8 | S W | 1 | fair | |
| | 7 p m | 29 | 8 | 13 | [illegible] | [illegible] | 7 | S W | 1 | fair | |
| 19 | 9 a m | 29 | 7 | 13 | 7 | 1 | 6 | W | 2 | cloudy | |
| | 7 p m | 29 | 7 | 13 | 6 | 1 | 7 | S W | 2 | fair | |
| 20 | 9 a m | 29 | 8 | 13 | 3 | 1 | 7 | W by S | 2 | fair | |
| | 7 p m | 29 | 9 | 12 | 7 | 1 | 5 | W | 1 | fair | |
| 21 | 9 a m | 30 | 1 | 12 | 8 | 1 | 7 | W | 1 | fair | |
| | 7 p m | 30 | 1 | 13 | 6 | 1 | 7 | E | 1 | cloudy | |
| 22 | 9 a m | 29 | 9 | 13 | 7 | 1 | 9 | S E | 2 | cloudy | |
| | 7 p m | 29 | 8 | 13 | 6 | 1 | 9 | S by E | 1 | rain | |
| 23 | 9 a m | 29 | 6 | 14 | 0 | 2 | 5 | S E | 2 | rain | |
| | 7 p m | 29 | 4 | 14 | 4 | 2 | 1 | S W | 1 | rain | |
| 24 | | | | | | | | | | | |
| 25 | 9 a m | 29 | 5 | 13 | 8 | 1 | 9 | W | 2 | fair —— | 0,060 |
| | 6 p m | 29 | 5 | 14 | 4 | 1 | 5 | W by S | 2 | fair | |
| 26 | 9 a m | 29 | 3 | 14 | 8 | 1 | 7 | W by S | 3 | clear —— | 0,020 |
| | 7 p m | 29 | 4 | 13 | 1 | 1 | 8 | W | 3 | clear | |
| 27 | 9 a m | 29 | 7 | 13 | 5 | 1 | 9 | W | 3 | cloudy | |
| | 7 p m | 29 | 7 | 13 | 2 | 1 | 8 | W | 1 | cloudy | |
| 28 | 9 a m | 29 | 8 | 13 | 5 | 2 | 1 | W | 1 | fair | |
| | 7 p m | 29 | 7 | 13 | 0 | 1 | 9 | E | 1 | rain | |
| 29 | 9 a m | 29 | 4 | 12 | 7 | 2 | 7 | S E | 2 | rain | |
| | 7 p m | 29 | 4 | 13 | 8 | 1 | 5 | E by S | 1 | cloudy | |
| 30 | 9 a m | 28 | 9 | 13 | 3 | 2 | 1 | S W | 1 | fair —— | 0,035 |
| | 7 p m | 28 | 8 | 13 | 1 | 1 | 8 | S W | 1 | cloudy | |
| 31 | 9 a m | 28 | 7 | 13 | 8 | 1 | 9 | S W | 1 | fair | |
| | 7 p m | 28 | 8 | 13 | 9 | 1 | 9 | S W | 2 | cloudy | |
| H. at a med. | | 29 | 6 | 13 | 3 | 1 | 9 | | | Total depth | 1,285 |
| Gr. height | | 30 | 1 | 15 | 6 | 4 | 9 | | | | |
| L. height | | 28 | 7 | 12 | 5 | 1 | 4 | | | | |

## SEPTEMBER 1734.

| D. | Hour. | Baro. In. D. | Ther. In. D. | Hyg. I. D. | Wind. Dir. | Wind. For. | Weather. | Rain. In D. |
|---|---|---|---|---|---|---|---|---|
| 1 | 9 a m | 28 9 | 13 3 | 1 9 | W | 2 | fair | 0,025 |
| | 7 p m | 29 2 | 12 0 | 1 9 | W | [illegible] | fair | |
| 2 | 9 a m | 29 4 | 12 7 | 1 8 | W | 3 | fair | |
| | 7 p m | 28 9 | 11 8 | 1 2 | W | 3 | rain | |
| 3 | 9 a m | 28 9 | 11 8 | 1 7 | W | 3 | fair | |
| | 6 p m | 28 9 | 12 3 | 1 6 | S W | 3 | fair | |
| 4 | 9 a m | 29 3 | 12 7 | 1 5 | N W | 3 | fair | 0,078 |
| | 5 p m | 29 5 | 13 0 | 1 5 | N W | 2 | fair | |
| 5 | 9 a m | 29 6 | 12 9 | 2 3 | W | 1 | cloudy | |
| | 5 p m | 29 7 | 12 8 | 2 1 | W | 1 | fair | |
| 6 | 9 a m | 29 8 | 12 8 | 1 9 | S E | 2 | cloudy | |
| | 5 p m | 29 7 | 12 7 | 1 7 | W | 1 | rain | |
| 7 | 9 a m | 29 8 | 13 5 | 2 4 | W | 2 | fair | |
| | 5 p m | 29 9 | 12 7 | 1 6 | W | 2 | fair | |
| 8 | 9 a m | 29 9 | 12 5 | 1 9 | S W | 2 | cloudy | |
| | 5 p m | 29 8 | 13 1 | 1 8 | S W | 1 | cloudy | |
| 9 | 9 a m | 29 6 | 11 3 | 2 3 | N E | 2 | rain | 0,130 |
| | 5 p m | 29 7 | 11 3 | 1 7 | N | 2 | fair | |
| 10 | 9 a m | 29 8 | 11 6 | 1 6 | N W | 2 | fair | 0,065 |
| | 6 p m | 30 0 | 11 2 | 1 5 | N W | 2 | fair | |
| 11 | 9 a m | 30 0 | 11 6 | 1 6 | N W | 1 | cloudy | |
| | 5 p m | 30 0 | 12 5 | 1 7 | W by S | 1 | cloudy | 0,090 |
| 12 | 9 a m | 30 0 | 12 3 | 1 8 | S W | 1 | cloudy | |
| | 5 p m | 30 0 | 13 2 | 1 5 | S W | 1 | cloudy | |
| 13 | 9 a m | 30 0 | 12 3 | 1 8 | W | 1 | fair | |
| | 5 p m | 29 9 | 12 5 | 1 6 | W | 1 | cloudy | |
| 14 | 9 a m | 29 8 | 11 7 | 1 9 | W | 2 | fair | |
| | 5 p m | 29 8 | 12 4 | 1 5 | N W | 2 | fair | |
| 15 | 9 a m | 29 8 | 12 1 | 1 8 | W | 2 | cloudy | |
| | 5 p m | 29 8 | 12 8 | 1 9 | W | 2 | cloudy | |
| 16 | 9 a m | 29 8 | 12 1 | 2 0 | W | 1 | fair | 0,156 |
| | 6 p m | 29 9 | 12 1 | 1 7 | W | 1 | fair | |
| | | | | | | | | 0,544 |

## SEPTEMBER 1734.

| D. | Hour. | Baro. In | D. | Ther. In. | D. | Hyg. I. | D. | Wind. Dir. | For. | Weather. | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 17 | 9 a m | 29 | 9 | 1[illegible] | 8 | 1 | 8 | S | 2 | cloudy | 0,544 |
| | 4 p m | 29 | 9 | 14 | 4 | 1 | 7 | S W | 2 | fair | |
| 18 | 9 a m | 30 | 0 | 13 | [illegible] | 1 | 9 | S W | 3 | fair — | 0,260 |
| | 5 p m | 30 | 0 | 14 | 1 | 1 | 7 | S W | 2 | cloudy | |
| 19 | 9 a m | 30 | 0 | 12 | 5 | 1 | 9 | W | 1 | cloudy | |
| | 5 p m | 29 | 9 | 11 | 3 | 2 | 1 | W | 1 | cloudy | |
| 20 | 9 a m | 29 | 7 | 11 | 8 | 2 | 9 | S W | 1 | fair — | 0,127 |
| | 5 p m | 29 | 6 | 12 | [illegible] | [illegible] | 8 | S W | 1 | cloudy | |
| 21 | 9 a m | 29 | 4 | 11 | 9 | 2 | 0 | S W | 1 | fair | |
| | 5 p m | 29 | 2 | 11 | 9 | 1 | 8 | S W | 1 | fair | |
| 22 | 9 a m | 28 | 7 | 10 | 7 | 1 | 9 | W | 3 | rain | |
| | 5 p m | 28 | 8 | 11 | 9 | 1 | 9 | W | 2 | cloudy | |
| 23 | 9 a m | 29 | 4 | 11 | 2 | 1 | 7 | N W | 2 | fair | |
| | 4 p m | 29 | 5 | 11 | 8 | 1 | 7 | W | 1 | rain | |
| 24 | 9 a m | 29 | 3 | 11 | 5 | 2 | 2 | N W | 1 | fair | |
| | 4 p m | 29 | 3 | 12 | 2 | 1 | 9 | N | 1 | cloudy | |
| 25 | 9 a m | 29 | 7 | 10 | 7 | 2 | 0 | N | 2 | fair — | 0,090 |
| | 5 p m | 29 | 9 | 11 | 1 | 1 | 6 | N | 2 | fair | |
| 26 | 9 a m | 30 | 0 | 10 | 1 | 1 | 7 | W by N | 2 | cloudy — | 0,035 |
| | 4 p m | 29 | 9 | 11 | 0 | 1 | 8 | W | 2 | cloudy | |
| 27 | 9 a m | 29 | 8 | 12 | 0 | 2 | 6 | N E | 1 | cloudy | |
| | 5 p m | 29 | 8 | 11 | 8 | 1 | 9 | E by N | 1 | cloudy | |
| 28 | 9 a m | 29 | 7 | 11 | 6 | 1 | 9 | E | 1 | fair | |
| | 4 p m | 9 | 7 | 12 | 2 | 1 | 7 | E | 1 | fair | |
| 29 | 9 a m | 29 | 7 | 11 | 6 | 1 | 8 | W | 2 | cloudy — | 0,116 |
| | 5 p m | 29 | 7 | 10 | 0 | 1 | 8 | W | 2 | cloudy | |
| 30 | 9 a m | 29 | 3 | 9 | 9 | 2 | 2 | S W | 3 | rain | |
| | 4 p m | 29 | 3 | 10 | 6 | 2 | 0 | S W | 1 | cloudy | |
| H. at a med. | | 29 | 6 | 12 | 0 | 1 | 8 | | | Total depth | 1,172 |
| Gr. height | | 30 | 0 | 14 | 4 | 2 | 9 | | | | |
| L. height | | 28 | 7 | 9 | 9 | 1 | 2 | | | | |

## OCTOBER 1734.

| D. | Hour. | Baro. In. | Baro. D. | Ther. In. | Ther. D. | Hyg. I. | Hyg. D. | Wind. Dir. | Wind. For. | Weather. | Rain. In D. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 9 a m | 28 | 8 | 12 | 4 | 2 | 2 | S W | 4 | rain | |
| | 5 p m | 28 | 8 | 11 | 0 | 2 | 0 | S W | 2 | fair | |
| 2 | 9 a m | 28 | 8 | 11 | 4 | 1 | 9 | S W | 3 | fair | |
| | 4 p m | 29 | 1 | 11 | 6 | 1 | 7 | S W | 2 | fair | |
| 3 | 9 a m | 29 | 1 | 10 | 6 | 1 | 9 | S W | 1 | fog | 0,096 |
| | 5 p m | 29 | 0 | 10 | 4 | 2 | 2 | N W | 0 | cloudy | |
| 4 | 9 a m | 29 | 3 | 10 | 6 | 2 | 2 | S | 0 | fair | |
| | 5 p m | 29 | 4 | 11 | 2 | 2 | 1 | S | 1 | fair | |
| 5 | 9 a m | 29 | 5 | 11 | 2 | 2 | 3 | N | 1 | cloudy | |
| | 4 p m | 29 | 5 | 11 | 5 | 2 | 1 | N | 0 | cloudy | |
| 6 | 9 a m | 29 | 5 | 11 | 6 | 3 | 0 | E | 1 | fog | |
| | 4 p m | 29 | 3 | 11 | 6 | 2 | 4 | E | 1 | cloudy | 0,107 |
| 7 | 9 a m | 29 | 1 | 10 | 4 | 3 | 1 | N E | 1 | rain | |
| | 4 p m | 29 | 2 | 10 | 7 | 3 | 2 | N | 2 | lowring | 0,037 |
| 8 | 9 a m | 29 | 5 | 10 | 6 | 2 | 6 | W | 2 | cloudy | |
| | 5 p m | 29 | 5 | 10 | 5 | 1 | 8 | W | 2 | cloudy | |
| 9 | 9 a m | 29 | 4 | 10 | 5 | 2 | 2 | W | 3 | cloudy | |
| | 4 p m | 29 | 4 | 10 | 0 | 2 | 2 | W | 2 | cloudy | |
| 10 | 9 a m | 29 | 5 | 10 | 2 | 2 | 4 | W | 3 | fair | |
| | 5 p m | 29 | 6 | 10 | 4 | 2 | 0 | S W | 2 | cloudy | 0,130 |
| 11 | 9 a m | 29 | 5 | 10 | 3 | 2 | 3 | S | 1 | cloudy | |
| | 4 p m | 29 | 3 | 10 | 6 | 2 | 3 | S | 2 | rain | |
| 12 | 9 a m | 29 | 1 | 9 | 9 | 2 | 3 | S | 1 | fair | |
| | 4 p m | 29 | 1 | 10 | 8 | 2 | 0 | S | 1 | fair | |
| 13 | 9 a m | 29 | 1 | 10 | 6 | 2 | 2 | S E | 1 | fair | |
| | 4 p m | 29 | 2 | 11 | 0 | 1 | 9 | N E | 2 | cloudy | |
| 14 | 9 a m | 29 | 5 | 10 | 3 | 3 | 0 | N | 0 | cloudy | 0,090 |
| | 5 p m | 29 | 6 | 10 | 4 | 2 | 9 | N | 0 | fair | |
| 15 | 9 a m | 29 | 6 | 10 | 7 | 2 | 9 | N E | 0 | fair | 0,030 |
| | 4 p m | 29 | 6 | 10 | 4 | 2 | 7 | N E | 1 | cloudy | |
| 16 | 9 a m | 29 | 6 | 9 | 4 | 2 | 5 | N W | 2 | fair | 0,290 |
| | 5 p m | 29 | 7 | 10 | 2 | 2 | 2 | N W | 2 | fair | |
| | | | | | | | | | | | 0,780 |

## OCTOBER 1734.

| D | Hour. | Baro. In. | D. | Ther. In. | D. | Hyg. I. | D. | Wind. Dir. | For. | Weather. | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | 0,780 |
| 17 | 9 a m | 29 | 9 | 9 | 7 | 2 | 3 | N W | 2 | fair | |
| | 4 p m | 29 | 9 | 10 | 2 | 2 | 0 | N by W | 1 | fair | |
| 18 | 9 a m | 30 | 0 | 9 | 2 | [illegible] | 1 | N W | 1 | fair | |
| | 4 p m | 29 | 9 | 10 | 6 | 2 | 0 | N W | 1 | cloudy | |
| 19 | 9 a m | 30 | 0 | 9 | 5 | 2 | 0 | N | 4 | fair | |
| | 5 p m | 30 | 1 | 9 | 3 | 2 | 0 | N E | 2 | fair | |
| 20 | 9 a m | 30 | 0 | 9 | 5 | 2 | 0 | W | 2 | cloudy | |
| | 5 p m | 30 | 0 | 9 | 2 | [illegible] | 9 | W by N | 0 | cloudy — | 0,075 |
| 21 | 9 a m | 30 | 0 | 10 | 2 | 2 | [illegible] | N W | 1 | hazy | |
| | 4 p m | 30 | 0 | 10 | 3 | 2 | 8 | N W | 1 | cloudy | |
| 22 | 9 a m | 30 | 2 | 9 | 5 | 2 | 6 | W | 1 | fair | |
| | 5 p m | 30 | 2 | 10 | 0 | 2 | 4 | W | 1 | fair | |
| 23 | 9 a m | 30 | 1 | 10 | 0 | 2 | 6 | W | 2 | fair | |
| | 4 p m | 30 | 0 | 10 | 8 | 2 | 6 | W | 2 | cloudy | |
| 24 | 9 a m | 29 | 9 | 11 | 8 | 3 | 0 | W | 2 | rain | |
| | 4 p m | 29 | 8 | 12 | 2 | 3 | 0 | W by S | 1 | cloudy | |
| 25 | 9 a m | 30 | 0 | 10 | 9 | 2 | 5 | S W | 3 | cloudy | |
| | 4 p m | 29 | 9 | 11 | 5 | 2 | 8 | S W | 3 | cloudy | |
| 26 | 9 a m | 29 | 8 | 11 | 3 | 2 | 0 | N W | 4 | fair —— | 0,179 |
| | 4 p m | 29 | 7 | 11 | 0 | 1 | 9 | N W | 4 | cloudy | |
| 27 | 9 a m | 29 | 7 | 9 | 5 | 1 | 8 | N W | 3 | fair —— | 0,094 |
| | 4 p m | 29 | 8 | 9 | 3 | 1 | 6 | N W | 3 | cloudy | |
| 28 | 9 a m | 29 | 8 | 9 | 0 | 1 | 7 | N W | 2 | fair — | 0,055 |
| | 4 p m | 29 | 8 | 9 | 8 | 1 | 6 | N W | 1 | cloudy | |
| 29 | 9 a m | 29 | 5 | 9 | 3 | 2 | 2 | S W | 0 | fog | |
| | 4 p m | 29 | 4 | 9 | 4 | 2 | 3 | S W | 1 | fair | |
| 30 | 9 a m | 29 | 5 | 9 | 3 | 2 | 3 | S W | 2 | fair — | 0,085 |
| | 4 p m | 29 | 6 | 9 | 8 | 2 | 0 | S W | 2 | fair | |
| 31 | 9 a m | 29 | 4 | 10 | 9 | 2 | 1 | S W | 1 | cloudy — | 0,053 |
| | 4 p m | 29 | 3 | 11 | 5 | 2 | 0 | S E | 2 | cloudy | |
| H. at a med. | | 29 | 5 | 10 | 3 | 2 | 2 | | | Total depth | 1,321 |
| Gr. height | | 30 | 2 | 12 | 4 | 3 | 2 | | | | |
| L. height | | 28 | 8 | 9 | 0 | 1 | 6 | | | | |

## NOVEMBER 1734.

| D. | Hour. | Baro. In. | Baro. D. | Ther. In. | Ther. D. | Hyg. I. | Hyg. D. | Wind. Dir. | Wind. For. | Weather. | Rain. In D. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 9 a m | 29 | 7 | 9 | 3 | 2 | 0 | S W | 1 | fair | |
| | 4 p m | 29 | 9 | 10 | 4 | 2 | 0 | S W | 0 | fair | |
| 2 | 9 a m | 30 | 0 | 10 | 3 | 2 | 1 | S | 1 | fair — | 0,130 |
| | 4 p m | 30 | 0 | 10 | 7 | 2 | 0 | S | 1 | fair | |
| 3 | 9 a m | 29 | 8 | 11 | 6 | 2 | 1 | S | 1 | cloudy — | 0,070 |
| | 4 p m | 29 | 9 | 10 | 8 | 2 | 1 | S | 1 | fair | |
| 4 | 9 a m | 29 | 8 | 10 | 4 | 2 | 1 | S | 1 | cloudy | |
| | 4 p m | 29 | 7 | 10 | 6 | 2 | 0 | S | 2 | fair | |
| 5 | 9 a m | 29 | 8 | 10 | 4 | 2 | 1 | S W | 2 | fair | |
| | 4 p m | 29 | 8 | 10 | 5 | 2 | 1 | S W | 2 | fair | |
| 6 | 9 a m | 29 | 9 | 10 | 2 | 2 | 3 | S W | 1 | cloudy | |
| | 4 p m | 29 | 9 | 10 | 6 | 2 | 1 | S by W | 1 | cloudy | |
| 7 | 9 a m | 29 | 8 | 10 | 6 | 2 | 0 | S W | 1 | cloudy | |
| | 4 p m | 29 | 9 | 10 | 1 | 2 | 0 | S W | 1 | fair | |
| 8 | 9 a m | 30 | 2 | 9 | 0 | 2 | 3 | S W | 1 | froſt | |
| | 4 p m | 30 | 2 | 9 | 0 | 2 | 2 | S W | 1 | fair | |
| 9 | 9 a m | 30 | 2 | 9 | 1 | 2 | 5 | S W | 1 | miſt — | 0,180 |
| | 4 p m | 30 | 2 | 9 | 3 | 2 | 7 | S W | 1 | fair | |
| 10 | 9 a m | 30 | 2 | 8 | 5 | 2 | 3 | S W | 1 | froſt fog | |
| | 4 p m | 30 | 2 | 8 | 6 | 2 | 2 | S W | 1 | froſt fair | 0,075 |
| 11 | 9 a m | 30 | 2 | 8 | 2 | 2 | 1 | S W | 1 | fair | |
| | 4 p m | 30 | 2 | 8 | 2 | 2 | 1 | S W | 1 | fair | |
| 12 | 9 a m | 30 | 2 | 7 | 9 | 2 | 2 | S W | 1 | froſt fog | 0,094 |
| | 4 p m | 30 | 3 | 8 | 3 | 2 | 1 | S W | 1 | froſt fair | |
| 13 | 9 a m | 30 | 4 | 8 | 2 | 2 | 4 | S W | 1 | fog | |
| | 4 p m | 30 | 4 | 8 | 9 | 2 | 4 | S W | 1 | cloudy | |
| 14 | 9 a m | 30 | 4 | 9 | 0 | 2 | 5 | S by E | 1 | fog | |
| | 4 p m | 30 | 4 | 9 | 4 | 2 | 2 | S by E | 1 | fog | |
| 15 | 9 a m | 30 | 4 | 9 | 5 | 2 | 1 | S E | 1 | fog | |
| | 4 p m | 30 | 4 | 9 | 8 | 2 | 0 | S E | 1 | fog | |
| 16 | 9 a m | 30 | 4 | 9 | 4 | 2 | 0 | S E | 1 | cloudy | |
| | 4 p m | 30 | 3 | 9 | 2 | 2 | 0 | S W | 1 | cloudy | |
| | | | | | | | | | | | 0,549 |

## NOVEMBER 1734.

| D. | Hour. | Baro. In | Baro. D. | Ther. In | Ther. D. | Hyg I. | Hyg D. | Wind. Dir. | Wind. For. | Weather, | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | 0,549 |
| 17 | 9 a m | 30 | 2 | 9 | 2 | 2 | 0 | W | 1 | cloudy | 0,144 |
| | 4 p m | 30 | 2 | 9 | 1 | 2 | 0 | W | 2 | cloudy | |
| 18 | 9 a m | 30 | 1 | 8 | 1 | 2 | 3 | W | 1 | fair — | 0,090 |
| | 4 p m | 30 | 0 | 8 | 9 | 2 | 2 | W | 1 | fair | |
| 19 | 8 a m | 29 | 9 | 9 | 2 | 2 | 5 | S | 1 | fog | |
| | 4 p m | 29 | 9 | 9 | 4 | 2 | 3 | S by E | 1 | fog | |
| 20 | 9 a m | 29 | 8 | 8 | 8 | 2 | 2 | S W | 1 | fog | |
| | 4 p m | 29 | 7 | 9 | [illegible] | [illegible] | 3 | S W | 2 | fog | |
| 21 | 9 a m | 29 | 7 | 8 | 7 | 2 | 3 | W | 1 | fair | |
| | 4 p m | 29 | 7 | 9 | 5 | 2 | 3 | W | 1 | cloudy | |
| 22 | 9 a m | 29 | 5 | 8 | 6 | 2 | 3 | S W | 2 | fair — | 0,285 |
| | 4 p m | 29 | 5 | 8 | 5 | 2 | 2 | W | 2 | fair | |
| 23 | 9 a m | 29 | 4 | 7 | 8 | 2 | 2 | W | 0 | fog | |
| | 4 p m | 29 | 3 | 8 | 5 | 2 | 3 | S E | 1 | fog | |
| 24 | 9 a m | 29 | 4 | 9 | 0 | 1 | 9 | N E | 2 | cloudy — | 0,130 |
| | 4 p m | 29 | 5 | 9 | 0 | 1 | 6 | N E | 2 | cloudy | |
| 25 | 9 a m | 29 | 7 | 8 | 1 | 2 | 6 | S E | 1 | ſnow | |
| | 4 p m | 29 | 8 | 8 | 6 | 2 | 2 | S E | 1 | cloudy | |
| 26 | 9 a m | 29 | 9 | 8 | 3 | 2 | 4 | N | 3 | fair — | 0,150 |
| | 4 p m | 30 | 0 | 7 | 9 | 2 | 0 | N | 2 | fair | |
| 27 | 9 a m | 30 | 2 | 7 | 7 | 2 | 0 | W | 1 | froſt | |
| | 4 p m | 30 | 2 | 7 | 9 | 2 | 0 | W | 1 | froſt | |
| 28 | 9 a m | 29 | 6 | 10 | 3 | 2 | 3 | S W | 3 | thaw — | 0,090 |
| | 4 p m | 29 | 7 | 11 | 2 | 1 | 6 | S W | 2 | fair | |
| 29 | 9 a m | 29 | 6 | 11 | 6 | 2 | 6 | S W by W | 2 | fair — | 0,070 |
| | 4 p m | 29 | 6 | 11 | 7 | 2 | 7 | S W by W | 2 | cloudy | |
| 30 | 9 a m | 29 | 6 | 9 | 7 | 2 | 6 | W | 2 | fair | |
| | 4 p m | 29 | 5 | 10 | 5 | 2 | 3 | W | 2 | cl udy | |
| H. at a medi. | | 29 | 9 | 9 | 3 | 2 | 1 | | | Total depth | 1,608 |
| Gr. height | | 30 | 4 | 11 | 7 | 2 | 7 | | | | |
| L. height | | 29 | 3 | 7 | 7 | 1 | 6 | | | | |

## DECEMBER 1734.

| D | Hour. | Baro. In. | Baro. D. | Ther. In. | Ther. D. | Hyg. I. | Hyg. D. | Wind. Dir. | Wind. For. | Weather. | Rain. In D. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 9 a m | 29 | 1 | 11 | 5 | 2 | 3 | S W | 3 | cloudy — | 0,094 |
| | 4 p m | 28 | 9 | 11 | 5 | 2 | 3 | S W | 2 | cloudy | |
| 2 | 9 a m | 29 | 0 | 8 | 7 | 2 | 3 | S W | 2 | fair — | 0,067 |
| | 4 p m | 29 | 1 | 8 | 8 | 2 | 2 | S W | 2 | fair | |
| 3 | 9 a m | 29 | 1 | 8 | 3 | 2 | 3 | S by E | 2 | fair | |
| | 4 p m | 29 | 1 | 8 | 8 | 2 | 3 | S by E | 1 | fair | |
| 4 | 9 a m | 29 | 2 | 8 | 0 | 2 | 4 | S | 1 | fog — | 0,190 |
| | 4 p m | 29 | 2 | 8 | 4 | 2 | 3 | S | 1 | cloudy — | |
| 5 | 9 a m | 28 | 9 | 9 | 1 | 2 | 5 | E | 2 | fog — | 0,050 |
| | 4 p m | 28 | 9 | 9 | 5 | 2 | 8 | E | 2 | rain | |
| 6 | 9 a m | 29 | 1 | 9 | 5 | 3 | 0 | W | 2 | cloudy | |
| | 4 p m | 29 | 2 | 9 | 7 | 3 | 0 | W | 2 | hazy | |
| 7 | 9 a m | 29 | 3 | 9 | 0 | 2 | 8 | W by N | 2 | fair — | 0,090 |
| | 4 p m | 29 | 5 | 8 | 8 | 2 | 5 | N W | 2 | fair | |
| 8 | 9 a m | 29 | 4 | 9 | 0 | 2 | 6 | S W | 1 | cloudy | |
| | 4 p m | 29 | 3 | 10 | 1 | 2 | 4 | S W | 2 | cloudy | |
| 9 | 9 a m | 29 | 0 | 10 | 0 | 2 | 5 | S W | 2 | fair | |
| | 4 p m | 29 | 2 | 9 | 6 | 2 | 4 | S W | 2 | fair | |
| 10 | 9 a m | 29 | 3 | 8 | 9 | 2 | 3 | S | 3 | fair | |
| | 4 p m | 29 | 3 | 9 | 5 | 2 | 3 | S | 2 | fair | |
| 11 | 9 a m | 28 | 8 | 8 | 5 | 2 | 8 | N W | 3 | cloudy — | 0,156 |
| | 4 p m | 29 | 0 | 8 | 8 | 2 | 3 | N W | 3 | fair | |
| 12 | 9 a m | 29 | 2 | 9 | 3 | 2 | 4 | S | 1 | fair — | 0,060 |
| | 4 p m | 29 | 1 | 9 | 2 | 2 | 3 | S | 1 | fair | |
| 13 | 9 a m | 28 | 6 | 9 | 4 | 3 | 0 | S E | 1 | fog | |
| | 3 p m | 28 | 6 | 9 | 4 | 2 | 8 | S E | 2 | cloudy — | 0,075 |
| 14 | 9 a m | 28 | 0 | 9 | 1 | 2 | 6 | S | 2 | hazy | |
| | 4 p m | 28 | 0 | 9 | 1 | 2 | 3 | S | 2 | cloudy | |
| 15 | 9 a m | 28 | 2 | 9 | 3 | 2 | 2 | N E | 2 | fair | |
| | 4 p m | 28 | 6 | 9 | 1 | 2 | 5 | N E | 2 | rain | |
| 16 | 9 a m | 29 | 0 | 8 | 4 | 2 | 3 | W by N | 2 | fair — | 0,460 |
| | 4 p m | 29 | 1 | 8 | 8 | 2 | 5 | W by N | 2 | fair | |
| | | | | | | | | | | | 1,242 |

## *DECEMBER* 1734.

| D. | Hour. | Baro. In | Baro. D. | Ther. In | Ther. D. | Hyg. I. | Hyg. D. | Wind. Dir. | Wind. For. | Weather. | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | 1,242 |
| 17 | 9 a m | 28 | 9 | 8 | 6 | 2 | 5 | S E | 1 | fog — | 0,265 |
| | 4 p.m | 28 | 9 | 9 | 0 | 2 | 5 | S E | 1 | hazy | |
| 18 | 9 a m | 28 | 9 | 9 | 0 | 2 | 5 | S W | 2 | cloudy — | 0,240 |
| | 4 p m | 28 | 9 | 8 | 9 | 2 | 3 | S W | 2 | fair | |
| 19 | 9 a m | 29 | 0 | 9 | 5 | 2 | 3 | S W | 3 | cloudy | |
| | 4 p m | 29 | 1 | 9 | 8 | 2 | 3 | S W | 3 | fair | |
| 20 | 9 a m | 29 | 5 | 9 | 3 | 2 | 5 | S W | 2 | fair | |
| | 4 p m | 29 | 6 | 9 | 2 | [illegible] | 5 | S W | 1 | fair | |
| 21 | 9 a m | 29 | 6 | 8 | 6 | 2 | 3 | S W | 2 | fog | |
| | 4 p m | 29 | 6 | 8 | 8 | 2 | 6 | S W | 2 | cloudy | |
| 22 | 9 a m | 29 | 8 | 7 | 9 | 2 | 5 | W | 1 | fair | |
| | 4 p m | 29 | 8 | 8 | 5 | 2 | 6 | W | 1 | fog | |
| 23 | 9 a m | 29 | 8 | 7 | 4 | 2 | 8 | S by E | 2 | fair | |
| | 4 p m | 29 | 8 | 8 | 1 | 2 | 7 | S by E | 1 | fog | |
| 24 | 9 a m | 29 | 6 | 10 | 6 | 2 | 7 | S W | 2 | cloudy — | |
| | 4 p m | 29 | 5 | 10 | 3 | 2 | 7 | S W | 1 | cloudy | 0,385 |
| 25 | 9 a m | 29 | 7 | 8 | 3 | 2 | 5 | W | 2 | fair — | |
| | 4 p m | 29 | 7 | 8 | 8 | 2 | 5 | W | 2 | fair | 0,090 |
| 26 | 9 a m | 29 | 5 | 9 | 9 | 2 | 3 | S by W | 4 | cloudy | |
| | 4 p m | 29 | 1 | 10 | 7 | 2 | 3 | S by W | 4 | cloudy | |
| 27 | 9 a m | 29 | 3 | 9 | 2 | 2 | 2 | S by W | 3 | fair | |
| | 4 p m | 29 | 1 | 10 | 0 | 2 | 2 | S by W | 3 | cloudy | |
| 28 | 9 a m | 28 | 6 | 9 | 9 | 2 | 6 | S by W | 2 | fair | |
| | 4 p m | 28 | 9 | 10 | 2 | 3 | 0 | W by N | 3 | cloudy | |
| 29 | 9 a m | 29 | 0 | 8 | 8 | 2 | 2 | S by E | 1 | fog — | 0,170 |
| | 4 p m | 29 | 0 | 9 | 0 | 2 | 2 | S by E | 1 | cloudy | |
| 30 | 9 a m | 29 | 5 | 8 | 3 | 2 | 3 | S by W | 1 | fair | |
| | 4 p m | 29 | 8 | 9 | 0 | 1 | 9 | S by W | 2 | fair | |
| 31 | 9 a m | 29 | 6 | 9 | 6 | 2 | 1 | S W | 4 | cloudy | |
| | 4 p m | 29 | 8 | 9 | 0 | 2 | 3 | S W | 2 | cloudy | |
| H. at a med. | | 29 | 0 | 9 | 1 | 2 | 4 | | | Total depth | 2,332 |
| Gr. height | | 29 | 8 | 11 | 5 | 3 | 0 | | | | |
| L. height | | 28 | 0 | 7 | 4 | 1 | 9 | | | | |

## *JANUARY* 1735.

| D. | Hour. | Baro. In. | D. | Ther. In. | D. | Hyg. I. | D. | Wind. Dir. | For. | Weather. | Rain. In D. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 9 a m | 29 | 9 | 9 | 2 | 2 | 3 | W by S | 2 | fair | |
| | 4 p m | 30 | 0 | 9 | 3 | 2 | 3 | W | 2 | fair | |
| 2 | 9 a m | 29 | 8 | 10 | 7 | 2 | 3 | S W | 4 | cloudy — | 0,147 |
| | 4 p m | 29 | 6 | 11 | 5 | 2 | 3 | S W | 4 | rain | |
| 3 | 9 a m | 29 | [illegible] | 9 | 0 | 2 | 3 | S W | 3 | fair | |
| | 4 p m | 29 | 9 | 9 | 7 | 2 | 3 | W | 3 | cloudy | 0,096 |
| 4 | 9 a m | 30 | 2 | 9 | 0 | 2 | 3 | W | [illegible] | fair — | |
| | 4 p m | 30 | 2 | 10 | 5 | 2 | 2 | S W | 3 | cloudy | |
| 5 | 9 a m | 30 | 0 | 11 | 1 | 2 | 6 | S by W | 3 | cloudy | |
| | 4 p m | 29 | 8 | 11 | 3 | 2 | 6 | W | 4 | cloudy | |
| 6 | 9 a m | 29 | 6 | 11 | 4 | 2 | 6 | W | 3 | cloudy | |
| | 4 p m | 29 | 3 | 11 | 6 | 2 | 4 | S W | 4 | rain | |
| 7 | 9 a m | 28 | 7 | 8 | 7 | 2 | [illegible] | S W | 4 | fair | |
| | 4 p m | 28 | 6 | 9 | 1 | 2 | 2 | W | 3 | cloudy | |
| 8 | 9 a m | 28 | 4 | 8 | 3 | 2 | 3 | S | 1 | fog | |
| | 4 p m | 28 | 2 | 8 | 5 | 2 | 3 | N | 1 | fair | |
| 9 | 9 a m | 28 | 4 | 8 | 3 | 2 | 7 | W | 1 | fair — | 0,195 |
| | 4 p m | 28 | 6 | 8 | 4 | 2 | 6 | W | 2 | fair | |
| 10 | 9 a m | 28 | 9 | 7 | 8 | 2 | 5 | W | 2 | fair | |
| | 4 p m | 28 | 9 | 7 | 6 | 2 | 4 | W | 1 | fog | |
| 11 | 9 a m | 28 | 9 | 8 | 8 | 2 | 5 | E | 2 | cloudy | |
| | 4 p m | 29 | 0 | 8 | 7 | 2 | 7 | N | 2 | cloudy | |
| 12 | 9 a m | 29 | 5 | 7 | 7 | [illegible] | 2 | N W | 2 | fair — | 0,540 |
| | 4 p m | 29 | 5 | 8 | 0 | 2 | 0 | N W | 2 | fair | |
| 13 | 9 a m | 29 | 3 | 7 | 9 | 2 | 0 | S | 2 | ſnow — | 0,430 |
| | 4 p m | 29 | 0 | 8 | 2 | 2 | 1 | S E | 3 | cloudy | |
| 14 | 9 a m | 28 | 6 | 8 | 8 | 2 | 2 | W by N | 4 | cloudy | |
| | 4 p m | 28 | 7 | 8 | 5 | 2 | 0 | W by N | 3 | cloudy | |
| 15 | 9 a m | 29 | 1 | 9 | 1 | 2 | 2 | W | 3 | fair | |
| | 4 p m | 29 | 2 | 9 | 2 | 2 | 3 | W | 2 | fair | |
| 16 | 9 a m | 29 | 3 | 8 | 4 | 2 | 6 | S W | 2 | fair | |
| | 4 p m | 29 | 3 | 8 | 6 | 2 | 5 | W | 2 | fair | |
| | | | | | | | | | | | 1,408 |

## JANUARY 1735.

| D | Hour. | Baro. In. | D. | Ther. In. | D. | Hyg. I. | D. | Wind. Dir. | For. | Weather. | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | 1,408 |
| 17 | 9 a m | 29 | 5 | 8 | 5 | 2 | 5 | W | 2 | fair | 0,290 |
| | 4 p m | 29 | 5 | 9 | 3 | 2 | 3 | W | 2 | cloudy | |
| 18 | 9 a m | 29 | 2 | 9 | 8 | 2 | 5 | S W | 2 | cloudy | 0,057 |
| | 4 p m | 29 | 1 | 10 | 6 | 2 | 6 | S W | 2 | cloudy | |
| 19 | 9 a m | 29 | 2 | 9 | 5 | 2 | 5 | W | 2 | cloudy | |
| | 4 p m | 29 | 3 | 9 | 5 | 2 | 5 | N | 3 | cloudy | |
| 20 | 9 a m | 29 | 7 | 8 | 7 | 2 | 3 | N | 2 | cloudy | |
| | 4 p m | 29 | 8 | 8 | 2 | 2 | 2 | N | 2 | fair | |
| 21 | 9 a m | 29 | 9 | 8 | 6 | [illegible] | 0 | W by N | 2 | fair | 0,250 |
| | 4 p m | 29 | 8 | 8 | 7 | 2 | 0 | S W | 2 | cloudy | |
| 22 | 9 a m | 29 | 8 | 8 | 1 | 2 | 3 | W | 2 | fair | |
| | 4 p m | 29 | 9 | 8 | 6 | 2 | 2 | N | 2 | fair | |
| 23 | 9 a m | 29 | 8 | 8 | 4 | 2 | 1 | E | 2 | fog | |
| | 4 p m | 29 | 8 | 8 | 3 | 1 | 9 | E | 2 | cloudy | |
| 24 | 9 a m | 29 | 8 | 8 | 0 | 1 | 9 | N W | [illegible] | fair | 0,410 |
| | 4 p m | 29 | 8 | 8 | 8 | 2 | 0 | N W | 2 | fair | |
| 25 | 9 a m | 29 | 8 | 8 | 8 | 2 | 3 | W | 2 | cloudy | 0,160 |
| | 4 p m | 30 | 1 | 8 | 6 | 1 | 9 | W | 2 | cloudy | |
| 26 | 9 a m | 30 | 0 | 9 | 7 | 2 | 6 | W | 2 | fair | |
| | 4 p m | 29 | 9 | 10 | 6 | 2 | 7 | W | 2 | cloudy | |
| 27 | 9 a m | 29 | 9 | 9 | 7 | 2 | 6 | W | 2 | fair | 0,130 |
| | 5 p m | 29 | 9 | 10 | 0 | 2 | 2 | W | 2 | fair | |
| 28 | 9 a m | 29 | 9 | 10 | 8 | 2 | 3 | W | 3 | cloudy | |
| | 4 p m | 30 | 2 | 9 | 6 | 2 | 5 | E | 2 | rain | |
| 29 | 9 a m | 30 | 3 | 9 | 6 | 2 | 4 | W | 2 | cloudy | |
| | 4 p m | 30 | 2 | 10 | 1 | 2 | 4 | W | 2 | fair | |
| 30 | 9 a m | 30 | 2 | 9 | 9 | 2 | 8 | W | 2 | fair | 0,290 |
| | 5 p m | 30 | 1 | 10 | 4 | 2 | 5 | W | 2 | cloudy | |
| 31 | 9 a m | 30 | 2 | 9 | 7 | 3 | 0 | W | 2 | fair | |
| | 4 p m | 30 | 3 | 11 | 6 | [illegible] | 3 | W | 2 | fair | |
| H. at a med. | | 29 | 5 | 9 | 2 | 2 | 7 | | | Total depth | 2,995 |
| Gr. height | | 30 | 3 | 11 | 6 | 3 | 0 | | | | |
| L. height | | 28 | 2 | 7 | 6 | 1 | 9 | | | | |

## FEBRUARY 1735.

| D. | Hour. | Baro. In. | Baro. D | Ther. In. | Ther. D. | Hyg. I | Hyg. D | Wind. Dir. | Wind. For. | Weather. | Rain. In D. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 9 a m | 30 | 4 | 9 | 3 | 2 | 8 | W | 1 | fair | 0,094 |
| | 5 p m | 30 | 4 | 10 | 4 | 2 | 4 | W | 1 | fair | |
| 2 | 9 a m | 30 | 4 | 8 | 9 | 2 | 6 | W | 1 | fair | |
| | 4 p m | 30 | 5 | 9 | 7 | 2 | 5 | W | 1 | cloudy | |
| 3 | 9 a m | 30 | 4 | 8 | 9 | 2 | 3 | W | 2 | cloudy | 0,165 |
| | 5 p m | 30 | 3 | 8 | 8 | 2 | 2 | W | 2 | cloudy | |
| 4 | 9 a m | 30 | 2 | 8 | 9 | 2 | 5 | W | 2 | cloudy | |
| | 5 p m | 30 | 2 | 8 | 7 | 2 | 4 | W | 2 | cloudy | |
| 5 | 9 a m | 30 | 1 | 9 | 7 | 2 | 2 | W | 3 | fair | 0,290 |
| | 5 p m | 30 | 0 | 10 | 4 | 2 | 1 | S W | 2 | cloudy | |
| 6 | 9 a m | 30 | 0 | 10 | 8 | 2 | 8 | S W | 2 | cloudy | |
| | 4 p m | 29 | 9 | 10 | 6 | 2 | 2 | S W | 2 | fair | |
| 7 | 9 a m | 29 | 6 | 10 | 3 | 2 | 1 | S W | 4 | fair | 0,460 |
| | 5 p m | 29 | 5 | 11 | 5 | 2 | 2 | S W | 4 | cloudy | |
| 8 | 9 a m | 29 | 5 | 12 | 4 | 2 | 3 | S W | 2 | fair | 0,195 |
| | 5 p m | 29 | 5 | 12 | 1 | 2 | 2 | S W | 2 | cloudy | |
| 9 | 9 a m | 29 | 6 | 10 | 5 | 2 | 4 | S W | 2 | fair | |
| | 5 p m | 29 | 7 | 10 | 8 | 2 | 0 | S W | 2 | cloudy | |
| 10 | 9 a m | 29 | 6 | 9 | 4 | 2 | 0 | S W | 3 | fair | |
| | 5 p m | 29 | 5 | 10 | 2 | 2 | 1 | S W | 4 | rain | |
| 11 | 9 a m | 29 | 9 | 9 | 3 | 2 | 0 | W | 2 | fair | |
| | 4 p m | 30 | 0 | 10 | 5 | 1 | 8 | W by N | 1 | fog | |
| 12 | 9 a m | 30 | 1 | 10 | 7 | 2 | 7 | W | 1 | cloudy | 0,316 |
| | 4 p m | 30 | 0 | 11 | 3 | 2 | 2 | W | 1 | drizling | |
| 13 | 9 a m | 29 | 9 | 10 | 9 | 2 | 3 | S W | 1 | cloudy | 0,070 |
| | 5 p m | 29 | 8 | 10 | 8 | 2 | 1 | W | 1 | cloudy | |
| 14 | 9 a m | 29 | 7 | 10 | 6 | 2 | 2 | S W | 2 | fair | |
| | 5 p m | 29 | 7 | 10 | 9 | 2 | 1 | S W | 2 | fair | |
| 15 | 9 a m | 29 | 8 | 8 | 9 | 2 | 1 | W by N | 2 | fair | |
| | 5 p m | 29 | 9 | 9 | 2 | 1 | 8 | W by N | 2 | fair | |
| 16 | 9 a m | 30 | 0 | 9 | 6 | 2 | 3 | S W | 3 | cloudy | 0,560 |
| | 4 p m | 29 | 8 | 10 | 8 | 2 | 2 | S W | 3 | cloudy | |
| | | | | | | | | | | | 2,150 |

## *FEBRUARY* 1735.

| D. | Hours | Baro. In. | D. | Ther. In. | D. | Hyg. I | D. | Wind. Dir. | For. | Weather. | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | 2,150 |
| 17 | 9 a m | 29 | 5 | 9 | 8 | 2 | 2 | S W | 2 | fair | |
| | 5 p m | 29 | 4 | 9 | 5 | 2 | 3 | W | 2 | fair | |
| 18 | 9 a m | 29 | 3 | 8 | 7 | 2 | 3 | N | 2 | ſnow | |
| | 4 p m | 29 | 3 | 9 | 4 | 2 | 0 | N E | 2 | fair | |
| 19 | 9 a m | 29 | 4 | 8 | 5 | 2 | 2 | N E | 3 | cloudy — | 0,496 |
| | 4 p m | 29 | 5 | 8 | 6 | 1 | 8 | N E | 3 | fair | |
| 20 | 9 a m | 29 | 8 | 7 | 4 | 1 | 9 | W | 2 | fair — | 0,170 |
| | 5 p m | 29 | 8 | 9 | 1 | 1 | 8 | W | 2 | fair | |
| 21 | 9 a m | 29 | 9 | 8 | 6 | 2 | 2 | N W | 2 | fair | |
| | 4 p m | 29 | 9 | 9 | 2 | 1 | 8 | N W | 2 | fair | |
| 22 | 8 a m | 29 | 7 | 8 | 7 | 1 | 8 | S W | 3 | fair | |
| | 5 p m | 29 | 5 | 9 | 6 | 1 | 9 | S W | 3 | fair | |
| 23 | 9 a m | 28 | 9 | 9 | 5 | 2 | 1 | S W | 4 | cloudy | |
| | 6 p m | 28 | 9 | 9 | 8 | 1 | 9 | S W | 3 | fair | |
| 24 | 9 a m | 28 | 9 | 9 | 3 | 2 | 2 | S W | 3 | fair —— | 0,296 |
| | 4 p m | 8 | 8 | 9 | 5 | 2 | 0 | S W | 2 | fair | |
| 25 | 9 a m | 28 | 9 | 9 | 2 | 2 | 4 | W | 2 | fair — | 0,100 |
| | 5 p m | 29 | 2 | 9 | 2 | 1 | 8 | W | 2 | fair | |
| 26 | 9 a m | 29 | 4 | 9 | 5 | 2 | 2 | W | 2 | fair | |
| | 5 p m | 29 | 4 | 10 | 8 | 1 | 7 | S W | 2 | fair | |
| 27 | 9 a m | 29 | 0 | 10 | 3 | 2 | 5 | S E | 2 | drizling | |
| | 5 p m | 28 | 9 | 10 | 9 | 1 | 8 | S W | 2 | fair | |
| 28 | 9 a m | 29 | 1 | 10 | 6 | 2 | 1 | S | 2 | cloudy - | 0,295 |
| | 6 p m | 29 | 2 | 10 | 6 | 1 | 9 | S | 2 | fair | |
| H. at a med. | | 29 | 7 | 9 | 0 | 2 | 1 | | | Total depth | 3,507 |
| Gr. height | | 30 | 5 | 12 | 4 | 2 | 8 | | | | |
| L. height | | 28 | 8 | 7 | 4 | 1 | 7 | | | | |

## *MARCH* 1735.

| D. | Hour. | Baro. In. | Baro. D. | Ther. In | Ther. D. | Hyg. I. | Hyg. D. | Wind. Dir. | Wind. For. | Weather. | Rain. In D. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 9 a m | 29 | 2 | 10 | 4 | 2 | 1 | S W | 2 | fair — | 0,180 |
| | 5 p m | 29 | 1 | 10 | 5 | 2 | 0 | N W | 2 | cloudy | |
| 2 | 9 a m | 29 | 0 | 10 | 0 | 2 | 5 | S by W | 2 | fair | |
| | 5 p m | 29 | 1 | 10 | 6 | 2 | 6 | S E | 2 | cloudy | |
| 3 | 9 a m | 29 | 2 | 9 | 8 | 2 | 5 | S | 2 | cloudy — | 0,050 |
| | 5 p m | 29 | 1 | 10 | 4 | 2 | 2 | W | 2 | cloudy | |
| 4 | 9 a m | 29 | 2 | 9 | 8 | 2 | 2 | S | 1 | fair | |
| | 5 p m | 29 | 2 | 10 | 2 | 2 | 2 | S | [illegible] | cloudy | |
| 5 | 9 a m | 29 | 4 | 11 | 0 | 2 | 3 | S W | 2 | fair | |
| | 6 p m | 29 | 5 | 11 | 3 | 2 | 0 | S W | 2 | cloudy | |
| 6 | 9 a m | 29 | 5 | 11 | 0 | 2 | 2 | W by N | 2 | fair | |
| | 5 p m | 29 | 7 | 11 | 0 | 2 | 0 | N W | 2 | fair | |
| 7 | 9 a m | 30 | 0 | 9 | 2 | 2 | 1 | S W | 2 | fair | |
| | 4 p m | 30 | 1 | 10 | 7 | 2 | 1 | E | 2 | fair | |
| 8 | 9 a m | 30 | 1 | 9 | 8 | 3 | 0 | S E | 2 | fog | |
| | 4 p m | 30 | 1 | 10 | 6 | 2 | 8 | S E | 2 | fog | |
| 9 | 9 a m | 30 | 2 | 9 | 7 | 3 | 6 | S E | 2 | fog ---- | 0,190 |
| | 6 p m | 30 | 2 | 10 | 1 | 3 | 3 | S E | 2 | fog | |
| 10 | 9 a m | 30 | 1 | 9 | 6 | 3 | 4 | S E | 2 | fog ---- | 0,170 |
| | 6 p m | 30 | 0 | 9 | 9 | 3 | 0 | S E | 2 | fog | |
| 11 | 9 a m | 29 | 9 | 9 | 0 | 3 | 0 | N E | 3 | cloudy | |
| | 5 p m | 29 | 9 | 9 | 7 | 2 | 0 | N E | 3 | fair | |
| 12 | 9 a m | 29 | 9 | 9 | 5 | 2 | 0 | E | 1 | fog | |
| | 6 p m | 29 | 8 | 10 | 4 | 1 | 8 | E | 1 | fog | |
| 13 | 9 a m | 29 | 7 | 9 | 6 | 1 | 9 | S E | 1 | fair | |
| | 5 p m | 29 | 6 | 10 | 0 | 1 | 8 | S E | 1 | fair | |
| 14 | 9 a m | 29 | 4 | 8 | 6 | 2 | 0 | S E | 2 | fair — | 0,290 |
| | 5 p m | 29 | 3 | 10 | 5 | 1 | 8 | S E | 2 | fair | |
| 15 | 9 a m | 29 | 2 | 9 | 3 | 1 | 2 | S E | 2 | fair | |
| | 6 p m | 29 | 1 | 9 | 9 | 2 | 5 | E | 2 | cloudy | |
| 16 | 9 a m | 29 | 0 | 9 | 6 | 3 | 8 | E | 3 | rain | |
| | | | | | | | | | | | 0,880 |

## *MARCH* 1735.

| D. | Hour. | Baro. In | D. | Ther. In | D. | Hyg I. | D. | Wind, Dir. | For. | Weather. | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | 0,880 |
| 17 | 9 a m | 29 | 4 | 8 | 7 | 2 | 8 | N E | 3 | cloudy | |
| | 5 p m | 29 | 5 | 9 | 3 | 2 | 2 | N E | 3 | hail | |
| 18 | 9 a m | 29 | 7 | 8 | 5 | 2 | 2 | N E | 2 | fair —— | 0,450 |
| | 5 p m | 29 | 8 | 9 | 3 | 1 | 9 | W | 2 | fair | |
| 19 | 9 a m | 29 | 8 | 9 | 4 | 2 | 0 | N W | 2 | fair —— | 0,270 |
| | 4 p m | 29 | 8 | 9 | 6 | 1 | 8 | N W | 2 | fair | |
| 20 | 9 a m | 30 | 0 | 8 | 5 | 2 | 3 | N | 2 | fair —— | 0,165 |
| | 5 p m | 30 | 0 | 9 | 8 | 2 | 0 | N | 2 | cloudy | |
| 21 | 9 a m | 30 | 1 | 9 | 3 | 2 | 1 | W | 2 | cloudy | |
| | 5 p m | 30 | 1 | 10 | 7 | 2 | 1 | W | 2 | fair | |
| 22 | 9 a m | 29 | 7 | 10 | 4 | 2 | 5 | W | 2 | rain | |
| | 5 m | 29 | 6 | 11 | 5 | 2 | 7 | W | 2 | drizling | |
| 23 | 9 a m | 29 | 6 | 11 | 5 | 3 | 2 | W | 2 | cloudy — | 0,450 |
| | 5 p m | 29 | 6 | 11 | 9 | 2 | 5 | W | 2 | rain | |
| 24 | 9 a m | 29 | 4 | 11 | 4 | 2 | 8 | N W | 2 | rain | |
| | 6 p m | 29 | 3 | 10 | 3 | 3 | 1 | N W | 2 | rain | |
| 25 | 9 a m | 29 | 3 | 9 | 8 | 2 | 7 | N E | 2 | cloudy | |
| | 5 p m | 29 | 2 | 9 | 9 | 2 | 2 | N E | 2 | fair | |
| 26 | 9 a m | 29 | 1 | 10 | 0 | 2 | 5 | N E | 2 | cloudy — | 0,390 |
| | 5 p m | 29 | 1 | 9 | 0 | 3 | 1 | N W | 2 | rain | |
| 27 | 9 a m | 29 | 3 | 9 | 6 | 3 | 0 | W | 2 | fair — | 0,910 |
| | 5 p m | 29 | 3 | 10 | 7 | 2 | 3 | N E | 2 | fair | |
| 28 | 9 a m | 29 | 3 | 9 | 9 | 3 | 5 | N E | 2 | rain — | 0,060 |
| | 6 p m | 29 | 3 | 9 | 7 | 4 | 0 | N E | 3 | rain | |
| 29 | 9 a m | 29 | 2 | 10 | 0 | 3 | 3 | N W | 2 | cloudy — | 0,970 |
| | 5 p m | 29 | 4 | 9 | 8 | 2 | 4 | N W | 2 | fair | |
| 30 | 9 a m | 29 | 6 | 9 | 5 | 2 | 3 | W | 2 | fair — | 0,640 |
| | 6 p m | 29 | 5 | 10 | 9 | 1 | 9 | S | 1 | cloudy | |
| 31 | 9 a m | 29 | 3 | 9 | 4 | 2 | 4 | W | 1 | cloudy — | 0,190 |
| | 6 p m | 29 | 4 | 10 | 8 | 2 | 0 | W | 1 | fair | |
| H. at a med. | | 29 | 3 | 9 | 9 | 2 | 4 | | | Total depth | 5,375 |
| Gr. height | | 30 | 2 | 11 | 9 | 4 | 0 | | | | |
| L. height | | 29 | 0 | 8 | 5 | 1 | 2 | | | | |

## APRIL 1735.

| D. | Hour. | Baro. In. | Baro. D. | Ther. In. | Ther. D. | Hyg. I. | Hyg. D. | Wind. Dir. | Wind. For. | Weather. | Rain. In D. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 9 a m | 29 | 5 | 10 | 2 | 2 | 3 | W | 2 | fair | |
| | 6 p m | 29 | 6 | 11 | 7 | 1 | 9 | N E | 2 | cloudy | |
| 2 | 9 a m | 29 | 7 | 11 | 8 | 2 | 0 | S | 1 | fair | 0,085 |
| | 5 p m | 29 | 8 | 13 | 3 | 1 | 9 | S | 1 | fair | |
| 3 | 9 a m | 30 | 0 | 12 | 3 | 2 | 3 | S E | 2 | fair | 0,070 |
| | 5 p m | 30 | 0 | 13 | 0 | 1 | 7 | S E | 2 | fair | |
| 4 | 9 a m | 30 | 1 | 10 | 7 | 2 | 4 | S E | [illegible] | cloudy | 0,190 |
| | 6 p m | 30 | 1 | 10 | 5 | 2 | 1 | E | 2 | fair | |
| 5 | 9 a m | 30 | 1 | 10 | 3 | 2 | 9 | E | 2 | cloudy | |
| | 6 p m | 30 | 1 | 9 | 8 | 2 | 7 | E | 2 | cloudy | |
| 6 | 9 a m | 30 | 0 | 9 | 7 | [illegible] | 0 | E | 2 | rain | 0,250 |
| | 6 p m | 30 | 0 | 10 | 1 | 3 | 2 | E | 2 | drizling | |
| 7 | 9 a m | 29 | 9 | 10 | 0 | 3 | 4 | E | 2 | fog | 0,060 |
| | 6 p m | 29 | 8 | 10 | 1 | 3 | 3 | E | 2 | fog | |
| 8 | 9 a m | 29 | 7 | 10 | 5 | 3 | 2 | E | 2 | rain | |
| | 7 p m | 29 | 6 | 10 | 8 | 2 | 9 | E | 1 | cloudy | |
| 9 | 9 a m | 29 | 4 | 11 | 6 | 2 | 9 | E | 1 | fair | |
| | 6 p m | 29 | 4 | 12 | 1 | 2 | 1 | S | 1 | cloudy | |
| 10 | 9 a m | 29 | 4 | 10 | 8 | 2 | 9 | E | 2 | cloudy | |
| | 7 p m | 29 | 3 | 10 | 2 | 4 | 1 | E | 2 | fog | |
| 11 | 9 a m | 29 | 2 | 10 | 8 | 4 | 1 | E by N | 2 | fog | |
| | 6 p m | 29 | 3 | 10 | 5 | 4 | 1 | E by N | 2 | fog | |
| 12 | 9 a m | 29 | 4 | 11 | 9 | 3 | 9 | S W | 0 | cloudy | 0,140 |
| | 6 p m | 29 | 5 | 11 | 5 | 3 | 1 | W | 1 | cloudy | |
| 13 | 9 a m | 29 | 7 | 11 | 4 | 2 | 7 | W | 2 | fair | 0,050 |
| | 7 p m | 29 | 8 | 11 | 7 | 2 | 4 | W | 2 | fair | |
| 14 | 9 a m | 29 | 8 | 12 | 1 | 2 | 6 | S | 0 | cloudy | |
| | 6 p m | 29 | 6 | 12 | 7 | 2 | 5 | S | 2 | cloudy | |
| 15 | 9 a m | 29 | 6 | 12 | 0 | 2 | 5 | W by S | 2 | cloudy | |
| 16 | 9 a m | 30 | 0 | 11 | 4 | 2 | 0 | W | 2 | fair | |
| | 6 p m | 30 | 0 | 13 | 0 | 1 | 7 | W | 2 | fair | |
| | | | | | | | | | | | 0,845 |

## *APRIL* 1735.

| D. | Hour. | Baro. In | Baro. D. | Ther. In. | Ther. D. | Hyg. I. | Hyg. D. | Wind. Di. | Wind. For. | Weather. | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | 0,854 |
| 17 | 9 a m | 29 | 7 | 12 | 9 | 2 | 1 | S by E | 1 | fair | |
| | 7 p m | 29 | 6 | 12 | 5 | 2 | 0 | S | 2 | cloudy | |
| 18 | 9 a m | 29 | 6 | 11 | 9 | 2 | 0 | S W | 3 | cloudy | |
| | 6 p m | 29 | 7 | 11 | 5 | 1 | 9 | S W | 2 | cloudy | |
| 19 | 9 a m | 29 | 4 | 11 | 5 | 1 | 9 | S W | 3 | cloudy — | 0,110 |
| 20 | 9 a m | 29 | 5 | 11 | 3 | 2 | 9 | W | 3 | cloudy — | 0,030 |
| | 7 p m | 29 | 4 | 11 | 7 | 2 | 0 | W | 2 | cloudy | |
| 21 | 9 a m | 29 | 2 | 11 | 8 | 2 | 4 | W | 4 | fair | |
| | 7 p m | 29 | 3 | 11 | 4 | 1 | 8 | W | 3 | fair | |
| 22 | 9 a m | 29 | 0 | 11 | 9 | 2 | 0 | W | 3 | cloudy | |
| | 7 p m | 29 | 0 | 11 | 2 | 2 | 0 | W by N | 3 | cloudy | |
| 23 | 9 a m | 29 | 5 | 11 | 1 | 1 | 8 | W by N | 2 | fair | |
| | 6 p m | 29 | 6 | 12 | 1 | 1 | 7 | W by N | 2 | cloudy | |
| 24 | 9 a m | 29 | 7 | 13 | 1 | 1 | 9 | S W | 2 | cloudy | |
| | 7 p m | 29 | 6 | 12 | 5 | 1 | 8 | S W | 2 | cloudy | |
| 25 | 9 a m | 29 | 5 | 12 | 6 | 1 | 8 | S W | 2 | fair — | 0,070 |
| | 7 p m | 29 | 5 | 13 | 5 | 1 | 5 | S W | 1 | fair | |
| 26 | 9 a m | 29 | 6 | 12 | 8 | 1 | 7 | S W | 1 | fair — | 0,290 |
| | 6 p m | 29 | 5 | 13 | 4 | 1 | 5 | S W | 2 | fair | |
| 27 | 9 a m | 29 | 3 | 11 | 4 | 2 | 0 | N E | 2 | rain — | 0,090 |
| | 7 p m | 29 | 3 | 10 | 8 | 2 | 7 | N E | 2 | fog | |
| 28 | 9 a m | 29 | 2 | 11 | 7 | 2 | 0 | N W | 2 | cloudy | |
| | 7 p m | 29 | 3 | 12 | 0 | 1 | 9 | N W | 3 | fair | |
| 29 | 9 a m | 29 | 5 | 11 | 6 | 1 | 9 | S W | 2 | fair | |
| | 7 p m | 29 | 6 | 12 | 8 | 1 | 6 | S by E | 1 | fair | |
| 30 | 9 a m | 29 | 8 | 11 | 5 | 2 | 1 | E by S | 2 | fair — | 0,195 |
| | 6 p m | 29 | 8 | 12 | 5 | 1 | 0 | E | 2 | fair | |
| H. at a med. | | 29 | 7 | 11 | 2 | 2 | 3 | | | Total depth | 1,630 |
| Gr. height | | 30 | 1 | 13 | 5 | 4 | 1 | | | | |
| L. height | | 29 | 0 | 9 | 7 | 1 | 5 | | | | |

## *MAY* 1735.

| D. | Hour. | Baro. In D. | Ther. In D. | Hyg. I. D. | Wind. Dir. | For. | Weather. | Rain. In D. |
|---|---|---|---|---|---|---|---|---|
| 1 | 9 a m | 29 9 | 12 7 | 1 9 | S W | 2 | cloudy | 0,020 |
| | 8 p m | 29 7 | 11 7 | 1 9 | S W | 2 | cloudy | |
| 2 | 9 a m | 29 7 | 12 0 | 1 9 | W by N | 2 | fair | |
| | 8 p m | 29 7 | 11 2 | 1 6 | W by N | 2 | fair | |
| 3 | 9 a m | 29 6 | 11 4 | 1 6 | W by N | 2 | fair | |
| | 7 p m | 29 7 | 12 8 | 1 5 | W by N | 2 | fair | |
| 4 | 9 a m | 29 9 | 11 7 | 1 5 | N | [illegible] | cloudy | |
| | 8 p m | 29 9 | 11 6 | 1 5 | N E | 1 | cloudy | 0,055 |
| 5 | 9 a m | 29 9 | 12 0 | 1 6 | N E | 2 | fair | |
| | 7 p m | 29 9 | 11 7 | 1 5 | N E | 2 | fair | |
| 6 | 9 a m | 29 9 | 12 3 | 1 5 | W by N | 2 | fair | |
| | 5 p m | 29 9 | 13 5 | 1 5 | W | 2 | fair | |
| | 7 p m | 29 9 | 12 5 | 1 7 | N W | 2 | cloudy | |
| 7 | 9 a m | 30 0 | 12 5 | 1 5 | N W | 2 | fair | |
| | 7 p m | 30 0 | 11 6 | 1 6 | N W | 2 | fair | |
| 8 | 9 a m | 30 0 | 12 8 | 1 9 | S W | 3 | cloudy | |
| | 7 p m | 30 0 | 12 7 | 1 6 | W | 3 | fair | |
| 9 | 9 a m | 30 0 | 13 2 | 2 0 | W | 4 | cloudy | 0,070 |
| | 8 p m | 29 9 | 11 8 | 1 5 | W | 3 | fair | |
| 10 | 9 a m | 29 9 | 12 3 | 1 3 | N W | 3 | fair | |
| | 8 p m | 29 9 | 11 0 | 1 3 | N W | 3 | fair | |
| 11 | 9 a m | 29 7 | 10 9 | 1 3 | N W | 3 | fair | |
| | 7 p m | 29 5 | 9 5 | 1 9 | N W | 3 | rain | |
| 12 | 9 a m | 29 5 | 10 8 | 1 7 | N W | 2 | cloudy | |
| | 7 p m | 29 6 | 10 2 | 1 4 | N W | 2 | cloudy | |
| 13 | 9 a m | 29 6 | 11 7 | 1 2 | N W | 2 | cloudy | 0,030 |
| | 8 p m | 29 7 | 10 6 | 1 2 | N W | 2 | fair | |
| 14 | 9 a m | 29 6 | 11 6 | 1 4 | N W | 2 | fair | 0,190 |
| | 8 p m | 29 5 | 11 5 | 1 3 | N W | 2 | cloudy | |
| 15 | 9 a m | 29 3 | 10 7 | 1 5 | S W | 3 | fair | |
| | 7 p m | 29 2 | 11 2 | 1 1 | S W | 3 | fair | |
| 16 | 9 a m | 29 4 | 11 6 | 1 4 | W by S | 2 | cloudy | |
| | 7 p m | 29 4 | 12 5 | 1 2 | W by S | 2 | cloudy | |
| | | | | | | | | 0,365 |

## *MAY* 1735.

| D | Hour. | Baro. In. | D. | Ther. In. | D. | Hyg. I. | D. | Wind. Dir. | For. | Weather. | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 17 | 9 a m | 29 | 5 | 12 | 1 | 1 | 1 | W | 2 | fair | 0,365 |
| | 7 p m | 29 | 7 | 12 | 9 | 1 | 2 | W | 2 | fair | |
| 18 | 9 a m | 30 | 1 | 12 | 2 | 1 | 0 | N W | 2 | fair | 0,150 |
| | 7 p m | 30 | 1 | 13 | 7 | 0 | 9 | N W | 1 | fair | |
| 19 | 9 a m | 30 | 2 | 13 | 5 | 1 | 2 | W | 2 | cloudy | |
| | 8 p m | 30 | [illegible] | 14 | 3 | 1 | 0 | S W | 2 | cloudy | |
| 20 | 9 a m | 30 | 0 | [illegible] | 7 | 1 | 4 | S W | 2 | cloudy | |
| | 8 p m | 29 | 8 | 12 | 9 | 1 | 2 | W | 2 | fair | |
| 21 | 9 a m | 29 | 8 | 13 | 7 | 1 | 4 | N W | 2 | fair | |
| | 8 p m | 29 | 8 | 12 | 5 | 1 | 3 | N W | 2 | cloudy | |
| 22 | 9 a m | 29 | 6 | 11 | 9 | 1 | 6 | N | 2 | rain | |
| | 8 p m | 29 | 7 | 12 | 6 | 1 | 8 | N | 2 | cloudy | |
| 23 | 9 a m | 29 | 9 | 11 | 0 | 2 | 0 | N | 2 | cloudy | |
| | 7 p m | 29 | 9 | 11 | 1 | 2 | 0 | E by N | 2 | rain | |
| 24 | 9 a m | 29 | 9 | 11 | 1 | 2 | 0 | N E | 2 | cloudy | |
| | 8 p m | 29 | 9 | 12 | 4 | 1 | 9 | N E | 2 | fair | |
| 25 | 9 a m | 29 | 8 | 12 | 3 | 1 | | NE | 1 | cloudy | |
| 26 | 9 a m | 29 | 9 | 11 | 3 | 3 | 3 | N E | 2 | fog | 0,100 |
| | 8 p m | 29 | 9 | 11 | 0 | 2 | 1 | N E | 2 | rain | |
| 27 | 9 a m | 30 | 0 | 11 | 0 | 2 | 3 | N E | 2 | fair | |
| | 8 p m | 30 | 1 | 11 | 9 | 2 | 3 | N E | 2 | fair | |
| 28 | 9 a m | 30 | 2 | 12 | 2 | 2 | 3 | E by N | 2 | fair | |
| | 8 p m | 30 | 1 | 13 | 6 | 1 | 9 | E by N | 1 | fair | |
| 29 | 9 a m | 30 | 1 | 13 | 4 | 1 | 3 | S E | [illegible] | fair | 0,070 |
| | 8 p m | 30 | 0 | 14 | 7 | 1 | 5 | S E | 2 | fair | |
| 30 | 9 a m | 29 | 9 | 13 | 5 | 1 | 8 | E | 2 | cloudy | |
| | 8 p m | 29 | 8 | 14 | 1 | 1 | 6 | E by N | 2 | fair | |
| 31 | 9 a m | 29 | 7 | 12 | 4 | 2 | 5 | E | 2 | cloudy | 0,035 |
| | 8 p m | 29 | 7 | 13 | 4 | 2 | 0 | N E | 1 | fair | |
| H. at a med. | | 29 | 8 | 12 | 1 | 1 | 6 | | | Total depth | 0,720 |
| Gr. height | | 30 | 2 | 14 | 7 | 3 | 3 | | | | |
| L. height | | 29 | 2 | 9 | 5 | 0 | 9 | | | | |

## II. *An Account of the DISEASES that were most frequent last Year* (1734) *in* Edinburgh.

THE tertian ague, which we already mentioned to have begun in March 1734, continued till the warm weather in June thereafter put an end to it.

In May, June, and July 1734, several children in the neighbourhood of Edinburgh, and some within the city, laboured under the *tussis convulsiva*, hooping-cough, or kink-cough.—In August more children were attacked by it. It became more frequent in September. In October few children in the villages near Edinburgh escaped it; and it was frequent all winter within the town, several adults being also seized with it. The symptoms of the sick were no other than what commonly attend this disease.

The method of cure most commonly followed here was, to keep the vessels empty enough, by the evacuations of bleeding, vomiting, purging and blisters; which did not seem so much to shorten the disease, as to prevent its proving fatal; for, notwithstanding the liberal use of these evacuations, the disease frequently continued several months: For most part, however, there was a sensible remission for some days, after blooding or purging, especially when the purgatives worked upwards too. Pectorals of the soft balsamic kind, and the most attenuant were also given, but with little or no good effect. Opiates rather did hurt. A great many specifics were likewise employed, but, so far

as

as we could obſerve, with as little ſucceſs, as the numerous charms the good women thought fit to make uſe of.

Other coughs were alſo rife, and difficult to remove through the winter.

It is worthy of a remark, that both common and convulſive cough, after being milder in January 1735, ſuddenly became much worſe in the firſt week of February, and continued ſo the greater part of that month, gradually decreaſing afterwards as the ſpring and ſummer advanced.

Rheumatic pains and ſtitches, ſome with, others without fever, ſeemed to begin, continue, and decreaſe with the cough. Repeated bloodings and antiphlogiſtic purgatives, with diluent, cooling, attenuant medicines, proved the moſt effectual remedies.

Towards the end of September, and in October, many people were ſeized with a dyſentery, of which there were ſome remains in this place all winter. It had the ordinary ſymptoms of a ſlight fever, frequent ſtools, for moſt part bloody and mucous, violent gripes, and an almoſt conſtant teneſmus.

This diſeaſe was fatal to ſome, and very tedious in others, who neglected the evacuations in the beginning, and had too ſoon recourſe to opiates and aſtringents.

Patients under the dyſentery were generally bled, vomited with ipecacuanna, and purged with rhubarb, with opiates ſometimes in the intervals, and mild mucaginous food and drink. In ſome caſes, where the ordinary method failed, the *vitrum antimonii ceratum* was given with ſucceſs.

In October, there were ſome fevers of a bad kind, in which the head was much affected, and the pulſe low and ſunk. Such ſick could not bear blooding; and bliſters did very little ſervice to them.

In February agues began, increaſed in March and April, and then gradually went off; and, at the ſame time, ſome remitting fevers were obſerved.

In the end of March, and beginning of April, many children were ſeized with a very irregular fever, which ſeldom continued any number of hours in the ſame way. They were ſometimes hot; then turned cold. Their pulſe was now very quick; ſoon after became moderate.—Sometimes they had difficult breathing, thirſt, purging, like the diarrhoea in teething; at other times they were free of theſe ſymptoms. Notwithſtanding any medicines that were given, the diſeaſe continued about ten days, and then terminated in a cough; which remained ſome time with moſt of them, and, in others, was very difficult to remove.

III. *An Extract from the public Register of Burials in* Edinburgh.

| 1734. | Men. | Women. | Child. | Still-born. | Sum. |
|---|---|---|---|---|---|
| June - | 10 | 25 | 28 | 5 | 68 |
| July - - | 18 | 18 | 22 | 9 | 67 |
| August - | 18 | 21 | 39 | 4 | 82 |
| September | 18 | 21 | 34 | 6 | 79 |
| October | 28 | 34 | 39 | 4 | 105 |
| November | 24 | 35 | 41 | 5 | 105 |
| December | 27 | 36 | 51 | 4 | 118 |
| 1735. | | | | | |
| January | 25 | 38 | 48 | 4 | 115 |
| February | 21 | 18 | 53 | 5 | 97 |
| March - | 25 | 37 | 61 | 6 | 129 |
| April - | 17 | 21 | 53 | 3 | 94 |
| May - - | 13 | 29 | 46 | 2 | 90 |
| Total | 244 | 333 | 515 | 57 | 1149 |

IV. *The good Success of opposite Caustics, and of a strong alterative Mercurial Medicine; by Dr* EDWARD BARRY, *Physician at* Cork, *and F. R. S.*

I Visited, with Mr Osburne and Mr Wilson, two eminent surgeons in this place, a gentleman of about forty-five years of age, of a sober life, and in a married state, who had a hard tumor formed in the coats of the testes, which lightly adhered to the right testicle, and extended to the epididymis, and was then as large as a turkey-egg. It came on him without any cause which he could account for, and was equal to the size of a walnut when he first perceived it.

He ſays it made this progreſs in the ſpace of one night.

Some months before he came to town, emollients and ſuppuratives were uſed, a collection of pus was ſuſpected, and a puncture made: a ſmall diſcharge of blood and ichor followed, the tumor continued large and ſchirrous, with the appearance of a cancerous ulcer.

Various internal medicines, and external applications were made uſe of to no purpoſe. But the following method removed this diſorder.

The common cauſtic, which is named by ſome *lapis ſepticus*, and by others *lapis infernalis*, was applied; after the ſeparation of the eſchar, about two inches in length, the *lapis infernalis* and *oleum vitrioli* were alternately uſed, by rubbing the part, firſt with the *lapis infernalis*, and, in leſs than a minute afterwards, with a piece of fir-ſtick dipped in the *oleum vitrioli*; which inſtantly removed the pain occaſioned by the *lapis infernalis*. At each dreſſing, this alternate application of theſe oppoſite cauſtics was repeated, till as much was waſted as was then thought convenient; the moiſture was abſorbed by an armed probe, and a digeſtive applied.

By theſe means the tumor was gradually waſted every day, without any continuing pain, or ſucceeding inflammation: A ſmall part was ſuffered to remain adhering to the teſticle. This was thought more prudent than to run the hazard of injuring the teſticle. This application anſwered lately in another very obſtinate ſchirrous tumour in the coats of the teſtes, and, in many caſes, ſeems to be preferable to common

cauſtics;

cauſtics; the one correcting by its oppoſite quality the too active ſalts of the other, and by that means inſtantly removing pain, and, by producing a *ſal tertium*, which has a mild opening quality, prevents an inflammation and callous lips, the common conſequences of cauſtic applications.

About three months afterwards, a tumor appeared in the other teſticle of this gentleman, of about the ſize of a walnut, which was removed, by giving him, going to bed at night, 15 grains of *pil. ex duobus*, 10 grains of turbith mineral, and the ſame quantity of camphire. It was ſeldom omitted any night. In the beginning it vomited him ſometimes, and purged four or five times; but at laſt operated chiefly as an alterative, and, in three weeks, not only carried off this tumor, but entirely removed the ſmall ſwelling that was left on the other teſticle. This medicine, which has a rough appearance, acts as a mild, though a powerful alterative. The evacuation which attends it is generally very gentle. I have often known it ſucceſsful in obſtinate venereal and ſcrophulous diſorders. Mr Moore, a ſurgeon in the army, to whom I communicated it ſome years ago, aſſures me it never fails him in obſtinate gonorrhoeas, and in many caſes for which he was formerly obliged to direct a ſalivation.

V. An

V. *An Essay towards ascertaining the Doses of vomiting and purging Medicines; by Dr* CHARLES BALGUY, *Physician at* Peterborough.

AFTER finding out a disease, and prescribing such medicines as shall answer the true indications of cure, there is nothing in practice more material, and yet less understood, than the art of adjusting their doses so nicely to the case in hand, and to the age, size, and strength of the person, that he shall receive the most speedy and certain relief these medicines are capable of giving, without the hazard of burdening nature, and overdoing the constitution. Dr Cockburn attempted this some time since, in vomiting and purging medicines; but, as he went upon a wrong principle, he must needs be mistaken in his consequences. Perhaps what I am going to advance may be liable to objections; I know it is, and I shall show that an absolute certainty cannot be expected: But yet I think I may venture to say, that you go upon surer grounds by following this method, imperfect as it is, than no method at all. First then, I suppose it will be readily granted me, that part of the medicine is spent on the *primæ viæ*, where it acts as a stimulus; and that the other part is carried into the blood, and has its effect there, by thinning and rarifying it: The first is plain, from their acrid burning taste, and the blisters which the most powerful will raise in the mouth: The other is certain, from the pulse being raised after taking them, and from

from known experiments upon the blood: But in what proportion they exert their different faculties is the difficulty; could this be precifely determined, the dofes of thefe medicines might be fixed with the utmoft exactnefs and certainty. All that can be done then is, to affign fuch a proportion as feems moft agreeable to experience. Thus we all know, that refinous purges exert themfelves chiefly on the firft paffages; whereas the greateft part of fuch as are of a more lax texture is carried into the blood, and, by attenuating it, promotes every other fecretion, as well as that by the inteftines. It may feem reafonable therefore to fuppofe, that, of fcammony, elaterium, and the ftrongeft refinous purges, not more than one fourth gets into the blood: Of jalap, ipecacuanna, &c. one third: Of rhubarb, fenna, aloes, &c. one half: Of *cremor tartari*, and the purging falts, two thirds. This being allowed, I proceed in the following manner: And, 1ft, I fay, If the medicine acted only on the *primæ viæ*, the dofe, in perfons of the fame fize, would be directly as the conftitution; for, as the fibres of the reft of the body, fo are thofe which compofe the ftomach and inteftines; and the ftronger they are, fo much the more able will they be to fhake off, and difengage themfelves from the particles of the medicine, which are fuppofed to prick and irritate them: And therefore fo much the larger dofe will be required to have a certain effect. Where the conftitution is the fame, it is eafy to fee that the dofe will be as the fize; when both differ, then it follows, that the dofe will be as the fize into the

the conſtitution. 2dly, Suppoſe the whole medicine to paſs into the blood; and the doſe will be as the ſize into the ſquare of the conſtitution. This is demonſtrated by Dr Cockburn: And therefore, 3dly, You are to doſe ſo much of the medicine as is ſpent on the ſtomach and inteſtines, directly as the conſtitution; and ſo much as is carried into the blood, as the ſquare of the conſtitution, and the ſum into the perſon's ſize is the quantity required. There are ſome caſes exceptions to this rule, which ſhould be conſidered, and provided againſt: And 1ſt, in conſtitutions which abound with acids in the *primæ viæ*, we find the force of reſinous purges ſo weakened, that they ſcarce operate at all. They are alſo leſs active, or a larger doſe is required, when the body is full of aqueous humours; for the ſmall proportion of bile in ſuch conſtitutions not being ſufficient to cauſe a compleat ſolution of the reſin in the water, a conſiderable part paſſes off, without imparting any of its ſubſtance: As, on the contrary, to perſons of a dry habit, and in hot climates and ſeaſons, they often cauſe intolerable grippings and hypercatharſes, for want of due moiſture. But theſe, and others of the like ſort, the cauſe being known, are eaſily remedied. And though not attending to, or being unacquainted with the ſtate of the body in theſe particular caſes, may render this method leſs uſeful, it is no leſs true on that account.

VI. *The*

VI. *The bad Effects of Opium given too soon to stop the Operation of Emetics; by Mr* JOHN STEDMAN, *Surgeon at* Kinross.

NOtwithstanding all the caution that can be used, emetic medicines will sometimes do more than is intended they should, either by vomiting more frequently, or by purging. In such cases the patients become alarmed, frighted, and impatient; and those who have prescribed for them, either to humour their patients, or perhaps too anxious about the consequences, have recourse immediately to opium, to stop any further effects of the medicine. I know there is no such powerful and good medicine as opium, when properly given, to stop vomiting or purging: But I cannot forbear to caution young practisers, not to be too hasty to give opium in the case mentioned, till the emetic (and I may say the same of purgatives) has wrought itself well out of the body; for I have seen several bad consequences from its being used too soon, of which the following case is a remarkable example.

A gentleman, aged forty-nine, being troubled with a cough, was prescribed 5 grains of emetic tartar by his physician: The patient obliged his apothecary to give him 6; which, after vomiting him six or seven times, began to purge him with gripes. The gentleman turned impatient; and, without advice, took 20 gutts of laudanum in a glass of white wine; opium having been a familiar medicine, for some time, to remove a watchfulness he was subject

ſubject to. In half an hour after taking the laudanum, he was free of the gripes and purging; but, in half an hour more, became ſhort-breathed, with ſweating about the heart; then I was firſt ſent for in a hurry, but, before any thing could be got done for his relief, he died.

## VII. Pulvis Stypticus *recommended particularly in uterine Hæmorrhagies; by Dr* ALEXANDER THOMSON, *Phyſician at* Montroſe.

SCribonius Largus, the Roman empyric, made uſe of ſimple allum in the evacuations of the ſex exceeding their due bounds; and I have been told by ladies, that it has very good effects.

Helvetius improved on this, by adding *ſanguis draconis*, whether as a larva, to make it his own, or to prevent the uneaſineſs of the ſtomach, which he might ſuſpect the allum might give, I cannot determine: But Dr Pitcairn, whoſe memory muſt continue as long as phyſic is known, was the firſt who introduced the uſe of it into this country; at leaſt, it was he who firſt deſired me to make experience of it in a caſe which had reſiſted a great many other medicines. Its reputation kept up many years, under the name of *pulvis Helvetii*, as an aſtringent, eſpecially in uterine hæmorrhagies; and I ſee it inſerted in the pharmacopœia of your college of phyſicians, by the name of *pulvis ſtypticus*; though in ſome different proportion, and different manner of preparing, from what I have commonly uſed.

ufed. The difpenfatory powder being prepared of a double quantity of allum to one of the gum, and made into a powder, without being put near to the fire; whereas what I have ufed was equal parts of both, the allum being firft melted in a crucible, and the powder of the *fanguis draconis* added to it, and then powdered together in a mortar; poffibly the difference of their effects may notwithftanding be very little.

The ufe of both I think is now much laid afide, which I cannot but regret; fince I never found any medicine (and I have tried feveral) fo much to be depended on in all the uterine hæmorrhagies; whether to correct the too frequent return of the menfes, or their too great abundance; to ftop the flooding which women with child are fo fubject to, or to moderate the flow of too plentiful lochia. I have tried it in fo many cafes with fuccefs, that it would be altogether tedious to give you their hiftories.

The quantity I give of the *pulv. helvetii* is more or lefs according to the exigencies of the patient: In violent bloodings I give half a drachm every half hour, and feldom or never mifs to ftop it before three drachms or half an ounce is taken.

The fuccefs of this medicine in thefe bloody evacuations, has encouraged me to prefcribe it alfo in the *fluor albus*, that obftinate pernicious difeafe of the fex, in which I have been furprifed at its good effects.

VIII. *Violent Effects of a* Mercurial Suffumigation; *by Mr* JAMES HILL *Surgeon in* Dumfries.

A Tall gigantic woman, sixty three years of age, complained to me of a hoarseness and sore throat, which she had laboured under some months, owing, as she said, to a cold; but, having observed some scabby crusts on her arms and fore-head, I suspected her disease to be venereal; and at last was informed it was a lues of four years standing. Her other symptoms were a weak low pulse, intermitting every third or fourth stroke; which she was sensible of, by a painful fluttering at her heart, as she called it. She had so many excrescences of every sort about the pudenda, she could neither sit nor walk without pain. The cephalalgia and other nocturnal pains were so violent, that frequently she slept none all night. By the long continuance of her disease, and the bloodings and rough medicines she had undergone, she was reduced almost to a skeleton.

Her case appeared desperate; but the woman begged so movingly for relief, and Dr Turner recommends his method of cinnabarin fumigations in the very worst circumstances so much, that I resolved to make trial of their effects.

On Monday, 1st April 1734, at nine o'clock of the forenoon, I burnt half a drachm of factitious cinnabar under her nose and mouth; which she bore very well, sucking in the fumes greedily,

greedily, with little cough. I left her wrapped up sweating and spitting. At twelve, she had spit half a pound, was coughing little, and the room smelled pretty strong of a rising salivation. Her pulse was quicker and fuller, but very irregular and intermitting. She would not acknowledge herself to be sick, but said her throat was easier. I ordered her to keep warm. At three afternoon, the room smelled as strong as any I ever felt, when the patient was spitting three or four pounds a day. She had had three stools, was very sick, and complained of excessive gripes. Her pulse was quick, low, quivering, and intermitting; I gave her 10 drops of laudanum in a glass of a cordial astringent julep, put her into bed with her cloaths on, and wrapped her up for sweating. At nine in the evening, she was in a very profuse sweet; her pulse going at a high career, full, strong, and intermitting only one stroke of twenty one or twenty two. She told me she was altogether free of sickness, without any fluttering at her heart. I left other 10 guts of laudanum in a haustus, to be given if the gripes should return; which they did after midnight, she having then exposed herself to cold, by undressing to go naked to bed. She had three stools before the laudanum had effect, but grew easy again as soon as the sweating returned.

In this condition, she continued all Tuesday; the gripes, sickness, and intermitting pulse returning every ten or twelve hours, and the profuse sweat succeeding after taking the opiate draught.

On Wednesday morning, the opiate was omitted, and she had twenty stools with the sickness, gripes, and intermission of the pulse, by which she was much weakened. I again ordered the opiate in a glass of warm claret, in the afternoon; which brought back the sweat, and removed the other symptoms; and I gave her some *ung. citrinum* to anoint the sores with.

She passed Thursday the same way as Tuesday. The foetid salivating smell still continuing.

Friday morning, at seven o'clock, she took a purging potion, which did not operate at nine, she lying and sweating. At eleven, I sent a stimulus, to be taken if her potion had then no effect; she concealed her having had three stools, and swallowed the stimulus: By which she was purged so severely, that at six in the evening she seemed almost like one about to expire; but was soon relieved by her ordinary anodyne, and lay perfectly easy in a sweat. By this time her throat was almost quite well, and all her other sores were entirely healed.

Saturday morning, she had the gripes, sickness, and irregular pulse; notwithstanding this, she walked that day six or seven miles, and rode one or two home, in wet cold weather.

On Wednesday I was told she was no worse; the purging still continuing till the middle of May. Her throat appearing a little tender, I gave her a solution of calomel in *aq. rosar.* to gargle with; desiring her to swallow none of it. This however purged her a little. Afterwards she took Dr Plummer's pills, with the *sulphur. aurat. antim.* and calomel, and drank the

the decoction of the woods. With these she sweated plentifully in the warm weather, but became free of all her former complaints, riding about to markets. When the cold season came on, the purging succeeded the sweating now and then, till it went quite off in February 1735, when her legs began to swell; of which she was relieved by two doses of purgatives. But the swellings returning again, and she being neglected, they increased; she wasted, and at last died toward the end of April.

## IX. *The Effects of a very small Quantity of* Arsenic; *by Dr* ALEXANDER THOMSON *Physician at* Montrose.

A Lady finding some arsenic, which she knew not, among other things for the use of the family, put a little of it into her mouth, as people ordinarily do when they would discover things by their taste. Soon after, she came to know what it was; but being, as she thought, certain that she had swallowed none of it, would use no precaution, and felt no change on herself for twelve hours. Then she became suddenly vertiginous; and being carried to her bed, her body was all over convulsed, so as by her motions the bed and chamber were shaken. I saw her four hours after this attack in this condition; it was too late to give her a vomit, neither did she complain of any disorder in her stomach or other bowels. I gave her as much *ol. amygd. d.* as she could bear, and ordered an injection of *ol. olivarum*; whereby hard fœces, of the shap and figure of ſheeps dung, and as deep-green a colour as capers, were brought away. I repeated the injections, till the clyſter came away as it was injected, without any of theſe fœces.

The convulſions and frights in her ſleep, procured by liquid laudanum given in emulſions, ſtill continued; and ſhe awaked often with ſuch ſtartings, that, had not one in the bed held her firmly, ſhe would have been thrown out of it. In this condition ſhe remained a whole day and night.

The day following, her body, but eſpecially her head, face, and neck, were all covered over with red ſpots like meaſles, with exceſſive glowings, and the *aurium tinnitus*. Her other ſymptoms, however, began to abate.

To advance the eruption, and thereby to carry off the internal diſeaſe, I gave diaſcordium and volatile medicines, whereby ſhe recovered of all the ſymptoms from the poiſon in five or ſix days, but remained deprived of a fine conſtitution ſeveral years after.

X. *Hiſtories of* Gangrenes *cured by the* Peruvian bark; *by* ſeveral hands.

TILL the good effects of medicines are aſcertained by a ſufficient number of well vouched hiſtories, phyſicians and ſurgeons muſt be cautious in giving, far more in depending on ſuch as are recommended only by report, or people whom they have no reaſon to confide in: We believe therefore it may be of uſe to the public to collect here the hiſtories of the ſervice of the

the Peruvian bark in gangrenes from different caufes, which we have been favoured with from our correfpondents. We have taken the liberty to abridge them, without, we hope, fuppreffing any thing that is material.

*Firft* cafe, communicated to us by Mr John Paifley furgeon in Glafgow, was a diary, kept by Mr John Hamilton ftudent in phyfic and furgery, of his father's difeafe, revifed and approved by the ordinary phyfician, Dr George Thomfon phyfician in Glafgow.

Archibald Hamilton of Weftburn, Efq; aged feventy-fix, who had enjoyed uncommon good health all his life, having been fcarce ever fick or out of order, till he was feized with an hemiplegia two or three years ago, of which he recovered fo well, by the affiftance of Dr Thomfon, that he walked abroad with the help of a ftaff; 10th April 1735, complained of a pain in the toe next to the little one of the right foot; but neglected it two or three days, till the pain increafing, with an ouzing of ichor from a fmall black fpot; and his ancle fwelling, fome tincture of myrrh and aloes was applied to it

On the 14th, the fwelling had gone a good way up his leg, and the fpot was very black and dry. Antifeptic fomentations and the tincture were ufed. Notwithftanding which, and a mixture of *fuccus cicutæ* and *fp. fal. ammon.* with the tincture, all the fymptoms increafed next day. The doctor prefcribed a draught of the *decoct. amar.* in which half a drachm of the *cortex Peruvianus* was boiled, to be taken every morning.

The

The day following, *viz.* 17th, the bone of the toe appeared bare, the flesh round it mortified, and black spots were seen upon the ancle and calf of the leg.

18th, The toe was cut off at the second joint; digestive was applied, and the leg was embrocated with *sp. anthos, sal ammoniac.* and *camphor.* All the medicines being continued, there was no great change for three days.

On the 21st, the black spots looked paler, but the toes were all livid.

22d, His pulse intermitted, and he was very uneasy.

23d, The toes very black.

24th, The sore very fœtid. The black spots in the leg began to disappear.

25th, The spots of a pale red colour. A great pain in the sole.

No great change on the 26th and 27th.

28th, The little toe was cut off. Appearance of suppuration, with violent pain in the sole of the foot.

29th, The patient slept none, was very feverish, and raved, with wild looks. An emollient poultice applied to the sole.

30th, Still raved. The swelling of the leg almost entirely gone. No spots on it.

1st May, Bloody sanious matter let out by an incision in the sole, where the tendons were bared, and very tender.

2d and 3d, As formerly.

4th, The two remaining lesser toes having mortified, were also taken off. The great toe a little livid.

5th, The

5th, The upper part and ſole of his foot ill-coloured. His appetite now for the firſt time failed him.

6th and 7th, little change.

8th, Fifteen grains more cortex added to each draught.

9th, The ſwelling in the leg much diminiſhed.

10th, The ulcers in the foot larger.

11th, As on the 10th.

12th, Towards night, he had frequent faintings, an intermitting pulſe, great oppreſſion and ſickneſs with ſtrugglings. Took *ſp. lavend. comp.* and *ſalin. aromat.*

13th, Much relieved, but ſtill confuſed with wild looks.

14th, He was calm and chearful, with regular pulſe.

15th, The ſwelling of the leg being now gone, the ſpirituous embrocation was laid aſide.

16th, The ulcer was cleaner. Half a drachm more bark was added to each draught.

From this to the 26th of June, his cure ſeemed to go on ſucceſsfully, with his dreſſings and decoction.

26th June, The foot began to ſwell with great pain.

27th, The ſwelling increaſed up the leg; and the great toe, which had been hitherto of a bluiſh colour, became nearer to a black colour. The decoction was continued, and the ſpirituous embrocation was again uſed.

The ſwelling, with black ſpots about the ancle, increaſed up to his knee before the 7th of July, when the ulcer in his foot was black.

Inſtead

Inftead of the *decoct. amar.* he was now ordered to take half a drachm of the bark in fubftance, morning and evening.

8th, July, black fpots appeared above as well as below the knee, one of them was two inches in diameter.

9th, The fwelling of his foot lefs.

10th, His right tefticle alfo fwelled.

Little change till the 15th, only the fwelling of the tefticle abated.

15th, He would fit up. The leg fwelled greatly.

16th, Many fmall, livid, or pale red fpots above the knee.

Till the 20th the appearances all better; the fwelling diminifhed; the fpots going off; the ulcer digefting.

20th, He had got a little fleep, had great pain; his pulfe was oppreffed, and he very faintifh: He took fome cordial drops, without any relief. The dofe of his bark was increafed to two fcruples, morning and evening.

Next day the leg looked worfe, but much better the two following days.

24th, In the night before, he had a great fweat in the thigh and leg, and the fwelling was fallen two inches.

The cure went well on with the cortex, without any new fymptoms, till Auguft 10th, when a fmall tumor was obferved near the heel; the matter from which difcharging at the ulcer in the fole of the foot, it was afterwards cured by compreffion. The fwelling of the leg, appearing now to be only oedematous, was bandaged up to the knee.

In the beginning of October, when the fores feemed to be near cured, he omitted the bark; but a blifter as big as a hen's egg, rifing on the great toe of the left foot; and, two days after, fuch another appearing on the great toe of the right foot, he returned again to the ufe of the bark: The fkin, which they covered, is frefh and clean. The other ulcers are now near healed; and we refolve to continue the bark fome time after the cure, to prevent more returns.

It is to be remarked, that during all the time of the cure, except when the patient was fick and oppreffed, he would not be confined to any regimen in diet, but indulged himfelf even in a plentiful ufe of falt meats and ftrong malt liquors.

Obf. II. Mr William Wood furgeon in Edinburgh informs us, that a young woman, who was brought very low both in flefh and ftrength, by what was judged an atrophia, from obftructions in the mefentery, had her feet greatly fwelled in the oedematous way, as all the depending parts of her body, even in a lying pofture, alfo were; which fhewed the *vis vitæ* to be very weak, and the fmall remains of her blood to be in a very watery ftate. The fkin on the fuperior part of the right foot having become black, with all the other fymptoms of mortification, Dr Francis Pringle, who had attended her in her former difeafe, and Mr Monro, who was called upon this gangrenous appearance, agreed with Mr Wood in opinion, that the mortified foot fhould ſhould be well fomented with an antiſeptic decoction, that all the gangrene ſhould be ſcarified, and baſilicon, with oil of turpentine, applied in the inciſions, and a cataplaſm of theriac put over all; and that ſhe ſhould immediately begin to take a ſcruple of the powder of the Peruvian bark four times a-day. Three days after, the mortified parts began to ſeparate from the ſound. In two days more, ſhe neglected her doſes of the bark; very ſoon after which, there was no more appearance of any further ſeparation; and, upon taking her medicine, the ſeparation went on as formerly. Her former diſeaſe killed her in few weeks, without any further progreſs, or new attack of the gangrene.

Obſ. III. Dr Thomas Simſon, profeſſor of medicine in the univerſity of St Andrew's, ſent us the following obſervation.

John Daw, about fifty years of age, ſervant to a miller here, after being employed all day in ſupplying corn to the mill in a ſieve, felt an uneaſineſs near the inſertion of the tendon of the flexor of the laſt joint of the fore-finger, where the edge of the ſieve, in which he had carried the grain, reſted. The joint being alſo a little ſwelled, he was adviſed by his neighbours to apply ſome white lilly root to make it ſuppurate. After applying this eight days, meeting me in the ſtreet, he ſhewed me his hand; the whole finger, and part of the metacarp were now ſwelled, and there was a ſmall opening at the ſecond and third joints of the finger, out of which a ſerous

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dark coloured matter iſſued. I deſired him to apply a warm poultice of boiled oat-meal, to let blood, to take a mercurial bolus at night; and ſome jallap, for a purgative, next day.

Three days after, the 20th March 1736, I was ſent for to viſit him. Ever ſince the purgative, which had operated mildly, he had been feveriſh, and his hand was much worſe; the two under joints of the finger being quite mortified, there was a large gangrenous bliſter both on the back and fore part of the metacarp next to that finger. A very conſiderable florid eryſipelous ſwelling, which pitted when preſſed, extended itſelf as far up as his elbow; and there was a tenſion and fulneſs on his arm and in the arm-pit. I ordered the gangren'd parts to be fomented with ſpirit of turpentine, and gave him half a drachm of the *pulv. cortic. Peruv.* every fourth hour.

Next morning, the inflammation had made no further progreſs; and in the evening the ſwelling and florid red colour extended no higher than his wriſt; and even below that the appearances were better, the parts being more ſenſible, and the ſuppuration beginning round the middle joint of the fore-finger. I continued his medicine; and on the 24th there was a compleat ſeparation of the mortified parts, the bliſtered ſkin caſt off, and there was an ulcer penetrating from one ſide to the other of the articulation of the firſt bone of the fore-finger with the metacarpal, from which a liquor in colour like to the decoction of the bark was evacuated.

The two mortified joints were cut off; I continued the use of the bark eight days longer, but diminished the number of doses. The stump was gradually covered with flesh; and after a tendon was cast out of the ulcer in the metacarp, all the sore speedily cicatrized.

Obs. IV. Dr Simson has also favoured us with another example of the good effects of the bark, in a case which he cannot determine to have been a gangrene, because he did not see the patient.

Mr Morton, an apothecary in Coventry, wrote to him, that he (Mr Morton) had laboured more than a year under a sore throat, occasioned by a spot at the root of his tongue no bigger than a sixpence; which had been judged unanimously to be cancrous, and for which he had undergone a variety of cures, prescribed by the best physicians. His description made Dr Simson suspect it rather of the nature of a gangrene; and, as he had had experience that the bark would do no harm in cancers, he desired him to try that medicine. After taking it some time, Mr Morton writes the Doctor, that, since using it, he had spit up a great deal of dead filth from the sore which was filled up, though still it was painful and hard about the edges.

Obs. V. Mr Gibson, town's professor of midwifery and surgeon of Edinburgh, favoured us with a remarkable instance of the good effects of the cortex in gangrenes.

Mr Alexander Bayne, merchant in White-horse yard near Drury-Lane, London, aged forty, of

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a groſs ſcorbutic habit, in mounting a vicious horſe on the 4th of June 1735, was thrown on the pin of a cart; by which a large penetrating wound was made in the under part of the umbilic region, and ſomewhat towards the right ſide, through which the omentum did fall down four or five inches, with its lower edge lacerated. There was alſo a ſimple fracture of the fibula of his left leg.

I attended him with two other ſurgeons; and, finding the colour of the omentum changed, by being expoſed ſome time to the air, I extracted a little more of it ſoftly, and made the uſual ligature upon the ſound part, and then cut it off to within half an inch of the tying, allowing afterwards the extremities of the ligature to hang out of the wound, till it ſhould fall off by the ſuppuration. I thought the omentum drawn together with the ligature would favour the diſcharge of blood or matter, as well as the tents which authors injoin in ſuch caſes, without the pain or inflammation which theſe cauſe. The external wound was dreſſed with pledgits dipt in a warm digeſtive, and ſupported with compreſſes and bandage, ſo tight as to prevent the prolapſus of any of the other viſcera. The fracture of the fibula was eaſily reduced, and dreſſed in the ordinary way. He was plentifully blooded, and an emollient clyſter was injected.

Dr John Jamieſon was called to our aſſiſtance, and attended him afterwards all the time of the cure.

The two following days no extraordinary ſymptom appearing, he was dreſſed as formerly.

On the 7th of June, the digeſtion was begun.

On the 8th, there was a reaſonable diſcharge of laudable well-digeſted pus.

But, on the 9th, in the morning, we were informed he had had a reſtleſs night, having drank much, yet making no particular complaint: Though his tongue was white, it was not parched, nor had he any conſiderable heat or degree of fever on him now, or ever after. The dreſſings, when taken away, were dry, and, ſome more than two inches round the wound, the parts were livid and inſenſible. The teguments were immediately ſcarified, and ſtupp'd with a proper heating fomentation. Pledgits dipt in warm oil of turpentine were applied to the ſcarifications, and a poultice of theriac, moiſtened with ſpirit of wine, was laid over all and half a drachm of the *pulvis cortic. Peruvian.* was ordered to be given every fourth hour, drinking a ſmall glaſs of old ſtrong claret after it.

In the evening, the mortification had made no further progreſs, and we obſerved a kind of dew on the dreſſings, which were renewed as before; and the cortex was regularly given through the night, which he paſſed with more eaſe than he had done the preceeding one.

Next day, there was a plentiful diſcharge from the wound of excellent matter, and an ichor from the ſcarifications.

The bark was continued till the 14th, when not only the diſcharge at the wound continued good, but the inciſions yielded laudable pus; and all our fears were over.

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On the 15th, the fracture was dreſſed, and appeared in a good way.

On the 16th, he complained of a ſenſe of cold in the foot of the fractured leg; which alarming us, we undid the dreſſings, and found the ſkin livid and cold as ice, with gangrenous veſiculæ here and there, which immediately were cut, and ſtupped with a warm ſtimulating fœtus. The wound in the belly, which was dreſſed at the ſame time, diſcharged only a ſmall quantity of a foetid bloody ichor, the colour of the ſkin round it being much paler than uſual: Wherefore we, without delay, had again recourſe to the bark; which anſwered our expectation ſo well, that at next dreſſing we had well concocted matter from the wound, and the foot had recovered its natural heat and colour next morning. We determined therefore to continue the uſe of this excellent and neceſſary medicine for a conſiderable time: And indeed we had occaſion oftener than once to obſerve how much we ſtood indebted to it in this cure; for if at any time of the firſt three weeks, the diſtance of time between taking each doſe exceeded eight hours, we were ſure to find the matter in leſs quantity, and of a much worſe quality.

Our patient was cured in five weeks, and was deſired to wear a poſting belt, with proper compreſſes upon the place where the wound had been for ſome time till the cicatrix was ſufficiently hardened, that upon any violent motion a hernia might be prevented.

*Obs.* VI. Mr James Calder junior, ſurgeon in Glaſgow, writes us, That a boy of twelve years of age, of a good conſtitution, having, by jumping, violently ſtrained the articulation of the foot, it ſwelled conſiderably; and a prentice having applied a tight bandage to it, before next night, when Mr Calder firſt ſaw it, the inflammation was greatly increaſed, and black ſpots appeared upon the ſurface of the ſkin. The boy's pulſe was high and quick He was blooded, and had clyſters given him; the part was well fomented. poultices and ſpiritous medicines were applied, and it was at laſt ſcarified. After a variety of the common medicines had been uſed eight days to no purpoſe, the gangrenous ſpots ſpreading and turning deeper, Mr Calder gave his patient a ſcruple of the *pul. cortic. Peruvian.* four times a-day; and in ſix days after his firſt beginning to take this medicine, the putrid parts ſeparated and caſt off, and the cure went on afterwards as that of a common mild ulcer uſes to do.

*Obſ.* VII. Mr Monro, profeſſor of anatomy, informs us, from the records of the infirmary here, That, on the 13th day of March 1735, Robert Biggins, a middle-aged labouring man, was received as a patient there. By a fall, he had broke both the bones of the leg, three inches above the articulation of the foot. There was a large wound on the arterior part of the fracture, and a violent inflammation and tenſion on the whole leg, with a mortification begun near the wound. In which ſituation it would

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not allow of the extenſion neceſſary for a reduction.

For the firſt four days he was treated in the common method for gangrenes, by fomenting the whole member, ſcarifications, and warm ſpirituous applications to the gangrened parts; and emollient poultices over all, with bleeding and low diet. The tenſion yielded a little to theſe, but the gangrene advanced.

On the 17th, he was ordered to take thrice a-day the bulk of a nutmeg of an electuary, compoſed of the powder of *cortex Peruvianus* and *ſyrup. cariophyl.* and to continue the former applications.

18th, There was a remarkable change for the better; but, on the 19th, the fomentation being neglected, the pain became more violent, and the appearances worſe.

The bark and fomentation being again uſed, the appearances were all good; and the bad ſymptoms decreaſing daily, the fracture was reduced on the 24th, and the cure went afterwards on in the common way.

*Obſ.* VIII. Mr John Douglas, ſurgeon in Edinburgh, informs, That —— Porteous, a labouring man at Lintoun, about twenty years of age, having fractured his arm by a fall from a cart, had it bandaged up by ſome of his neighbours, without reducing the fracture. Thirteen days after, Mr Douglas's advice was firſt aſked, when a large ſwelling and conſiderable mortification were brought on the part; his pulſe being felt at the wriſt of that arm in a natural ſtate, and,

there

there being little tumor in the hand, he advised the gangrened part to be scarified, and to give the patient half a drachm of the powder of the bark every three or four hours. Ten days after, when he was again called to him, the swelling of the arm was fallen, and a great share of the sphacelated parts were separated; he then attempted the reduction of the bone, which he could not accomplish till he sawed off a little of the lower piece of the fractured bone, when he observed all the parts which the bones hid before were mortified, except the artery and a small share of the tegument on the back part of the arm. He therefore performed the amputation close by the head of the *os humeri*, beyond which the mortification extended a considerable way; so that he had great difficulty in stitching the artery, the corrupted flesh always yielding as he passed the needle. He was obliged to apply bandages; and therefore could not scarify, nor use the common medicines in such cases, but was obliged to trust entirely to the bark, which he ordered to be given as formerly. In few days a good suppuration came on, and the patient cured easily, and is now in good health.

We have heard of several more cases of gangrenes, both from internal and external causes, where the bark was given, every one of which was successful: But we think these, with the histories published in Art. V. and VI. of our third volume, are sufficient to convince the most incredulous how valuable a discovery has been made by Mr Rushworth, who was the first

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who employed the Peruvian bark in gangrenes. See p. 323. of vol. II.

XI. *The Description and Uses of the* Intestinum Duodenum; *by* ALEXANDER MONRO *Professor of Anatomy in the University of* Edinburgh, *and* F. R. S.

ANatomists having generally copied Vesalius's * description and picture of the *intestinum duodenum*, which appeared to me very faulty, I caused Mr Cooper to draw that intestine in its natural situation several years ago. Since that time, I have read two authors, Santorini † and Winslow ‡, who have described this gut more accurately than Vesalius; but, neither of them having given any figure of the parts, and my description differing considerably from theirs, as will appear upon comparing them, I resolved to send you this paper, that the exact situation of this intestine might be more generally known; by which many phænomena in the animal œconomy and diseases may be understood and explained.

From the pylorus, which is raised upwards and backwards from the stomach, the duodenum descends obliquely to the right side, with the anterior lamella of the omentum fixed to its inferior part; and the little omentum, proceeding from the opposite part, to connect it to the liver. After this, the duodenum is involved for about an inch and a half, in a doubling of the omentum,

* De corp. hum. fabric. lib. 5. cap. 4.
† Observ. anat. cap. 9. § 7.
‡ Exposition anat. traité du bas ventre, § 105. &c.

tum, and then enters into the duplicature of the mesocolon, where it can't be seen without dissecting away that fatty membrane. It descends in this cellular sheath, till it is almost contiguous to the great sac of the colon, which properly is the human cæcum. In this descent the colon lies before it; the biliary duct, hepatic artery and nerve, *vena portarum*, and emulgent vessels, are behind it: The liver, gall-bladder, and right kidney, are on its right side, and the pancreas is on the left. This gut makes several turns in this progress; for it is raised into a convexity forwards, where it passes before the vessels of the liver. Immediately after, it bends backwards and to the right side, till it approaches the right kidney, and then turns forward, and a little to the left in its course towards the great sac of the colon. The duodenum then makes a considerable curve to the left side, where it is involved in a cellular substance, which may be looked on as the common root of the mesentery and mesocolon, through the membrane of which it may be seen commonly, even in very fat bodies, without any dissection. In the concave left side of this curve, the thick extremity of the larger pancreas and the little pancreas are lodged; the superior mesenteric artery and vein coming through the notch between the larger and lesser pancreas hang loose before the gut here; and the *ductus communis cholidochus*, after passing behind the gut a little higher, unites commonly with the pancreatic duct, very little above the lowest part of the curve, and after passing obliquely through the

the coats of the gut, the two ducts open by one common orifice in the posterior part of the duodenum. After the curve just now described, the duodenum is involved in the root of the mesentery, and mounts obliquely within it towards the left side, with the *vena cava* behind it; and, after a course of about four inches there, rises forwards, to acquire a proper mesentery, or to commence jejunum, the membranes of the root of the mesentery seeming to make a ring at which the gut comes out, though they are really continued on the intestine, and form its external membranous coat.

That the duodenum may be all exposed to view, without changing its natural situation in a body lying supine, it is necessary to cut through the great arch of the colon below the bottom, of the stomach, and after turning the cut extremity of the left side over on the left short ribs, to take hold of the other extremity of the colon; and having separated it with a pair of scissars from the stomach and liver, taking away with it as much of the omentum and mesocolon as obstruct the view of the duodenum and pancreas, to lay it likewise on the right loin. When the colon is removed, observe where the roots of the mesentery and mesocolon cover the duodenum so much as to prevent your seeing its course; at such places cut these membranes with a very sharp scalpel, directing the incisions according to the length of the gut, and then cautiously separate the membranes to each side, till all the intestine is in view. Lastly, draw the small guts gently down, raise the liver, and suspend the fundus

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of the ſtomach as much as is neceſſary to allow a full view of the whole courſe of the duodenum.

Thoſe who have ever diſſected the human body, muſt be ſenſible how difficult it is to lay the duodenum of an adult all in view, without diſturbing its ſituation; and the taſk of keeping all the parts in the ſame fit poſture till a painter delineates them, is ſtill much greater: Therefore, though the preceeding deſcription is taken from the adult body, I chuſed to lay the body of a fœtus, which I had preſerved ſeveral years in acidulated ſpirit of wine, before Mr Cooper, to draw the picture from; and afterwards I compared this picture with ſeveral adult bodies, to make ſure of there being no eſſential difference.

In *Tab.* I. *Fig.* 1. are repreſented:

AA The liver, larger proportionally than in the adult, and raiſed ſo that its concave ſide is in view.

B The umbilical vein entering the liver.

This vein is commonly deſcribed and painted as paſſing to the *vena portarum*, without ſending off any branches: But, in all the human fœtuſes, or young children which I have diſſected, after their veſſels were injected, I always ſaw the umbilical vein giving off veſſels to the liver, in its paſſage through it towards the *vena portarum*.

C The gall-bladder full of bile, of a more pyriform ſhape than it is for ordinary in an adult.

D The

D The ſtomach diſtended with air.
E The remains of the omentum.
FF The extremities of the divided arch of the colon laid to each ſide.
G The pylorus where the duodenim begins, and the little omentum connects it to the liver. From this to H it is covered by the omentum. Between H and I this gut is lodged in the cellular ſubſtance of the meſocolon, thence to K it is covered by the common root of the meſocolon and meſentery. It runs involved in the meſentery to L, where there is an appearance of a ring; but, inſtead of being turned down afterwards, as here repreſented, becauſe of the guts being drawn ſo much down to have a full view of the duodenum, this gut makes the curvature delineated in fig. 2.
M The large pancreas with its duct, which is more hid by the ſtomach in an adult.
N The little pancreas with its duct.
O The meſeraic artery and vein cut as they paſs in the niche between the larger and leſſer pancreas.
P The *ductus communis cholidochus* appearing on the left ſide of the gut, where it is about to join the pancreatic.
Q The right kidney.
R The ſmall guts.

From the deſcription of the duodenum, it muſt appear, 1. That, ſince it is involved in the cellular fatty ſubſtance of the omentum, meſocolon, and meſentery, without having the firm external membrane br[illegible]ed upon it as the other guts have; it muſt therefore more eaſily yield to any diſtending force: And, having the whole ſubſtances thrown into the ſtomach with the bile and pancreatic juice poured into it, it muſt receive more than any other inteſtine; and then whatever enters it muſt go out with ſome difficulty, becauſe its extremity next to the jejunum is fixed in a courſe almoſt perpendicular upwards. So that, upon the whole, it is no wonder that this inteſtine is frequently found of ſo much larger diameter than the other guts, as to be called *ventriculus ſuccenturiatus*, by ſeveral authors.

2. The aſcending courſe of the extremity of this gut, and the influx of the bile and pancreatic liquor into the moſt depending part of it, where the food muſt make the longeſt ſtop, are wiſely contrived, both for the more eaſy influx of theſe liquors, and for a ſufficient quantity of them being mixed with the food, to perform well the neceſſary offices for which they are deſigned in digeſtion.

3. A pendulous inteſtine here would, in our erect poſture, have drawn the ſtomach out of its due ſituation, and might have twiſted or overſtretched the biliary and pancreatic ducts, ſo as to have ſtopped the courſe of the liquors in them; and therefore it is ſo firmly tied down in its whole courſe, that it cannot change its ſituation.

The duodenum of brutes is likewiſe placed in ſuch a manner as to anſwer the ſame uſeful purpoſes, though in many of them this gut would appear to one who does not conſider the different

Vol. IV

different postures and way of life of animals, to be situated in an opposite manner to the human body. To shew how general this contrivance is, I shall cursorily mention a few examples.

Apes, whose posture is for most part erect, or near so, have these parts disposed in near the same way that man has.

In dogs, cats, cows, sheep, and most other quadrupedes whose posture is horizontal, the pylorus and beginning of the duodenum are firmly connected to the liver; after which a considerable piece of gut, with a mesentery, hangs pendulous, and then the gut is fastened to the loins and back-bone; therefore the pendulous part must be lowest in them. The biliary duct opens into the duodenum where it is tied to the liver. The pancreas is long, and lodged in the mesentery along the pendulous gut, and its duct is near the middle of that gland.

Hens, ducks, geese, and other fowls, whose posture of body is neither erect nor horizontal, but oblique, have the beginning of the first small gut well secured to the liver, from which the gut runs near to the podex, and returns again to near the same place where it began at, to be again tied to the liver; all between these two connections being pendulous. The pancreas is fixed between these pendulous parts, and its ducts open into the part of the gut where it had returned back to the liver, as the biliary ducts also do, but with a direction opposite to the course of the aliment in the gut. After the entry of these ducts, the intestine runs a good way along the concave part of the liver towards the membranous diaphragm, being fixed to the liver and to the air-membrane which lines the abdomen. At length the gut makes a curve to the right side, and takes its course towards the podex.

In cod, haddocks, and such fish, the cæcula, which are esteemed analogous to the pancreas of other animals, surround the first gut, soon after it comes from the stomach, and then the intestine is fastened to the liver, where the biliary ducts open into it.

I designedly here leave the reader to follow out the same contrivances in this comparative anatomy as were mentioned in the human.

If then we consider what quantities of different substances enter the duodenum, some of which have rough hard parts, to rub violently on its sides; others have sharp saline particles, to prick and corrode; others are irritating rancid oils; others, in their digestion, generate great quantities of distending air; others, by their moisture and watery nature, are apt to relax the fibres; others harden these fibres too much: If, I say, we consider this, we need not be surprised that this gut, the duodenum, should be more subject to maladies than any of the rest; especially that it has not such a firm covering of an external membrane, to assist it to resist the forces overstretching its fibres, as the other guts have. And if the gut itself suffers, how soon must it disturb many other functions of the animal œconomy, by the pressure it may make, when overstretched, upon so many large vessels, nerves, and other organs, to to which it is infeparably contiguous, or by communicating its afflictions to thofe parts which fympathize with it, by means of the common origin of their nerves, or by ftopping the influx of thefe two abfolutely neceffary liquors, the bile and pancreatic juice.

Any tolerable account of difeafes, from fuch caufes as I have juft now mentioned, would lead me far beyond the bounds of any effay you could admit of; and therefore I fhall do no more than, with Santorini *, recommend to practifers to have fuch in their view, when they treat patients who labour under difeafes of the epigaftric or hypochondriac regions, that they may not confound fuch which have their feat in this gut with others which require very different treatment. This is the more neceffary, becaufe authors generally take little or no notice of the duodenum as the feat or caufe of any particular difeafe.

XII. *Mifcellaneous Remarks on the* Inteftines; *by the* fame.

THE preceeding account of the duodenum has called to my remembrance fome remarks that are commonly neglected, which I think may be made on the other chylpoetic vifcera. If you are of opinion they deferve a place in your collection, you may tack them to the defcription of the duodenum.

The inteftines are generally defcribed as being covered all over with an external membranous



* Obferv. anatom. cap. 9. § 7.

mous coat which is ſaid to be derived from the peritonæum. But it ought to be remarked, that a conſiderable part of the circumference of the human inteſtines is not covered with ſuch a membrane: For the meſentery being formed by the peritonæum produced on each ſide, and including a conſiderable quantity of cellular ſubſtance, in which much fat is frequently contained, together with the numerous *glandulæ vagæ*, the large meſeraic arteries and veins, and the nerves and lacteals, and the fatty cellular ſubſtance being thick, till after the membrane of each ſide is continued ſome way upon the gut, all the ſpace there between the membranes, which is always more than the diſtance between the inſertion of the arteries that riſe on each ſide of the gut, that is, a fourth at leaſt of the circumference of the gut, muſt have no ſuch firm membranous coat covering it; and therefore will more eaſily yield to any ſtretching force, which may be of uſe, in allowing the guts to be more enlarged than otherwiſe they could be, without overſtretching their veſſels.

It is in this ſpace between the inſertion of the blood-veſſels that the longitudinal fibres of the guts can be ſeen moſt diſtinctly and eaſily; becauſe the cellular ſubſtance is ſeparated with little trouble, or collapſes ſo much as not to hinder the view of the muſcular fibres under it; whereas, in the part of the inteſtine oppoſite to the meſentery, the external membranous coat, whoſe fibres are very like to thoſe carnous ones, adheres ſo firmly to them, that it

cannot

cannot easily be distinguished or separated from them.

I need scarce observe, that the circular fibres of the guts are often so interlaced, that one can scarce trace the same muscular fibre round the whole gut.

It is now generally enough known, that all the substance between the interior side of the circular muscular fibres of the guts and the villous coat, which used to be divided into the vascular, nervous, and glandular coats, can, by blowing into a piece of gut whose interior side is turned out, be raised into a *tunica cellularis*, with numerous vessels running every where through it, without any fat in it, where, however, it would appear there is some secretion performed; for, by injecting water into the arteries, these cells are filled with it; and frequently, after making injections of grosser coloured liquors, I see a secretion performed, by the cells being here and there distended with the white injected substance, while the colouring powder does not pass with it, but is left behind. When this secreted liquor hardens, it forms a number of small round or oblong tubercles, which I have seen several willing to imagine were Peyer's glands filled with the injection; and this Ruysch * seemed likewise to think. I can't yet bring myself to that opinion, because water diffuses itself so equally every where, and the injections which harden do frequently the same for a large space; at least, these make it evident, that if Peyer's glands are sometimes injected, there are also vessels

* Epistol. xr. respons. & sparsim.

veſſels which convey liquors into this internal cellular membrane; which may lead us into a more reaſonable account of the very great diſcharges of mucus after an excoriation of the guts, and of the large extended hard tubercles that are frequently ſeen within their muſcular coats, and of ſeveral other phænomena of diſeaſes, than otherwiſe we could give.

In a piece of gut diſtended with air in the manner mentioned for demonſtrating its internal cellular coat, we ſee the villous coat in its membranous form, and without the downy, papillous, and mamillary appearance, which an inverted undiſtended inteſtine floating in water has.

Upon obſerving this villous membrane when it is ſtretched, remarking how thin and flexible the cuticula becomes upon the lips, with the continuation of the ſame membrane in the mouth, tongue, fauces, œſophagus, ſtomach, and inteſtinal canal; and, upon comparing the properties which the moſt external common covering of the body has, with thoſe of this villous membrane, we muſt be convinced they are very much of the ſame kind, if not the ſame continued ſubſtance. I do not know that all the properties of the cuticle have been conſidered by the authors who deſcribe it, far leſs that a compariſon has been made between it and any of the internal membranes, to ſhew their analogous uſes and advantages; therefore beg to be allowed to conſider them a little in this way.

The cuticula is notourly pervious to liquors going out of the body, and to others coming into

into it; so is the villous coat of the intestines; and both of them have other passages through them, whereby they allow certain substances to penetrate to the nerves which they cover. Thus a numbness is brought on the skin by immersing any part of the body in several sorts of liquors, and pain is raised by substances which do not destroy the cuticle; thus sapid objects affect our tongue, and the different sensations arise which we frequently feel from the contents of the stomach and intestines.

The external epidermis, by being exposed to a variety of different forces acting upon it, is of very different thickness and firmness in several parts; but it is naturally formed so flexible, as to allow a sufficient impression of tangible objects on the nerves below it. The internal membrane of the guts is less exposed to a variety of such causes, and therefore is more uniform; but can be changed in the same manner by like causes: And hence frequently we find the interior surface of the stomach and guts of a callous hardness, and almost insensible, while for ordinary it is very sensible, and so flexible, that, by being connected to such a loose cellular substance as already described, it hangs floating, and assumes any shape the contractile fibres of these cells give it, whether of large rugæ, called *valves*, or of smaller papillæ of different forms.

Thin watery saline liquors wash away the cuticula; thick mucaginous substances protect it against them and the bad effects of friction: Therefore, where-ever the cuticula is exposed expofed to fuch injuries, its defence is likewife provided. Thus the eye-lids are defended againft the tears and their mutual collifion, by the febaceous matter feparated in their glands; the nipples, arm-pits, glans, urethra, perinæum, *&c.* are all protected in the fame way. When their defence is wanting, we fee the troublefome confequences, excoriation, pain, inflammation, *&c.* The internal membrane of the guts, being more expofed to the action of watery liquors, has a much more plentiful fupply of the protecting liquors, and is always, in a found ftate, lined over with mucus. Whenever therefore this mucus is carried off too quickly, as in diarrhoeæ and dyfenteries, or is not fecerned in fufficient quantity, as in inflammations, or other obftructions of the inteftinal veffels, we may eafily judge what the confequences muft be, and are led to fupply by art what nature then is deprived of.

A certain moderate degree of friction makes little or no change upon the cuticula, nature eafily fupplying what is carried off. When it is greater, but gradual, and not fo violent as to deftroy its texture, or to feparate it from the parts it is connected to, the effect is not a little furprifing; the cuticula becomes thicker, ftronger, and firmer, as we fee every day in the foles of the feet, and in the hands of labouring people. When fudden violent friction is applied to the cuticula, either it is rubbed imperceptibly off, or it is feparated from the fkin: This daily experience alfo fhews every one. The villous coat of the inteftines is not expofed to fuch accidents as the external

nal ſurface of the body, and is better defended by the ſlime from the bad effects of rubbing forces, while the abraſion or ſeparation of this villous coat may well paſs ſo unobſerved, that one cannot give examples to prove circumſtances in it analogous to thoſe mentioned in the cuticula. The moſt ſurpriſing, and leaſt to be accounted for phænomenon, to wit, the thickening and hardening of the cuticula by friction, may however be alſo ſeen in the inteſtines, when any hard concreted ſubſtance is lodged a conſiderable time in any particular part of the guts; for then the internal ſurface of the inteſtines becomes there thick and hard.

The epidermis ſeems to ſerve for contracting the extremities of the cutaneous veſſels, probably by forming their extremities; for, whenever it is ſeparated, theſe veſſels throw out their liquors in much larger quantities than ordinary. I know the writers on this ſubject generally expreſs themſelves in ſuch words as would perſuade their readers they thought the cutaneous liquors were all thrown out of their veſſels between the ſkin and cuticula, and thence gradually eſcaped through the interſtices of the cuticular ſcales. But, if this was the caſe, there would perpetual bliſters be raiſed in the depending parts of the body, if not all over the ſurface of it; and the liquor in bliſters would eſcape through theſe interſtices; which it does not. The ſame effuſion of liquors is made into the inteſtines, upon the ſeparation of the villous coat, in the end of conſumptions, and in other diſeaſes, where the

the tongue and throat ſhew the excoriated ſtate of the alimentary canal.

When any part of the cuticula is ſeparated from the ſkin, but ſtill is continued with the adherent ſcarf-ſkin, it becomes thicker, eſpecially if ſoaked with liquors: Thus the cuticle of bliſters, and what ſeparates from the edges of wounds and ulcers, is frequently very thick. The ſame thing happens in the alimentary tube, as is evident in aphthæ: And this obſervation only can account for the tubular thick ſubſtances frequently voided at the anus; which have been taken for pieces of the guts, becauſe of their ſhape and firmneſs.

The epidermis is the moſt incorruptible and leaſt ſubject to eroſion of any part of the body. In abſceſſes, the pus has little other effect upon it, than to ſeparate it from the ſkin, and to tear it by its weight, but not to diſſolve it. In gangrenes and ſphaceli, it remains uncorrupted, after all that it covers is converted into a putrid maſh; nay, it can allow the common *lapis ſepticus* to penetrate through it, and deſtroy the parts below, without ſuffering a ſolution of union in its own ſubſtance. Poſſibly this may be owing to its having no proper veſſels or liquors. Whatever the reaſon of it is, it is certain the *tunica villoſa* of the inteſtines enjoys the ſame properties, which are of the utmoſt advantage in both, conſidering how many ſubſtances of different natures are applied to them; ſome of which would certainly diſſolve them, if they were capable of diſſolution, and would expoſe us to the inconveniences cies of pain, inflammation, effuſion of liquors, &c.

Becauſe theſe membranes of which I have treated, though indiſſolvable, are however ſeparable from the parts they cover, the conſequences of which ſeparation are ſo bad, therefore they are the moſt eaſily and quickly regenerated of any organs in the body that are not of the ſame ſtructure.

Whoever calls to remembrance the analogous ſtructure and uſes of the moſt internal coat of all the hollow viſcera, of the arteries, veins, &c. will ſee that I point at concluding all of them of the ſame nature with thoſe I have now inſiſted on.

I never ſaw the *appendix vermiformis*, of any of the human fœtuſes which I have diſſected, diſtended with meconium; and therefore cannot allow it to ſerve as a reſervoir of the fœces during geſtation, which ſeveral authors have imagined; but muſt join with thoſe who aſſign it the uſe of furniſhing mucus to lubricate the internal ſurface of the great ſac of the colon, and to moiſten the fœces in it, that they may more eaſily be puſhed forward out of this part of the gut where there is the greateſt difficulty in their progreſs, and where, by ſtagnating too long, they may bring on troubleſome ſymptoms; witneſs the diſeaſe called *placenta inteſtinalis* in women with child, which I have ſeen more than once in hazard of being miſtaken for ſome other diſeaſe, that required a very oppoſite method of cure to what ought to have been uſed. The numerous mucous lacunæ obſervable in the human appendix, and the like ſtructure in the cæca of brutes, are proof enough of the appendix ſerving the uſe mentioned, both in the human fœtus and adults.

It will be ſaid, that the appendix being ſo much proportionally larger in the fœtus than the adult, ſeems to indicate ſome other uſe it is alſo of to the fœtus. But this proportional leſſer ſize of this little gut in an adult will appear to depend upon the preſſure it ſuffers, and being emptied ſo frequently of its contents; whereas in a fœtus there is no reſpiration to ſqueeze it, and the meconium in the ſac of the colon prevents its being emptied; ſo that the liquor ſeparated by its glands being collected there, ſoftens and relaxes its fibres, and diſtends it.

The neglect of conſidering what the different forces are, which act upon the ſeveral organs of the body, while in a fœtus-ſtate and after birth, has, in my opinion, contributed to many diſputes, which might eaſily have been put an end to, by accounting for the phænomena, which were the ſubject of them, in this way of reaſoning. I ſhall mention one remarkable difference in the circulation of the blood, and ſome few conſequences from it.

Though the heart and arteries of animals are able, by their action, to keep up a circulation in the larger veſſels, yet, without aſſiſtance from ſome other powers, they cannot propel the liquors with velocity enough, and in in ſufficient quantity, through the ſmall veſſels. Theſe aſſiſting powers, after birth, are the alternate preſſure of reſpiration, and the actions of the muſcles. We can obſerve at any time how much the circulation is quickned, by increaſing theſe; and, on the contrary, how much all the ſecerning organs are infarcted and ſtretched by their almoſt ſtagnating fluids, whenever one of them, muſcular motion, to wit, is little exerciſed. Thus creatures turn fat when they have not exerciſe. Hence a recruit of all the neceſſary liquors in time of ſleep. Hence the ſtrong ſlow pulſe of ſleeping people. Hence the deſire of continuing ſleep, after a perſon has ſlept beyond his ordinary time. Hence the ſmall waſte of ſuch creatures that continue long in a dormant condition, without any ſupply of food. Hence a dry parched mouth in the morning, which is ſoon relieved by chewing. Hence a ſtiffneſs and lazineſs after abſtaining from exerciſe too long. And a great many other phænomena, which will occur to any upon the leaſt reflection.

Since then the heart and arteries of fœtuſes have little or no aſſiſtance from any alternate preſſure, in propelling their liquors, their ſecerning organs, (where, of all the parts of the body, there is the greateſt complication of veſſels, divided into the ſmalleſt ramifications, with the leaſt firmneſs, and conſequently reſiſtance, in their ſolids), muſt all be infarcted and diſtended, and therefore of a larger proportional ſize than in the adult, as we ſee their brains, liver, pancreas, kidneys, breaſts, &c.

*&c.* are. So that the thymus and *glandulæ renales*, about which so much inquiry has been made, have so far nothing but what is common to other glandular parts.

It probably will be alledged, that the thymus and *glandulæ renales* lose more of their proportional size in the adult than the other organs named: Admitting this generally received fact, though, upon comparing them with the brain, and some others, I doubt it ought not to be admitted as commonly stated; admitting, I say, this fact, it will not bring us under any necessity of being obliged to search out some particular use they serve in the fœtus: For a view of their circumstances, as to situation and pressure will account for all the differences observable in them. To understand this aright, it may not be amiss previously to consider one or two causes that may influence the growth of animal organs.

1. Then, it will not, I believe, be denied, that the growth of the parts of the body will be greatest where they are least confined, and least exposed to pressing forces. The brain is at first inclosed in membranes, and is prodigiously large, in proportion to the other members; as the bones of the scull become firm, its proportional size diminishes; and, after they are fully joined, its proportional increase is very little. The testicles, on the contrary, are at first confined within the abdomen, and very small; afterwards, when they fall down into the loose bag, the scrotum, they increase much faster.

2. The greater the force is with which our fluids

fluids are thrown into parts, or the greater the resistance is to the secerned, or to the returning liquors, whether that resistance is owing to external pressure, smallness, or unfavourable situation of the vessels, or to the viscidity of the fluid, *cæteris paribus*, the bulk of a part will be increased. A hand swells, upon pressing the veins of the arm. A tumor in the urethra, near the *caput gallinaginis*, occasions a swelling of the testicles.

To apply these principles to the thymus and *glandulæ renales*, we need only call to our remembrance the situation of the one, in the double mediastinum, between the heart with its large vessels, and the sternum, with the lungs on each side. The other lies on the muscular appendix of the diaphragm, covered before by the chylopoietic organs. Neither of them have any exc[illegible]on for [illegible]nal, except the lymphatic vessels are esteemed such. The veins of both have a short course, and that of the *glandulæ renales* is remarkably large.

The greater pressure which the thymus suffers after birth, from the increased action of the heart and of the lungs, is altogether evident. The immediate play of the diaphragm upon the other shews as evidently that there are scarce any glandular parts in the body, the change upon which, as to pressure, is greater after birth, to what it was before, than in these two; and therefore, by our first proposition, they should suffer in their growth upon this account. But to this is to be added the thinness of the fluids sent from them, and their short course in large vessels, which are

 almost

almoſt peculiar to them; by which their veſſels muſt be leſs diſtended, and conſequently their increaſe leſs, by propoſition 2.

Having endeavoured thus to account for the leſſer proportional ſize of theſe organs in the adult, I would join in opinion with thoſe gentlemen, who aſſign them the office of lymphatic glands, both before and after birth, ſerving to dilute the chyle and thick blood, that is ſoon after to be returned to the heart.

Having thus exerciſed the common privilege miſcellany writers aſſume to themſelves, of digreſſing at pleaſure, I chuſe to conclude with a ſubject ſomething nearer to what I began with.

At the part of the meſocolon which connects the ſigmo[illegible] flexure of the colon near the left cavity of th[illegible] where in moſt adult bodies there is [illegible]iliform *cul de ſac*, or thimble-like cavity, I could never obſerve in children more than one part of the meſocolon laid over the other, becauſe of the great flexure of the gut at this place; and therefore conclude the cavity to be accidentally formed, by the growing together of the contiguous parts of the meſocolon: And that it is as needleſs to aſſign uſes to it, as it would be to ſhew how uſeful the concretion of the lungs and pleura is, which ſeldom miſſes to be obſerved greater or leſs in adult bodies.

XIII. *Ob-*

XIII. *Obſervations concerning the* Placenta, *the two Cavities of the* Uterus, *and* Ruyſch's *Muſcle* in fundo Uteri; *by Dr* THOMAS SIMSON, Chandois *Porfeſſor of Medicine at* St. Andrew's: *In a letter to Dr* John Pringle, *Phyſician, and Profeſſor of Ethics in the Univerſity of* Edinburgh.

SIR,

YOU know I had ſome ſuch a reſolution in publiſhing a new edition of my treatiſe of the uterus, as to make public advertiſement of it. But, upon a fuller conſideration of that part which related to the practice, finding it deficient in ſeveral material points, which I have ſome hopes of making more compleat from further obſervations, I perſuaded myſelf to delay the publication for ſome time. But, that I might make ſome atonement for my delay, reſolved to lay before the ſociety with you for collecting Medical Eſſays, ſome of the improvements I had made in the theoretical or anatomical part, reckoning that ſuch things cannot be too ſoon publiſhed, to give opportunity to ſuch as have a variety of ſubjects, to examine into what is alledged; which is the more neceſſary as to my doctrine, becauſe the ſubjects that can be ſerviceable for this end are the worſt of all to be had, and in a ſmall proportion to thoſe that in general are ſubſervient to anatomy: I was obliged in what I have advanced, to have recourſe to the ſtanding obſervations of anatomiſts, many of which had hitherto

hitherto never been employed to ſolve any queſtions relating to the animal œconomy; and I hope the way that I have collated them, and the concluſions I have drawn from them, when conſidered conjunctly, ſhall ſhew the uſefulneſs of inſiſting upon particular obſervations, in order to the making out of ſyſtems. Except we take this way, all our theories ſhall be mere hypotheſes: But, by full collections of obſervations, we may come to ſolve the moſt intricate problems.

The example I am to give you, is in finding out the true riſe of the placenta, and the deſign of dividing the uterus into two cavities, which we find the antients did not ſo much as attend to; at leaſt Fallopius ſeems to have been the firſt who deſigned them by different names; and, by ſo doing, has given occaſion to the later anatomiſts to make a great many particular obſervations as to their ſtructure, and to me, if I miſtake not, of their uſe. It was the variation that is obſervable in the poſition of the umbilical cord that gave the firſt riſe to my ſcheme, upon which I built the moſt of my theory upon this head in my ſyſtem: But ſince, a great many other arguments have ariſen to me from different topics, eſpecially from comparative anatomy, as you will find in the ſequel.

It has been hitherto generally received, that the placenta is an original part among the ſecundines. But to me, from collating obſervations, it ſeems to have no place in the ovarium, nor in the uterus, till once the ovum becomes contiguous to the fundus; at which time [illegible] every part contiguous becomes really placenta, which is the whole of the chorion, except that ſmall portion that lies contiguous to the cervix: So that, according to my doctrine, at firſt the placenta involves the whole embryo, except ſo much as is contiguous to the paſſage from the fundus to the cervix, where ſometimes one part and ſometimes another of the ovum happens to fix; and conſequently ſometimes one part and ſometimes another is placenta, which is vaſtly larger than the membranous part at its firſt appearance. Such a phænomenon as this could not have eſcaped the attention of the anatomical inveſtigator, if he had not been prejudiced with the notion, that the placenta, at its firſt appearance, ſhould have ſomewhat of that proportion to the other parts of the ſecundines which it has at the birth. The celebrated Ruyſch ſeems ſo prejudiced with this notion that he could not believe his own eyes, that the placenta was ſo large at its firſt appearance, as he found in every caſe, except one; and therefore forced himſelf into the conceit, that what then appeared as placenta round the whole embryo, was nothing but a coat of blood coagulated round it; though he is ſo juſt as to own, that this blood was ſo involved with the placenta, that he could not ſeparate them without violence to the placenta *. Dr Harvey's account of the invelopements of the embryo was leſs diſguiſed, who tells us, That their external ſurface was all over wrinkled, and daubed over with a kind of gelly

* Obſerv. decas 2. Obſerv. 10. theſ. 6. num. 40.

gelly or glutinous substance, without any appearance of the after burden *; that is, of an after-burden distinguished from the other integuments, as in the last months. And indeed, amongst most of our more curious observators, we find a like indistinctness in their account of the invelopements at that time. I shall, in Zanottus's words, give an instance in a history from Galeatius, as Zanottus has given it, where he treats *de institut. et Academia Bononiensi.* "Dis-
" secta muliere, quam, secundo circiter gravidi-
" tatis mense, febris oppresserat, uterum cum o-
" variis & tubis continuo Galeatius extraxit,
" scire cupiens, quemadmodum hæc haberent.
" Uterus siccerat. Placenta ejus fundo satis
" firmiter adnectebatur, quanquam adhuc erat
" informis; nihil, quod fœtum profiteretur,
" apparuit; tantum sacculus se prodidit car-
" neæ cuidam [illegible] quam vix a placenta
" distinguisses, adhærens, apertumne dicam
" per longum an laceratum? Id Galeatio
" aliisque cum eo idem contuentibus suspi-
" cionem injecit, vel mulierem nihil perfectum
" concepisse, vel fœtum, si quem conceperat, an-
" te ejus obitum ex illo claustro excessisse." The best commentary to this history is a case I had occasion to examine, when occasionally at Coventry, where a lady of my acquaintance miscarried about the third month, with a great effusion of the lochia, which gave me reason to suspect, that the after-burden was some time loose in the uterus before it was discharged: The whole conception was about the bigness

* De generat. differt. 69.

ſize of a gooſe-egg, and uniform through its external ſurface, which had ſomewhat the appearance of a gelatinous ſubſtance, as Harvey obſerves. But, ſcraping upon this, I found it much of the ſame fibrous thick texture as the after-burden in the laſt months, till I came as far as the chorion, from whence I ſcraped the fibrous part all round, ſo as to leave it a pure clean membrane, ſuch as what Ruyſch mentions as the only inſtance where he did not find the placenta involved with blood; which, I think, gives juſt ground to ſuſpect, that the fibrous part had been ſome how or other torn off, which is not difficult to do in that tender ſtate. When I came to the membranous part, I ſaw diſtinctly the fibrous part every way inſerted into it; nor did I, through the whole circumference, find the leaſt difference as to the manner the fibrous part was attached to the membranous, ſo as to ſuſpect one part for placenta more than another: Only at one part I found a ſmall ſlit that led into the membranous bag into which the ſhoulders of an apoſteme lancet would have had difficulty to have entered. When I opened the bag fully, there was nothing in it of fœtus or humour; only to one part I found hanging about an inch of the umbilical cord: But, though I ſhewed this to the midwife and other by ſtanders, and argued from it, that there had certainly been a fœtus there, but that it had been ſqueezed in the time of labour, through the obſervable ſlit, I could not prevent its paſſing abroad, that the lady had been with a falſe conception; the whole appearance and the want of an embryo ſatisfying

fying them upon that head: But to me it gave a very diftinct notion of Galeatius's cafe, of his facculus, and adhering flefhy fubftance, which he could not diftinguifh from the placenta, and that the embryo had certainly been fqueezed, as in my cafe, through the aperture found in the conception. How thefe apertures happen, fo as to allow the fmall embryo to pafs them (which we may fuppofe to take place frequently, fince Ruyfch tells us the embryo is frequently amiffing, *Thef.* vi. *num.* 81.) may eafily be underftood upon my hypothefis, that the part of the fecundines next to the cervix, where they are not contiguous to the fundus, keeps always membranous, and has nothing to fupport it; fo that it muft eafily yield, and being deftroyed, the part that is covered muft appear only as a flit: But this will be more manifeft in the fequel of our treatife. Heifter, in the fourth edition of his anatomy, has given us the moft diftinct account of this affair, as you will find in the 27th figure, where he has elegantly reprefented a fœtus about three months, included in its integuments: In his explanation of this figure, he tells us, that the conception was almoft quite furrounded with fuch a fet of vafcular fibres, as he has reprefented in the lateral and under margin of his figure; but that he had fcraped fo much of them off, as to let us have a view of a fœtus through the membranous part. This conception of Heifter's feems to have every thing in its natural ftate; whereas, for moft part, thefe early conceptions are more difguifed, by lying longer in the uterus, or being more fqueezed when difcharged;

ged; and thus they are found frequently without the embryo, and having the fibrous excrescences obliterated: So that Ruysch could take them for clotted blood; and to Harvey they appeared like so much gelley: Which was the external appearance of the instance I mentioned; though, as I said, in scraping into it, the fibrous or vascular part was most distinct; De Graaf, towards the end of his 15th chapter, speaks of the same massy appearance of the external integuments of the more early embryo's, as what is ordinarily to be observed, and looked upon it as preternatural; while he tells us, that others reckoned them to be mola's: Which, according to la Motte's opinion, has no membranous cover, has no embryo, nor any think like an after burden †. I suppose, from the histories I have given, and the light I have set them in, the ground of their mistakes, and the definition of la Motte's, may be easily seen into. All these cases are mola's with la Motte, where the small membranous part is torn, and the water and embryo discharged; which happens most frequently. And I mistake it, if this has not given ground to the numbers of mola's we have recorded amongst observators, who are very copious upon this subject, and has given occasion of their being distinctly treated of by every writer almost in midwifery, who have given us very strange accounts of them; such as have frequently distracted the poor woman, and put her to much trouble and expence: While the



† Des accouchements, liv. 1 obs. 3.

the practitioner took his indication from such an appearance, to put her under a severe regimen and course of drugs, to purge the uterus, from whose bad state they deduce the rise of the mola.

As we have the concurring testimony of almost all observators as to the surrounding of the embryo in the two or three first months with the placenta, though somewhat disguised: So, after this, it is agreed upon, that the placenta has still a lesser proportion to the membranous part of the integuments, the nearer they come to the ninth month. Thus Blasius, in the first of his anatomical observations, takes notice, that the placenta covered the whole of the embryo in the fourth month, excepting a space that could be covered with a crown. In this the placenta had much the appearance as in Heister's figure, according as Blasius describes it; and the extent of the placenta, and its proportion to the membranous part, is agreeable to Ruysch's representations. I had occasion lately to see a miscarriage of twins, the first of which came away without its integuments, and the other with them, all entire, in the midst of the waters, alive, though but of five months, as I guessed from their bigness, and as the mother reckoned in her usual way of computation. In both, the placenta was much of the same extent with the membranous part, and not much inferior to what I usually find it at full growth in the ninth month: So that, for the four last months, the growth of the placenta is very obscure, but that of the membranous part very considerable, being at least five

five times larger than the placenta at that time, as may be reckoned from the bulk of the infant, and waters it furrounds; which is the only way we have to judge of them in the laſt months, when the integuments ſeldom come away entire.

When we look into theſe hiſtories then, every where to be met with, which ſhew us that the embryo is wholly ſurrounded with the placenta in the firſt months, we cannot doubt of what I aſſerted at the beginning of this chapter, that the whole exterior ſurface of the ovum, that at firſt is contiguous to the cavity of the fundus, is in fact placenta: But, this being a new doctrine that will require to have our arguments ſet in different lights, to convince the prejudiced mind, I ſhall conſider the riſe of the placenta in other animals; for I find in this caſe, as in a great many other inſtances, comparative anatomy contributes a great deal to form our judgment.

In animals, where the ovum has its ſupplies by mediation of cotyledons, which are original parts in the uterus or its horns *, nothing can be more evident than that ſuch parts of the chorion, or exterior ſurface of the ovum, becomes caruncle or placenta, as comes in contact with the cotyledons, and theſe only: For example, in the hind, in whoſe uterus Harvey takes notice of 10 cotyledons, 5 on each ſide, there are exactly ſo many caruncles upon the chorion, and of the ſame extent and figure: And in ſheep and cows, which we have ocH 2 caſion

* Ad finem theſ, 5. articul. 5. Ruyſch.

casion to see every day, and round whose chorion Needham has sometimes found 60, 70, 80 cotyledons, the caruncles always correspond in number and shape; and that even in cows, who at former calvings had lost many of their original cotyledons, which they frequently do: Than which nothing can give us greater evidence that the caruncle is produced upon the chorion by the influence of the cotyledon. But further, in mares and swine, all the time the ovum keeps disengaged from the uterus, which it does for more than half the time it is there, nothing fibrous or like a caruncle appears upon the chorion; nor do the cotyledons appear; but, upon the conception coming in contact with the uterus, the cotyledons appear, though but small, and caruncles answering to them *. The same thing holds in those animals, who have particular cells alongst the horns for receiving and nourishing the ovum; such as rats and mice, in which the ovum is joined to the cell by mediation of one cotyledon; upon their invelopements there is but one caruncle exactly shaped like the cotyledon: But in dogs and cats, and such like, where there is a particular cell fitted for the ovum, without the mediation of a cotyledon, the placenta is exactly of the shape of the cell, which is a cylindrical portion of a cylindrical tube; and, the cell being open at both ends, the membranous part extends itself to them: So that in these creatures the placenta appears as a belt round the chorion. And as in the separation of the human placenta,

* Needham embryotomia.

centa, blood always appears; so it does in them upon that occasion, but not in animals with cotyledons, except the cotyledons be brought away with the caruncles, as I have seen frequently in cows: So that there is no doubt but the cells, in which the ovum fixes without cotyledons, are prepared for the engraftment of the ovum, in the same manner as is the fundus of the human uterus; since, as in them, all becomes placenta, contiguous to the cells, and that part membranous which extends beyond them. And thus we find in all animals who as yet have come under our consideration, that it is manifest that the fibrous part of the secundines or placenta is owing to the soil; and that nature has been solicitous to have a proper soil for such a production in every animal, though she has taken a different way to prepare it.

But though what I have now advanced gives the strongest evidence for this doctrine; yet there are other material considerations which add to the beauty and force of our arguments: For this is the peculiar excellency of truth, that, upon whatever side you view it, it appears uniform and of a piece; and, the more it is examined, it appears with the greater simplicity and symmetry. By this doctrine of ours, by which we determine the whole of the ovum to become placenta which is contiguous at first to the cavity we call the *fundus uteri*, it of necessity follows, that, when two or more ova arrive at once at that cavity, that all their placenta's will be confined to it, and consequently amongst them will only take up the space ordinarily poſſeſſed by one; and all of them being contiguous and taken together, will be of the ſhape that the ſingle placenta has. This is a neceſſary conſequence of what we have formerly advanced; and I refer to all practitioners in midwifery, if, in births where there are more than one at a time, they do not find either all the placenta's together, or certain marks of their having been contiguous in the form I aſſerted; only that between them they make a larger placenta than what we find in caſe of a ſingle birth: For every part concerned, when twins are produced, being extended more than in ſingle births, it is but juſt to allow that the fundus, with the placenta or placenta's, is ſo likewiſe. In the caſe of twins I mentioned before, where I was preſent after the miſcarriage, I obſerved the place where the two had been contiguous, as evidently as we ſee the correſponding ſides of two kernels that are contiguous in one ſhell: Nay further, I evidently ſaw marks of their being torn away from one another by violence, the mutual parts of contact in both being evidently torn. In others I conſidered this in, when the infants had come to their full time, though I ſaw not ſo much the marks of violence, yet I eaſily diſcerned the ſides of the placenta's that had been contiguous: But inſtances, where they come away ſeparate, are not ſo frequent as thoſe where they come away ſtill united, as Deventer has obſerved in his chapter *de geminis prave verſis*, and every one may obſerve. But it is ſtill more curious to obſerve how this holds where there are three or four children at a birth,

birth, of which we want not examples. Thus la Motte, in the 42d chapter of his book we formerly cited, has two inſtances of three at a birth; in one of which the placenta's were all joined together as one; and in the ſecond, two were joined; and the third came away by itſelf. Mr Saviard, in his *Nouveau recueil d'obſervations, num.* 82. gives us two examples exactly parallel. Vieuſſens, in his treatiſe upon the uterus, tells us, that Meſſrs. Codonian and Olavier ſhewed him an inſtance where the three were in one, though their boundaries were moſt diſtinct. But, what is ſtill more obſervable, Luca Shræckius gives us a diſtinct account, in the *Miſcell. n. curioſ. dec.* 11. *ann.* 2. *obſerv.* 9. *p.* 26. of four produced at a birth by the wife of one James Thomas, all of whoſe placenta's were combined as one; though he takes notice, that they were ſeparated by certain furrows into four parts, each of which had an umbilical cord inſert into it, which is always the caſe when different placenta's are thus combined. Now, what account can be given of ſuch a phænomenon as this, from the common hypotheſis, that the placenta is an original part in the ſecundines, and is fixed only to a certain place of the fundus? What ſtrange chance is it, that the four placenta's ſhould meet ſo neatly, as to have the appearance of one ſingle placenta? If we ſhould ſuppoſe but two, it would, even in that caſe, be a great chance if they ſhould meet, becauſe the membranous part is ſuppoſed greater than the fibrous; and then, if they did meet, it ſhould only be as two circles in a point; whereas, in fact, they are joined the whole

whole length of one of their diameters. So that the whole circumftances argue for what I advance, that the *fundus uteri* is of fuch a virtue, that every part of the ovum that lies next it becomes placenta; and, if at once there are received two, three, or four ova, then fo much of every one as touches the *fundus* becomes placenta; and of neceffity all the placenta's muft be contiguous, they filling up the cavity of the fundus exactly amongft them: For, whether there are more or fewer, they are confined here, having their exiftence from it. And as, by the light I have fet the placenta in, we fee how the placenta's are always fo crowded together, when there are more than one at a birth; fo from it we come to underftand how, in cafes where there are but one, as well as where there are more, the place of the placenta, into which the umbilical cord is infert, fhould be altogether uncertain; that is, that the cord fhould be found fometimes near the circumference, fometimes at the center, and again, at very different parts of the area; as I every day obferve, and as Ruyfch has given us to know, in a variety of examples. By our doctrine, the phænomenon is inevitable; for the ovum, with the embryo attached to it, landing at the fundus, that part of it into which the umbilical cord is infert will anfwer fometimes to one part of the fundus, and fometimes to another; there being nothing to determine the loofe egg into one pofture there more than another. If we fuppofe then that the part of the ovum, into which the cord is infert, fixed at the moft vertical part of the fundus, and that all

all of it becomes placenta contiguous to the fundus, then it is plain, in that case, that the cord will be found at the center of the placenta. But, if we shall suppose, that the part of the ovum with the cord fixes near to the boundary of the fundus and cervix, then of necessity the cord is at the circumference, as I have seen it several times, and in case of twins, where the placenta's were unite, both cords were insert within half an inch of the circumference of their placenta's. I have seen it in a variety of places of the area different from the center; in which cases, the insertion of the cord had got a different place of the fundus to settle in from the other two. So that all these phænomena become most plain and intelligible, upon admitting our hypothesis, and are most certain proofs of its authenticness.

There is one thing follows upon this doctrine, which, perhaps, will be somewhat debated, and that is, that extra-uterine conceptions can have no placenta. I have consulted authors upon this; nor can I get any instances to the contrary: They indeed speak, as if they had no doubt, but they should have had them. But this seems to have arisen wholly from prejudice; for, upon examining into the distinctest cases, we have no ground to reckon that they had. Thus Cyprianus tells us, in the famous history he gives us, where he saved a woman with an extra-uterine fœtus, that the placenta was dried up, and become membranous; so that, in fact, there was no placenta found at all, that hypothesis of the author's, of its drying up being gone into, only to account for the want of a part which he reckoned eſſential amongſt the ſecundines, wherever found. Sanctorinus ſeemed likewiſe at a loſs about the placenta of the tubarian fœtus, which he gives the hiſtory of amongſt his obſervations; in ſurveying which, he tell us, that he met with ſome thing *placentæ ſimile*; and, in his deſcription of his figure, he ſays, *effuſa membranis adhærebat*. So that nothing can be concluded of a true placenta's being there, though he reckons the embryo to have been near the fifth month. Nor could I get any greater ſatisfaction in any caſe I examined. So that what I have yet met with upon this head rather confirms my doctrine than oppoſes it, that the *fundus uteri* is a place peculiarly fitted for the growth of the placenta, as well fitted ſoils encourage the growth of the roots of trees and ſhrubs, many of which are propagated by the branches, however placed; ſo that every part of them ſeems equally fitted to be root or branch.

Againſt this doctrine, of every part of the chorion becoming placenta that is contiguous to the fundus, it may be urged, that ſometimes in the middle of the placenta there have been found membranous portions, ſuch as in that inſtance Rohault mentions in the Memoirs of the French academy, Anno 1715; like to which I ſaw another in the cuſtody of the ingenious Dr James Douglas, who has not a little ſupported the credit of his country, by his anatomical diſcoveries. But ſuch examples as theſe, in my opinion, rather add than detract from the

the force of my arguments; ſince by them it would appear, that what is placenta was originally membranous, which in theſe caſes was not changed from ſome bad diſpoſition of the chorion or uterus.

I muſt next conſider whence the cavity is formed, which in the laſt months contains the infant, the greateſt ſhare of the waters, and all the ſecundines except the placenta: For ſince, by our doctrine, the placenta poſſeſſes all the fundus, the reſt of the cavity, which contains the other parts, muſt ariſe and have its formation from ſome other part; concerning which we need not be at any great loſs, ſince the uterus has, contiguous to the fundus, another cavity, which, agreeable to the name now received, we call the *cervix;* contiguous to which we obſerved the ovum membranous. So that, ſince the fundus does not diſtend beyond what we find the extent of the placenta, the reſt of the cavity at the laſt months muſt ariſe from the cervix: And, agreeable to this, we find, that then the *os tincæ* opens immediately into one uniform cavity, where cervix and fundus are confounded together. And thus, in Deventer's figure of the uterus, immediately after child-bed, the uterus is drawn of a globular ſhape, comprehending both fundus and cervix; and, from what Veſalius, Ruyſch, and Deventer obſerve concerning the poſition of the round ligaments and tubes of the uterus † in the firſt and laſt months, it is

† Veſal. lib. v. cap. 15. Ruyſch. theſ. 8. n. 3. not. 3 Devent. ars obſtetr. cap. 9, fig. 4.

is manifeſt that the uterus does not diſtend equally in all its parts: For not only theſe appendices of the uterus keep very near to the *os tincæ*, ſo that the greateſt part of the bulk of the uterus is above them towards the laſt months, but thoſe upon the one ſide are much lower than their neighbours upon the other ſide, as we may obſerve in Deventer's figure. So that we muſt ſuppoſe the uterus to extend ſometimes to one ſide more than to another. And from this it muſt follow, that ſometimes the placenta will be found in one place of the cavity, and ſometimes at another; and not, as Ruyſch and Deventer maintain, that it is always fixed at the vertical part, reckoning that the inclination of the uterus to a ſide deceived thoſe who thought otherwiſe. But, in an uterus I ſaw with Dr Douglas I mentioned formerly, where the ſecundines were ſtill *in ſitu*, the placenta was wholly to a ſide; which gave me a certain proof againſt their authority I had formerly relied upon. But, however inconſtant the uterus be, as to the manner of its diſtenſion, this is certain, that the placenta inviolably adheres to the cavity of the fundus; with this it is ingraft, and can never again ſhift its place. And therefore, as we allow that the placenta is found ſometimes in the lateral parts of the uterus, ſo it is a ſign that the uterus has diſtended much more on one ſide than another: And this ſeems very much confirmed by the obſervation of ſuch as have been frequently pregnant; few of which there are, but who have been ſenſible that the bulk of the uterus, in one courſe of pregnancy,

has

has had a very different ſituation from what it has had at another.

That long-experienced anatomiſt Mr Ruyſch, in examining into the ſtructure of the part into which the placenta was fixed, found, that there the fibres ran in ſomething of a circular courſe, and were ſome-how centrical one with another; and theſe he reckons were placed at the *fundus uteri*. But he ſeems to take the fundus there in a different ſenſe from what we have done all along; he meaning by it nothing elſe than the uppermoſt and moſt vertical part of the capacity of the uterus, when in its enlarged ſtate: Whereas we ſtrictly underſtand by it that cavity Morgagni deſcribed under that name, in his third figure of the firſt of his *Adverſaria*, we formerly cited. And, by what we have advanced, we reckoned it demonſtrated, that this fundus, in its whole extent, makes up what Ruyſch called his *muſculus uterinus*, this being the part to which the placenta infallibly adheres in all caſes: So that what Ruyſch obſerved of the circular courſe and centrical poſition of theſe fibres, in the enlarged ſtate of the uterus, muſt inſtruct us in the make and ſtructure of that cavity, which hitherto was not obſerved; that it has its fibres diſpoſed in a circular courſe, having the moſt vertical part for their common center, about which they all turn at a leſs or greater diſtance, being in miniature that very courſe of fibres Ruyſch has deſcribed as one muſcle, and repreſented as ſuch in his *Tractatio anatomica de muſculo in fundo uteri*; where we may obſerve, that though he has repreſented none of of the fibres as a compleat circle, that yet they are all circular, keeping the courſe we have defined. In that tract he ſpeaks as if part of the placenta did not anſwer to the muſcle in ſome caſes; but he tells us no inſtance where he found it ſo: Wherefore we muſt look upon this only as a ſuppoſition, to anſwer for the difficulty there is ſometimes in ſeparating the placenta; and it is plain, from the author's ſtrain of arguing, it was no more; which he would never have fallen into, if he had ſeen things in the light we have placed them; it being certain, from our account of things, that the placenta can never be found ſeparate from this muſcle, which, as we ſaid, is nothing elſe than what originally was the whole of the cavity of the *fundus uteri*, to which the ovum inevitably fixes. Nor do I ſee that there is occaſion to change its name, that has been given it in this enlarged ſtate of the uterus, that of *Ruyſch*'s *muſcle*; ſince he was the firſt who obſerved its boundaries and figure in that ſtate; and ſince it, as he aſſerted, anſwers well the office of a muſcle, in ſeparating the placenta; for the placenta of itſelf does not contract; and therefore, upon the contents of the uterus being diſcharged, and ſo the overſtretched fibres left at liberty, theſe circular ones, attached to the placenta, in contracting, muſt deſert the placenta and leave it looſe, which is the office Ruyſch allotted to it. And as he has cauſed this part to be drawn in the tract we have mentioned, he has deſcribed it as very concave and hollow; and I make no doubt that thoſe who have occaſion to ſee the uterus immediately

mediately after delivery, will ſee this cavity forming by itſelf, as has appeared to Ruyſch. What confirms me in this notion is, that, upon managing an adhering placenta, I have found it included in a very diſtinct pouch by itſelf; which I remember particularly to have happened to me in a caſe where there were twins, and where the ſecond child was ſo included in this diſtinct cavity, that after the delivery of the firſt, when I was ſearching for the placenta, not ſuſpecting twins, I felt no more of the ſecond than part of the head, though now I was in the region of the ſpleen; and before this place there was a very large vacancy between it and the *os tincæ*: After I made way for the child, I had occaſion to bring both placenta's, though they were diſtinct, from the ſame cavity, which was contracting very faſt. If I had not had a particular notion of the fundus at that time, I would have been very ready to have ſuſpected an uterus divided in two, or an uterus with horns. Since that time, I was called to a woman, who continued in very hard labour, though delivered of her child, and whoſe belly, immediately above the *os pubis*, was bulky and hard: I could not determine what the caſe was, till, examining with my hand introduced into the uterus, I felt the womb hard and inflamed all round its cavity, and part of it forced down below the *os pubis* into the vagina; which certainly was the occaſion of the labour: But ſomewhat to the right ſide, above the pubis, I was very ſenſible of a diſtinct cavity, where two or three fingers had difficulty to enter.

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This I reckoned the fundus taking its proper form, by means of its circular fibres; which, according to what we have ſhewed from Ruyſch, are to be diſtinguiſhed from the other parts of the uterus in its enlarged ſtate. From which hiſtory I would perſuade myſelf, that the deſign of parting the uterus into an upper and under cavity is manifeſt; the firſt being deſigned for the convenient ingraftment of the placenta into one certain place of the uterus, and the other for the receiving and giving place to the membranous part of the ſecundines: By which contrivance we ſee, that the *muſculus Ruyſchii* and placenta muſt always be together, and that the membranous part of the ſecundines muſt always be contiguous to the *os tincæ*. And ſince the conſidering the two diſtinct cavities of the uterus has given occaſion to theſe reflections, we ſee how lucky it was to have had them diſtinguiſhed by different names. It is this which has made authors ſo pointedly examine into their different ſtructure and uſe; and it is this that conducted us in our ſpeculations. And after I have carried them the length I have here narrated, in which, I hope, I have adduced ſufficient authority, I would venture to make one ſuppoſition, relating to the globular ſmall bodies ſcattered through the whole bounds of the cervix, amongſt the many that have been made concerning them; and that is, that they ſeparate a mucous humour, to keep ſeparated the membranous part of the ſecundines and the contiguous cervix: For, by our doctrine, the whole membranous part of the ſecundines lie contiguous to the cervix.

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It was moſt convenient then, that they ſhould be kept from uniting; and nature, for this end, in other places, has contrived ſuch a ſet of ſmall glandular bodies; ſo that it is natural to ſuppoſe they may be employed the ſame way here: For, in pregnancy, by our ſuppoſition, the inner ſurface of the cervix is ſo dilated, as to be of equal extent with the membranous part of the ſecundines; and, of conſequence, the glandular bodies will be proportionally ſcattered through that ſpace; and, in their moſt enlarged ſtate, come to ſupply that great quantity of mucous humours that flow from women near the time of the birth: Which muſt certainly flow from the whole bounds between the membranous part of the ſecundines and contiguous part of the uterus, and ſo lubricate much the *os tincæ*, as Sanctorinus would have them, whoſe account of this part, both as to the largeneſs of the cervix towards the birth, and the diſpoſal of theſe glandular bodies, anſwering much our hypotheſis, I ſhall ſet down here, for the ſatiſfaction of my readers, *Obſerv. Anat. cap.* 11. § 9. "Ad puerperarum autem "repetitas diſſectiones quod attinet, incredibili "pene numero mucoſas eas veſiculas per ampliſ- "ſimam tum temporis uterinæ cervicis cavitatem "disjectas comperi; ut vel harum numerum "pregnationis tempore augeri, vel, quod majus "conſonum eſt, earundem vix viſibilia corpuſ- "cula uſque adeo adoleſcere dicendum, quo, ex- "ſtillante humore, tum ſenſim earum membra- "narum relaxentur fibrae, tum praeter aliud "fortaſſe quodpiam pregnationis commodum "facilius illac pertranſeunti foetui via ſternatur."

And thus, Sir, I have laid before you the facts which I think conduce moſt for forming a right judgment of theſe two cavities, into which we find the body of the uterus naturally divided; and I am apt to perſuade myſelf, that thereby I have ſomewhat contributed to the clearing the hiſtory of that obſcure, though efficacious viſcus. It is but very lately ſince we came to have any notion in what manner its appendices, the ovarium and tubes, were employed: But the obſervations which put it out of doubt, that the conception is ſometimes found in the ovarium and tubes, and the experiments made upon brutes, have cleared and demonſtrated each of their offices beyond contradiction. Morgagni had of late given us a beautiful ſcheme of the *fundus uteri* from the life, whereby we could not but be ſatisfied with the uniformity of that cavity, and what immediate acceſs there was from it to the maſs of blood; and demonſtrated further, that the placenta was certainly contiguous to the blood in pregnancy. Ruyſch, about the ſame time, took notice, that, where the placenta fixed, there was a muſcle ſome-how proportioned to it. What I pretend to determine is, that all this uniform cavity has the placenta attached to it; and that it was moſt certainly calculated for its ſervice: This the figure of early placenta's demonſtrates; this the combination of placenta's, when there is a plurality of children; this the variation of the umbilical cord; this comparative anatomy makes appear, with that conſiſtency and ſimplicity wherewith truth is always accompanied. And can we imagine,

imagine, ſince the placenta was to have a muſcle ſurrounding it, that a more certain method could have been fallen upon to adjuſt their relative poſition; or that a ſecurer method could have been contrived, for the ſteady ingraftment of the ovum and uterus; and for determining that the membranous part of the ſecundines ſhould always be adjacent to the *os tincæ?* And I hope that the whole of the hiſtory will be the more acceptable to the anatomiſt, that it gives us a rational account of mola's, which hitherto had ſo much obſcured the hiſtory of the uterus.

## XIV. *An Eſſay concerning the Motions of our Eyes; by* WILLIAM PORTERFIELD, M. D. *Fellow of the College of Phyſicians at* Edinburgh.

### PART II.

### *Of their internal Motions.*

HAVING, in the former part of this eſſay, treated of the external motions of our eyes, I ſhall now, without much preface or introduction, inquire into the internal motions of theſe moſt beautiful and uſeful organs.

This is indeed a very curious and entertaining ſubject; but it is very difficult and extenſive: It takes in not only thoſe motions, whereby our eyes are fitted for ſeeing diſtinctly at different diſtances; but it alſo takes in the motions of the uvea, ſerving to dilate and contract the pupil: To each of which belongs a good deal of ſubtile anatomy, both human and comparative; and both together comprehend almoſt an entire ſyſtem of optics; not a mathematical ſyſtem, but, which is more, a phyſical one, which includes and ſuppoſes all that is mathematical in this ſcience.

It is not therefore to be expected that I ſhould here attempt at exhauſting this ſubject. This is a work that is not to be executed in the narrow bounds of ſuch a paper as this. And to write a volume, or even any methodical finiſhed treatiſe, is neither my preſent buſineſs nor inclination. All I propoſe is an eſſay fit for theſe collections, in which my chief purpoſe ſhall be, to fix and eſtabliſh ſuch of the fundamental principles appertaining to this ſubject, as may be of moſt general uſe for explaining the phaenomena of viſion, and for tracing up morbid ſymptoms to their firſt cauſes; without which there is no foundation on which a ſure and rational practice can be built. As for other things of leſs general uſe, and that have but little influence on practice, they are no part of my preſent deſign; and therefore ſhall not be meddled with, but by the by, and when they may ſerve for illuſtrating and confirming the the doctrine I embrace.

Now, the internal motions of our eyes are either ſuch as reſpect the change of conformation, that is neceſſary for ſeeing diſtinctly at different diſtances, or ſuch as only reſpect the dilatation and contraction of the pupil.

That our eyes change their conformation, and accommodate themſelves to the various diſtances of objects, will be evident to every body,

body, who but reflects on the manner and most obvious phænomena of vision. It is now, well known, and it has been demonstrated, beyond all exception, that, when a man views any object, the light which comes from its several points is so refracted by the transparent skins and humours of the eye, as to converge and meet again, at so many points, in the bottom of the eye, and there to paint the picture of the object upon the retina, with which the bottom of our eye is covered; which picture being propagated by motion, along the fibres of the optic nerves, into the brain, is the cause of vision; for accordingly as these pictures are perfect or imperfect, the object is seen perfectly or imperfectly.

Thus in general vision is performed; and, to be convinced thereof, we need only take off, from the bottom of the eye, a small bit of the sclerotis and choroides, and having, instead of them, applied the membrane of an egg, or a bit of oiled paper, that the humours may not escape; place this eye at a hole in the window-shut of a dark chamber, so as the bottom of the eye may be toward you; for you shall then see, through the membrane or oiled paper, the pictures of external objects lively painted on the retina, with their proper figures and colours; only these pictures will be inverted, just as it happens in the vulgar experiment of the *camera obscura*, where the images of external objects are received upon a sheet of white paper, placed at a due distance behind the lens. But it must still be remembered, that, according as these pictures of external objects formed on the re-

retina are more or lefs perfect and exact, the object itfelf will be feen more or lefs perfectly and diftinctly: For, when the rays that come from any point of the object are not exactly united upon the retina, the picture of this point will not be a point, but a fpot; which, being confounded with the pictures of the neighbouring points, muft render vifion very confufed and imperfect. We have an example of this in the eyes of old men, whofe humours are fo much wafted and decayed, that, through a penury thereof, the cornea fhrinks and becomes lefs convex, and the cryftalline grows flatter than before; by which means the light is not fufficiently refracted, and, for want of fufficient refraction, does not converge to the bottom of the eye, but to fome place beyond it; and, by confequence, paints in the bottom of the eye a confufed picture; and, according to the greater or leffer confufion of this picture, in the more or lefs flat eyes, the object itfelf appears more or lefs confufed and indiftinct. This is the reafon of the decay of fight in old men, and fhews why their fight is mended by fpectacles; and the contrary happens in fhort-fighted men, whofe eyes are too convex and plump: For, the refraction being now too great, the rays which come from the feveral points of the object will be made to converge, fo as to convene, in fo many diftinct points, within the eye, before they come at the retina; and therefore will, after croffing one another, where they meet within the eye, again diverge, fo as the picture made in the retina, by thefe diverging rays, and the viſion cauſed thereby, will not be diſtinct, unleſs the object be brought ſo near the eye, as that the place, where the converging rays convene, may be removed to the retina, or that the plumpneſs of the eye be taken off, and the refraction diminiſhed, by a concave glaſs, of a due degree of concavity; or, laſtly, that by age the eye grows flatter, till it come to a due figure: For ſhort-ſighted men ſee remote objects beſt in old age; and therefore they are accounted to have the moſt laſting eyes.

But, though it is certain that all objects are ſeen perfectly or imperfectly, according as their image on the retina is perfect or imperfect; yet we are not from this to imagine, that the eye, or rather the mind, by means thereof, does ever ſee any ſuch image on the retina; or that it judges of the object from what it obſerves in this image. This is a vulgar error, which, on reflection, every one's experience muſt ſoon convince him of; and it cannot be denied, but every body is himſelf beſt judge of what he ſees. This I have taken notice of in the former part of this eſſay; where I have alſo demonſtrated, that all the perceptions of the mind are preſent with it, and in the ſenſorium; and that, in ſeeing objects, the mind, by means of an original and connate law, traces back its own perceptions, from the ſenſorium to the retina, and, from thence, along right lines drawn perpendicularly to the retina, from that point of it where the impreſſion is made by the image, to the object itſelf; whence it is, that the mind, or viſive faculty, does

does always ſee every point of the object without the eye in theſe perpendicular lines: From which it is eaſy to underſtand, how the object appears perfect or imperfect, according as its image on the retina is perfect or imperfect, without having recourſe to the groundleſs ſuppoſition of the mind's ſeeing a picture in the retina; for, when the rays that come from the ſeveral points of the objects are not exactly united upon the retina, the picture of each point being a ſpot that takes up a conſiderable ſpace upon the retina, and that is confounded with the pictures of the neighbouring points, which alſo are ſpots, it muſt make theſe points to be ſeen in a great many places, and a great many points to be ſeen in the ſame place: From which confuſion the appearance of the object will be confuſed and indiſtinct. Thus (in Tab. II. Fig. 1.) let COB be an object, whoſe points O, B, and C, by emitting rays that are not reunited at the retina, but beyond it as far as X, do upon the retina form the circular images *o*, *b*, and *c*; and let F be the center of the eye, through which every line that is drawn perpendicular to the retina muſt paſs. From the extreme points of theſe circular images on the retina, *o*, *b*, and *c*, draw right lines to the point F, and continue them to the horopter, as in the figure; theſe lines, by reaſon they paſs through the center of the eye F, will be perpendicular to the retina: Whence it is evident, that the points O, B, and C, muſt be ſeen without the eye, in the whole of the circular ſpaces OCIB, BOLH, and CGKO, which are comprehended within the ſtraight lines

lines drawn perpendicularly to the retina, from the extreme points of the images of the respective points; which circles being confounded with one another, it follows that the points O, B and C must, for the reason above observed, appear confused and indistinct, though the eye sees not the confusion that is in their images at the retina.

From what has been now said concerning the manner of vision, it follows, that, in order to see objects at different distances distinctly, it is necessary that there should be a change in the eye, lest the place, in which the picture of the object is exact, should fall short of, or beyond the retina, and so cause the vision to be confused: For instance, if just now my eye is of such a conformation, as that, when I look upon an object at a foot distance, I see it perfectly and distinctly, by reason that the rays, which, in coming from the several points of the object, fall upon my eye, are so refracted by the humours thereof, as to converge and meet again in so many distinct points at the retina; if this same object be removed to five or six feet distance, and the eye, at the same time, retain unalterably its former conformation, it must appear confused and indistinct; because the rays, which come from the object at this distance, are less diverging than when it was at a foot distance; and consequently will, in passing the humours of the eye, be made to conveen before they reach the retina, and so paint thereon a confused image of the object: Whence it seems evident, that, in order to see objects equally distinct, at the foot's distance, and six feet distance, it is necessary that the eye change its conformation; either, by having its humours made more or less flat, or having the distance betwixt the crystalline and the retina increased or diminished. And this does likewise further appear by the analogy of the images painted on the retina, and those painted on a sheet of white paper, by means of a lens placed at a hole in the window-shut of a dark chamber; for if the lens be of such a convexity as is necessary to paint the image of a body, at a foot distance from it, distinctly, upon a sheet of white paper, five or six inches behind the lens, the same object, removed to the distance of six feet from the window, will not be painted exactly upon the paper, unless, in place of the former lens, you substitute one less convex, or diminish the distance betwixt the lens and paper, by bringing the paper nearer the window.

And here it may not be improper to observe how careful nature has been to form the eyes of all animals so as, upon all occasions, to see objects distinctly at an ordinary distance. Every body knows that the cornea is always protuberant, and more convex than the rest of the globe; but, in all animals, this protuberancy is not always the same: In man, and the greatest part of quadrupeds, the cornea is a part of a sphere, whose diameter is an eighth part less than that of the sclerotica; but birds have their cornea much more elevated and convex, being part of a sphere whose diameter is only the half of the diameter of the sclerotis.

tis. Now, the uſe of the cornea, with the aqueous humour which lies behind it, being to begin the refraction of the rays of light, which is afterwards perfected in the cryſtalline and vitrous humours, ſo that all that proceed from the ſeveral points of the object are again united in ſo many correſponding points on the retina, it may be aſked how it comes to paſs, that this great convexity of the cornea in birds does not render their ſight confuſed and indiſtinct, by increaſing the refraction, and making the rays meet at ſome point before the retina, as always happens to ſhort-ſighted men, who never ſee objects diſtinctly at an ordinary diſtance, by reaſon their too plump and convex cornea makes the rays conveen too ſoon behind the cryſtalline? To this we anſwer, that this great convexity of the cornea of birds is ſo far from rendering their ſight confuſed, that it is abſolutely neceſſary for diſtinct viſion; for it is to be obſerved, that, as in man and quadrupeds, the figure of their eye is almoſt ſpherical; in birds, as well as fiſhes, it is flat and depreſſed, both in its fore and back parts; by which means the retina is placed near the cryſtalline humour: And therefore, were not the cornea of a convexneſs anſwerable to the flatneſs of their eyes, and diſtance of the retina, the diſtinct image of the viſible object would fall behind it, and thereby the ſight would be rendered confuſed and imperfect, like that of old men who cannot ſee objects diſtinctly, eſpecially at a ſmall diſtance, by reaſon their eyes have become too flat in proportion to the diſtance of the retina: And therefore provident nature, who was never found to neglect any thing for perfecting the organs of animals, has very wifely increafed the convexity of the cornea in fuch creatures as have their retina brought near to the cryftalline, by the flatnefs of their eyes; for, being thereby enabled to fee objects diftinctly at an ordinary diftance, they can, by changing the conformation of their eyes, adapt them to all other diftances neceffary.

To the fame purpofe alfo belongs the different figures that have been obferved in the cryftallines of different animals. In men, quadrupeds, and the greateft part of the bird kind, it is always lenticular; but, in fuch as always recide in water, as do the greateft part of fifhes, its figure is that of a fphere or globe; and in thofe creatures that are fometimes upon land, and other times in water, as the fea-calf, the cryftalline has a middle figure betwixt that of a lens and globe.

Thefe are the differences which have been found in the figures of the cryftalline; all which are the very beft that could have been for perfecting the fight of thefe animals, and are exactly fitted to their feveral circumftances and occafions: For, with refpect to man and fuch animals as live conftantly in air, it is not doubted that they are of fuch a degree of convexity as qualifies them for feeing diftinctly at an ordinary diftance; and this is what all of us conftantly experience: But then it may be inquired, how it comes to pafs, that fince, in land-animals, the lenticular cryftalline is fufficient to refract the light as much as is neceffary ceſſary for viſion, the ſpherical cryſtalline of fiſhes does not, by its greater refractive power, conveen the rays, before they come at the retina, and thereby render their ſight confuſed and indiſtinct, excepting only when the object is very nigh? To this I anſwer, *1mo*, That the eyes of fiſhes are flat, both behind and before, by which means the retina is not ſo far removed from the cryſtalline, as in man and quadrupeds, whoſe eyes are nearly a perfect ſphere; and therefore, that the rays might be made to meet at a point on their leſs diſtant retina, it was neceſſary the refraction ſhould be increaſed by the ſpherical figure of the cryſtalline. But this is not all; for their ſpherical cryſtalline would be more than ſufficient for the diſtance betwixt it and the retina, were it not that the rays of light ſuffer no refraction in their cornea and aqueous humour. Every body knows that the light is never refracted, but when it falls obliquely on a ſurface which intercedes mediums of different denſities, and therefore it can ſuffer no refraction in falling upon their cornea and aqueous humour, becauſe they are of equal denſity with the water in which they ſwim; to compenſate which, it was abſolutely neceſſary that their cryſtalline ſhould have a ſpherical figure for increaſing its refraction, that the rays might be brought together to a point in the retina: But in land-animals, their lenticular cryſtalline is ſufficient for that end; becauſe the rays of light which paſs from the object, through the rare medium air, ſuffer a refraction in falling upon their convex and more denſe cornea, and therefore need not afterwards ſo much refraction in the cryſtalline; and this is the reaſon of that difference that is found in the figure of the cryſtalline in the inhabitants of air and water. From which it is eaſy to ſee, why this humour is of a middle figure, betwixt that of a lens and a globe, in the ſea-calf, cormorant, &c. For it being neceſſary that theſe and ſuch like animals as dive in purſuit of their prey under water, ſhould ſee both when in water and upon land. This could be no better effectuated, than by giving the cryſtalline that middle figure, which, as is evident, muſt refract the rays too much when upon land, and too little when in water; but, by the change that is made in the conformation of the eye, they are enabled to ſee diſtinctly enough in both. Hence it is that the cormorant, that large voracious bird, about the bigneſs of a large capon, does purſue its prey under water with ſuch nimbleneſs and agility, and for a long time together, till at laſt it catch it, which it doth with a dexterity which is very ſurpriſing: And therefore having firſt put on an iron-ring at the bottom of its neck, to the end that the fiſh being received into the oeſophagus, which is very large, making a kind of craw, may not enter into the ventricle; it is frequently employed for fiſhing, and is ſaid to afford a very agreeable diverſion; and what adds thereto is, that, after it has ſeized a fiſh, it always throws it up into the air, and catches it again by the head as it falls down, that it may ſwallow it entire, and without loſs of time: But, becauſe of the ring about its neck, the fiſh gets no further than its gullet, which being

being large and yielding, ſtretches into a large pouch or bag, in which the fiſhes are kept, till it be full; when they force it to come to the land, and throw them up entire.

From theſe and ſuch like arguments taken from the manner of viſion, moſt phyſicians, as well as philoſophers, have been brought to believe, that we have a faculty of changing the conformation of our eyes, in order to ſee objects diſtinctly at different diſtances; yet the famous French academiſt Mr de la Hire is of a contrary opinion, and contends, that, at whatever diſtance objects be placed, yet the eyes never alter their conformation; and this he endeavours to demonſtrate from ſeveral reaſons, and particularly from the following experiment, which is truly very ingenious and beautiful.

Take a card, and pierce it with a pin in two, three, or more places, ſo as the moſt diſtant holes be not further from one another than the wideneſs of the pupil: This done, ſhut one of your eyes, and apply the card cloſe to the other, ſo as to view a ſmall object through its holes; you ſhall be ſurpriſed to ſee this object multiplied as many times as there are holes in the card, providing it be placed out of that preciſe place, where it would be moſt diſtinctly ſeen by the naked eye; *e. g.* If I ſee an object diſtinctly when at a foot diſtance, it will appear ſingle at that diſtance when viewed through the perforated card; but, if it be removed to four, five, or ſix feet diſtance, it will always appear multiplied as often as there are holes in the card. In like manner,

ner, if the conformation of the eye be fuch, as it cannot fee objects diftinctly but at four feet diftance, it will at that diftance appear fingle through the card, but at all leffer diftances it will be multiplied.

This experiment I have taken notice of in the former part of this effay, where I have obferved, that, to make it with exactnefs, you muft for an object look to a fmall luminous point in a dark place, fuch as a little hole in a card placed before a candle, or elfe you muft look at a fmall black object placed before a white furface.

Now it is certain, that, if the rays of light that come from each point of the object are exactly united in a correfponding point of the retina, the object will always appear fingle, tho' it be viewed through feveral fmall holes; for the little luminous cones, OHH, O*hh* (Fig. 2.) which have for their apex or top a point of the object O, and for their bafis the little holes in the card, HH, *hh*, will alfo have all their oppofite tops *oo*, in one and the fame point *o*, of the retina RR, which muft needs make the object appear fingle: But, if the eye have not that conformation which is neceffary to reunite thefe rays in a point in the retina, each of thefe little cones will be cut by the retina, either before or after their reunion, and therefore each point of the object fhall, by its rays, touch the retina in as many diftinct places as there are holes in the card, and confequently the object will appear multiplied according to the number of holes. Thus, if the rays conveen before the retina, let AB be the retina,

retina, it is evident from the figure that this muſt receive the luminous pencils at two diſtinct places *x* and *x*. And, if the rays conveen behind the retina, let CD be the retina, which alſo muſt receive the luminous cones at the diſtinct places *c* and *c*. In both which caſes the object muſt appear double, by reaſon that its picture falls on two diſtinct places of the retina: Whence it is eaſy to ſee, that if the card be pierced in three or more holes, ſo as the moſt diſtant holes may not be further from one another than the diameter of the pupil; the luminous pencils, and the places in the retina where theſe pencils do fall, muſt be multiplied according to the number of holes; from which multiplication the object itſelf muſt alſo be equally multiplied. From all which, the above named author concludes, that the conformation of our eyes is never changed, at whatever diſtance objects be placed. For, ſuppoſe that I ſee an object diſtinctly at a foot diſtance, and at the ſame diſtance it appears ſingle, when viewed through the perforated card; if, to ſee the ſame object at four feet diſtance, it were requiſite that the eye changed its conformation, then he concludes it would do ſo when the object is viewed at that diſtance through the card, which does not happen, as is evident from its being multiplied.

This is the great argument whereby M. de la Hire, both in the *Journal des Sçavans*, *ann.* 1685, and in his diſſertation *ſur les differens accidens de la vüe*, publiſhed in the year 1693, endeavours to prove that the cryſtalline does not change its figure or ſituation, and in general

neral that the eye receives no new figure or conformation in viewing objects at different distances. And, to do justice to the learned author, it must indeed be acknowledged, that, at first view, the argument seems to go a great way towards a full demonstration of what he alledges; nor, so far as I know, has any thing been yet offered by any author, whether physician, anatomist, or optician, that can in the least weaken or disprove it; and yet all of them, excepting Maitre Jean and some few others, continue to teach, that our eyes change their conformation according to the distance of objects, without so much as once taking notice of De la Hire's reasoning, or attempting at an answer; which must appear very strange to every body that considers the character of the author, the strength of his reasoning, and how long ago it is that his opinion has been published to the world

In answer to this argument of De la Hire, I once suspected that, when an object is viewed through a perforated card, the eye, by endeavouring to see the card, adapted itself to as near a distance as it could, and by continuing in that state, occasioned the object to appear multiplied when at a greater or lesser distance than that to which the eye is then accommodated: But, by some experiments to be mentioned below, it soon appeared that the eye did not endeavour to see the card, nor by any such endeavour was it accommodated to the nearest distance possible; and, therefore, something else must be sought for, in order to reconcile this multiplication of the object with our

our having a power of accommodating our eyes to its diſtance.

But, for the better underſtanding this matter, it may be proper, before I go further, to clear up the ſtate of the queſtion, by admoniſhing the reader, that it is not here meant to inquire, why a ſmall object is thus multiplied when placed without the limits of diſtinct viſion: It being evident, that it ought then to appear multiplied, by reaſon that the eye can never adapt itſelf to its diſtance. Thus, if I cannot ſee diſtinctly any object that is nearer than half a foot, it muſt appear multiplied at four inches; and, if I cannot ſee an object diſtinctly that is further off than two feet, it muſt appear multiplied at three feet, and all greater diſtances. But my meaning is to account for this multiplication, when the object is placed within the limits of diſtinct viſion, which we have here ſuppoſed to be at a foot and a half diſtance from each other: And, after various conjectures on the matter, I am now at laſt fully ſatisfied, that there are two cauſes that concur in producing this phænomenon, by hindering the eye to accommodate itſelf to the diſtance of objects viewed through the perforated card, viz. the diſtinct appearance of the object, and the miſtake that the mind commits with reſpect to its diſtance.

That the object appears diſtinct, when viewed through a perforated card, is evident from reaſon as well as from experience; for the little luminous cones OHH, O*hh*, (ſee fig. 2.) which have for their apex, or top, a point in the object O, and for their baſis the little holes in in the card HH, *hh*, will, by reafon of their acutenefs, proceeding from the fmalnefs of the holes, take up but a very little fpace upon the retina, whence the object muft appear pretty diftinct. Thus, if the object is at too great a diftance, let *o* be the place where the rays conveen, and let AB be the retina; it is plain that the luminous pencils will fall on the retina at *x* and *x*, where, for the reafon juft now mentioned, they muft take up but a very little fpace, and confequently the confufion muft be very fmall. In like manner, if the object is too near, let CD be the retina, and *o* the focal point where the rays are united, thefe pencils will, at *c* and *c*, occupy fo fmall a fpace on the retina, as to occafion no fenfible confufion in the object; whereas, in both cafes, had it not been for the interpofition of the card, the luminous cone *m o m*, would, on the retina, have taken up the whole fpace *xx* or *cc*, which muft have rendered the appearance of the object very confufed and indiftinct. To correct which confufion, the eye changes its conformation, and adapts itfelf to the diftance of objects feen with the naked eye. But when, by means of the perforated card, this confufion is taken off, the mind will not then change the conformation of our eyes, there being nothing that fhould influence it to fuch an action. And this is one reafon why the object is fo frequently found multipled, according to the number of holes through which it is viewed, though it be placed within the limits of diftinct vifion, to which the eye can perfectly accommodate itfelf.

But

But there is yet another caufe which muft concur towards this multiplication, and that is, the miftake into which the mind falls, with refpect to the diftance of the object. It is not enough that the mind perceives no confufion: For, though this confufion in our fight is commonly believed to be the only thing that can influence our mind to change the conformation of our eyes; yet, by reafon of that neceffary connection and dependence, that will be hereafter fhown to have been eftablifhed by habit and cuftom betwixt thofe motions whereby the conformation of our eyes is changed, and certain correfponding motions of the axes of vifion, thefe motions come at laft always to accompany one another, and that fo neceffarily as to make it impoffible for us, by any act of volition, to direct our eyes to any object within the limits of diftinct vifion, without, at the fame time, giving them that difpofition that is neceffary for feeing diftinctly at that diftance; and therefore, though there fhould be no confufion in the object, when feen through the perforated card, it would not then appear multiplied, if placed within the limits of diftinct vifion, did not the mind miftake its diftance: For, when the mind judges rightly of the diftance of any object, both eyes are neceffarily directed towards it, and that as well when one of them is fhut, as when both are open; from which direction of our eyes, they muft alfo be accommodated to its true diftance: Whence the object will not appear multiplied; and therefore there muft be another caufe, befides the diftinct appearance of the object, that muft muſt concur in this multiplication; and that is, the miſtake the mind commits with reſpect to its diſtance.

I know that M. De la Hire affirms, that we judge rightly of the diſtance of objects viewed through a perforated card, and indeed moſt people, upon trial, will be apt to fall into the ſame miſtake; but we will afterwards have occaſion to touch upon all the means the mind can poſſibly employ for judging of the diſtance of objects; from which it will appear, that, in the caſe before us, we can ſcarce form any judgment with reſpect to diſtance, but what is wholly founded upon prejudice and anticipation, which cannot fail of betraying us into error and miſtake. Seeing then that we are ſo liable to be miſtaken in the judgment we form of the diſtance of objects ſeen through a perforated card, it needs be no ſurprize that the eye ſhould not be accommodated to their true diſtance; and that, for want of this accommodation, they ſhould appear multiplied according to the number of holes through which they are viewed.

Thus I have fully anſwered the argument wherein De la Hire places his main ſtrength, and have ſhown that the eye may be poſſeſſed of a power of changing its conformation, and of adapting itſelf to the diſtance of objects, though this power ſhould not be exerted when the object is viewed through a perforated card. But then our author alledges, that, from an anatomical examination of all the parts belonging to our eyes, it will be found that none of them are capable of making any of thoſe changes

ges in the eye, that are ſuppoſed neceſſary for ſeeing diſtinctly at different diſtances; but this we ſhall conſider afterwards, when we come to inquire into the cauſes of theſe inward motions in our eyes.

There is yet another weighty argument brought by the learned author againſt this change in our eyes; and that is, that there is no need of ſuppoſing any ſuch change; and that the eye can ſee objects diſtinctly enough at different diſtances, ſo as not to be ſenſible of any defect in the ſight, without being obliged to have recourſe to any change in its conformation.

For underſtanding this, we muſt firſt obſerve, that, if any object appear diſtinct at ſix feet diſtance, that is, if the conformation of the eye be ſuch as is neceſſary to refract the rays which come from a point of the object at that diſtance, ſo as that, in falling upon the retina after refraction, they impreſs it with a diſtinct image of that point from whence they came, then, at whatever greater diſtance the object be placed, it will alſo appear diſtinct: The reaſon of which is, that, when the object is at ſix feet diſtance, the rays which, in coming from a point thereof, fall upon the pupil, are nearly parallel; and therefore, at whatever greater diſtance the object be placed, the rays may be conceived as parallel, and conſequently the ſame conformation of the eye that is neceſſary to refract them, ſo as to make the object appear diſtinct at ſix feet diſtance, will alſo refract them in the ſame way, and thereby make it alſo appear diſtinct at all greater di-ſtances.

Now this being underſtood, let us ſee how De la Hire accounts for diſtinct viſion at different diſtances, without changing the conformation of the eye.

Suppoſe then that a man's ſight is good, that is, that he ſees objects diſtinctly enough at a foot diſtance, and likewiſe at ſix feet diſtance; it follows, from what has been ſaid, that, to ſee objects at all greater diſtances than ſix feet, there is no need of any change in the conformation of the eye: So that the only queſtion is, How the object can appear diſtinct, both at the diſtance of ſix feet, and of one foot, without ſuffering any change in its conformation?

To this the above named author anſwers, That to ſee objects ſo diſtinctly, ſo as not to be ſenſible of any defect in the ſight, it is not needful that the rays, which come from a point in the object, ſhould be united accurately in a point in the retina, but that it is ſufficient they ſhould be nearly ſo: Whence he concludes, that if the conformation of the eye be ſuch, as when an object, viewed through two holes in a card, at two feet diſtance, appears ſingle, becauſe all the rays that come from the ſeveral points of the object are united accurately in ſo many points in the retina; then, at one foot diſtance, the place where the rays meet will be a little behind the retina, and, at ſix feet diſtance, it will be a little before it, though not ſo much in either caſe as to render the object indiſtinct; becauſe caufe the rays which come from the feveral points in the object do, in falling upon the retina, meet nearly, though not accurately, in fo many correfponding points: And therefore he concludes, that thofe who have their eyes of a conformation proper to fee objects moft diftinctly at two feet diftance, will alfo fee them diftinctly enough both at one foot diftance, and fix feet diftance; and if they fee diftinctly at fix feet diftance, then they muft alfo fee diftinctly at all greater diftances: And thus he accounts for that perfect vifion which ftands in the middle betwixt fhort and long fight, without any change in the eye.

And as for the fight of old men who cannot fee diftinctly at any lefs diftance than three feet, he fuppofes that their eyes are of a proper conformation to fee objects at four feet diftance moft diftinctly; from which he infers, that, at three feet and all greater diftances, the picture of objects upon the retina will be pretty diftinct, and confequently they will be feen without any fenfible confufion, though the eye fuffers no change in its conformation.

In like manner, in thofe that are fhort fighted, and cannot fee objects diftinctly at a greater diftance than a foot, he fuppofes the eye to be of a conformation proper to fee moft diftinctly at half a foot's diftance; and thence concludes, that the picture made on the retina, when the object is at any diftance betwixt four inches and a foot, will not be confufed; and confequently the object will be feen diftinctly enough, without any change in the eye, unlefs its diftance be greater than a foot, or leſs than four inches; in which caſe the image on the retina will begin to be confuſed, and conſequently the object itſelf will alſo appear confuſed and indiſtinct.

This is, in few words, the ſum of what De la Hire advances, concerning our ſeeing objects diſtinctly at different diſtances, without having recourſe to any change in our eyes. And indeed it cannot be denied but the eye has ſome latitude of ſeeing objects diſtinctly, without changing its conformation, though they be a little further from, or nearer to the eye, than what is neceſſary for collecting the rays that come from the ſeveral points of the object, in ſo many preciſe points in the retina; and that becauſe, when the object is not far removed from that place, at which the rays coming from the object meet again at the retina, the image thereof will be pretty diſtinct, and therefore will not occaſion any ſenſible confuſion of ſight: But it does not from thence follow, that our eyes do not change their conformation when objects are much removed from that place where they appear moſt diſtinctly: For, beſides what we have ſaid before, in ſpeaking of the images of external objects, caſt upon a ſheet of white paper, by means of a lens placed at the hole in the window-ſhut of a dark chamber, where we obſerved, that, in order to make the image diſtinct, it was neceſſary, according to the different diſtance of the object, either to change the lens, for one more or leſs convex; or to change the diſtance betwixt it and the paper, by bringing the paper nearer to, or further from the lens, ac-

cording

cording to the different diſtances of the external object; I ſay, beſides this, experience teaches us, that the conformation of our eyes is changed, in viewing objects at different diſtances. For every body knows, that the eye cannot ſee equally diſtinctly at the ſame time objects at different diſtances, *e. g.* if with one of your eyes, the other being ſhut, you look attentively to a ſmall object, ſuppoſe a pin, at half a foot or foot from the eye, and at the ſame time place another at ſix feet diſtance, that at ſix feet will appear exceeding confuſed; but if you apply yourſelf to obſerve accurately that at ſix feet diſtance, then it will be ſeen diſtinctly, but the other next the eye will appear very confuſed and imperfect; which plainly ſhews, that when the diſpoſition of the eye is ſuch as is neceſſary for making a diſtinct picture of the pin at one diſtance, the place where the diſtinct picture of the other pin is made muſt fall ſhort of, or beyond the retina; and conſequently upon the retina itſelf the picture muſt be confuſed, from which confuſion viſion is rendered imperfect and indiſtinct; and therefore, ſince at pleaſure I can ſee diſtinctly either of the pins I will, while at the ſame time the other appears confuſed, it follows, that I have a power of changing the conformation of my eye, and of adapting it to the different diſtances of objects; and this is the only reaſon can be given, why objects without doors do not appear diſtinct through a window glaſs, when the eye is attentive in obſerving the little ſcratches or particles of duſt upon the ſurface of the glaſs; and, on the contrary, when attentive

tentive to the external objects, it does not distinctly observe the scratches or opaque particles of dust upon the glass; the conformation of the eye in the one case being such as to paint distinctly upon the retina the images of the scratches and particles of dust, but not to paint those of the external objects but confusedly; and in the other case, the conformation of the eye is adapted to paint exactly upon the retina the images of external objects; and therefore the place where the distinct images of the scratches are made must fall behind the retina, from which they must appear confused and imperfect. And indeed, were it not for the change that is made in the disposition of the eye, it were very difficult to explain how birds, that dive in pursuit of their prey, should be enabled to see both in air and water, seeing the refraction that happens in the eye is so far different in the one case from what it is in the other.

To weaken the force of these objections, M. De la Hire has recourse to the mobility of the pupil, from which he endeavours to account for distinct vision at all distances, without any change in the conformation of the eye; but with what success will appear afterwards.

Having thus considered what De la Hire brings in support of his hypothesis, I shall now proceed to some experiments I made for measuring the strength and weakness of sight; whereby not only the fallacy of De la Hire's reasoning will be made further manifest; but it will also be demonstrated, beyond all exception, that our eyes change their conformation, and adapt themselves to the various distances

ſtances of objects, within certain limits; which limits will alſo be accurately determined: But, that theſe experiments may be the better underſtood, I muſt firſt premiſe the following axioms.

## AXIOM I.

*When an object ſeen with both eyes appears double, by reaſon that its diſtance is leſs than that to which the eyes are directed, upon covering either of the eyes, the appearance that is on the the contrary ſide will vaniſh; and if it appear double, becauſe its diſtance is greater than that to which the eyes are directed, upon covering either of the eyes, the appearance that is on the ſame ſide will vaniſh.*

*Illuſtration.* To illuſtrate this, ſee fig. 3. 4. and 5. where A and B are the eyes, *x* the object, which is at a ſmaller diſtance than the point C, to which both eyes are directed. It is evident, that while the eyes continue directed to C, the object *x* muſt be ſeen in two different places, which, with reſpect to the horopter, to which all objects are referred, will be D and E; for being ſeen by the right eye B, in the direction of the viſual line B*x*D, it muſt, at D, hide a part of the horopter DCE; and, being ſeen by the left eye A, in the direction of the viſual line A*x*E, it muſt hide a part of the horopter at E; and therefore, with reſpect to the horopter on which the eyes are fixed at C, the object *x* muſt appear to the right eye B, as at D, and to the left eye A, as at E; and, in covering either of the eyes, the appearance that

that is on the contrary ſide will be made to vaniſh.

In like manner, if the eyes are directed to $x$, the object C, which is further off than $x$, will be ſeen by the right eye B, in the direction of the viſual line B$m$C; and by the left eye A, it will be ſeen in the direction of the viſual line A$o$C: And therefore, with reſpect to the horopter $mxo$, to which all objects are referred, it muſt appear double, as at $m$ and $o$; and in covering the right eye B, the appearance that is on the right ſide towards $m$ will vaniſh; and in covering the left eye A, the appearance that is on the left ſide towards $o$ will vaniſh; all which is exactly agreeable to experience.

## AXIOM II.

*When an object appears double, from its being ſeen with one eye through too ſmall holes made in a card, or any other opaque thin body, if its diſtance be greater than that to which the eye is accommodated, upon covering either of the holes, the appearance that is on the ſame ſide will be made to vaniſh; and if its diſtance be leſs than that to which the eye is accommodated, upon covering either of the holes, the appearance that is on the contrary ſide will be made to vaniſh.*

*Illuſtration.* Let E be the eye, (See fig. 6. and 7.), QT the card, in which are two ſmall holes $d$ and $r$, and let A be a ſmall body, at a greater or leſſer diſtance than that to which the eye is accommodated. The rays of light A$d$, A$r$, will not, after refraction, converge

verge to a point in the retina; but, by reaſon that the diſtance of the object A is greater or leſs than that to which the eye is accommodated, they will be made to converge to ſome other point, either before or behind the retina, ſuch as *o*; but, on the retina itſelf, they will fall on the different points *i* and *m*, at both which a picture of the object will be formed; from which duplication of the picture the object itſelf will alſo appear double at C and B, viz. in the right lines *i*C and *m*B, which are ſuppoſed to be drawn perpendicular to the retina from the points *i* and *m*, where the pictures fall. Whence it is evident, that, if the hole at *d* be covered, there will be no image at *i*, and conſequently the appearance at C will vaniſh; and, if the hole at *r* be covered, there will be no image at *m*, and conſequently the appearance at B muſt vaniſh: But, when the object A is at a greater diſtance than that to which the eye is accommodated, as in Fig. 6. the appearance that is made to vaniſh, by covering either of the holes *d* or *r*, lies on the ſame ſide with the covered hole. But, when the object A is at a leſs diſtance than that to which the eye is accommodated, as in in Fig. 7. the appearance that is made to vaniſh, lies on the contrary ſide of the hole that is covered, as has been affirmed in the axiom.

Exper. 1ſt, I took a ſmall plate of white iron IK, (ſee Fig. 8.) in which I had cut two parallel narrow ſlits, whoſe diſtance from one another did not exceed the diameter of the pupil. Theſe ſlits gave paſſage to more light than what could paſs through ſmall holes, and therefore

therefore were fitter for my purpofe, it being neceffary that the object fhould be clearly feen. This plate I held clofe to my right eye B, in fuch a manner as the flits might have a vertical pofition; and, having fhut my left eye A, through thefe flits I viewed the fmall object o, which alfo had a vertical pofition, and confequently was parallel to the flits. In this experiment, the object o was at fuch a diftance from the eye B, as to appear fingle, when viewed in this manner through the flits: But when both eyes were opened, and directed to a more diftant point, fuch as P, three appearances were feen, *a*, *b*, and c; which appearances were nearer to, or further from each other, according as the point P was nearer to, or further from the object o; and in covering the left eye A, the appearance, *a*, that was on the contrary fide, did vanifh, which appearance did therefore belong to the eye A. And, in covering the right eye B, the appearances on the contrary fide *b* and c, belonging to the eye B, did vanifh, from which I was certain, that the diftance of the object o was lefs than that to which the eyes were directed, (fee Ax. 1.) This being done, my next bufinefs was to examine, whether thefe double appearances *b* and c, that were feen thro' the flits, did not alfo proceed from the object o its being at a lefs diftance than that to which the eye B was then accommodated, and, upon trial, I found it was fo; for, by covering either of the flits with my finger, the appearance on the contrary fide was always made to vanifh, (fee Ax. 2.) Having fatisfied myfelf as to thefe particulars,

culars, I changed the direction of my eyes, and turned both inwards towards a nearer point, such as $x$, by which also three appearances were seen, $d$ $e$ and F, and these appearances were also nearer to, or further from one another, according as the point $x$ was nearer to, or further from the object o, but they were always in a contrary order to those that were seen, when my eyes were directed as above: For the appearance F, seen by the left eye A, was on the left side, and the appearances $d$ and $e$, which were seen through the slits by the right eye B, were on the right side; whence I was certain, that the distance of the object o was greater than that to which my eyes were directed. I then covered one of these slits with one of my fingers, and I found that the appearance that was on the same side did always vanish; from which, when compared with the second axiom, it follows that the object o is at a greater distance than that to which the eye is accommodated.

In making this and all the following experiments, it was necessary that the object o should be as conspicuous as possible: What upon trial I found to answer best, was a narrow slit made in a dark lantern in which a lighted candle was put, to render it luminous, though sometimes I also made use of a black line upon white paper, or a white line upon black paper, both which answered very well, in all the experiments wherein the distance of the object did not exceed two feet; but, when the distance was greater, these lines began to be obscure, and by reason of their obscurity, the experiment did not succeed so well. It must also be observed here, once for all,

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that though, in the above experiment, it was easy for me to direct my eyes to a distance that was rather greater or less than the distance of the object o, without the assistance of any other object, on which my eyes might be fixed; yet in this, as well as in many of the subsequent experiments, I was sometimes obliged to put an object in that place, towards which both eyes were to be directed; and this was always necessary, either when a great effort was needful to give the eyes the designed direction; or when, for observing the phænomena more accurately, the experiment required that the eyes should for some time be kept fixed in a certain determined direction, both which are made much easier, by having an object on which the eyes may be fixed. When it was required that my eyes should be directed, to a very near distance, for the object o I made use of a black or white line, made on paper of an opposite colour; and at the place *x*, to which my eyes were to be directed, I held in a horizontal position, and parallel to my eyes, any small object *zx*, such as a bit of the stem of a quill, whose extremity *x* I looked at for an object; but, when the experiment required that my eyes should be directed to some point at a considerable distance beyond the object o; for the object o I made use of the narrow slit in the lantern, and at the distant point p, to which my eyes were to be directed, I placed another dark lantern, in which was the horizontal slit PQ, whose extremity P, which was seen by the right eye, in the visual line B*o*P that passed immediately above the upper

per end of the object o, ferved me as a point of view, on which I could eafily fix both eyes, while I attended to the appearance of the object o.

Now, from this experiment, compared with the preceeding axioms, it clearly follows, 1*mo*, That we are poffeffed of a power of changing the conformation of our eyes, and of adapting them to various diftances. 2*do*, This change in our eyes, whereby they are fitted for feeing diftinctly at different diftances, does always follow a fimilar motion in the axes of vifion with which it has been connected by ufe and cuftom; for, when the eyes were directed to P, the object o was feen double through the flits, and, by covering one of the flits, it appeared that the eye was adapted to too great a diftance; and, as the point P was brought nearer and nearer the object o, thefe appearances approached nearer and nearer to one another continually, till at laft, when the point P became very nigh to o, they coincided in one at o, which fhews that the eye was then adapted to its diftance. But, when the point P was moved along the line o*x*, from o to *x*, two appearances of the object o were again feen through the flits; which appearances being in a contrary order from what were feen, when the point P was on the other fide of the object o, it follows, that the eye was then adapted to too fmall a diftance. And as the point P, in its motion from o to *x*, receded further and further from o, thefe appearances receded further and further from one another continually. From all which it is very evident, that there is a neceffary connection and dependence eftablifhed

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betwixt thoſe motions, whereby the conforma-tion of our eye is changed, and certain cor-reſponding motions in the axes of viſion, which makes it impoſſible for us to direct our eyes to any object within the limits of diſtinct viſion, without, at the ſame time, giving them that diſpoſition that is neceſſary for ſeeing diſtinctly at that diſtance; but theſe two corollaries will be further confirmed by the experiments that follow:

*Exper.* 2. The diſtance of the object o, (Fig. 8.) being five inches, I viewed it through the ſlits, the other eye A being ſhut or covered, and it appeared double; and, upon covering either of the ſlits, the appearance that was on the contrary ſide was made to vaniſh, and therefore the diſtance of the object was leſs than that to which the eye was accommodated; and both eyes being open, and directed to *x*, whoſe diſtance from the eye was about three or four inches, three appearances were ſeen, *d*, *e*, and F, whereof the appearances *d* and *e* belonged to the right eye B, and when with my finger I covered either of the ſlits, the appearance that was on the contrary ſide did vaniſh; whence it is evident, that I cannot, by any effort, fit my eyes to ſo ſmall a diſtance as five inches.

*Exper.* 3. 4. and 5. At ſix, ſeven, and eight inches diſtance, when one eye was ſhut, the object o, ſeen through the ſlits, appeared double, and, by covering one of the ſlits, it was evident that its diſtance was leſs than that to which the eye was accommodated. And in looking with both eyes to *x*, whoſe diſtance

from

from the eye was about half the diſtance of the object p, a double appearance was ſeen, one at P belonging to the eye A, and the other at $x$ belonging to the eye B; but this appearance at $x$ was always ſingle, though ſeen through the ſlits; whence it follows, that my eye cannot accommodate itſelf to a diſtance that is much leſs than ſix, ſeven, or eight inches.

*Exper.* 6. At the diſtance of nine inches, the object o ſeen through the ſlits, the other eye being ſhut, appeared ſometimes ſingle, but moſtly double, and when it appeared double, it was evident, by covering either of the ſlits, that it was too near, with regard to the diſpoſition of the eye, and when both eyes were open, and directed to the quill $x$, which was at half diſtance preciſely, three appearances were ſeen, whereof the appearance $d$ and $e$ did belong to the right eye B, to which the ſlits were applied; and, in covering one of theſe ſlits, the object on the ſame ſide diſappeared: Whence I was certain that the object was too far off, and that my eye can be accommodated to a leſs diſtance than nine inches, but not much, as may be learned from the nearneſs of the appearances, as well as from the four laſt experiments.

From the five laſt experiments laid together, we may ſafely draw the following corollary. viz. The neareſt limits of diſtinct viſion in my eyes, is at about ſeven inches diſtance; for, by the ſecond experiment it appears, that my eyes cannot be fitted to ſo ſmall a diſtance as five inches; and by the laſt experiment it is plain, that they can be accommodated to a leſs

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diſtance than nine inches; and the third, fourth and fifth experiments make it manifeſt, that at ſix, ſeven, and eighth inches diſtance, the object ſeen through the ſlit appears always ſingle, whatever effort be made to double it, by ſtraining the eyes to ſee a nearer object: whence the middle diſtance, ſeven inches, ſeems to be nearly the neareſt limits of my eye, beyond which it cannot go: And therefore all objects that are nearer than ſeven inches muſt appear more and more confuſed, according as their diſtance is leſs and leſs than ſeven inches.

*Exper.* 7. In looking to an object at two feet diſtance, through the ſlits, the other eye being ſhut, it always appeared double and too far off; and, in looking with both eyes to a more diſtant object, it was then alſo ſeen double; but, in covering either of the ſlits, the appearance on the oppoſite ſide did vaniſh; whence it was evident, that the object was then too nigh, but theſe appearances were ſo cloſe, that they did almoſt touch one another; which ſhews that my eyes can ſcarce go further than to accommodate themſelves to the diſtance of two feet.

*Exper.* 8. At two feet and a half, three feet, and all greater diſtances, the object not only appeared double and too far off, when viewed with one eye through the ſlits; but, when both eyes were open, and directed to a very diſtant object, the double appearance that was then ſeen through the ſlits, was ſuch, as by covering one of the ſlits, made it evident, that even then the object was alſo too far off.; from which it follows, that my eyes can never, by any

any effort, be accommodated to ſo great a diſtance as two feet and a half.

*Corol.* From this and the immediately preceeding experiment, it ſeems probable, that the furtheſt limits of my ſight reaches to the diſtance of about twenty ſeven inches: For, by *exper.* 6. it is plain that I can accommodate my eye to a diſtance that is greater than two feet; and by the laſt experiment it is manifeſt that my eye cannot accommodate itſelf to ſo great a diſtance as two feet and a half: Whence it ſeems reaſonable to conclude, that the furtheſt limits of my ſight lies about the middle diſtance betwixt both.

*Exper.* 9. and 10. At ten and twelve inches diſtance, the object o, ſeen with one eye through the ſlits, did, as in the 6th *exper.* where it was at the diſtance of nine inches, appear ſometimes ſingle, but frequently double and too nigh.

*Exper.* 11. and 12. At the diſtance of fifteen and eighteen inches, one eye being ſhut, the object o, ſeen through the ſlits, appeared ſometimes ſingle, and at other times double; but when it was double, by covering one of the ſlits, it was always found to be too far off.

*Corol.* From the four laſt, as well as from ſome of the preceeding experiments, it is manifeſt, 1*mo*, that the eye does frequently miſtake the diſtance of the object ſeen through the ſlits; for, when its diſtance lies betwixt the limits of diſtinct viſion, to which the eye can eaſily accommodate itſelf, it would never appear double, did not the mind miſtake its diſtance. And this is the reaſon why, when both

both eyes are open, and directed to the object, it appears ſingle at all diſtances within the limits of diſtinct viſion, by reaſon the eye is then accommodated to its diſtance, which is then known to us, by means of the angle which the optic axes make at the object. *2do,* The judgement which the mind forms with reſpect to the diſtance of objects, ſeen with only one eye through the ſlits, is not always the ſame, but is fluctuating and inconſtant, as may be gathered from the four laſt experiments, where the object ſometimes appeared ſingle, and at other times double; and, when it appeared double, the diſtance betwixt the appearances was not conſtantly the ſame. *3tio,* If the object ſeen through the ſlits, the other eye being ſhut, is not much beyond the neareſt limits of diſtinct viſion, when the mind miſtakes its diſtance, it imagines it further off than it really is, as is evident from the 4th, 5th, 6th, 9th, and 10th experiments. But, *4to,* When the object is not a great deal nearer than the furtheſt limits of diſtinct viſion, when we make its diſtance, we imagine it nearer than it really is; whence it appears double, becauſe it is too far off with reſpect to the conformation of the eye, as does appear from the 7th, 11th, and 12th experiments.

If it ſhould be here inquired, why the mind miſtakes the diſtance of the object ſeen through the ſlits, the other eye being ſhut? To this I anſwer, that, by running over all the means the mind can poſſibly employ for judging of the diſtance of objects, which means we will have occaſion to touch upon below, it will appear, that

that in the caſe before us, we can ſcarce form any judgment with reſpect to diſtance, but what is entirely founded upon prejudice and anticipation; and therefore it needs be no wonder that we are frequently led into error and miſtake, and that the mind ſhould be ſo fluctuating and inconſtant in the judgment it forms of diſtance.

When I made the forgoing experiments, I deſigned to repeat them with more care and exactneſs, and to make ſome new ones of the ſame ſort, by means of an inſtrument I had contrived for that purpoſe; which, from its uſe in meaſuring the limits of diſtinct viſion, and in determining with great exactneſs the ſtrength and weakneſs of ſight, may be called an optometer. But I was then interrupted, and I have not now time to take thoſe things into further conſideration.

Having thus ſufficiently demonſtrated, that our eyes do change their conformation, and adapt themſelves to the different diſtances of objects, it remains that we examine wherein this change conſiſts, and by what mechaniſm it is introduced; about which authors are very much divided in their opinions: The chief of which we ſhall now conſider, and fix upon what we think moſt probable; leaving every body at liberty to differ from us as he ſees reaſon.

Some are of opinion, that the whole globe changes its figure by being lengthened into an oblong figure when objects are near, and by becoming flat when they are removed to a greater diſtance. This indeed very well ac-

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counts for the diſtinct appearance of objects at different diſtances; for, according as objects are nearer or further from our eyes, their images will be painted at different diſtances behind the cryſtalline humour: And therefore, if we have a power of rendering the eye flat or oblong, the retina will be brought to that preciſe place behind the cryſtalline, where the perfect image of the object is made, and conſequently will be ſeen diſtinctly.

Now this change in the figure of the eye is differently explained by authors. Some maintain, that it is rendered oblong by the joint contraction of the two oblique muſcles. And this opinion Dr Keill likewiſe embraces: His words are, "The aqueous humour, being the "thinneſt and moſt liquid, eaſily changes its figure, when either the *ligamentum ciliare* contracts, or both the oblique muſcles ſqueeze the "middle of the bulb of the eye, to render "it oblong when objects are too near us." (See his anat. chap. iv. ſect. 4.) But this is by no means probable; for, in order that the eye may be rendered oblong by the contraction of theſe muſcles, it is neceſſary to ſuppoſe, that they preſs its ſides inwards towards its axis; but this they cannot perform, becauſe their diſpoſition is not proper for that effect. Had they been ſo diſpoſed as to embrace the globe in the form of a ring, their contraction might then have ſqueezed the eye into an oblong figure: But their preſent diſpoſition is very far different from what ſeems neceſſary for producing this change in the eye; which we ſhall not now repeat, having in the former

part

part of the eſſay deſcribed them at ſome length. But beſides this, there is yet another argument againſt the eye's changing its conformation, when theſe muſcles contract; and that is, that, in ſeveral creatures, their diſpoſition is very far different from what it is in man: Thus in the pike they are both ſituated in the under ſide of the eye, where they decuſſate one another in form of a croſs, as has been obſerved from Aquapendente and Perrault, in the former part of this eſſay. In the *canis carcharias*, and in ſome other fiſhes of the dog kind, Steno has obſerved, that the ſuperior oblique had no trochlea, but that its origin and progreſs was altogether ſimilar to the inferior oblique. (See his *canis carchariæ diſſectum caput*, and his *diſſectio piſcis ex canum genere*). And Pierus the ſon, in his *Obſervationes Anatomicæ*, tells us, that the grand oblique is alſo without any trochlea, both in geeſe and hares; whence it ſeems very probable, that theſe muſcles, ſo differently diſpoſed in different animals, do never ſqueeze the eye, ſo as to render it oblong, and yet it muſt be allowed, that they have a power of accommodating their eyes to the different diſtances of objects, as well as other creatures, which therefore muſt be ſought for ſomewhere elſe than in the oblique muſcles.

Another opinion concerning this change of our eyes is, that the four ſtreight muſcles acting together, compreſs the ſides of the globe, and, by this compreſſion, reduce it to an oblong figure, when objects are near; and that by its natural elaſticity it recovers its former figure

figure when theſe muſcles ceaſe to act; but, though this opinion be received by the learned Boerhaave, as well as by the generality of other authors, yet there are many objections which render it very doubtful, if not altogether abſurd: For, when theſe muſcles act together, they muſt draw the eye inwards, and preſs its bottom againſt the fat, which touches it in that place: But all action and re-action being equal, it follows that the back part of the eye muſt be preſſed forwards by the fat with as much force as the muſcles draw the eye inwards; and conſequently, that the force whereby theſe muſcles endeavour to lengthen the eye, by compreſſing or ſqueezing its ſides, muſt be balanced and taken off by the preſſure of the fat againſt the back part of the eye. The other objections againſt this hypotheſis, muſt be taken notice of below; to which the reader muſt therefore be referred, for ſaving repetitions.

Others again are of a quite contrary opinion, and would perſuade us that, when theſe four ſtreight muſcles act together, they render the eye flat, by pulling it inwards, and preſſing its bottom againſt the fat; and that it is again reduced to its former figure, either by the joint contraction of the two oblique muſcles, or by the inherent elaſticity of its parts, which exerts itſelf when the ſtreight muſcles ceaſe to act: But neither does this opinion appear probable; for, when theſe muſcles contract, they not only endeavour, by preſſing the eye againſt the fat in the bottom of the orbit, to render it flat, but likewiſe ſqueeze the ſides of the eye, and

and by that means endeavour at the ſame time to render it oblong, which two actions being equal, becauſe proportional to the ſame cauſe, viz. the contraction of the muſcles, and being contrary to one another, they muſt deſtroy each other.

From what has been ſaid, it ſeems very probable, that the eye can neither become flat nor oblong, either by the action of the ſtreight or oblique muſcles. And this does yet further appear from the following reaſons: *1mo*, Did the eye accommodate itſelf to the diſtance of objects, by any change in its figure ariſing from the contraction of its muſcles, this change would be different in different poſitions of the eye, and only regular in one ſituation of it.

*2do*, If you preſs your eye gently with your finger, all objects ſeen with that eye will appear confuſed and indiſtinct, neither will they appear more perfect, at whatever diſtance they be placed. If you aſk the reaſon of this phænomenon, I know no better anſwer, than that that determined ſituation of the ſmall fibres compoſing the retina, which is neceſſary for diſtinct viſion, is by the preſſure of the finger diſturbed and diſordered: And therefore, it is not eaſy to underſtand, how the ſame diſpoſition ſhould not be equally diſordered by that ſuppoſed compreſſion of the muſcles, which is neceſſary for changing the figure of the eye.

*3tio*, A third argument againſt this change of figure in the eye, is, that in ſome creatures the ſclerotica is ſo very hard as does not allow of any ſuch change; and this diſpoſition in the ſclerotica is generally obſervable in all

 birds

birds and fishes, both which have it bony, from the middle of the globe, to its fore part where it joins the cornea, as has been observed by Aquapendente, the French Academists, and many other Anatomists. Mr Ranby has observed, that this bony circle in the ostrich consists of fifteen bony scales joined to one another, so as to make one circular bone round the cornea, of which he has given a figure in the Philosophical Transactions. And Mr Warren has since found, that the ostrich has this ring in common with other fowls both of the water and land, with this difference only, that that the ring in water-fowls consists of fifteen, and in land-fowls but of fourteen bones, and that they are so disposed, that one bone lies over the end of two others, then three or four lie over one another like the scales of fish; then one bone lies under the ends of two others, and then two or three more follow again like the scales of fish; but he thinks, that unless there be a *lusus naturae*, Mr Ranby's figure does not express it so very justly as it might be done, which Ranby himself in another paper seems to acknowledge, (see Philosoph. Transf. abrid. Vol. VI.) But, whatever be in this, one thing is certain, that, in all fowls, as well as fishes, a great part of the sclerotis is hard and inflexible: And, particularly in the owl, Mr Perrault speaks as if it were wholly bony; yet I find, that Peierus the son makes it a little softer towards the entry of the optic nerve. But what makes most for our purpose, is, that in some fishes the whole of the sclerotica is of a cartilaginous or bony substance; thus

thus it is in the whales, in which alfo its thicknefs is more than an inch, as Ruyfch obferves, (*Thefaur. anatom. maxim.* N. LI.), in the fea-fox, this tunicle, tho' thin, was by the French Academifts found "fo hard that it might rather pafs for a bone than a cartilage." See their memoirs for the natural hiftory of animals. And the like has been obferved by Steno in the *canis carcharias*, and fome other fifhes of the canine kind, *fclerodis tunicæ pars anterior, et tranflucens*, (fays he, in his *canis carchariæ diffectum caput*), *quæ cornea dicitur, hic plana erat, reliqua pars vere dura, cæteris in eodem pifce cartilaginibus fimilis; fic et in avibus, magna fclerodis pars offea reperitur, &c.* Santorini, in his *Obfervationes anatomicæ, cap. IV. fect.* 2. has alfo a very remarkable obfervation to this purpofe: His words are: *Quoniam nulla funt, quæ circa oculi mufculos adnotanda habemus, de eorundem ufu quædam proponere libet: Num fcilicet, præter ejufdem oculi motum illum fic vel retrahant vel producant, ut vel in planiorem, vel in acutiorem figuram ille conformetur? Hanc me in quaeftionem induxit offeam prorfus reperiffe in thinni oculis fclerotidem membranam, ob cujus quidem foliditatem ac duritiem, nullo mufculorum vel valentiffimo nifu conftituta poteft figura commutari. Quapropter, fi in eo pifce quidquam commodi ex ejus figuræ varietate natura fperaviffet, aliud quodpiam artificium in ejus vicem machinata fuiffet, &c.* Now, from thefe obfervations it is very plain, that in many animals it is impoffible that the eye can accommodate itfelf to the different diftance of objects, by varying its

N 2 figure,

figure, the action of its muſcles being inſufficient to overcome the reſiſtance of its cartilaginous or bony and almoſt inflexible tunicles; and yet it cannot be denied but they have a faculty of changing the conformation of their eyes, and of adapting them to the diſtance of objects, as well as other creatures; which therefore we muſt expect to find ſomewhere elſe than in any of its muſcles.

It may indeed be ſaid, that, though the change made in the eyes of birds and fiſhes does not proceed from the action of its muſcles; yet it does not from thence follow, that in man and other animals, who have the tunicles of the eye flexible and yielding, the contraction of theſe muſcles does not produce ſome variation in the figure of the eye: This I readily own; yet, if we conſider that nature is very conſonant and conformable to herſelf in all her actions, we can hardly doubt but the ſame cauſe, which in fiſhes and birds accommodates their eyes to the diſtinct viſion of objects at different diſtances, does likewiſe produce the ſame change in the eyes of men, eſpecially ſince there is nothing to be found in the eyes of theſe creatures capable of producing that change, but what alſo obtains in human eyes.

I am not ignorant, that ſome have feigned certain fibres going from the choroides to the cryſtalline in birds; and others have ſuppoſed, that in fiſhes there is likewiſe ſome peculiar diſpoſition for adapting their eyes to the diſtances of objects. But, with reſpect to birds, Perrault and the French Academiſts have particularly

cularly observed, that there is no ſuch fibres different from thoſe that compoſe the *marſupium nigrum*, which can never anſwer that end, being adapted to another purpoſe, to be explained afterwards; and as for fiſhes, that pretended mechaniſm is ſo darkly explained, and that only by authors of ſo little character and reputation, that it does not deſerve credit. But,

*4to*, To put this matter out of all diſpute, we muſt have recourſe to the following obſervation, viz. a man having a cataract in both eyes, which entirely deprived him of ſight, committed himſelf to an oculiſt, who, finding them ripe, performed the operation, and couched the cataracts with all the ſucceſs could be deſired; but, after they were couched, he could not diſtinctly ſee objects, even at an ordinary diſtance, without the help of a very convex lens; which is what every body has obſerved to be neceſſary to all thoſe who have had a cataract couched. Neither is the reaſon thereof difficult; for, as a cataract is not a philm ſwimming in the aqueous humour, as has been generally believed till of late, but an opacity in the cryſtalline itſelf, and, as the couching of a cataract conſiſts in introducing a needle into the eye, and turning down that opaque humour below the pupil, it is evident that the cryſtalline cannot be diſplaced and turned down to the under part of the eye, but the vitrous humour muſt, in giving way to it, be puſhed into its place; but, becauſe its denſity is leſs than that of the cryſtalline, it follows, that the rays of light will be leſs refracted, and therefore will not meet at a point at the retina, but at some diſtance behind it; from whence the ſight muſt be confuſed, unleſs a convex glaſs, of a due degree of convexity, be brought to aſſiſtance, which, by refracting the light, may render its rays more converging, and thus ſupply that refraction which is wanting in the eye by the depreſſion of the cryſtalline: And this is the true reaſon why there can be no diſtinct viſion after the couching of a cataract, unleſs when objects are viewed through a convex glaſs of a due degree of convexity; nor has the efflux of the aqueous humour any concern in this phaenomenon, ſeeing it is again reſtored, as was known to Galen. But this is not all that happens after the depreſſion of the cataract; for it was obſerved, that the ſame lens was not equally uſeful for ſeeing all objects diſtinctly, but that he was obliged, for ſeeing them diſtinctly, to uſe glaſſes of different degrees of convexity, ſtill the more convex the nearer the object.

To make this experiment with great exactneſs, and to provide againſt all poſſibility of miſtake, it were proper to cover that ſide of the lens which is next to the eye, with black paper, in the middle of which, two narrow parallel ſlits have been made, whoſe diſtance from one another does not exceed the diameter of the pupil By this means, if the eye ſtill retains its faculty of changing its conformation, a ſmall object, that is at ſuch a diſtance as to appear ſingle through the ſlits, when the other eye is ſhut, may be made to appear double, by opening both eyes, and directing them to a nearer

nearer or more remote object, as has been explained above; whence, if no ſuch double appearance can be ſeen, we may conclude with great certainty, that the eye has loſt its power of accommodating itſelf to the diſtance of objects. I have never had an opportunity of making the above experiment myſelf; but, when any ſuch offers, I deſign to make it in this manner, or rather to employ the inſtrument formerly mentioned; which, for its uſe in meaſuring the limits of diſtinct viſion, and in determining with the utmoſt exactneſs the ſtrength and weakneſs of ſight, I have called an optometer. In the mean time, from the experiment as it ſtands, we may ſafely draw the following corollaries.

*Cor.* 1. From what happens in couching the cataract, the eye loſes the faculty of adapting itſelf to the various diſtances of objects.

*Cor.* 2. Did that change in the eye, that is neceſſary for ſeeing objects at different diſtances, depend upon the action of its muſcles, then, after the depreſſion of a cataract, the ſame lens will anſwer all objects of whatever diſtance; but, ſince this is not fact, it follows, that however the muſcles of the eye may be ſuppoſed to change a little its figure, yet this change is not ſufficient to provide for the diſtinct viſion of objects at all diſtances.

*Cor.* 3. Seeing that nothing happens in the eye, in couching the cataract, but that the cryſtalline is depreſſed, it follows that the change made in our eyes, according to the diſtance of objects, muſt be attributed to this humour.

It

It remains now that we inquire what this change of the cryſtalline is, and by what mechaniſm it is produced.

Some maintain, that, according as objects are at different diſtances, this humour becomes more or leſs convex, which does indeed very well account for diſtinct viſion at all diſtances; for objects, painted on a ſheet of white paper, by means of a lens placed in a hole of a window ſhut of a dark chamber, have their images always diſtinct, at whatever diſtance they be from the window, provided that the lens be of a convexity anſwerable to that diſtance.

Others again are of opinion, that the cryſtalline never changes its figure, but that it is moved to and from the retina, according to the diſtance or proximity of the object in view; and this alſo does equally well account for the diſtinct appearance of objects at all diſtances, as is evident from the laws of optics, as well as from the vulgar experiment of caſting the ſpecies of objects from abroad, upon a ſheet of white paper, by means of a lens placed at a hole in the window-ſhut of a dark chamber: For the picture will always be diſtinct, at whatever diſtance the object may be, provided that the paper be at a due focal diſtance behind the lens.

Thoſe that embrace the firſt opinion ſay, that the *ligamentum ciliare*, which ariſes all round from the inſide of that circle of the choroides where it joins the uvea, does, by its contraction, draw the edge of the cryſtalline, to which it is attached all round, towards that circle;

circle; and by that means makes it broader and flatter than before, when objects are at a diſtance from the eye; and that, when we view nearer objects, this ligament is relaxed, and the cryſtalline recovers its convexity by the elaſticity of its parts: And, to render this opinion ſtill the more probable, they contend that it is for this end that nature has made the outer part of this humour of a ſubſtance eaſily flexible and yielding, that it may with greater facility yield to the contraction of this ligament. But, if we obſerve accurately the ſituation of the *ligamentum ciliare*, we will find that it is ſuch as diſqualifies it for rendering the cryſtalline more flat by increaſing its breadth; for its fibres are not in the ſame plane with the cryſtalline, but have an oblique direction, as in *Fig.* 9. where C is the cryſtalline humour, *aCa* its tranſverſe diameter, *ao ao* the *ligamentum ciliare* (ſometimes alſo called the *ciliary proceſs*). Now, in order to draw out the cryſtalline into a broad flat figure, or, which is a juſter way of conceiving this matter, in order to draw out and extend its capſule, ſo as it may compreſs the cryſtalline into this figure, it ſeems neceſſary it ſhould be drawn according to the direction of the lines, *ab* and *ab*, which are in the ſame plane with the tranſverſe diameter of this humour *aCa*; but this cannot be performed by the *ligamentum ciliare*, becauſe its direction is oblique; and therefore it can never by its contraction change the figure of the cryſtalline. Nor is this opinion rendered more probable from the different ſubſtances of which the cryſtalline is compoſed:

It

It is indeed true, and has been obſerved by anatomiſts, that, though this humour be all very ſolid, in reſpect of the other humours of the eye; yet it is not all throughout of the ſame conſiſtence, being externally like a thick gelly, but internally, towards its center, of a conſiſtence equal to that of hard ſewet. This external ſoft part of the cryſtalline is by ſome reckoned to be about the third of its whole bulk; and, in fiſhes, this difference of conſiſtency is in a particular manner remarkable, who are therefore ſaid to have a double cryſtalline, the one very ſmall and ſolid, in the center of the other, which is larger, but of a ſofter and leſs ſolid ſubſtance. This little cryſtalline, which is as it were a nucleus or kernel to the other in whoſe center it is placed, is never found wanting in the eyes of fiſhes; and indeed in all animals, ſo far as has been obſerved, this humour is always much ſofter externally than towards its center. But it does not from this follow, that nature has thus ſoftened the external part of this humour, that its figure may be the more readily varied for ſeeing diſtinctly at all diſtances, but for another very wiſe and neceſſary purpoſe: For it is certain that the rays of light which fall upon the extremities of the cryſtalline, by reaſon of their greater obliquity, muſt be more refracted than thoſe which fall upon its middle, near its axis, by which means they will be made to meet at different diſtances behind the cryſtalline humour, theſe towards its extremity nearer, and theſe near its axis at a greater diſtance; ſo that it is impoſſible for all to be united exactly upon the

the retina, for rendering the ſight diſtinct: And therefore, to prevent this inconveniency, provident nature, which is never known to do any thing in vain, but always for the beſt purpoſes, has very wiſely, towards the center of the cryſtalline, made its ſubſtance more denſe and ſolid; that the rays of light that fall on the cryſtalline, near its axis, may, in paſſing this nucleus, have their refraction increaſed, and by that means may be made to converge, and meet at the ſame point with thoſe that paſs the cryſtalline towards its edge or extremity.

This is the reaſon why the cryſtalline of all animals is more ſolid in its center than externally, and why in fiſhes this difference is ſo remarkable; for in them this humour being ſpherical, as has been obſerved above, the rays that fall thereon, at ſome diſtance from its axis, by reaſon of their great obliquity, would be made to meet at a greater diſtance from the point of union of the other rays that paſs near its center, than in land-animals who have this humour lenticular; and therefore, to prevent this inconveniency, which would have rendered the ſight prodigiouſly indiſtinct, nature has provided them with that ſmall ſolid cryſtalline in the center of the other, whoſe denſity far exceeds that of the nucleus of land-animals.

All this might be demonſtrated mathematically; but, if a glaſs lens be covered with opaque paper in which there are two holes, one at the axis of the glaſs, and another towards its edge; and if this glaſs be placed in the hole of the window-ſhut of a dark room, ſo as to refract a beam of the ſun's light upon a ſheet

of

of white paper, placed at a due focal diſtance behind the lens, it will be found that the beam, that paſſeth the hole towards the edge of the lens, will cut the axis before the focus of the glaſs, and fall on the oppoſite ſide of the paper. From all which it is evident, that the different conſiſtency obſervable in the cryſtalline humours, does not prove that they are rendered flatter by the contraction of the ciliary proceſs, as ſome authors would perſuade us, but to diminiſh the refraction where the rays fall moſt obliquely, and thereby to diſpoſe them to meet in the ſame point with thoſe which paſs through its middle, which was abſolutely neceſſary for diſtinct viſion, unleſs the pupil had been much leſs than it now is, in which caſe our ſight had not been near ſo clear as it is at preſent. If it ſhould be ſaid, that the cryſtalline changes its conformation and becomes more or leſs convex, by the action of certain muſcular fibres that enter its compoſition, it is incumbent on thoſe who entertain this opinion to ſhew us theſe fibres. The cryſtalline, when dried, doth manifeſtly enough appear to be made up of many thin concentrical laminae or ſcales lying one upon another, of which Mr Lewenhoeck reckons there may be two thouſand in one cryſtalline from the outermoſt to the center, and every one of theſe ſcales, he faith, he hath diſcovered to be made up of one ſingle fibre, or fineſt thread, wound in a moſt ſtupenduous manner this way and that way, ſo as to run ſeveral courſes, and meet in as many centers, and yet not to interfere or croſs one another in any one place. In oxen, ſheep, hogs, dogs and

and cats, the thread ſpreads into three ſeveral courſes, and makes as many centres; in whales five, but in hares and rabbits only two; in the whole ſurface of an ox's cryſtalline, he reckons there are more than 12000 fibres juxtapoſited. But, for the better underſtanding the manner of this admirable piece of mechaniſm, I muſt refer to the cuts and deſcriptions in his works, and in the Philoſophical Tranſactions, No. 165. and 293. from which it will appear, that this diſpoſition is but ill qualified for changing the figure of the cryſtalline, and for adapting it to the diſtance of objects. But ſuppoſing it were otherwiſe, and that it could be made appear, that the diſpoſition is well fitted for that effect, I am afraid, it would not be ſo eaſy to prove thoſe fibres to be muſcular, and capable of contraction.

There is yet another argument againſt this hypotheſis of the cryſtalline's changing its figure, by means of muſcular fibres that enter its compoſition, which muſt not be omitted; and that is, that it has no viſible attachment or communication with any part of the body, but is kept in its place, by means of a membranous capſule, with which it has not the leaſt connection; whence it is, that when this capſule is opened, the cryſtalline eſcapes of itſelf, without the leaſt violence, as has been obſerved by Maitre-Jean, in his *Maladies de l'œil*, chap. xi. and by Dr Petit, in the *Memoires de l'Academie Royale, anno* 1730; who therefore make no ſcruple to affirm, that, of all the parts of our body, the cryſtalline is the only one that has no continuity with the parts adjacent, by any fibre, blood-veſſel, or nerve: And this opinion is very much ſtrengthened by a paſſage I find in Steno's *Canis carchariae diſſectum caput.* "Cryſtallini humoris propria tunica contenti" (ſays he, ſpeaking of this animal) "ſubſtantia "triplex erat, intima, centrum, centroque vicina loca occupans, dura, et ex lamellis compoſita erat, quae integrae, cryſtalli inſtar, diaphanae apparebant, ſectae vero, albae ſimul et "opacae evadebant; extima cryſtallini ſubſtantia, tunicae proxima, aquae inſtar diffluebat; "reliqua, ut centrum inter et tunicam, medium "locum invenerat, ſic etiam conſiſtentiae mediae erat, viſciditate ſua gluten aemulans. Solidus globus viſco ſuo circumdatus libere in aqua volvebatur." From theſe words it is plain, that the author, who was one of the moſt accurate anatomiſts in his time, diſcovered no attachment of the cryſtalline to its membrane or capſule, which, had there been any, could not eaſily have eſcaped his obſervation, where ſo much water ſurrounded the ſolid cryſtalline: And this will be ſtill more evident, if we conſider the following paſſage; from which it appears, that he had frequent opportunities of repeating the like obſervations. See his *Diſſectio piſcis ex canum genere*; where, ſpeaking of the cryſtalline in one of thoſe canine fiſhes, he ſays, "Cryſtallini humoris ſubſtantia triplex erat; media dura, et ex lamellis compoſita; huic undique adhaerens alia multum "glutinoſa; tertia tunicae proxima, omnino aquea, ſed et hoc piſcibus aliis plurimis datum eſt."

The famous Morgagni has alſo obſerved, that there is water in the capſule of the cryſtalline, not only in men, but in ſeveral other creatures,

tures, (Adverfar. vi. p. 90.), and yet he takes no notice of any attachment. But, of all the authors that have written on this fubject, Dr Petit feems to have carried his obfervations the furtheft; for he found this water not only in the human eyes, but in the eyes of dogs, cats, wolves, hares, rabbits, fheep, lambs, calves, oxen, horfes, turkies, ducks, &c.; but could never difcover the leaft attachment, though he feems to have been at a good deal of pains in fearching after it. See *les Memoires de l'Academie Royale, anno* 1730.

Had the cryftalline any continuity with its capfule, it is probable that Ruyfch's fubtile injections would have reached it; but we find he could never go further than its membrane, and that only by pufhing forward the blood in its veffels by the ceraceous matter, from which they became confpicuous, though the ceraceous matter itfelf could never be made to enter them, (*Ruyfch. Thefaur.* 2 *locul. arc.* 4.) Seeing then that the cryftalline has no vifible attachment or communication with any part of the body, it can never receive into its fibres any blood or fpirits; and confequently it cannot be adapted to the diftance of objects by the contraction of thofe fibres.

If any body fhould afk me, how it is poffible for the cryftalline to be nourifhed, without having fome communication with the neighbouring parts, from which it may derive blood and fpirits? To this I anfwer, That I fee no abfurdity in giving it a kind of vegetative life, and, in fuppofing that it draws nourifhment from the water in which it fluctuates, as Maitre-

Jean and Petit have fuppofed; and this may be the reafon, whence it is, that, when this water is wanting, as fometimes happens in morbid cafes, the cryftalline becomes dry and opaque, much like what it is when taken out of the eye, and dried, as Briffeau, Morgagni, and Petit have obferved.

The laft opinion concerning the change made in our eyes, is what we embrace, and confifts in the motion of the cryftalline, whereby the diftance betwixt it and the retina is increafed or diminifhed according to the different diftances of objects; fo that, at whatever diftance objects are placed, the retina is always at a due focal diftance behind the cryftalline.

Now the *ligamentum ciliare* is an organ whofe ftructure and difpofition excellently qualify it for changing the fituation of the cryftalline, and removing it to a greater diftance from the retina, when objects are too near us; for when it contracts, it will not only draw the cryftalline forwards, but it will alfo comprefs the vitrous humour lying behind it; by which compreffion it muft prefs upon the cryftalline, and pufh it forwards further from the retina. For underftanding which, let C (fig. 9.) be the cryftalline, and let the curve lines *ao*, *ao* reprefent the *ligamentum ciliare*; it is eafy to fee that, when this ligament contracts, it muft draw the cryftalline forwards in the direction of the right lines *aod*, *aod*; by which means this humour will be brought nearer the forepart of the eye *oo*. But this is not all; for the fibres, compofing this ligament or mufcular

procefs,

procefs, do not run in a ftraight line from their origin in the choroides to their infertion in the edge of the cryftalline, but by their inflexion form a hollow, behind which lies the vitrous humour, as reprefented in the figure; and therefore, when they contract, they muft come nearer to the ftraight lines *ao*, *ao*, by which means this concavity will become lefs, and the vitrous humour will be compreffed; which therefore muft, by preffing on the back of the cryftalline, pufh it forwards further from the retina, when we look at near objects, its axis all this while remaining the fame.

Plempius afcribes the difcovery of the ufe of this ligament, in changing the conformation of our eyes, to the celebrated philofopher and mathematician Johannes Keplerus; of which anatomifts need not be afhamed, it being only from mathematical principles, that the neceffity of any fuch change was ever difcovered. But, in explaining this matter, not only Kepler, but Plempius himfelf, feems to have fallen into a miftake; for they fuppofe that, by the contraction of this procefs, the fides of the eye are drawn inwards towards the cryftalline, by which means the eye is elongated, and the retina is pufhed back to a greater diftance behind the cryftalline, when objects are near; which is repugnant to the above-noticed fituation of this procefs, as well as to the hardnefs and inflexibility of the fclerotis of feveral animals. See *Plemp. Ophthalmogr. lib.* iii. *cap.* 9.

M. de la Hire denies this motion of the cryftalline, as well as all other changes made in the conformation of the eye, all whofe arguments have already been examined at ſome length, excepting thoſe taken from the ſtructure of the parts; which now we muſt conſider in ſo far as they have any relation to this above-deſcribed motion of the cryſtalline. This author maintains, that it is impoſſible the cryſtalline can change its ſituation, becauſe the ciliary ligament is not muſcular, and conſequently has no power of contraction: And of this opinion are likewiſe a great many anatomiſts, and, in particular, Hovius; but it appears that all of them have been led into this miſtake, by an unjuſt notion they have entertained about the colour of muſcles. Every body knows that our muſcles are generally of a red colour; but it does not from thence follow, that what is not red, is not muſculous: The muſcular fibres of the guts and ſtomach have ſcarce any thing of redneſs in their colour; and it is alſo certain, that the pupil does contract and dilate itſelf according as objects are more or leſs luminous, and yet none of the fibres which perform that action are in the leaſt red; whence it follows, that the fibres of the *ligamentum ciliare* are not to be deprived of a power of contraction, becauſe of a colour different from what generally obtains in other muſcles; nor are we to be ſurpriſed that ſo many accurate anatomiſts, after a careful examination of this proceſs, have not ſcrupled to affirm it to be truly muſcular.

On what has been ſaid, I ſhall now make a few obvious reflections by way of corollary. And,

1*ſt*, Seeing that the natural ſtate of the *ligamentum*

*ligamentum ciliare*, like that of all other muſcles, is a ſtate of relaxation, it is eaſy to ſee that the cryſtalline muſt then be as near to the retina as poſſible; whence it follows, that the eye is naturally diſpoſed to ſee diſtinctly only diſtant objects, and that that diſpoſition whereby it is fitted for the diſtinct viſion of near objects, ariſing from the contraction of this ligament, is a ſtate of violence introduced at the command of our will: For confirmation of which we might appeal to every one's experience, who we doubt not will acknowledge, that, when they are ſitting careleſsly, without attending to any object, nothing at an ordinary diſtance appears diſtinct, till a certain effort be exerted, which will be remarkably greater in proportion as the viſible object is nearer; and this alſo agrees perfectly well with that neceſſary connection and dependence that habit and cuſtom has eſtabliſhed betwixt the motions of the cryſtalline and certain correſponding motions in the axes of viſion, which makes it impoſſible for us to direct our eyes to any object, without at the ſame time giving them that diſpoſition that is neceſſary for ſeeing diſtinctly at that diſtance; for, as our eyes are naturally adapted for ſeeing diſtinctly only diſtant objects, and as that diſpoſition, whereby they are fitted to near objects, is a ſtate of violence that requires an effort greater or ſmaller as the object is nearer or further off; ſo the axes of our eyes are naturally parallel, which is the direction proper for diſtant objects: And, when they are directed to a near object, an effort muſt be exerted, which alſo will be greater or ſmaller,

ſmaller in proportion as the object is nearer or further off; which harmony and agreement of motions I eſteem a very great confirmation of this doctrine.

2*dly*, From what has been ſaid, we may clearly ſee, whence it is that our eyes are ſo ſoon fatigued in looking to near objects, which ſeldom happens when the object is at any conſiderable diſtance; for, when the object is near, an effort muſt be exerted, both by the muſcles of our eyes, for giving them the neceſſary direction, and by the *ligamentum ciliare*, for giving them the neceſſary conformation; which effort being always greater in proportion as the object is nearer, muſt be painful and laborious when the object is very nigh; whence ariſes that fatigue ſo often felt, in looking long at near objects; but, when the object is at any conſiderable diſtance, ſo great an effort is not required, eſpecially by the *ligamentum ciliare*, which by ſome experiments taken notice of above, is in my eyes totally relaxed at the diſtance of twenty-ſeven inches, whence at that, and all greater diſtances, no uneaſineſs can be felt, unleſs the object be too bright and luminous, but what ariſes from the direction of my eyes, which is very inconſiderable; and therefore my eyes are not ſoon fatigued in looking at an object, whoſe diſtance is greater than twenty ſeven inches; but, when the diſtance is leſs, they ſoon become ſenſible of an uneaſineſs, which, being proportionally greater, as the object is nearer, does ſoon require that the eyes be relaxed: and this is the true reaſon, why none

none of us are able to look long to a very nigh object.

*3dly*, From this also it is easy to understand, whence it comes to pass, that, after the eye has been very attentive, in considering an object at a certain determined distance, it cannot presently see another object distinctly, at a greater or less distance, though both objects seem to touch one another, being nearly in the same line: For, since the conformation of the eye must be fitted to the distance, some time will be required for finding out, by repeated trials, that precise disposition which is necessary for seeing the object at that distance; and therefore it must appear confused and imperfect till the eye has exactly adapted itself to the distance of the object.

*4thly*, This motion of the crystalline, whereby our eyes are accommodated to the distance of objects, being entirely voluntary and subjected to our mind, which, being a wise agent, wills its motion that objects may not appear confused, it follows, that, when by any other means this confusion is taken off, the mind will not then change the conformation of the eyes, unless there be something else that can influence it to such an action; and this, after many conjectures upon the matter, I take to be the true reason why the eye is not adapted to the distance of objects, viewed through a small hole made in a card; and why, when viewed through several small holes, whose distance from each other does not exceed the diameter of the pupil, they appear multiplied according

to

to the number of holes, as has been obſerved above.

*5thly*, Though this motion of the cryſtalline be ſubjected to our mind, which, when the object appears confuſed, changes its ſituation, till, by repeated trials, it finds out the preciſe place it ought to poſſeſs, for rendering our ſight as diſtinct as poſſible; and though this confuſion in our ſight ſeems to be the only thing that ſhould influence our mind to ſuch an action; yet, by reaſon of a habitual or cuſtomary connection that has grown up between the motions of the cryſtalline and correſponding motions of the axes of viſion, theſe motions come at laſt always to accompany one another, and that ſo neceſſarily as to make it impoſſible for us to ſeparate them by any act of volition: Thus, when we view any object at two feet diſtance, we not only accommodate our eyes to that diſtance, but we alſo move our eyes, ſo as their axis produced may meet in ſome point of the object; whence it comes to paſs, that theſe motions, which at firſt had no neceſſary connection or dependence on each other, do in time come to cohere ſo cloſely, as to make it impoſſible for us to direct our eyes to an object at two feet diſtance, without at the ſame time giving them that diſpoſition that is neceſſary for ſeeing diſtinctly at that diſtance; and what has been ſaid of objects at two feet diſtance, is alſo true of objects at all other diſtances within the limits of diſtinct viſion. Whence it is eaſy to ſee how our eye may be made to change its conformation, when an object is viewed through a ſmall

hole

hole made in a card, though, by reafon of the fmallnefs of the hole, the object appears always diftinct, even when the eye is not adapted to its diftance; for, feeing the motion of the cryftalline is, by cuftom and habit, made to follow a correfponding motion in the axes of our eyes, it follows, that, by changing the direction of our eyes, the eye muft alfo, at the fame time, accommodate itfelf to the diftance at which the optic axes meet. It is for this reafon, that, when a fmall body appears fingle, when viewed thro' two fmall holes, whofe diftance does not exceed the diameter of the pupil, it may be made to appear double, and if its diftance be fuch as makes it appear double, the diftance betwixt the appearances may be increafed or diminifhed, and all this only by changing the direction of our eyes.

*6thly*, This motion of the cryftalline, by which our eyes are adapted to the diftance of objects, has its limits beyond which it cannot go; whence it is, that none of us can fee diftinctly with the naked eye, but within certain limits, beyond which, if the object be placed, it muft appear confufed more or lefs, as it is further removed from the limits of diftinct vifion. Thefe limits are at different diftances, according to the difference of peoples eyes; and very often, in the fame man, both eyes have not the fame limits, which is oftentimes of the fame ufe, as if the limits of both eyes were more diftant from one another; for one may fee an object diftinctly enough with only one eye; but, if the neareft limit of one eye is further off than the furtheft theſt of the other, then near objects and diſtant objects may be ſeen diſtinctly, but the intermediate ones muſt appear confuſed, which is a caſe that is very apt to ſurpriſe ſuch as deſpiſe or neglect theory.

*7thly*, The *ligamentum ciliare* being the only inſtrument by which our eyes can be fitted for ſeeing diſtinctly at different diſtances, it follows, that, whatſoever affects the oeconomy and action of this ligament, muſt alſo affect our ſight. Thus, *1mo*, When it has become paralytic, no near object will appear diſtinct; for an example of which, ſee *Foreſti obſervationes*, lib. XI. obſerv. XXXVI. His words are, "Generoſus vir et Dominus de Banthuyſen, anno 1567, menſe Majo, cum ad eum accitus eſſem, conquerebatur ſe propius admota hebetius videre, longinqua vero optime," &c.——Quibus praeſidiis tandem hoc vitium ceſſavit," &c. Nor are we to imagine that the caſe here was only a common *viſus ſenilis*: For who is it that calls a phyſician on ſuch an occaſion? And as this diſeaſe never yields to medicine, it would not have been ſaid, "quibus praeſidiis tandem hoc vitium ceſſavit."

*2do*, If this ligament ſhould be convulſed, no diſtant object will appear diſtinct. We have a beautiful caſe to this purpoſe recorded by Timaeus, which I ſhall alſo ſet down in the author's own words. "Studioſus quidem juvenis" (ſays he) "queritur ſe menſae aſſidentem legere poſſe etiam minutis literulis conſignata, non tamen internoſcere ſi qui hominum conclave ingrediuntur, donec proximius accedant, longius vero diſſita plane ſe non cernere: Duravit iſte affectus effectus jam fere sesquiennio." Timæi Cas. medicinal. lib. I. cas. XXV. The author indeed resolves this case into a thickness and muddiness of the humours of the eye; but to me it seems more reasonable it should have been resolved into a contraction or spasm of the ciliary process, and if, by means of a concave glass of a due degree of concavity, distant objects could have been distinctly seen, of which the author has taken no notice, this would have served as a proof of our conjecture; for, on Timæus's supposition, as also on the supposition, that this symptom proceeded from a certain degree of insensibility in the retina, or immediate organ of sight, such a glass would have rendered the sight yet more dark and confused than before.

From this we may possibly see why, in hysteric and nervous cases, a certain dimness of sight is so frequently complained of; and in particular, why Piso's matron was seized therewith for an hour before her hysteric and convulsive paroxysm (*Piso de colluv. seros. obs.* XXV. *p. m.* 146.). For though this symptom may arise from a numbness or certain degree of paralysis, and insensibility in the optic nerve, yet as certain it is, that it may also proceed from a spasm in this muscular process; and, seeing both are equally possible, I see no reason for admitting the one and rejecting the other, without a very exact and impartial examination of all the phaenomena, which in such cases are commonly overlooked. The great Hippocrates, in many parts of his writings, has observed this dimness of sight to be the attendant, as well as the harbinger of spasms, and convulsive motions; whence it it ſeems reaſonable to ſuppoſe, that, in many ſuch caſes, it ſhould alſo itſelf be occaſioned by a ſpaſm in this proceſs, whereby the eye is diſqualified for ſeeing diſtinctly at an ordinary diſtance. But all this I propoſe only by way of conjecture, leaving it to be further examined by others, as occaſion ſhall offer. In the caſe of ſome poiſons, the matter ſeems ſomewhat plainer; for, as Nicander, Diaſcorides, and others, have long ago obſerved, that this dimneſs and obſcurity of ſight, together with ſpaſms and convulſions of various kinds, are the common conſequences of hemlock taken internally; ſo it ſeems pretty evident, from a paſſage in Ægineta, that this does not proceed from any degree of inſenſibility in the organ of viſion, but from a ſpaſm in this proceſs. See lib. V. cap. XLI. where, ſpeaking of this ſubject, the following words are very remarkable: "The ſight becomes ſo dim, that a man is wholly deprived of the ſight of diſtant objects." But I muſt go on.

*3tio*, If this muſcular proceſs ſhould be paralytic in one ſide, and ſound in the other, the cryſtalline muſt get an oblique ſituation, when we look at near objects, whence they will not appear diſtinct, unleſs the eye be turned aſide from the object. And,

*4to*, If this ſame proceſs is convulſed on the one ſide, while the other ſide is healthful, the cryſtalline will alſo get an oblique ſituation, but not unleſs we view diſtant objects, in which caſe alſo it will be neceſſary to turn our eye away from the object it would view, that its picture

may

may fall on the retina towards the axis of the eye, where it is moſt ſenſible. But,

*5to*, If in the one ſide it ſhould be convulſed, whilſt it is paralytic in the oppoſite ſide, the cryſtalline will always have an oblique poſition, at whatever diſtance the object may be placed; and therefore the ſtrabiſmus ariſing from this cauſe muſt be conſtant and uninterrupted: whereas, in the two former caſes, it only takes place in certain circumſtances. But, for the better underſtanding what has been ſaid on this and the two immediately preceeding heads, it may be proper to review what I have ſaid on the ſubject of the ſtrabiſmus in the former part of this eſſay; from which it will alſo appear, that, in all theſe obliquities of the cryſtalline, the object will not be ſeen in its proper place, where it is ſeen with the other eye, but will be thence tranſlated to ſome other place, from which tranſlation it muſt neceſſarily appear double.

*6to*, When this ligament has become rigid and ſtiff, the cryſtalline will have but very little motion, whence the limits of diſtinct viſion will be very narrow: Thus it is with all thoſe who are much employed in any ſubtile work, ſuch as engravers, jewellers, watchmakers, painters in miniature, &c. who are very apt to become ſhort ſighted from the conſtant application to ſmall objects, which cannot be diſtinctly ſeen but at a very ſmall diſtance; and therefore they are obliged, by the contraction of this ligament, to bring the cryſtalline as near to the uvea as poſſible; but all muſcles that continue long in the ſame ſtate become rigid

gid and ſtiff, and loſe much of their activity; and therefore this ligament, by its conſtant contraction, muſt at laſt ſhrink, and have its fibres ſhortened, which will keep the cryſtalline fixed in that ſituation, by which the eye is diſqualified for ſeeing diſtant objects diſtinctly. This has been obſerved by *Ramazzini, de morbis artificum, cap.* XXVI. who therefore judiciouſly adviſes all ſuch, from time to time, to intermit their work, and recreate their eyes with a diverſity of objects, leſt they ſhould loſe their mobility and become ſhort-ſighted. The reverſe of this diſeaſe happens to ſuch as are ſeldom employed in obſerving near objects, but who from their infancy have accuſtomed themſelves to look much to diſtant objects, ſuch as hunters, falconers, ſailors, &c. In thoſe this ligament is much relaxed, by which they can ſee at a great diſtance; but, by reaſon they are ſo little accuſtomed to obſerve near objects, it loſes much of its faculty of contraction, whence they cannot accommodate their eyes to near objects: And thus we ſee how the *viſus ſenilis*, as well as the myopia, may be acquired by uſe and cuſtom. But I muſt go on.

*8thly*, The eight and laſt reflexion I ſhall make on this ſubject ſhall reſpect the cauſe of this change of conformation of our eyes, which is either efficient or final.

As to the efficient cauſe, it has been already demonſtrated, that this lies in the *ligamentum ciliare*, which being muſcular, does by its contraction change the ſituation of the cryſtalline, according as objects are nearer or further off. But, leſt it ſhould be imagined, that our mind

does

does not preside over this motion of the crystalline, by reason we are so very little conscious of its influence, it must here be observed, that there are many other motions that are no doubt voluntary and depending on our mind, of which we are every bit as little conscious. No body denies but the mind presides over those muscles which tune the ear, and yet we are not conscious of their acting. The motions of the eye-lids are also all voluntary, though we are often insensible of them, and even in many cases cannot, by any act of volition, hinder them to move in a particular manner: Thus, when the eyes are turned up or down, the eye-lids always follow their motion, and keep at the same distance from the pupil; and if a body be hastily moved towards our eyes, they will shut without our being conscious thereof: Neither is it in our power to do otherwise, because we have accustomed ourselves to do so on the like occasions; for such is the power of custom and habit, that many actions which are no doubt voluntary, and proceed from our mind, are in certain circumstances rendered so necessary, as to appear altogether mechanical and independent on our wills; but it does not from thence follow, that our mind is not concerned in such motions, but only that it has imposed upon itself a law, whereby it regulates and governs them to the greatest advantage. In all this there is nothing of intrinsical necessity; the mind is at absolute liberty to act as it pleases, but, being a wise agent, it cannot chuse but to act in conformity to this law, by reason of the utility and advantage that arises from

from this way of acting: Thus the ear is tuned to different ſounds, the eye is accommodated to objects at different diſtances, the pupil is proportioned to the intenſity and weakneſs of light, while at the ſame time we are not conſcious of our having done any thing: And when theſe and ſuch like motions have become neceſſary in certain circumſtances, ſo as to render any other motion impoſſible, this does not make them mechanical and independent on our will, but only ſhews us, that the mind, which at firſt always acted from a principle of intereſt, comes at length to be determined by habit and cuſtom, without examining how far ſuch motions may be profitable or hurtful to us, or at leaſt without being ſenſible of any ſuch examination: And this is the only reaſon can be given why none of us are now able to move our eyes differently, though when children we were poſſeſſed of that power, and, with reſpect to the eye lids, the caſe is exactly the ſame; for, though we are not now at liberty to keep them open when any thing is haſtily moved towards our eyes, yet that this proceeds entirely from cuſtom, and not from any abſolute or mechanical neceſſity in the thing itſelf, is evident from this ſingle conſideration, that ſome may be found who can keep them open, though the organs ſubſervient to their motions are the ſame as in other men. Thus it was with the two Roman gladiators taken notice of by Plempius (Ophthalmogr. lib. I. cap. II.) who, being of uncommon fortitude and courage, had not accuſtomed themſelves on every trifling occaſion to ſhut their eye-lids for the defence of their eyes,

eyes, whence their motions continued arbitrary in the ſtricteſt ſenſe. Thus alſo it ſeems to have been with that excellent philoſopher Socrates, who, in the judgment of Apollo, was the wiſeſt man on earth; and yet, for no better reaſon than that, to accuſtom himſelf to patience, he was wont to ſtand for a whole day like a ſtatue, without the leaſt motion, not ſo much as of his eyes or eyelids, (*Gell. Noct. Att. Lib.* II. *Cap.* I).

But the matter does not ſtop here; our mind does not only preſide over the motion of the cryſtalline, and ſuch other motions as are commonly ſaid to be voluntary, but there is good reaſon to ſuſpect, that it extends its dominion and influence even over all the vital and natural motions.

Some very great philoſophers, and particularly Des Cartes and his followers, have been of opinion, that our mind always thinks, and yet this is what we are often very little conſcious of; and, if the mind can think without our knowledge, I ſee not why it may not alſo be allowed to exert its active power in the government of the vital and natural motions, without our knowledge or attention.

I know it has been alledged by Locke and others, That all the thoughts and operations of the mind muſt neceſſarily be attended with conſciouſneſs; from whence it may be argued, that the mind is not concerned with theſe motions, becauſe it is altogether inſenſible of its influence. But, without determining how far the thoughts and operations of the mind may or may not imply conſciouſneſs, which is a meta-

metaphyſical queſtion, I leave to be diſputed by thoſe who have better thoughts of that matter; it is ſufficient for my preſent purpoſe to have ſhewn in a few of many inſtances that might have been brought, that there are motions unqueſtionably voluntary and depending on the mind, which, by cuſtom and habit, have become ſo eaſy as to be performed without our knowledge or attention, and that ſo neceſſarily as to make it impoſſible for us, by any effort of mind, to hinder them from going on in their uſual manner. And, ſeeing every one's experience and obſervation may afford him many inſtances of ſuch motions as theſe, I would gladly know why the vital and natural motions ſhould be eſteemed of a different kind.

That they cannot be accounted for from mechaniſm alone, is very certain; for all the hypotheſes that have hitherto been, or may hereafter be invented, for explaining thoſe motions mechanically, do and muſt proceed upon the ſuppoſition of the poſſibility of a perpetual motion. By a perpetual motion, I mean an uninterrupted communication of the ſame degree of motion, from one part of matter to another, in a circle, ſo as perpetually to return undiminiſhed upon the firſt mover, which, in the preſent conſtitution of things, is repugnant to the laws of nature; for ſeeing 'all motions and changes made in the motions of 'bodies are always proportional to the impreſſed 'moving force;' and ſeeing all motions on this globe are performed in a reſiſting fluid, viz. the air, a conſiderable quantity of the motion

tion must be spent in the communication on this medium; and consequently it is impossible the same quantity of motion should return undiminished upon the first mover, which is necessary towards a perpetual motion. Moreover, the nature of all material organs is such, that there is no avoiding a greater or lesser degree of friction, though the machine be formed according to the exactest principles of geometry and mechanics, there being no perfect congruity, nor exact smoothness in nature: And this also must very considerably diminish the communicated force; so that, on both these accounts, it is impossible there should be a perpetual motion, unless the communicated force were so much greater than the generating force, as to recompence the diminution made herein by these causes; so that the impressed motion may return undiminished to the first mover: But, that being contrary to the above mentioned law of motion, it is clear that the motion must continually decrease, till it at last stop, and consequently there can be no perpetual motion in the present state of things: "Liquet ex "calculo mathematico," says the learned Dr Clarke, in *Rohault. phys.* § 1. *cap.* 22. "omnem "de motu perpetuo quæstionem eo redire, ut "pondus inveniatur seipso ponderosius, vel vis "elastica seipsa fortior, quod est absurdum.'

No body, now-a-days, that understands any thing of nature and philosophy, can so much as imagine that any animal, how abject soever, can be produced by mechanism, without any active immaterial cause; we all know there is nothing in the animal machine but an infinity

of

of branching and winding canals, filled with liquors of different natures, going the ſame perpetual round, which are no more capable of producing the wonderful fabric of another animal, than a thing is of making itſelf. Beſides, in the generation of an animal, there is a neceſſity that the head, heart, nerves, veins, and arteries, ſhould be formed at the ſame time; which can never be done by the motion of any fluid, what way ſoever moved: For the heart cannot move, unleſs animal ſpirits be ſent from the head through the nerves into it. The animal ſpirits cannot be derived into the heart, unleſs the blood be ſqueezed by the heart through the arteries into the brain; ſo that it is evident, that the head and heart, the arteries, veins, and nerves, muſt all be formed at the ſame time, and not ſucceſſively, if the animal is mechanically produced. But this is altogether impoſſible; for no motion of any fluid or fluids, howſoever diſpoſed, can form all theſe at the ſame inſtant; and we know all the internal mechanical actions of animals are performed by the force of their circulating fluids. From theſe and ſuch like conſiderations, it is evident, that an animal cannot be produced mechanically. This is too hard a problem to be ſolved from ſo few data as matter and motion; and it is doing penance to read the wretched accounts of the wiſeſt and moſt learned phyſicians and philoſophers on this head: Read but Des Cartes, who, by a bold, not to ſay impious attempt, was the firſt ſince Prometheus and Democritus's days, who endeavoured to make an animal; I ſay, read but

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Des Cartes, and you'll ſee how, in every ſtep, he contradicts the known laws of motion: And indeed the manner after which he will have them generated, is as much above the power, and beyond the laws of mechaniſm, as the true and genuine manner and method of their production is. Seeing then that the formation of animals does not depend on mechaniſm, but on the powerful influence and operation of ſome immaterial cauſe, whether of GOD himſelf, the ſoul of the animal, or of ſome other ſubordinate being, ſome vital principle, plaſtic power, or Archæus, concerns not the preſent queſtion: I ſay, ſeeing that animals are not at firſt produced by mechaniſm, but by ſome active immaterial cauſe, why, after that, ſo great concern ſhould be ſhewn to reduce all to mere mechaniſm, and to exclude an intelligent and active principle from having any ſhare in the government of thoſe motions on which life depends; and why it ſhould be thought that theſe motions ſhould never ſtand in need of new impreſſions from ſome ſuch vital principle as firſt ſet them a-going, I cannot ſo eaſily conceive.

If it ſhould be ſaid, that theſe motions do not depend on mechaniſm alone, but on mechaniſm joined with certain active powers or forces, imprinted by the Author of nature upon all the bodies of this univerſe, ſuch as are the powers of attraction and repulſion, by which indeed the greateſt part of the phænomena of nature are unqueſtionably produced; it is incumbent on thoſe who entertain this opinion, to explain particularly how theſe motions

tions are thus continued by these active principles, before they can expect we should believe them. Could one single instance be brought of any motion that goes on perpetually in this manner, without standing in need of new impressions from any active and intelligent cause, there might be some more pretence for this hypothesis; but, as no such instance is to be found, I see no necessity of having recourse to such a supposition, for explaining the vital and natural motions.

Is it not certain that these motions are powerfully influenced by the passions of fear, grief, joy, rage, &c.? And does not this clearly shew their dependence on the mind? The learned Dr Cheyne (on the English malady, p. 307.) gives the case of Colonel Townshend, who, for some time before his death, could at pleasure suppress all the vital motions, so as in all respects to appear dead, and yet by an effort, or some how, he could come to life again, and restore these motions. The case is indeed singular, but not at all to be accounted for, without allowing the mind to preside over the vital motions. But this is not all; for in some creatures the motion of the heart has been found so irregular, as to give cause for esteeming it altogether arbitrary in the common acceptation of the word. There is a remarkable instance of this recorded by Lister, which I shall set down in the author's own words. 'Denique (says he, speaking of the cochlea) cor 'nudatum, et oculis, ut hac sectione fit, plane 'objectum, modo sponte ad aliquam multa mo'menta, immo quadrantem horae, pulsare desiit;

'iterumque

"iterumque fponte fe ftrenue, et celeriter move-"bat. Ut certiffimum fit, cordis motum, in id "genus animalibus, etiam voluntarium effe, et "non mere naturalem, ut in homine reliquifque "fanguineis animalibus; fed pro earum lubitu "ad quodvis tempus fifti, aut moveri poffe, &c." Lifter de cochleis et limacibus, p. 38.

All thefe things being confidered, it feems probable to me that the mind does not only prefide over thofe motions commonly called voluntary, but that it is alfo conftantly employed in the government of all the vital and natural motions, which of themfelves would foon ftop, were it not for the influence and interpofition of this active principle. It feems to me further, that thefe motions in the beginning of life are altogether arbitrary, in the common acceptation of the word, and that it is only from ufe and cuftom that they have become fo neceffary as to make it impoffible for us, by any effort of mind, to hinder them from going on in their ufual manner. The above mentioned cafe of Colonel Townfhend feems inexplicable on any other hypothefis; but, when this is once granted, it admits of a moft eafy folution, it being no more difficult to conceive how, by repeated endeavours, we fhould get the better of that neceffity, whereby the heart moves, according to the laws the mind has originally impofed on itfelf, than it is to conceive how the mind can get the better of that neceffity whereby our eyes are moved uniformly. It is indeed no eafy matter to fquint, after that the uniform motions of our eyes have once been eftablifhed by habit and cuftom, yet we are taught by experience, that the thing is not altogether impoſſible; and ſeeing the neceſſity whereby the vital motions go on in a conſtant uniform manner is of the ſame kind with that neceſſity whereby our eyes continue their uniform motion, if the mind can get the better of the one, I ſee no reaſon why it may not alſo ſometimes get the better of the other, and for a time ſuppreſs all thoſe motions on which life depends: Nor is it more ſurpriſing that none of us are now able, by any act of volition, to retard, accelerate, or put a ſtop to the vital motions, than it is that we cannot now give our eyes different motions, nor move our eyes up or down, without at the ſame time moving our eye-lids.

It is a common obſervation, that the fœtus in the mother's womb paſſes almoſt its whole time in an unactive ſleepy ſtate; and even infants newly come into the world are ſeldom awake, but when either hunger calls for the teat, or ſome pain, (the moſt important of all ſenſations), or ſome other violent impreſſion on the body, forces the mind to perceive and attend to it: And, if we follow a child from its birth, and obſerve the alterations that time makes, we ſhall find that it comes to be more and more awake, thinks more and more of external objects, and at laſt begins to know the objects, which, being moſt familiar with it, have made laſting impreſſions. If all this be duly conſidered, perhaps ſome reaſon may be found for imagining, that in the beginning of life the ſoul is wholly taken up in regulating and governing the internal mo-

tions,

tions, which are yet difficult, by reason it has not been much accustomed to them; and therefore do require a close and constant application of mind, which draws off its attention from things external, whence the child must appear to be in an unactive sleepy state: But, as these motions grow easier, the mind by degrees attends more and more to external objects, seems less and less sleepy and unactive, till by use and custom these motions do in time become so easy as to be performed without our knowledge or attention, and that so necessarily as to make it impossible for us, by any act of the mind, to hinder them from going on in their usual manner; and then the mind being at full liberty to employ all its thoughts about other matters, the creature appears no longer sleepy and unactive.

It were easy to illustrate and confirm this doctrine by many familiar examples; observe but children when they first begin to walk, and you will find that the whole mind is employed in conducting the motions necessary for their progression, insomuch that if any thing shall divide the mind, and draw off its attention, they presently tumble down, by reason of the difficulty that attends the government of these motions, which cannot be rightly conducted, while the mind gives attention to any thing else; but, when use and custom have once made these motions easy and familiar, then they need but little attention, and allow the mind to employ its most serious and anxious thoughts about other matters. And what has been said of walking, is in some measure also true of

 speak-

ſpeaking, ſinging, playing on muſical inſtruments, and many other exerciſes, whoſe difficulty is only overcome by habit and cuſtom. Seeing then that there are many actions that in the beginning require ſo entire and undivided an application of mind, which nevertheleſs do in time become ſo eaſy as to need but little attention, I ſee not why it may not now, after all that has been ſaid on this ſubject, be allowed that the ſoul in the beginning of life is wholly taken up in the government of the internal motions, which, by uſe and cuſtom do in time become ſo eaſy as to be performed without our knowledge or attention, and that ſo neceſſarily as to make it impoſſible for us to hinder them from going on in their uſual manner. But I am wearied of this ſubject, upon which I ſhould not have dwelt ſo long, had it not been that I underſtood that ſome were greatly ſurpriſed that I ſhould have ſuppoſed in the former part of this eſſay, (*Med. Eſſ.* Vol. III. p. 258.) that 'the mind may poſſibly preſide over the vital and natural as well as the animal motions.' To take off which ſurprize, and to ſhow that the thing is at leaſt poſſible, I have been obliged to enlarge beyond what I intended. Let us now proceed to the final cauſes of this change of conformation in our eyes.

It has already been ſhewn in general, that, had the eyes continued invariably the ſame, there could have been no diſtinct viſion, but at one determined diſtance, either great, middle, or ſmall, according to the particular diſpoſition of peoples eyes: But that the ſeveral phænomena, ariſing from this defect in our eyes, and

and the benefits we receive from the change that happens in their conformation, may be the better underſtood, it may not be improper that I ſhould here briefly explain the phaenomena that attend ſhort and long ſight, to which the caſe before us is altogether ſimilar and analogous; this only excepted, that, in the myopia and *viſus ſenilis*, there is always ſome latitude of ſight; whereas, did not the cryſtalline change its ſituation, diſtinct viſion would be confined to one determined diſtance, and, with reſpect to objects at a greater or leſſer diſtance, the eye would be myopical or preſbytical.

By myopes, or people that are ſhort-ſighted, I do not mean ſuch as have a ſmall pupil or turbid humours, who on that account are alſo ſhort-ſighted, by reaſon that a ſufficient quantity of rays do not paſs to the retina, unleſs when the object is near; nor do I mean ſuch as have a weakneſs of ſight, proceeding from a certain degree of calloſity, paralyſis, or inſenſibility in the retina, by which the viſion is very faint and obſcure, unleſs when the object is very bright and luminous, or when, becauſe of its proximity to the eye, it ſends a greater number of rays to the retina: But by myopes I underſtand, ſuch as have the cornea and cryſtalline, or either of them, too convex, or that have the diſtance betwixt the retina and cryſtalline too great. From the diſpoſition of the eye it is plain, that,

*1mo*, The diſtinct picture of objects at an ordinary diſtance will fall before the retina, and therefore the picture muſt be confuſed on the retina itſelf; from which confuſion in the picture, the vision will also be confused and indistinct. Whence,

2*do*, In order to see distinctly, they are obliged to bring the object very nigh to their eyes; by which means the rays that are now more diverging are made to converge and meet at the retina, where a distinct picture will be made, from which the object will be distinctly seen.

3*tio*, They that are short-sighted never look attentively to those that speak to them; for, by reason of this defect in their sight, they cannot exactly observe the motion of the eyes of those that speak, which contributes greatly to explain their thoughts, and augment the force of their words; and therefore they are only attentive to their discourse, without fixing their eyes on any object. Pliny calls those who have big and prominent eyes *hebetiores*; but it is not to be supposed that this bigness in their eyes can any way impair their genius, or lessen their vivacity: But as those who have such eyes are commonly short sighted, they do not look attentively to those that speak to them; whence they may be thought to be more dull and stupid than others, for we commonly judge of peoples attention from the disposition of their eyes.

4*to*, Short-sighted persons need less light for seeing clearly than others, and can easily read the smallest print at the light of the moon, or in the twilight, when such as have good eyes, are not able to distinguish one letter from another: The reason of this is plain; for, when the object is near, more light enters the pupil, and acts more powerfully upon the retina than when it is at a greater diſtance; but they who are ſhort-ſighted muſt, in order to ſee diſtinctly, bring the object near to their eyes: Whence the impreſſions made on the retina by the rays of light, and the viſion that is cauſed thereby, will be ſtronger than in thoſe who cannot ſee diſtinctly but at a greater diſtance. But beſides this, in the myopia the pupil is very large, on which account alſo more light will enter the eye for rendering the ſight clear. That the pupil is large in myopical eyes, is a common obſervation; nor will the reaſon thereof be difficult to any one who ſhall conſider that, 1*ſt*, The natural ſtate of the pupil is a ſtate of dilatation, as is manifeſt from its being very large in faintings, and upon firſt waking, as alſo after death. 2*dly*, The cauſe of the contraction of the pupil lies in the mind, which wills the contraction of its orbicular fibres, either when the light is too ſtrong, or, when the picture is not diſtinct upon the retina. 3*dly*, When the ſight is perfect, that is, when it is neither too ſtrong or too weak; and more eſpecially when the ſight is weak, as in old men, all objects that are very near the eyes will appear confuſed; on which account, as well as on account of the quantity of light that enters the eye, the pupil will be contracted: Whereas in ſhort-ſighted people, near objects appear diſtinct, and therefore the pupil does only contract, by reaſon of the too great quantity of light that enters the eye, which alſo they can eaſily avoid, by retiring into a darker place, as every body commonly

commonly does, when the light is too ſtrong: And this is one reaſon why the pupil, which in children is very large, does always continue ſo in thoſe who are ſhort-ſighted, and who are not obliged to contract it for ſeeing more diſtinctly. But, in the *viſus perfectus*, and eſpecially in the *viſus ſenilis*, the pupil muſt become ſmaller and ſmaller by degrees; for, by reaſon of its frequent contraction for ſeeing near objects more diſtinctly, the orbicular fibres ſhrink, and become ſhorter; by which means the pupil becomes narrower, juſt as the fingers of work-people are much bended from the frequent contractions of the *flexores digitorum*.

5*to*, Myopes have their ſight mended by a concave lens of a due degree of concavity; for the refraction being here too ſtrong, in proportion to the diſtance of the retina from the cryſtalline, this refraction will be diminiſhed by the interpoſition of ſuch a glaſs, and the diſtinct image of the object will be made to fall upon the retina: But as ſuch glaſſes repreſent objects under a leſs angle, they muſt appear leſs than to the naked eye; which is what ſurpriſeth the moſt ſuch as are ſhort-ſighted, and who, for want of knowledge in optics, are prepoſſeſſed with the opinion, that objects are always beſt ſeen when they appear largeſt.

6*to*, Their ſight will alſo be mended, by looking through a ſmall hole, ſuch as that made by a pin in a card; for the little luminous pencils, which have for their apex a point in the object, and for their baſis the little hole,

hole, will, by reafon of their acutenefs, proceeding from the fmallnefs of the hole, take up fo fmall a fpace on the retina as to occafion but little confufion in the picture; whereas, without the interpofition of the perforated card, the luminous pencils will have a bafe as large as the pupil itfelf, and confequently muft occupy a much larger fpace upon the retina, from which the picture, and the vifion thereby occafioned, will be a great deal more confufed than when the object is viewed through the fmall hole. And this is the reafon why fhort-fighted people, to fee diftant objects more diftinctly, call to affiftance their eye-lids, which they fhut fo as to leave open only a very fmall flit, by which the confufion in the picture is in fome meafure corrected, and the fight is made more diftinct, though lefs clear.

*7mo*, Short-fighted perfons commonly become lefs fo, as they advance in years, and that becaufe the humours of the eye do daily wafte and decay; from which decay in the humours, the cornea fhrinks, and becomes lefs convex, and the cryftalline becomes flatter than before, by which means the rays of light will be lefs refracted, and will not meet fo foon behind the cryftalline; and therefore the image on the retina, and the vifion caufed thereby, will be more perfect and diftinct, and the eye will be enabled to fee at a greater diftance, than when the refraction was ftronger in the more plump and convex eyes; Whence we may fee, why very young children never take notice of any thing but what is clofe upon their eyes; for in them the cornea is too

convex

convex and prominent, to allow them to see distinctly, or attend to objects at an ordinary distance. This has been taken notice of by Des Cartes; and skilful painters seem to be well acquainted with it, who therefore, when they paint young children in profile, give the cornea an uncommon convexity.

8*vo*, Small lucid bodies, when at a considerable distance, appear great, round, and frequently full of spots. For understanding this, let H (fig. 10.) be the eye, and let the candle A be the object, which is here supposed to be at the distance of about sixty feet, and which by reason of its distance may be conceived as a point. The rays of light AB, AC, &c. will, after refraction in this myopical eye, converge and meet in a point before the retina as at o, and after that they will diverge and form on the retina the large image *de*, which will have the same figure with the pupil, and consequently will be round. From the extreme points of this image *d* and *e*, draw through the centre of the eye L, the right lines *d*LD, *e*LE: These lines will be perpendicular to the retina, at the points *d* and *e*; and consequently the object A will, by means of its luxuriant picture on the retina *de*, be seen under the angle DLE. If therefore, about the centre A, with the radius AD or AE, the circle ADE be described, the small object A will be seen uniformly extended over all that circle, and consequently must appear big and round.

Whence they that are skilled in optics will easily see that, 1st, When the small object A is at a very great distance, the appearance will alſo be very great. At ſixty feet diſtance, a candle commonly appears a luminous circle of about a foot diameter; but this will be greater or ſmaller, according to the degree of ſhortneſs of ſight, and magnitude of the pupil.

2*dly*, The nearer the object is, the appearance will be the leſs, and will approach the nearer to the natural figure of the object.

3*dly*, When two or more candles of unequal magnitude are ſeen at a great diſtance, they will appear equal, and, if they are not far from one another, their circular appearances will cut each other; thus a luſtre full of candles puts on the appearance of a globe of fire.

4*thly*, If the hand be gently brought before the eye, before that any one of theſe circular appearances are hid, ſome part of each circle will be made to diſappear; and this part that is made to diſappear will lie on the ſame ſide with the interpoſed hand; whereas in the *viſus ſenilis* it lies on the oppoſite ſide, as will be ſhewn below.

5*thly*, From this alſo it is eaſy to underſtand, why to all of us the ſtars appear larger than they ought; for the eye with reſpect to them being purblind, they are ſeen under a greater angle, and conſequently muſt appear greater, for the ſame reaſon that a diſtant candle appears ſo to a myops: Whence it is, that, when the fixed ſtars are viewed through a ſmall hole made in a card, they ſeem much leſs than when ſeen with the naked eye; for the luxuriancy of the image being in ſome meaſure corrected by the ſmallneſs of the hole in the card, the ſtars

themſelves

themſelves muſt neceſſarily appear ſmaller. And this alſo is one reaſon why the teleſcope, which increaſes the magnitude of all other objects, diminiſhes that of the ſtars; for this optical inſtrument does not here magnify ſo much by increaſing the angle under which they are ſeen, as it diminiſhes by uniting the rays at the retina, and by that means correcting the luxuriancy of the picture: Nor is it any ſolid objection to this, That the ſun and moon, with reſpect to the diſtance of both which no doubt the eye is alſo myopical, do appear larger when ſeen with a teleſcope than with the naked eye; for it muſt be obſerved, that, at a given diſtance, the luxuriancy in the image, proceeding from the rays not being accurately united at the retina, is always the ſame, and is not augmented according to the magnitude of the object; and therefore, in large bodies, ſuch as the ſun and moon, it bears but a very ſmall proportion to the true magnitude of the image, and conſequently, when this luxuriancy is cut off by the teleſcope, it makes no ſenſible detraction from the magnitude that ariſes from the augmentation of the viſual angle. And of the ſame kind with the appearance of the ſtars, is alſo the appearance of a diſtant candle, which in the night-time ſeems larger than it ought to moſt eyes; becauſe the eye, with reſpect to its diſtance, is ſomewhat purblind, and the pupil being then much dilated, muſt greatly increaſe the luxuriancy of the picture: But if this ſame candle be viewed in day-light, or even if it be viewed by night from a well-lighted room, or if a

ſlaſh

flaſh of lightning ſhould happen at the time it is viewed, it will not appear much bigger than it ought, by reaſon of the contraction of the pupil, which corrects the luxuriancy of its picture on the retina.

As for what concerns the ſpots that are ſo frequently obſerved by myopical eyes, theſe may proceed from ſome little extravaſations, varicous ſwellings, or other defects in the retina, which, by intercepting the rays, will occaſion a defect in the picture, and by conſequence, a ſimilar and correſponding defect or ſpot on the object. Theſe ſpots commonly vaniſh, or at leaſt become leſs ſenſible, when the object is brought nearer the eye, and within the limits of diſtinct viſion; for the rays, which are now exactly united upon the retina by being more crowded, have their force augmented; by which means, a ſenſible impreſſion is made upon the retina through theſe extravaſations which are too thin to intercept wholly theſe impreſſions, unleſs when they are weak and faint: And this is the reaſon why theſe ſpots are always moſt ſenſible to thoſe who have a ſmall pupil, and eſpecially to thoſe who are ſhort or long ſighted: From which alſo we may ſee, why in a preſbytical eye the ſpots, which were formerly very ſenſible, become ſo faint, when the object is viewed through a convex glaſs of a due degree of convexity; for by means of this glaſs more rays are made to enter the eye, which, being united exactly at its bottom, muſt ſtrike the retina ſtrongly enough to make a ſenſible impreſſion through theſe extravaſations, which

 will

will render the ſpots ſo faint and obſcure, as to be ſcarce any more obſervable. And what has been ſaid with reſpect to theſe ſpots, when occaſioned by ſmall extravaſations, or other defects in the retina that intercept the rays, will alſo hold, when they are occaſioned by a calloſity, or any degree of paralyſis or inſenſibility in ſome parts of the retina, by which the impulſe or ſtroke received from the rays is made leſs ſenſible.

But, beſides theſe defects in the retina, there is yet another cauſe which may give occaſion to thoſe ſpots both in the myopia and *viſus ſenilis*, and that is, certain ſmall opaque marks on the cornea itſelf, or any where within the eye, which, by intercepting ſome of the rays, muſt occaſion a defect in the picture, from which defect a kind of dark ſpot will be ſeen in the object. Thus in the eye of a myops, (See fig. 10.) if there is any opacity in the cornea, or within the eye, which intercepts the rays B*be*, C*cd*, and A1*a*, there will be a defect in the picture at *e*, *d* and *a*, from which alſo the external appearance will be deficient at the correſponding points E, D, and A, where, by conſequence, dark ſpots will be ſeen: For it is here to be obſerved, that there is not one point in the picture that is formed by a plurality of rays which conveen at that point, but every ray goes to a different point of the picture, both in myopical and preſbytical eyes; and therefore, when any of the rays are intercepted, that part of the picture to which ſuch rays belong, will not be illuminated, which muſt occaſion a correſponding defect in the

appearance

appearance of the object; but in the *visus perfectus*, where the rays that come from the several points of the object are so refracted as to meet again at so many corresponding points in the retina, every point of the picture is formed by a cone or pencil of rays, whose basis is the pupil; and therefore, tho' some of those rays be intercepted, yet no part of the picture will be darkened, and consequently no defect will be seen in the object from any such opacity in the cornea or humours of the eye, unless this opacity be in the back part of the vitrous humour, where the pencil is narrow, and intercepts the whole pencil.

If any desire an experimental proof of this, let them repair to a *camera obscura;* and having made some dark spots in the lens, by applying patches, or any such like small opaque bodies, let this glass be placed at a round hole made in the window-shut of the dark chamber for refracting the light which comes from an object without doors, so as its picture may be painted on a sheet of white paper, placed behind the lens; if this paper be at a due focal distance from the lens, no defect or spot will be found in the picture; but, if the distance of the paper be greater or smaller, than that at which the rays conveen, for making the picture distinct, as many dark marks will be seen in the picture, as there are spots in the glass: And it is only from this principle that any satisfactory account can be given how it comes to pass, that when, a small opaque body, that is, a body that is less than the pupil, is held close to the eye, before which several candles are placed at a great diſtance, ſuppoſe ſixty feet, if the eye attend to this ſmall object, or, which is the ſame thing, if it endeavour to accommodate itſelf to its diſtance, this object will be ſeen multiplied according to the number of candles, and will appear like a dark ſpot in each of the candles. This phænomenon ſeems very extraordinary to ſuch as have no knowledge in optics; but, from the principles already eſtabliſhed, it admits of a moſt eaſy ſolution. For, when the eye endeavours to ſee the ſmall object, it becomes myopical with reſpect to the candles; and therefore, on each of their pictures on the retina, the dark ſhade of the interpoſed body will be caſt; from which a correſponding dark ſpot muſt be ſeen in each of the candles: But, when the eye does not attend to this ſmall body, but is well diſpoſed for ſeeing the candles diſtinctly, this interpoſed body will not be multiplied, nor will any dark mark be ſeen in any of the candles, becauſe every point of their pictures is now compoſed of a cone or pencil of rays, which, after refraction, are made to conveen at that point; and therefore, tho' ſome of the rays belonging to each pencil are intercepted, yet every point of the picture will be ſufficiently, and indeed equally illuminated by the rays that are not intercepted; and conſequently no defect or obſcurity will be ſeen in any part of the andles, and the ſmall opaque body will itſelf ſeem perfectly diaphanous.

9no, Another phænomenon belonging to myopes is, that they read and write very ſmall characters, by reaſon that the viſual angle is enlarged by the proximity of the object: Whence

Whence alſo it is, that great characters fatigue their eyes, becauſe of the motion that is required to run over a word.

10*mo*, In reading, they generally hold the book towards the ſide of their head, that it may be ſufficiently illuminated and not darkened by the ſhade of their head.

11*mo*, No object being diſtinctly ſeen but what is very nigh, in order to ſee it with both eyes, their axes muſt be very converging; which ſituation of their eyes being painful and laborious, becauſe of the ſtrong effort that muſt be exerted by the adducent muſcles, they are oftentimes obliged to turn away one of their eyes; whence proceeds a double viſion, which, in reading, does frequently oblige them to ſhut one of their eyes, that they may avoid the confuſion that is occaſioned thereby.

12*mo*, There is yet another phænomenon which happens to all kinds of ſights, but it is commonly a great deal more ſenſible to thoſe that are purblind than to others, becauſe that in them the cornea is more convex and elevated, viz. they ſee an object that they do not look at, and they do not ſee this ſame object when their eyes are turned towards it. This ſeems a paradox in optics, but it is agreeable to experience; for having applied to the ſide of the head any thin black body, ſuch as the brim of a hat, ſo as it may abſcond the objects that are upon that ſide: Without moving the eye, turn the head with the black body that is fixed to its ſide, till you ſhall perceive a certain ſmall white object placed upon a black or brown ground; then keeping the head fixed n this ſituation, let the eye be directed to the object, and it will vaniſh.

This experiment is apt to ſurpriſe at firſt, but it is very eaſy to account for it from the conformation of the eye; for if AIK (Fig. 11.) be the eye, BL the black body placed near to the eye, and if O be the white object, the pupil *fc* being turned towards M; the rays which come from the object O, in paſſing by the black body BL, will fall obliquely upon the cornea at A, and will be refracted in the aqueous humour, ſo as to paſs through the pupil *fc*, and conſequently will fall upon ſome part of the retina; by which means the object O will be ſeen, though the eye is not directed to it: But if, without moving the head, the eye be turned towards O, it muſt move nearly upon its center H, from which the cornea and pupil will get the poſition given them in the figure; and therefore, though, even in this poſition of the eye, the rays that come from O, and that paſs over the extremity of the obſtacle at B, ſhould be ſuppoſed to fall upon the cornea, yet, by reaſon of the obliquity of their incidence, they can never be ſo refracted as to enter the pupil ED, but muſt be all loſt upon the uvea; from whence it is evident, that the object O will not be ſeen when the eye is directed to it.

I have done with the ſhort-ſight, and ſhall now ſhortly explain the phænomena that belong to the weak or preſbytical ſight. Preſbytæ, or weak-ſighted people, are ſuch as have the cornea and cryſtalline, or either of them, too flat, in proportion to the diſtance betwixt the cryſtalline and retina: From which fault in the conformation

formation of their eyes, it clearly follows, that,

*1mo*, The rays of light that come from the several points of an object at an ordinary distance will not be sufficiently refracted, and for want of sufficient refraction will not meet at the retina, but beyond it; and therefore the picture on the retina, and the vision caused thereby, will be imperfect and undistinct, more or less, as the object is nearer or further off. Whence,

*2do*, In order to read, they must remove the book to the distance of two or three feet; whereas in their youth they used to read at one foot distance. But,

*3tio*, As the picture on the retina, and the visual angle under which the object is seen are then lessened, and that in proportion as the distance of the object is increased; it is evident that small objects will not be seen well, even when their distance is such as is necessary for making their picture on the retina distinct. Whence it is, that sometimes they cannot read at all, especially if the characters be small, without the assistance of spectacles; which are still the more necessary, because that, when the object is at a distance, less light enters the eye, and consequently the picture, and the impression made on the retina thereby, will be fainter.

But, for a fuller explication of this point, and to show how small objects may become invisible to the naked eye, even when their picture is distinct upon the retina, it must be observed that there is a *minimum visibile*, and that all objects, however small, if at all seen, are seen of that bigness. For the retina being composed of small fibres, not unlike a piece of of plush, with the ends of the threads turned towards the cryſtalline, all the other ends of them being terminated in the brain, there can be no more diſtinct ſenſations than there are diſtinct threads to convey the impreſſion on them, and the eye will be incapable of diſtinguiſhing the parts of any picture that is no bigger than one of theſe fibres compoſing the retina; ſo that, if any object be ſo far removed from the eye, as to make the picture of it on the retina leſs than one ſingle fibre, that object becomes inviſible, if it be but of a dull radiation, becauſe of the weakneſs of the impreſſion made on the fibre; but, if it be of a very bright and powerful radiation, the whole filament is moved, by having one part of it powerfully acted on, and therefore the ſenſation is the ſame as if the object were much bigger, and did take up or cover the whole end of the filament: And this to me ſeems to be the reaſon why the ſtars appear all of the ſame bigneſs, and why, even to the naked eye, they appear many thouſand times bigger than really they are, and even as big as through a long teleſcope, which would not be, if our ſight were ſufficiently fine and nice. I have ſaid, that when an object is ſo far removed from the eye, as to make the picture of it on the retina leſs than one ſingle fibre, that object becomes inviſible, if it be of a dull radiation, by reaſon of the weakneſs of the impreſſion: For the ſtrength of the impreſſion, when the picture covers the whole end of the fibre, will be to its ſtrength, when it only covers a part of it, as the magnitude of the whole fibre is to the magnitude

nitude of that part of it that is taken up by the picture.

But there is yet another reason, why an object is not seen, when its picture is less than one single fibre, and that is, that this same fibre does not only receive an impression from this object, but it does also receive an impression from the extreme parts of the contiguous objects by which it is terminated; which, if they be of a bright radiation, must prevail over the other impression, and, by their prevalence, render the object itself invisible: Thus, if one of the fibres composing the retina, be supposed as big as *ao*, (*fig.* 12.) the small objects IE will, on the retina, make a picture betwixt *i* and *e*, and the contiguous objects OI and AE will on the same fibre *ao*, form a picture at *oi*; and *ae*, which being white will act more powerfully on the fibre *ao*, than does the picture of the small black object IE; and consequently this same object IE must become invisible, and the more bright and luminous bodies OI and AE must appear extended over all the space OIEA. The learned Dr Hook, by an easy experiment, found the *minimum visible* in most eyes to be comprehended within an angle of one minute, (See his posthumous works, p. 12. and 97.) Whence it is, that whatever is seen, is seen of that bigness, or under that angle: Thus every star that the eye discovers appears to be of the bigness of a minute at least, and so it is conceived really to be; though yet, when we come to examine its diameter by the help of a telescope, we really find it to be but some few seconds, or 60th parts of such an angle: And this also is the

the reafon why, if there be two, three, or a hundred fmall ftars fo near together, as that they are all comprifed within the angle of one minute, the eye has a fenfation of them all as if they were one ftar, and diftinguifhes them not one from another; becaufe all their pictures, falling upon the fame nervous fibre, make but one impreffion on the fenforium; which being ftrong and powerful, prevails over and deftroys the more faint and languid impreffion made by the picture of the interval that is betwixt them: And the cafe is exactly the fame when an old man removes the book to a very great diftance from his eyes; for, if the black lines forming the letters are feen under a lefs angle than a minute, they will be wholly obliterated by the more powerful impreffions of the white paper that terminates them.

And here by the way it may not be improper to obferve, that this experiment of Dr Hook's ferving to determine the *minimum vifibile* affords us a pretty certain proof of the magnitude of our nervous fibres: For if *ao* (Fig. 12.) be the end of one fingle fibre, the fmall object IE, which is here fuppofed to be bright and luminous, will, by means of its picture on the retina *ie*, move the whole fibre, and the appearance of the object will be the fame as if its picture were extended over the whole end of the fibre *ao*; and therefore, if, from the extreme points of the fibre *a* and *o*, the right lines *ax*A, *ox*O are drawn thro' the center of the eye *x*, thefe lines will be perpendicular to the retina at the points *a* and *o*, and confequently the fmall object IE will be feen under

the

the angle O$x$A; which angle being given, the angle $oxa$, which is equal to it (both being angles at the vertex $x$) will alſo be known, from which the diameter of the nervous fibre $ao$ may eaſily be found. Thus, if the angle O$x$A be one minute, as Dr Hook found it in moſt eyes, though there were ſome that could ſee to the third of a minute, the angle $oxa$ will alſo be one minute, which is the 60th part of a degree, or the 21,600 part of a circle: Whence, if the eye be ſuppoſed to be one inch diameter, or three inches in circumference, the diameter of the nervous fibre $ao$ will be the 21,600 part of three inches, or the 7200 part of one inch, which is the 600 part of a line; and, if ten hair-breadths make a line, which is pretty near the truth, its diameter will not exceed the 60 part of the diameter of a hair: Whence the nervous fibres themſelves will be no bigger than the 3,600 part of an ordinary hair. And if it be ſuppoſed that one can ſee under an angle that is no bigger than the third of a minute, as Dr Hook found that ſome could do, then the bigneſs of the nervous fibres compoſing the retina will not exceed the 32,400 part of an ordinary hair, which is a ſurpriſing and almoſt inconceivable ſmallneſs; and the more, becauſe each of theſe fibres are ſuppoſed hollow canals or tubes in which the ſpirits flow. How fine and ſubtile muſt then the ſpirits themſelves be? But this is not all; for, if birds can ſee diſtant objects as well as man, which ſeems very probable, becauſe of the facility which they have, in returning to places at a great diſtance from which they had gone,

gone, and especially becaufe birds of prey are obferved to perceive very fmall animals at a great diftance; I fay, fuppofing that birds fee objects at a diftance as well as we do, it is neceffary that the fibres which compofe their optic nerves and retina be much more fine and delicate than in men; for, fince their eyes are fmaller than ours, the image of objects on the retina will alfo be fmaller: Whence it is manifeft, that a fimilar conformation of the humours is not alone fufficient to make an equal perfection in the fight: For inftance, an eye of two lines diameter, (than which there are many fmaller) which has the humours of a fimilar or like figure to thofe of a human eye, whofe diameter is an inch, can never fee objects at a great diftance as diftinctly as we do, unlefs the organ of fight on which the pictures of objects are received be 36 times finer and more fenfible than it is in our eyes: For the picture of the object will be 36 times fmaller in their fmall eye than in ours, the furfaces of the globes of their eyes being to one another as 1 is to 36. And therefore, if the nervous fibres of our retina do not exceed the 32,400 part of a hair, in animals whofe eyes are only two lines diameter, they will be no bigger than the 1,166,400 part of an ordinary hair, which is truly a prodigioufly furprifing and almoft incredible fmallnefs; and yet, upon calculation, it is as certain as any propofition in Euclid, that they can be no bigger, if we allow them to fee objects at a diftance as diftinctly as men do. But I muft go forward.

*4to*, They who are long-fighted require more light than others for enabling them to read; for being obliged to remove the book to a confiderable diftance, that the rays which come from the feveral points of the object may meet again in fo many correfpondent points on the retina, without which there can be no diftinct vifion, lefs light will enter the eye, and the impreffion made thereby on the retina will be too faint, unlefs the object be more ftrongly illuminated, than what is neceffary either in the fhort or perfect fight; in both which, the proximity of the object does in fome meafure recompenfe its obfcurity. Add to this, that in the prefbytical eye the pupil is always fmaller, on which account alfo more light will be required for making a fufficient impreffion on the retina. And this is the reafon why,

*5to*, The prefbytical eye receives greater benefit from the ufe of a convex lens, than the eye of a myops does from one that is concave: For the property of fuch glaffes being to collect the rays, more of them will be made to enter the pupil; and, as in fuch eyes the refraction is too weak, the rays which flow from a point at an ordinary diftance, and which, for want of fufficient refraction, do not concur at the retina, but at fome place beyond it, will, by means of this glafs, be made to meet at the retina: Whence they that are long-fighted muft receive a double advantage from convex glaffes; for by them the picture is not only diftinct upon the retina, but is alfo as ftrong and lively as if the pupil had been much larger.

*6to*, Long fighted people fee more diftinctly through a fmall hole, fuch as that made by a pin in a card; for by this hole the picture will be rendered more diftinct upon the retina, and that for the fame reafon that has been given, why in a myops the fight is mended, by looking through a fmall hole: But, as part of the light is intercepted by the card, and as long-fighted people require more light than others for rendering their fight equally clear, they will not reap fo much advantage from the interpofition of the perforated card as thofe who are fhort-fighted, and who, by reafon of the proximity of the object, can eafily read in the twilight, when fuch as have not that defect in their eyes cannot diftinguifh one letter from another.

*7mo*, They who are long-fighted commonly become more fo as they advance in years; for the cornea and cryftalline become flatter and flatter continually, becaufe of the daily wafte and decay that happens in the humours of the eye; whence at laft they cannot fee at all without the affiftance of fpectacles, which fupply the refraction that is wanting in the eye, by rendering the rays converging, which can never be done by the alone pofition of the object from which they proceed: For, if it is near, they enter the eye diverging; and, if it is far off, they enter nearly parallel But, though the prefbytical eye does commonly become more and more fo by degrees; yet fome have been found who at laft recover their fight again, and have no further occafion for their glaffes to enable them to read. There are many caufes which

which may produce this effect, but to me it seems probable that it should chiefly arise from a decay of the fat in the bottom of the orbit; whence the eye, for want of the usual pressure at its fund, is, by the pressure of the muscles and fat towards the sides of the eye, reduced to an oblong figure, by which the retina is removed to a due focal distance from the crystalline. From this it is easy to see how, from a contrary cause, the sight, which was perfect till about the twentieth or twenty-fifth year of their age, does in some for a certain time after that become more and more myopical by degrees: For if at this time the muscles of the eye become bigger and more fleshy than before, or if the fat should be collected in greater plenty towards the side of the eye, the eye will, by reason of the pressure on its sides, be reduced to an oblong figure, and the retina will be pushed back to too great a distance from the crystalline, which obliges them to bring the object they would see distinctly nearer to their eyes, that its picture may be distinct upon the retina; whereas, before that, they used to read at an ordinary distance.

*8vo*, In the presbytical or weak sight, as well as in that which is perfect, the eye is more sensibly affected, and suffers more by great light than when the sight is myopical with the same opening of the pupil; for the luminous bodies that surround us, and which are not very near us, send rays into the eye, which in the *visus perfectus* are brought together, and united upon the retina, and make but a very small base in the presbytical eye; whence the

 impression

impreſſion made on the retina will be ſtrong and lively in both theſe eyes, and muſt therein cauſe ſome pain or uneaſineſs, which does not happen in the myopical ſight, becauſe theſe ſame rays make a larger baſe on the retina; for, all things being equal, the myopical eye always ſees objects more confuſedly than does either the perfect or preſbytical eye, and this confuſion is cauſed by the ſpace which the rays that come from each point of the object occupy on the fund of the eye. And this by the by is another reaſon why the pupil, which in children is very large, continues more ſo in thoſe who are ſhort-ſighted, than in thoſe whoſe ſight is either perfect or weak, and who, by reaſon of the too ſtrong impreſſion made upon the retina by bright and luminous objects, are obliged to contract the pupil, for keeping out a part of the light.

*9no,* To a preſbytical eye, ſmall lucid objects, ſuch as a lighted candle, or a ſmall hole of a line or two diameter, which is luminous, becauſe of the light which paſſes it, appear big and round; and, if the rays on either ſide of the pupil be intercepted by the interpoſition of any opaque body, the oppoſite ſide of the appearance will be hid, and the opaque body itſelf will appear as if it were on the contrary ſide to that where it really is.

This phænomenon will, no doubt, ſeem very extraordinary to ſome; for ſince we always judge that objects are in a contrary poſition to that of their pictures on the retina, in the preſent caſe it would ſeem that the order of nature is inverted, for by this experiment we ought

ought to conclude, that the object forms its picture on that side of the retina on which it is, which is altogether contrary to all the laws of optics, and to all our other experiences.

For explaining this phænomenon, see fig. 13. where the candle A is the small luminous object, BC*de* the eye, and *a* the point of concourse, where the rays AB, AC, &c. that come from the point A, are united behind the retina; these rays being cut by the retina before their reunion, will thereon form the large image *ed*, which, by reason of the round figure of the pupil, will also be round, whence the candle itself will likewise appear round, and of a bigness answerable to the bigness of its luxuriant image; for, by means of the impression at *e*, it will be seen at E in the right line *ex*E, which being drawn through the center of the eye *x*, is perpendicular to the retina at the point *e*; and, by means of the impression at *d*, it will be seen at D in the right line *dx*D; which being drawn through the center of the eye *x*, is also perpendicular to the retina at the point *d*, and by means of the other impressions made by the other rays forming the circular picture *ed*, it will be seen in the other points of the circle AED described about the center A, with the radius AD or AE, and consequently will appear big and round: And, if, by the interposition of the opaque body F, the ray AC be intercepted, there will be a defect in the picture at *d*, and consequently a similar defect in the appearance at D; and therefore, if this same body F be slowly moved from C to *f*, this defect in the picture will, by degrees, extend itſelf from *d* to *o*, by which a ſimilar defect in the appearance will be made to extend itſelf from D to A, inſomuch, that when the extremity of the opaque body F has, by moving from C to *f*, come to *f*, the half of the picture at *do*, and the correſponding half of the appearance DA will be made to vaniſh, and the candle will get a ſemicircular appearance at AE: Whence it is eaſy to ſee, that the opaque body itſelf F muſt appear as if it were on the contrary ſide to that where it really is; for, being moved from C to *f*, it will have the appearance of a dark ſhade that is moved from D to A.

10*mo*, The laſt phænomenon that I ſhall notice is the little ſpots or marks that long-ſighted people are ſo liable to ſee before their eyes. I have already inquired into the cauſe of theſe ſpots, and have ſhown that, when the eye ſees diſtinctly, no ſpots will appear, unleſs there be ſome defect in the retina itſelf: But both in the *viſus ſenilis* and myopia, certain dark ſpots or marks will alſo be ſeen, when there are any ſmall opaque marks on the cornea, or any where within the eye, that intercept ſome of the rays in their paſſage to the retina; whence ſuch eyes may be ſaid to ſee all their own defects, which does not commonly happen when the eye ſees diſtinctly, becauſe then the rays that come from a point in the object are exactly united in a correſponding point in the retina; and therefore, though ſome of them be intercepted, yet that point will be ſeen by means of thoſe that paſs; yet, if any ſuch opaque ſpot be in the back part of the

the vitrous humour where the pencil is narrow, and intercept the whole pencil, the corresponding point of the object will be darkened. These spots or marks are not all of the same kind. There are some which may be called fixed and permanent, because they do not change their place with respect to the axis of vision, and these must proceed from some defect either in the retina or cornea, or in the vitrous and crystalline humours. Others there are which may be called fluctuating and inconstant, because they are in constant motion, and change their place continually; and these must arise from some corpuscles floating in the aqueous humour, which, being thin like water, does not hinder them from changing their situation; but, whether they are fixed or moving, they must always appear like dark marks or defects in the object, and that as well when they proceed from opaque spots on the cornea, or any where within the eye, as when they proceed from the above observed defects in the retina itself.

And this leads me to explain another kind of spots that are very common in the presbytical sight, and which are not dark and shady, like those that have been already accounted for, but more bright and luminous than the object itself before which they appear. These spots appear best in looking to bright objects at a considerable distance, and are always of the same colour with the object before which they are seen. In the middle, their colour is clear and strong, which is surrounded by a dark and shady border, not unlike the knots of a polished ed fir-board. They are commonly accompanied with certain irregular veins that proceed from each ſpot, and which, as well as the ſpots themſelves, change their order and diſpoſition. Theſe veins are alſo of the ſame colour with the object, and being bright and luminous in the middle, are likewiſe terminated by a dark and obſcure edge, as may be ſeen at fig. 14. Theſe ſpots and veins do not always remain in the ſame place, but change their poſition with reſpect to the axis of viſion, according as the eye is differently moved, being ſometimes in the axis of viſion itſelf, and at other times to the right or left of this ſame axis; but, when the eye is kept fixed in the ſame direction, they commonly deſcend gradually.

As for what concerns the cauſe of theſe ſpots and veins, it ſeems evident, that, *1mo*, They muſt proceed from ſome corpuſcles within the eye, which are at liberty to change their place, and which therefore muſt be ſuppoſed to float in the aqueous humour. *2do*, Seeing theſe ſpots do always deſcend when the eye is kept fixed and immoveable, the corpuſcles from which they ariſe muſt aſcend, and conſequently are lighter than the aqueous humour in which they ſwim. *3tio*, Theſe ſpots being more bright and luminous than the object itſelf before which they are ſeen, they cannot be occaſioned by any opaque corpuſcles, which, by intercepting the rays, do caſt a ſhade upon the retina. For from ſuch corpuſcles the ſpots would appear like defects or dark marks on the object, as has been ſhown above. What therefore ſeems

to

to bid faireſt for producing theſe ſpots and veins, is ſmall, oily, diaphanous particles and filaments, that ſwim in the aqueous humour before the cryſtalline; for ſuch particles and filaments, by reaſon of their lightneſs, will mount upward, when they are left to themſelves, and are not diſturbed by the motion of the eye, and, by their greater refractive power, they will produce theſe luminous ſpots and veins, terminated by dark and ſhady borders. That oily and ſulphureous ſubſtances, though leſs denſe than water, have a ſtronger refractive power, is evident from the obſervations of the incomparable Newton, who, in his admirable treatiſe of optics, has given us an exact table ſhewing the refractions of almoſt all kinds of bodies, by which it appears, that the refractive power of unctuous and ſulphureous ſubſtances is two or three times greater, in reſpect of their denſities, than the refractive powers of glaſs and other terreſtrial alkalizate concretes; and that rain-water, with which the aqueous humour may be ſuppoſed to agree, has a refractive power, in a middle degree between thoſe two ſorts of ſubſtances. From which it follows, that the rays of light which paſs through theſe oily particles, ſuffer a much greater refraction, and by conſequence will meet ſooner behind the cryſtalline, than the rays that paſs by and do not meet with ſuch particles: Whence, if the conformation of the eye be ſuch as renders it preſbytical, the rays of light that come from the ſeveral points of the object will not converge to ſo many other points in the retina, but behind it, by which the picture on the retina,

and

and the viſion cauſed thereby, will be rendered more dark and obſcure; but the rays which paſs through theſe oily grains and filaments, by having their refraction increaſed, will be made to meet nearly at the retina, where they will form ſmall luminous ſpots and veins, ſurrounded with dark and ſhady borders; juſt as a convex glaſs, when expoſed to the ſun, forms its luminous focus in the middle of a very ſtrong ſhade with which it is environed. For, as light is not generated whenever we ſee it increaſed, it is by robbing ſome other part of its light, or by bringing the light that ſhould have been diffuſed over ſome other part to the more enlightened place. When therefore the rays of light that paſs through theſe oily particles and veins, are ſo refracted as to conveen at the retina, and paint thereon ſmall luminous ſpots and veins, theſe ſpots and veins will be terminated by a dark and ſhady edge, becauſe the light which ſhould have illuminated the edge is now made to fall on the luminous picture: And this you ſee how, in the preſbytical eye, ſmall luminous ſpots and veins, encompaſſed with dark and ſhady borders, may be painted on the retina, and how, from ſuch pictures, ſimilar and like ſpots and veins will be ſeen moving before the object.

From what has been now ſaid concerning the phaenomena that are peculiar to the ſhort and long ſight, it is eaſy to deduce the many advantages that accrue to us from the motion of the cryſtalline humour; for it being by the motion of this humour that our eyes are fitted

for

for feeing diftinctly at different diftances, had we been denied the power of changing its fituation, there could have been no diftinct vifion, but at one determined diftance, either great, middle, or fmall, according to the particular difpofition of our eyes; and, with refpect to all objects at a greater or leffer diftance, the fight would have been myopical or prefbytical, and confequently would have been liable to all the fymptoms, defects, and inconveniencies above explained. But,

2*do*, Befides the advantage we receive from the mobility of the cryftalline, in enabling us to fee diftinctly at different diftances, there is yet another taken notice of by the greateft part of our optical writers, which confifts in enabling us to judge with more certainty of the diftance of objects.

There are fix things whereby we are enabled to difcover the diftance of objects, all which I have promifed to explain, in order that it may thence appear, that, when with one eye, the other being fhut, an object is viewed through fmall holes made in a card, we can fcarce form any judgement with refpect to its diftance, but what is founded on prejudice and anticipation, as has been affirmed above.

The firft mean whereby the mind judgeth of diftance, confifts in that difpofition of the eye that is neceffary for feeing diftinctly at different diftances. We have already demonftrated, that there can be no diftinct vifion, unlefs the rays of light, which are fent from the feveral points of the object, be, by the humours of the eye, refracted and brought together

ther in fo many correfponding points on the retina; and that the fame conformation in the eye is not able to perform this effect, but muft be changed by the contraction of the *ligamentum ciliare*, which being fenfible to us, becaufe it depends upon our mind which regulates it, will enable us in fome meafure to judge of diftances even with one eye; as for inftance, when I view an object at feven inches diftance, I diftinguifh its diftance by the difpofition of the eye, which at that diftance is not only fenfible, but in fome fort uneafy: And when the fame object is viewed at twenty feven inches diftance, the diftance is ftill perceived, becaufe the neceffary difpofition of the eye is ftill fenfible, though no longer uneafy. And thus you fee how, with one eye alone, we judge of leffer diftances from the change which happens in the conformation of the eye; but, as this change in the conformation of the eye has its limits beyond which it cannot go, it can be of no ufe in affifting us to judge of the diftance of objects placed without the limits of diftinct vifion, which in my eyes reach no further than from feven to twenty-feven inches: But, as the object does then appear more or lefs confufed, according as it is more or lefs removed from thefe limits, this confufion fupplies the place of the motion of the cryftalline, in aiding the mind to judge of the diftance of the object, it being always efteemed fo much the nearer or further off, by how much the confufion is greater; but this confufion has its limits alfo, beyond which it can never extend: For when an object is placed at a certain diftance diſtance from the eye, to which the breadth of the pupil bears no ſenſible proportion, the rays of light that come from a point in the object, and paſs the pupil, are ſo little diverging, that they may, in a phyſical, though not mathematical ſenſe, be looked on as parallel; and therefore the picture on the retina will not to ſenſe become more confuſed, though the object be removed to a much greater diſtance. What this diſtance is to which the diameter of the pupil bears no ſenſible proportion, authors are not agreed on, nor ſhall I at preſent take upon me to determine; but, conſidering the ſmallneſs of the pupil, it is obvious, that it cannot reach any far way; and conſequently this confuſion in the appearance of objects can only aſſiſt us in judging of ſmall diſtances.

The *ſecond* moſt univerſal, and frequently the moſt ſure mean we have for judging of the diſtance of objects, is the angle made by the optic axes at that part of the object on which our eyes are fixed: For our two eyes are like two different ſtations in longimetry, by the aſſiſtance of which diſtances are taken, as hath been explained in the former part of this eſſay. And this is the reaſon why thoſe that are blind of one eye do ſo frequently miſs their mark in pouring liquor into a glaſs, ſnuffing a candle, and ſuch other actions as require that the diſtance be exactly diſtinguiſhed; of which Mr Boyle has given ſeveral inſtances, in his obſervations upon the vitiated ſight.

The *third* mean for judging of the diſtance of objects conſiſts in their apparent magnitudes, or the magnitude of their image painted upon the retina. The diameter of these images does always diminish in proportion as the distance of the object they represent does increase; and therefore, from this change in the magnitude of the image, we easily judge of the distance of objects, as often as we are otherwise acquainted with the magnitude of the objects themselves. And this is the reason why painters always diminish the magnitude of objects in their pictures, in proportion as they would have them appear at a greater distance. But, as often as we are ignorant of the real magnitude of bodies, we can never, from their apparent magnitude, or the magnitude of their image on the retina, form any judgment of their distance; and this is one reason why the stars do all appear of the same distance, though it is certain that some are vastly nearer than others. For there are an infinity of objects whose distance we can never perceive, because we are entirely ignorant of their magnitude.

The *fourth* thing whereby we judge of the distance of objects, is the force wherewith their colour acts upon our eyes: For, if we are assured that two objects are of a smaller and like colour, and that the one appears more bright and lively than the other, we judge by experience that the object that appears most bright is nearer than the other.

There are some who, in explaining this matter, would have us believe, that the force wherewith the colour of objects strikes our eyes must decrease in a reciprocal duplicate proportion of their distances, because the intensity

tensity and vigour of light does always decrease in that proportion. That the intensity or vigour of light does always decrease in a reciprocal duplicate proportion of the distances from the radiant point, I readily acknowledge; for, since the light is diffused spherically, like rays drawn from the centre to the circumference, its intensity at any given distance from its centre of activity will be proportional to the density of its rays at that distance; and therefore, if A (fig. 15.) be any radiant or visible point, and if ABE, ACF, ADG, &c. represent the rays flowing spherically therefrom, the rays, which at the distance AB are diffused through the spherical surface BCD, at the distance of AE, are dispersed through the whole spherical surface EFG; but the density of any given quantity of rays is reciprocally as the spaces they occupy; that is, if the surface EFG be double the surface BCD, the rays at the surface BCD will be twice as thick or dense as the same rays at the surface EFG; and, if the surface EFG be triple the surface BCD, the rays at BCD will also be three times denser than the same rays at the surface EFG; and universally, whatever proportion the surface EFG has to the surface BCD, the same proportion will reciprocally obtain betwixt the density of the rays at the surface CD and the surface EFG: But (as is manifest from *Archimedes de sphera et cylindro*) the surfaces of spheres are in a duplicate proportion of their diameters or radii; and therefore the thickness or density of the rays, at the distance AB, is to their density at the distance AE, in a reciprocal duplicate proportion of the ſemidiameter or diſtance AE to the ſemidiameter or diſtance AB: But, as has been already ſaid, the vigour or intenſity of light, in any given diſtance, is always as the denſity of its rays at that diſtance; and therefore the intenſity of light at any diſtance as AB, will be to its intenſeneſs at any other diſtance, as AE, in a reciprocal duplicate proportion of the diſtance AE to the diſtance AB; that is, as the ſquare of AE is to the ſquare of AB.

But, though the intenſity and vigour of light does thus decreaſe in a reciprocal duplicate proportion of the diſtances from the radiant point, it does not from thence follow, that the force wherewith objects act upon our ſight does alſo decreaſe in the ſame proportion; and that for this obvious reaſon, *viz.* That as the intenſity of light decreaſes by the diſtance of the object, ſo does the magnitude of the image upon the retina alſo decreaſe in the ſame proportion; and therefore this image will be as ſtrong and lively, and will act as powerfully upon the retina, when the object is at a diſtance, as when it is near; and conſequently the object will at diſtances appear equally clear and luminous, unleſs there be ſome other cauſe that can make it otherwiſe.

For underſtanding what this cauſe is, we need only let into a darkened chamber, through a ſmall hole in the window-ſhut, a beam of the ſun's light: For this beam being ſeen like a luminous path, in all poſitions of the eye, it is evident, that the whole light does not go forward in its rectilineal courſe, but that at all points

points of the medium through which it paſſes, ſome part of it is reflected every way, by means of which the beam becomes viſible; and therefore this ſame beam, by reaſon of the continual diminution made in its light, muſt grow weaker and weaker continually, and that in proportion to the opacity of the medium through which it paſſes. If the air be pure and clear, little light will be reflected and more will be tranſmitted. If it be moiſt or ſmoaky, more will be reflected and leſs tranſmitted. But be it never ſo clear, ſome part of the light will always be reflected or ſtifled in its paſſage; and conſequently its intenſity muſt always decreaſe in proportion to the diſtance of the object from which it flows. Seeing then the intenſity and vigour of light does thus continually decreaſe, according as the diſtance of the object increaſes, it follows that objects muſt always appear leſs luminous, and more tinged with the colour of the medium through which they are ſeen, the further they are removed from our eyes; and therefore, when we are otherwiſe aſſured, that two objects are of the ſame colour, if the one appear more bright and lively than the other, we are taught by experience to conclude, that that which appears moſt bright is the neareſt; and it is for this reaſon that luminous and ſtrongly illuſtrated bodies do always appear nearer than really they are. Whence it is eaſy to ſee why a chamber appears leſs when its walls are whitened, and why the fields and hills appear leſs when covered with ſnow: For, in theſe and ſuch like caſes, the brightneſs and ſtrength of colour makes them ſeem nearer; from which we conclude they are ſmaller: For we always judge of the extenſion and magnitude of bodies, by comparing their apparent magnitude with their diſtance. From this alſo, we may ſee why fire and flame appear ſo ſmall when they are ſeen at a diſtance in the night-time: For the pupil being then much dilated, more light will enter the eye, which, by acting more powerfully on the retina, muſt make the object appear much nearer, from which it will be judged ſmaller.

And as bright and luminous bodies appear nearer, and leſs than they really are, ſo, on the contrary, dark objects, and objects that are faintly illuminated, do always appear further off and greater, by reaſon of the faintneſs and obſcurity of their colour. This is particulary obſervable, when dark bodies are ſeen in the twilight, which always ſeem further off and greater than when ſeen in the brighter light of day. And it is alſo for the like reaſon, that the apparent diſtance and magnitude of objects are increaſed when ſeen in miſty weather: For much light being intercepted or ſcattered irregularly in its paſſage through the miſt, leſs of it will enter the pupil, and conſequently it will act leſs forcibly on the retina, from which the object will be eſteemed at a greater diſtance, and bigger than it ought. And indeed the deception of ſight ariſing from this cauſe is ſo great, that I have been told that a diſtant ſheep has ſometimes in a miſty day been miſtaken for a horſe. And this opacity of the atmoſphere, which hinders part

of

of the light from coming to the eye, is alſo the reaſon why the ſun, moon, and ſtars appear very faint when near the horizon, and brighter continually as they riſe higher: For the tract of air and vapours which lies in the way of the rays, is longeſt and thickeſt near the horizon, and becomes thinner and ſhorter as the objects riſe higher, and conſequently does leſs obſtruct the paſſage of the rays: And this to me ſeems to be one reaſon why theſe bodies appear always the bigger the nearer they are to the horizon: For, ſince they appear fainter, they will alſo appear at a greater diſtance, from which they muſt appear bigger, for the ſame reaſon that objects appear ſo in miſty weather.

From all which I think we may ſafely conclude, that the apparent colours of bodies are very uſeful for us in judging of their diſtances, as often as we are otherwiſe well acquainted with the intenſity and vigour of their colour at any other determined diſtance. And it is from this principle, that ſkilful painters do upon the ſame plane repreſent objects at different diſtances, by increaſing or diminiſhing the intenſeneſs of their colour, according as they would make them appear nearer or further off.

It is indeed true, that the pupil, by its contractile power, does always proportion itſelf, as much as poſſible, to the vigour and ſtrength of light, from which ſome may think it ſhould be impoſſible for us to judge of the diſtance of objects from their apparent colour, or the force wherewith they act upon our eyes. But to this it is eaſy to anſwer, that the dilatation

and

and contraction of the pupil is ſenſible to us, becauſe it depends on the motion of the uvea that we feel, which motion proceeds from the different force wherewith the light acts upon our eye, which therefore muſt always be ſenſible to us; and therefore, though the pupil ſhould by its contraction allow no more light to paſs to the retina, when the object is near than when it is further off, yet we are very ſenſible of the intenſeneſs of its light, becauſe we know that the pupil is then contracted. And beſides, when the pupil is contracted, we ſee more diſtinctly than when it is dilated, by which alſo we are aſſiſted in judging of the diſtances of objects.

The *fifth* mean for judging of the diſtances of objects, conſiſts in the different appearance of their ſmall parts; when theſe parts appear diſtinct, we judge that the object is near; but, when they appear confuſed, or when they do not at all appear, we judge that it is at a greater diſtance.

For underſtanding this we muſt conſider, that the diameters of the images painted upon the retina are always diminiſhed, according as the diſtance of the object they repreſent is increaſed; and therefore, any object may be made to vaniſh, by placing it at ſuch a diſtance from our eyes, as to make its picture inſenſible, becauſe of its ſmallneſs; but the ſmaller the object be, it will ſooner vaniſh. Hence it is, that all the ſmall parts of an object are not ſeen at every diſtance; for the leaſt viſible part will always be ſmaller or greater, according as the object itſelf is nearer or further off. Thus the

the leaſt part that is viſible at one foot diſtance, will vaniſh at two feet diſtance; and the leaſt part that is viſible at two feet diſtance, will become inviſible at three feet diſtance; and ſo forth without end. From all which it is manifeſt, that, when the eye can ſee diſtinctly the ſmall parts of an object, it muſt judge that that object is nearer than any other, whoſe equal parts are not at all ſeen, or only ſeen confuſedly. Painters are well acquainted with this, who therefore, to repreſent objects at different diſtances upon the ſame plane, do always paint them diſtinct or confuſed, in proportion as they would make them appear nearer or further off; for when the picture is confuſed, its ſmall parts cannot be diſtinctly ſeen; and therefore we judge it at a greater diſtance than ſuch as have their parts more diſtinctly painted.

The *ſixth* and laſt mean which we have for the judging of the diſtance of objects, is, that the eye does not repreſent to our mind one object alone, but, at the ſame time, it makes us alſo ſee all thoſe that are placed betwixt us and the principal object whoſe diſtance we conſider; as for inſtance, when we look at any diſtant object, ſuch as a ſteeple, we commonly ſee at the ſame time ſeveral fields and houſes betwixt us and it; and therefore, becauſe we judge of the diſtance of theſe fields and houſes, and at the ſame time ſee the ſteeple beyond them, we conclude that it is removed to a much greater diſtance, and even that it is every way larger than when it is ſeen alone, without the interpoſition of any other viſible object; and yet it is certain, that the image thereof

thereof that is painted upon the retina, is always the ſame in both caſes, provided that we ſee it from a place equally diſtant; and thus you ſee how we judge of the magnitude of objects from their apparent diſtance, and how the bodies, that we ſee betwixt us and any object, aſſiſt us in judging of its diſtance, juſt as by the confuſed remembrance of all that we have done or ſuffered, and of all the thoughts we have had, we are enabled to form a judgment concerning the extent of our duration, or, which is the ſame thing, of the magnitude or extent of time which has paſſed ſince we performed any particular action: For it is theſe ſucceſſive thoughts and actions which enable our mind to judge of the paſt time, or the extent of any part of our duration; or rather the confuſed remembrance of all theſe ſucceſſive thoughts and actions, is the ſame thing with the judgment of our duration, as the confuſed ſight of fields and other objects that are betwixt us and the ſteeple, is the ſame thing with the judgment we have of its diſtance.

And this affords us another reaſon why the moon appears greater when ſhe riſes, than afterwards when ſhe is much elevated above the horizon; for, when ſhe riſes, by reaſon of the interpoſition of the fields, ſhe appears removed to the diſtance of ſeveral leagues, viz. beyond the ſenſible horizon, or that part of the terraqueous globe, which terminates our ſight; whereas, at a greater height, no body being interpoſed betwixt her and us, we don't judge that ſhe is above half a league diſtant. But, ſeeing objects do always appear ſmaller or

greater

greater according as we judge them nearer or further off, it follows that the moon muſt appear greater when near the horizon, than afterwards when ſhe is much elevated, by reaſon of the different judgments we form of her diſtance in thoſe caſes.

There are then ſix means which ſerve our ſight for judging of the diſtance of objects, viz. their apparent magnitude, the vivacity of their colour, the diſtinction of their ſmaller parts, the neceſſary conformation of the eye for ſeeing diſtinctly at different diſtances, the direction of their axes, and the interpoſition of other objects betwixt us and the principal object whoſe diſtance we conſider. Of theſe ſix things, which ſerve to make objects appear near or far off, there are only the three firſt that painters can poſſibly make uſe of in their picture; whence it is that it is impoſſible for them perfectly to deceive the ſight. But, in the decoration of theaters, where all theſe ſix things are artfully conjoined, one needs not be ſurpriſed, if he cannot help being deceived; for, in the ſcenes, care is taken to diminiſh the magnitude of objects, in proportion as one would make them appear far off, while, at the ſame time, they diminiſh the vivacity of their colour, and likewiſe paint them confuſedly, that the ſmaller parts may not be ſeen diſtinctly: And thus the three firſt means for judging of the diſtances of objects are perfectly ſatisfied. And for the three laſt, they repreſent on different planes obliquely placed, and a little removed from each other, the parts of the ſame object which they would make appear at different

rent distances, such as the pillars of any of the orders of architecture, by which means the two eyes are obliged to change their direction for seeing distinctly the different parts of the objects, whether on the same or different planes. And this representation of objects, or the different parts of the same object on inclined planes, placed at a distance one behind another, does also occasion that change in the conformation of our eyes, by which we likewise judge of distances that are within the limits of distinct vision; and, when the object is beyond the furthest of these limits, as indeed all the representations made in the theatres commonly are, the degree of confusion arising from this position of the planes supplies the place of this change in the conformation of the eye in forwarding the deceit; so that all the six means, whereby we judge of the distance of objects, are satisfied, excepting the last, which might a little discover the cheat, were not care taken to represent, according to the strictest rules of perspective, a series of objects lying at different distances on the inclined and differently removed planes, of which their scenes are composed: so that, when we view any of these objects, we cannot help judging them further off than really they are, because we see so many other objects placed betwixt them and our eyes. And thus you see how by art, all the means whereby we judge of the distances of objects may be satisfied, and we thereby deceived: And what contributes yet further to the perfection of the cheat, is the false light

where-

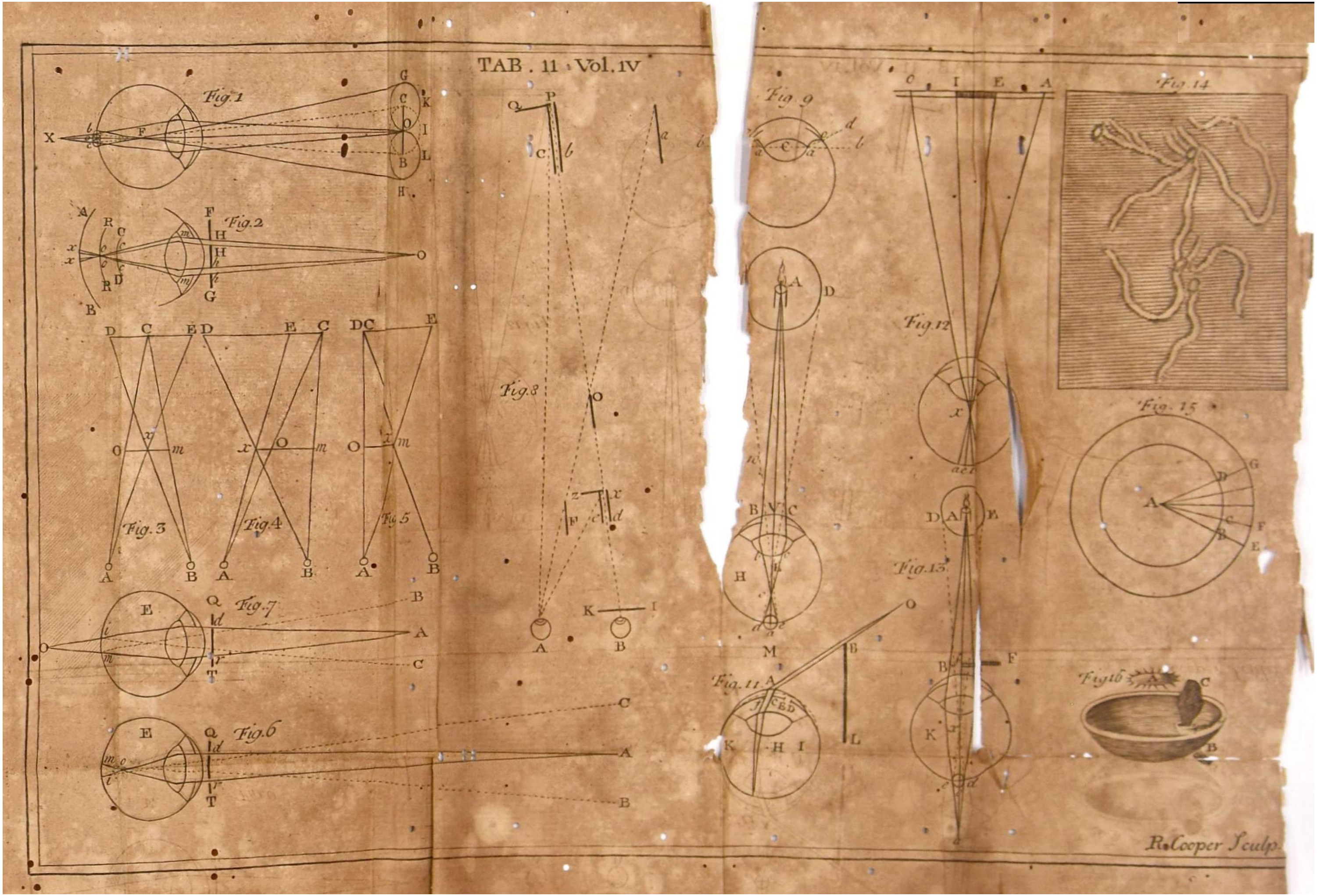
wherewith these decorations are always illuminated.

Having now finished what concerns the motion of our crystalline, whereby the eye is adapted to the various distance of objects, it may not be improper, before I dismiss this subject, to explain a little another motion of the crystalline, which only obtains in birds, and is performed by means of the *marsupium nigrum*, or *bourse noire*, as the French call it.

This is a membrane in form of a purse, which arises from the entry of the optic nerve, and passes through the vitrous humour, to its insertion in that part of the edge of the crystalline which is next the great canthus. Thus it is described by the French academists, and by M. Perrault in his *Mechanique des animaux*, from whom I have caused it to be copied at fig. 16. which represents the half of the globe of an ostrich's eye, in which A is the crystalline humour, B the optic nerve, and C the black purse attached above to the crystalline, and below to the optic nerve. But in some birds I have found this membrane of a rhomboidal figure, agreeable to the account given of it by Dr Petit, in the *Memoires de l'Academie Royale*, an. 1720.

This membrane is always covered with a black pigment, which is of a more intense colour than that of the uvea or choroides; whence it is that M. Perrault and the French academists conjecture, that its only use is to assist the choroides and uvea in preparing the nourishment of the humours of the eye, which, by reason of the transparent purity that is requiſite for them, muſt have an aliment very pure and wholly exempt from the groſs, earthy, and black parts, by which bodies are rendered opaque. For theſe parts, which may be called the *lees of the blood*, are ſeparated therefrom, and retained in the choroides and purſe of the optic nerve, which are ſullied and blackened therewith.

I know that the uſe of this blackneſs in the uvea and choroides is ſeldom extended to the preſervation of this tranſparency in the humours of the eye: For the moſt part of authors ſuppoſe, that the blackneſs of the uvea ſerves only for rendering this membrane more opaque, that no light may enter the eye but what paſſes the pupil, and that the blackneſs of the choroides has no other uſe than to ſtifle the rays of light that fall thereon, and keep them from being reflected back upon the retina; which might efface the images of objects, or at leaſt render them more confuſed and imperfect. But, if we conſider that the back ſide of the choroides, next the ſclerotica, is likewiſe covered with this black pigment, and that in all animals, even thoſe which have its concave ſide next the retina of another colour, as Aquapendente, in his treatiſe *de oculo*, § 1. cap. iv. obſerves, we cannot but think that it likewiſe contributes to the preſervation of that tranſparency in the humours, which is abſolutely neceſſary for the tranſmiſſion of light; and that becauſe there appears no other reaſon for the black colour upon the back part of the choroides: Thus the lion, camel, bear, ox, deer, ſheep, dog, cat, and many other quadrupeds,


TAB. 11 Vol. IV
Fig. 1
Fig. 2
Fig. 3
Fig. 4
Fig. 5
Fig. 6
Fig. 7
Fig. 8
Fig. 9
Fig. 11
Fig. 12
Fig. 13
Fig. 14
Fig. 15
Fig. 16
R. Cooper Sculp.

drupeds, and even ſome of the bird-kind that are not endued with a good ſight, ſuch as the owl, and other nocturnal birds, which have the inſide of the choroides of a blue, green, yellow, pearl, or other bright and reſplendent colour, are never found to want a conſiderable quantity of this black mucous pigment upon the convex or back-ſide of this membrane, which can ſerve for nothing elſe but for rendering the aliment which goes to the cryſtalline and other humours of the eye more pure and free from the groſs black parts, which might render them opaque and unfit for tranſmitting the light.

It therefore ſeems neceſſary, that this tunicle ſhould be provided with glands proper for ſecerning this black mucus, that the cornea and humours of the eye may not be tinged with any opacity: For, as in the jaundice, the whole body becomes yellow, by reaſon of the bile which is not duly ſecerned in the liver; ſo it is not to be doubted but the cornea and humours of the eye would ſoon loſe of their tranſparency, were it not for the ſecretory power of the uvea and choroides, by which the blood which goes to their nouriſhment is freed of its moſt opaque and black parts: Whence it is that animals, whoſe blood abounds moſt with blackiſh particles, have this membrane proportionally of a more intenſe black colour; for it is obſervable, that thoſe who have moſt blackneſs in their hair or feathers, have this membrane alſo moſt black.

There is a mechaniſm not unlike this, very remarkable in the ſepia or cuttle fiſh, which is known in the apothecary ſhops from a kind of ſhell commonly called *os ſepiae*, wherewith its back is covered: This animal is provided with a bag towards the throat, near the ſtomach, whoſe uſe is to ſeparate and contain all the opaque black particles of its blood and humours; hence it is that the ſubſtance of this fiſh is of a white colour, which otherwiſe probably would have been black: For the humour contained in this bag is ſo very black, that it exceeds that of ink itſelf, and one gut thereof is ſufficient to blacken many baſons of water. Whence it is that this fiſh does eaſily ſave itſelf from fiſhers and other fiſh that ſeek to devour it, by expreſſing a little of this liquor into the water in which it ſwims; and therefore Plutarch very agreeably ſays, that this fiſh imitates what Homer makes the Gods do, to hide and deliver their friends from the danger to which they are expoſed in battle, which is, to cover them with a dark cloud, that they may eſcape ſafe. Now, as the ſubſtance of this fiſh becomes white, from the ſeparation of all the opaque black particles contained in its blood and humours, ſo it is more than probable that the cornea and humours of the eye retain their tranſparency, becauſe the blood which goes to their nouriſhment is, by the ſecretory power of the choroides and uvea, freed of all theſe opaque black particles which could in the leaſt tarniſh them, or diminiſh their pure tranſparency. And this may poſſibly be one reaſon why thoſe creatures that ſee beſt, ſuch as eagles and other birds of prey, have the pupil very black; and on the contrary, the owl, lion, and

and other animals whose sight is not so good, have this hole less black; because the bottom of their eyes is not covered with this black pigment.

Thus being much premised concerning the use of the choroides and uvea, it will not be questioned but the *bourse noire* has a similar office, and that it serves to assist them in the separation and reception of all those opaque black parts of the blood which might have sullied the humours of the eye; and that for these reasons, 1*mo*, Because this membrane is never found in any creature but birds, and that because of all other creatures they have occasion for the best sight, by reason their flight places them at a considerable distance from objects which they ought to see. 2*do*, Because, as birds naturally fly more high, and by that means require a more piercing sight, this part is always proportionally of a more intense black colour: Thus, in eagles and other birds of prey, it is always covered with more of this black mucus than in our domestic birds, which either do not fly, or do not fly so high, such as geese hens, &c. 3*tio*, Because, in the demoiselle of Numidia, that strange dancing or buffoon bird, which is the celebrated and wonderful otus of the antients, and which, by reason of certain ways of acting, wherein it seems to imitate the gestures of a woman who affects a grace in walking and dancing, has of old by Aristotle been named *actor* or *comedian*; I say, this bird, which is the only one wherein the French academists found this black purse wanting, the choroides is a great deal blacker and thicker

 than

than ordinary, as if the whole dregs of the blood, which in the eyes of other birds ſhould be retained in the choroides and black purſe, had here been collected into the choroides alone.

Theſe are reaſons that determine us to agree with Monſ. Perrault and the French academiſts, in thinking that the uſe of this part is to preſerve that tranſparency in the humours of the eye ſo neceſſary for viſion, though at the ſame time we are of opinion that it has yet another uſe no leſs conſiderable, which I ſhall now explain.

Every body knows that in birds, their eyes are not, as in man, dogs, and ſuch other creatures as look the ſame way with both eyes, placed in the fore-part of their head, but ſo much towards the ſides thereof, as makes it impoſſible for them to direct both of them to the ſame object. Neither does this ſituation of their eyes ever allow them to be turned towards an object placed ſtraight before them: Hence it is you ſhall frequently obſerve, that, when any bird wants to ſee an object that is ſtraight before it, it does turn the ſide of its head that way, that the rays of light may fall directly upon its eye; but then their ſight muſt, in ſome meaſure, be weaker, becauſe the object is only ſeen with one eye. Now, this being underſtood, it is eaſy to ſee, that, without this *marſupium nigrum*, it would have been impoſſible for birds to ſee their food wherewith they are nouriſhed; becauſe the rays of light that come from an object placed near the extremity of their bill, would, in falling

falling obliquely upon their eyes, have rendered their figh prodigiously confused and imperfect; just as the image of a candle, or other object, is confused, when made by a lens placed obliquely; for the picture cast on the paper is alway more and more confused, in proportion to the oblique situation of the lens through which the light passes; and therefore, to prevent this defect in the sight of birds, nature has very wisely provided them with this part, which being of a muscular substance, does, by its contraction, draw that edge of the crystalline next the great canthus, towards the bottom of the eye, and render its situation such, as the rays of light which come from objects placed directly before them, and towards the extremity of their bill, may fall upon it more perpendicularly, which was absolutely necessary for distinct vision. This is an admirable and truly elegant, and surprisingly beautiful piece of mechanism for perfecting the sight of those animals that cannot turn their eyes to objects straight before them; and, to confirm this opinion, it may be worth while to observe, that this *bourse noire*, as the French call it, though it be tinged black, like the choroides; yet if it be washed, it appears to be composed of muscular fibres, not unlike the *ligamentum ciliare*. Nor is it any solid objection to this, supposing that this membrane should not always be found inserted into the crystalline; for it being so firmly fixed unto, or embodied in the vitrous humour, that the vitrous humour hangs firmly to it, and is not so easily parted from it, all the motions of this mem-

membrane are eaſily communicated to the vitrous humour, and by conſequence to the cryſtalline, which is connected to it.

I ſhould now proceed to the motions of the uvea, whereby the pupil is contracted and dilated, which is indeed by far the moſt beautiful and entertaining, as well as the moſt uſeful of all the motions that belong to our eyes: But the humour of ſcribbling will hold out no longer; and it is well that it is ſo, for I find this paper has already ſwelled much beyond its intended bulk; for which, as well as for ſeveral defects and imperfections, and poſſibly ſome miſtakes, which could not be ſo eaſily avoided, conſidering the hurry in which it has been written, I ſhould now make an apology. But, as a tedious apology for a tedious performance would be inſufferable, I ſhall add nothing further.

## XV. *A Wound with a hot Iron penetrating the* Pelvis; *by Dr* ANDREW WILSON *phyſician at* Dundee.

THE regard phyſicians and ſurgeons have for their own character and reputation, makes them often ſhy to undertake the cure of patients, where they have no hope of ſucceſs, and may be blamed by the ignorant, if the diſeaſe is fatal; nay, the deſpair of doing good is ſometimes a reaſon of proper remedies being neglected as needleſs. But, as every one can ſave himſelf from reproach, by making a due prognoſtic of the event to the relations, and people have recovered beyond expectation,

*præſtat*

*præstat anceps quam nullum tentare remedium*, the sick should have all the assistance which the healing art can give.

By this general reflexion I would introduce the history of a case, which contains indeed no new method of cure, but where nature, with a little assistance, made a cure of a disease which I looked on as desperate.

In the beginning of March 1735, a smith pushed a red-hot iron with such force into the buttock, an inch and a half from the anus, of a young man of twenty years of age, that the point of it came out through the *linea alba*, about an inch above the *ossa pubis*, having pierced thro' the pelvis. After some hours, I saw him. His pulse was low and intermitting. He had violent bilious vomiting from time to time, great pains in his belly, thirst, watchings, cold sweats, and faintings, with scarce any blood from the orifices of the wound.

I ordered fourteen ounces of blood to be let from him, and to inject an emollient clyster with turpentine, which operated well, and he was somewhat relieved of the pain in his belly. He rested ill all night; and, next morning, his symptoms continued, and he had passed no urine, now twenty hours after receiving the wound, notwithstanding his having drunk great quantities of liquor. His pulse was more frequent and harder. I caused twelve ounces of blood to be taken, and after fomenting his belly well with emollients, rubbed it with *ol. scorpionum*. By which the pain became much less. Thirty hours after receiving the wound, he voided a little urine with much mucus, such as people with stones

ſtones in their bladder commonly paſs. At night, the clyſter was repeated, and brought off a great deal of ropy ſlime with it. An emulſion with ſome nitre was given for ordinary drink, and a cordial julep did him much ſervice in moderating the vomiting.

The third day, he again uſed the fomentation, clyſter, and emulſion. His urine and excrements came now plentifully by the lower wound near the anus, little being evacuated the natural way, except a little ſlimy urine which he paſſed with ſharp pain. I ordered diluted digeſtive and mel-roſe warmed to be injected into the wound.

The bad ſymptoms continued ten days, and the ſame medicines were uſed. After this time, his urine came the natural way. In ten days more, the excrements did the ſame, and in ſix weeks he was cured, having taken a low vegetable diet all the time, with thin weak liquors for drink, and opiates at night.

Towards the end of the cure, he was emaciated, and had a cough, but recovered of both by a milk diet.

XVI. *A Stone in the Bladder formed on a Needle; by Mr* ANDREW BROWN *Surgeon in* Dalkeith.

A Gentleman's daughter in this place began, at two years of age, to be afflicted with colic pains and difficulty of making water, which were commonly removed by clyſters, purgatives, diuretics, and ſome other medicines. When three years of age, her diſeaſe had

had more the appearance of gravel, for the severe pains were about the *regio pubis*; she had partial obstructions of urine and frequent vomiting, but never complained of her back. These symptoms not yielding to her former medicines, I put her into the semicupium, which, with injections, relieved her. She was so sensible of the benefit of the semicupium, that she frequently desired it, and sometimes came unwillingly out of it.

Her pains and obstruction of urine increased all the following year, the warm bath being the only medicine that gave her relief, for in it only she made urine freely; at other times it either came away insensibly, or in very small quantity at once.

In the end of February 173[illegible], when she was four years and two weeks old, she complained of a great pain in the pudenda, putting her fingers there as if she would extract somewhat. All the external parts swelled greatly, and inflamed. Her mother then sent for me. Upon pressing the right labium, I felt like a fluctuation of liquor in it, and, on the posterior part of the rima, there was some pus. I fomented the parts with warm milk, and applied an emollient poultice upon them.

Next day the quantity of pus was greater, and she was easier, but still aimed at extracting what pained her. The same applications were continued.

On the second day, her mother observed a white hard substance in the passage, and the child was miserably tortured with pain. In the evening, while a fomentation was applying,

ing, the ſtone, which you will recieve with this, fell into the baſon. It weighed then more than half an ounce, and is, you ſee, grown round a needle, the ends of which ſtand out. See Tab. III. fig. 1. where it is repreſented of the natural ſize with the ends of the needle A and B ſtanding out.

The child was too young to give an account of what had happened to her ſo long before; and the parents knew of no needle ſhe had ſwallowed, or of any pains ſhe had had, till the colic pains, which I mentioned in the beginning of this hiſtory.

When this ſtone came away, there was neither blood nor pus with it, nor had ſhe paſſed any blood before. Since that time ſhe has ſtill complained of gravel pains, and the urine flowing involuntarily excoriates the ſkin.

XVII. *An* Aneuriſm; by ALEXANDER MONRO, *Profeſſor of Anatomy in the Univerſity of* Edinburgh, and F. R. S.

THE caſes of aneuriſms cured by operation are ſo few in books, that I perſuade myſelf you will not refuſe a place in your collections to a ſecond, eſpecially that it ſerves to confirm a general doctrine which you have already publiſhed, (See art. 15 16. and 17. of Vol. II,) and that ſome improvement on the operation is likewiſe to be propoſed along with the hiſtory.

Andrew Rady, living in Galloway, had the misfortune, in being bled in the baſilic vein of the right arm, by ſome gardener there, to

have

have his artery hurt, which was followed by an aneurifm. Some more than a year after, he came to town here, and was received into the Infirmary in may 1735. On the 22d day of that month, Mr George Cuningham, the furgeon then in attendance, performed the operation. After the tourniquet was applied, Mr Cuningham laid open the tumour from one end to the other, with one longitudinal incifion; then taking out the polypous fubftance, and a fmall quantity of liquid blood, the fmall aperture of the artery was fo plainly feen, that I put a probe into it, and raifed the trunk of the artery, while he paffed the needle behind it, the fides of the wound being held afunder in the mean time by two blunt hooks. The proper membrane of the tumor was confiderably thicker and ftronger than in James Foreft's aneurifm, and required force to pufh the blunt aneurifm-needle through it; but the nerve was preffed by the tumor a good way from the trunk of the artery, fo that there was no danger of taking the nerve within the ligature. After making the fuperior ligature, the tourniquet was untwifted, but no blood came by the orifice, which fhewed the anaftomofing canals to be very fmall; the fecond ligature was however made below the orifice, for fecurity. The cavity was filled with foft lint, and the other ordinary dreffings applied. That afternoon his hand fwelled and became warm, which removed all our fears of the circulation being intirely ftopped. No pulfe was to be felt on either fide of the wrift for feveral days; but, before the 5th of June, when

 both

both the ligatures suppurated off, the pulse was plainly to be felt on both sides of the wrist, and he cured soon, having as much strength and motion in that whole member as ever.

To make this operation more speedy and safe, I would propose, that as soon as the longitudinal incision is made, and the polypous with the blood is removed, the patient's elbow being bended some way, the operator should take hold of the humeral artery with the thumb and fore-finger of the left hand, and gripping it towards the back part, should push the needle close upon his own nails, by which he has a sure direction whereby he may shun the nerve, which he can readily distinguish from the artery by feeling, and can in that posture of the arm easily draw the artery so far outwards as to keep free of the nerve.

The operation then of the aneurism, which appeared by the description surgeons gave of it, to be very nice, difficult, tedious, and precarious, may be done easily, quickly, and safely, by opening the whole tumor at once, and then putting the ligature about the artery as just now described.

XVIII. *A white Swelling of the Knee*; by the same.

MAny instances are daily seen of that tormenting, dangerous disease, the White Swelling of the joints; but, before one has an opportunity of examining them by dissection, to understand the nature of the disease right, the matter has become so sharp that it erodes the

the bones themſelves, and then one ſees the ſame thing as he would do in a *ſpina ventoſa*. I met with one patient in our Infirmary, whoſe joint of the knee was juſt as far advanced as I wiſhed to examine, when it was amputated. It gave me a better idea of that diſeaſe than I had before, and poſſibly may be ſo to ſome of your readers, which induces me to ſend it to you.

Iſabel Blackadder, a young woman of a delicate tender conſtitution, having hurt her left leg by a fall ſome years ago, an ulcer broke out near her heel, and ſeveral pieces of bone caſt out at it; but it recovered ſo well, that ſhe went to ſervice again.

In the end of 1734, having hurt the ſame leg by another fall, the knee ſwelled, became very painful and ſtiff, for which ſhe was taken into the Infirmary; where, after blooding, a few doſes of *aquila alba*, and embrocation with *aq. mindereri*, the ſwelling and pain both ſeemed to abate, but ſoon became as bad as formerly, and never afterwards yielded to any medicines.

The ſkin of the ſwelled parts was not diſcoloured, and on the inſide of the joint a fluctuation was felt in one or two points; but the quantity of liquor appeared very ſmall, and the fluctuation had a different feeling to what commonly pus collected in a cavity has. Her pains were very ſharp, eſpecially upon the leaſt motion of the affected leg; her fleſh and ſtrength decayed daily, and the hectic ſymptoms increaſed; which at laſt brought her ſo low, that ſhe could not be raiſed to a ſitting poſture without fainting, which brought her under the neceſfity of fuffering the member to be amputated.

In this condition fhe was when the member was cut off above the knee by Mr Douglas; after which fhe recovered daily, and walks on a wooden leg of the fame form which Alexander Sheppard ufed, whofe cafe I fhall foon extract alfo from the records of the Infirmary to fend you. (See art. XXI.)

When the difeafed joint was diffected, all the cellular membranes, in which fat is naturally contained under the fkin, between the mufcles and tendons, and upon the ligaments, were found full of a glairy matter, which had infinuated itfelf fo much everywhere, and had made the other parts fo foft, that we could fcarce diftinguifh one from another. In feveral places of this glairy fubftance, there were fmall cavities full of pus. When the articulation of the knee was opened, all the mucous glands and fatty membranes were feen in the fame condition with the exterior parts; the femilunar cartilages themfelves between the tibia and femur being quite foft, and with the fame cellular mucous appearance that the glands had. We alfo obferved fome pus within the cavity of the joint, but the extremities of the bones were fcarce begun to be eroded.

## XIX. *Part of the Cartilage of the Joint of the Knee feparated and offified;* by the fame.

IN the letter inclofed in this paper, you have an account of a white fwelling from a very uncommon caufe, treated by my good friend Dr Simpfon profeffor of medicine in the univerfity

fity of St Andrew's. (See the following Article). In it the Doctor does not, and indeed could not, determine how the loofe bone he defcribes came into the cavity of the articulation. I believe it may not be difagreeable to you to relate what I faw once in the joint of the knee very like to the bone he took out, and which may ferve to explain that phænomenon.

In the body of a woman, aged forty, which I diffected in February 1726, I found, within the ligament of the articulation of the right knee, a bone of the fhape and fize of a fmall turky-bean, depending by a ligament half an inch long from the external fide of the tibia. The bone, when cut, had only a thin external firm plate, being compofed within of cells which were full of oil. On feparating the femur and tibia, I faw the ligament came out from the exterior edge of the cartilage covering the exterior cavity of the tibia; and more internally a part of the cartilage of the tibia, of the fame fhape with the bone, was wanting. In Tab. III. Fig. 2. A is the bone hanging by its ligament, and B is the bone cut open. The circumftances of this malefactor made it impoffible for me to know exactly her fymptoms or complaints before her execution.

XX. *An uncommon Tumor of the Knee, with Remarks on the* white Swellings *of the Joints; in a Letter to Mr.* MONRO *Professor of Anatomy, from Dr* THOMAS SIMPSON *Professor of Medicine in the University of* St. Andrew's.

*SIR,*

THE glory of medicine consisting in the cure of diseases, every discovery that contributes to make this more certain must be looked upon as of moment in that art. Upon which account, I look upon the history of the particular species and individuals of diseases, as a part of our art that can never be fully enough enlarged upon. For, if it be in diseases as it is in botany, that frequently the species, nay the individual, has such particular properties, as to satisfy us that they have a most peculiar nature of their own, however much besides they have in common with others, we cannot be sure, till a trial satisfies us, whether or not this peculiar nature will require a particular consideration in the cure, and consequently should be animadverted to, to make our method the more certain and extensive. It is this consideration, Sir, which makes me lay before you an instance of what is called by our English writers, the White Swelling at the joints, which arose from a most singular cause.

A countryman, in the neighbourhood of St Andrew's, had, for several months, an uneasiness in walking, from a pain in his left knee, which had got no observable injury: When the pain was greatest, he found something of a hard

hard body immediately under the rotula, generally at the inſide of the leg, though ſometimes at the oppoſite, and could get no eaſe, till, by chaffing it upwards with his hand, he made it diſappear: The parts about were tumified, as we find in the caſe of a white ſwelling at this joint, though to no great height: The hard body always made its appearance upon walking; ſo that, when I came to ſee him, he was obliged to take ſome turns through the houſe, before he could make me ſenſible of it. I eaſily catched it betwixt my thumb and finger, where it felt ſo diſtinctly, that I could not ſuppoſe but it had its ſeat in the *tunica adipoſa*, immediately under the ſkin, which made me immediately pull out a biſtory to open into it; but my patient, not having reſolution to allow me, did plead ſome days delay to think upon it. After which, he came to my houſe with ſome of his friends, and acquainted me that he would ſubmit to the operation, he being perpetually upon the rack, by the falling down, as he called it, of that body, which happened every moment of the day he offered to ſtir. It appeared ſo looſe and ſuperficial, that I had no manner of doubt of ſucceſs, and ſo with a ſcalpel immediately began to make an inciſion upon the body, which in the mean time I held betwixt my finger and thumb; but, to my great ſurprize, when I had made my firſt inciſion through the ſkin and fat, I found a membranous ſtrong bag between me and the tumor, which made me ſenſible for the firſt time where this floating body was lodged; however, though now I reckoned the operation

operation of more importance than formerly, I was ſatiſfied nothing elſe could help my patient than to continue the inciſion; which I did, and, upon entering the bag, there was at leaſt four ounces of the ſynovia, or a thick pellucid humour iſſued out with the hard body, which I found much of the ſhape, though larger than a kidney bean; it then appeared wholly cartilaginous, and very ſmooth and protuberant. But, upon drying, it ſhrunk in, and ſhewed itſelf a bone covered over with cartilage. In cutting through the bag, which was firm and thick, the patient expreſſed the utmoſt pain, which turned eaſier upon taking out the body, and after the evacuation. I earneſtly deſired the patient to ſtay in the town that night, that I might have the better acceſs to attend him; but nothing would perſuade him. So that, after ſome hours ſtay, he mounted horſe, and rode under night two miles to the country, in time of a moſt keen froſt, which raiſed the pain of his knee to the greateſt height, and obliged him to ſend expreſs for me at midnight. I ordered it to be fomented with the moſt anodyne ſoftning materials I could contrive, but with little ſucceſs; his knee ſwelled exceedingly all round: And what was obſervable, he did not make ſo much complaint of the place where the inciſion was made, as at the oppoſite ſide. He was bled and purged with calomel frequently, but all to no purpoſe, being, notwithſtanding all that could be done to him for a month's time, ſeldom free of horrid cries and complaints; nor could he allow his leg to be moved in the moſt gentle way we could contrive,

and

and never slept but when he took opiates. Bladders of water round his leg, as warm as he could bear, had little influence, but water syringed took more effect. Which I made two men do by turns for near an hour at once, from a large clyster syringe; but, though this caused the pain and swelling to abate, yet it did not carry it quite off, till I applied a caustic to the outside of the knee; which being kept running, and the syringing continued, it gradually wore off in about an year's time: So that he is quite free of all complaint and swelling, and walks about without any impediment.

Amongst the many causes of this swelling about the knee, I have not met in any author such an one as this loose body within the bag which environs the joint; I must leave it to my readers to suppose whence it had its rise: Only I must give my opinion, that it was the irritation of this loose body which had produced such a quantity of synovia, by which the bag was made to stretch and become so contiguous to the skin, and allowed its guest such an easy motion from side to side, though it appeared most at the inside, which is the most depending. But I would have it observed what an obstinate swelling the irritation caused by the wound, the hanging it in riding, and the cold of the night had produced; so that it stood out a very long time against the most efficacious remedies, as all these swellings about the knee, or about any other joint, do for most part. I have had several of them of a shorter standing with the worst symptoms, such as a considerable swelling round the joint, while a few places

places seemed to be rising by themselves, forming, as one would have reckoned at first likely, an abscess; which, with purging and the fall of cold water, according to Cheyne's method, I have in a few weeks carried off. To some I applied, with like success, the fall of warm water, according to Le Dran's method. To others I applied sometimes the cold and sometimes the warm water by turns with like success. In those of longer standing, though I never saw any of them carried off quite, yet I always found they got relief from the extravagant pain, against whose return they can never be secure, by the same methods. Several of those I have had under these cases could give no account of the rise of their trouble. Some women have contracted it under the diary fever (the weed) they are subject to in child-bed. A great many get it by wounds or bruises about the parts, which are capable of the easiest irritation, and never miss to bring all the neighbouring parts to suffer; and it seldom misses that the flexor-tendons turn rigid under the disease: The patient generally, for ease, keeping the parts about the joint in as relaxed a state as possible; and thus the flexor-tendons, which share the pain and irritation, turn rigid and stiff: Wherefore, I would rather look upon this as an effect of such a disease than the cause, as I find M. Maloet does in the Memoirs of the French Academy of Sciences for the year 1728.

As I have been witness to the greatest mischiefs from such a disease, I thought it just to observe, that it should be plied very earnestly

upon

on its firſt appearance; and that the impro-
s of our art ſhould lay themſelves out to ſee
they could fall upon a more certain and expe-
ious method of cure. I am,

your moſt humble ſervant,

Andrew's,
y 21. 1736.

THOMAS SIMSON.

*The Hiſtory of an Ulcer of the* Leg; *by* ALEXANDER MONRO, *Profeſſor of Anatomy in the Univerſity of* Edinburgh.

ALexander Sheppard, a ſmith, aged thirty ſix years, having accidentaly wounded the fore part of his leg with the point of a hook about the beginning of harveſt 1732, an inflammation and ſuppuration were brought on this member, and were neglected till october, when he was received into the Infirmary here.

In examining the ſtate of this diſeaſed leg, a ſinous ulcer was diſcovered to extend itſelf the whole length of the leg. This being laid open, ſent out only a very ſmall quantity of ſanious ichor, and, in a few days after, the knee was attacked with a painful ſwelling, which ſoon yielded to a fomentation of urine, in which wormwood, camomel, and mallows had been boiled. The ulcer ſeemed to be in a good way for ſome days after this; but then proud ſpongy fleſh roſe from it, which was kept down by ſprinkling *red precipitate* upon it; and, in order to correct his bad habit

habit of body, he was ordered to drink plentifully the decoction of guajac, and had repeated doses of mercurial purges given him. These medicines had a good effect for some time; but, upon interrupting the use of them a little, the matter of the ulcer turned more fanious, he felt a pain and stiffness in his knee, pimples broke out all round the ulcer of his leg, and the itch appeared every where else on his skin. The mercurial purgatives were therefore repeated, and again brought the leg to a better condition, but left a diarrhoea that continued several days.

In the beginning of January 1733, he was seized with a feverish paroxysm like that of an ague, and next day a red swelling of the erisipelatous kind was observed on the back part of the diseased leg near the ancle.

The day following, the tumour of the leg was less, but his knee was considerably swelled, though without any heat or redness, his pulse was frequent, with thirst, heat, and other feverish symptoms. A low vegetable diet was prescribed, with emulsion, or milk and water for his drink, and emollient fotuses and cataplasms were applied to his knee. Notwithstanding which the feverish symptoms continued, the knee swelled more, and became more painful, and a large suppuration began in the back part of his leg. These were soon followed by an obstinate diarrhoea; so that, before the end of this month, January, he was emaciated to skin and bone, was so weak as scarce to be able to turn himself in his bed, his appetite was quite lost, and he had constantly a quick

quick pulse and thirst, with night-sweats, and a colliquative diarrhœa. His knee was greatly swelled, with its ligaments so weak that the bones could be made to have the appearance of a partial luxation, and a certain grating was felt on moving the patella from one side to the other; at the same time a large collection of pus was made in the back part of the leg.

He had refused to allow the amputation of his leg to be made when it was first proposed to him; but, finding himself worse every day, and being convinced that the only chance he had for life, was to have that operation performed, he allowed it to be done, on the first day of February, by Mr. Hope, who was the surgeon then attending.

The member was taken off four inches above the knee. When this joint was dissected, the cartilages were found eroded, and the bones were become carious.

From the day of the operation, he had no more diarrhœa; and, in twelve days after, all the other hectic symptoms were gone, his flesh and strength being evidently recovered considerably.

The cure went successfully on all the months of February and March, except that, on March 16th, a livid-couloured spot, about the size of a six-pence, was observed towards the posterior part of the wound, which having a pledgit dipt in brandy applied to it, could not be seen next dressing.

April 6th, several granula of flesh that came out, with very small peduncles, from the solid ſubſtance of the bone, threw out a conſiderable quantity of blood, and four or five more ſuch hæmorrhagies, from theſe fleſhy papillæ, happened in this month, and were always ſtopped by applying oil of turpentine.

April 12th, A large livid fungus ſprouted out from the cavity of the bone, and ſeveral other ſuch fungi were ſeen upon the fleſhy parts alſo of the wound. Theſe were removed by repeated ſearing with a red-hot iron, and the application of oil of turpentine.

In the beginning of May, he was altogether free from the fungi and hæmorrhagies, and, May 15th, a piece of the thigh-bone, about the thickneſs of a crown, exfoliated.

He was then cured of his itch; appeared to be every way in good health and vigour, with the bone covered, and all the wound cicatrized, except about the breadth of half a crown in the middlemoſt prominent part where the bone was, on which a ſkin could not be brought; and therefore it was neceſſary to contrive ſuch an inſtrument for him to walk with, as would not allow the weight of his body to bear on this raw part, and that could make the ſoft parts to ſupport the body, without reſting on the bone.

The inſtrument he made uſe of with ſucceſs, was of the form which you ſee repreſented in the annexed figure, (See Tab III. Fig. 3. and 4.) which I chuſe to ſend you, becauſe, if ſuch an one is deſcribed by any author, his works are not very generally read in this country.

A is a box of wood made firm on the outſide by two rings of iron *a*, *a*, and covered within

within with a thick twilting of wool under chamois-leather.

B is the ſtick or leg, of ſuch a length as anſwers to the ſound extremity.

A piece of ſtrong bend-leather, ſhaped as in Fig. 4. is fixed to the brim of the box A, the two ends CC being at a diſtance from each other, and having py-holes for paſſing the lace D through; the middle long part E has a large piece of thick chamois, or thin well dreſſed buff-leather F fixed to it.

G, G is a belt of buff, at one end of which is the buckle H, and the other end I is pierced with holes for eaſily paſſing the tongues of the buckle.

K, L are two ſmall ſtraps coming from the lower edge of the belt G.

*m*, *n* are two ſmall buckles faſtened to the bend leather.

The patient having the thigh of his breeches fitted to his ſtump, ſo that the prominent raw part, with the dreſſings upon it, paſſes through a hole left in the end of the breeches, the ſtump is put through the bend-leather into the pyramidal box, which does not allow the raw part to ſink to its bottom; and the laced part D is brought to anſwer to the courſe of the large crural veſſels on the inſide of the thigh: Then the lace is drawn ſo tight, that the bend-leather may grip the thigh all round, by which the whole weight will not reſt upon the cicatrix of the ſtump on the ſides of the box; but the teguments of the thigh all bear a ſhare, while the tight lacing will have no bad effect in ſtopping the circulation, the larger veſſels being free from any compreſſion: E comes upon the outſide of the thigh as high as the great trochanter, and F covers the glutæi muſcles; and being pliable, allows them and the joint to move eaſily. The belt GG is then faſtened round the loins, and the ſtraps K, L are ſecured by the buckles *m*, *n*, to ſupport the inſtrument in the inſide of the thigh.

If the belt GG does not ſupport all well enough, a ſuſpenſory muſt be put over the ſhoulders, to be faſtened to it at two different parts both before and behind.

Any who read the preceeding hiſtory, and conſider the nature of the matter evacuated at the ſinus, its ſmall quantity, the feveriſh ſymptoms coming upon this, nature's effort to throw it off by a new ſuppuration, and by the ſkin where it erodes the conduits, it could not paſs; and the hæmorrhagies, fungi, &c. that came on afterwards, will obſerve very ſtrong marks of a ſharp purulent matter reaſſumed into the blood; and, from the ſucceſs of this caſe, ſurgeons may be encouraged to undertake operations to patients with very unfavourable ſymptoms, when they can thereby take away a *fomes purulentus*, or any other cauſe upon which the ſymptoms depend. That it may not be thought I recommend bold operations from the ſucceſs of this ſingle inſtance, I muſt tell you, that, among the ſmall number which our Infirmary, ſo lately erected, can maintain, there are ſeveral ſuch other caſes recorded. 1. Patrick Higgins, fourteen years of age, with the bones of his leg carious, hectic fevers, colliquative ſweats, and diarrhœa, was received into the

Infirmary,

Infirmary, September 17. 1730, where his leg was amputated, and went out of it plump and ſtrong, with a firm cicatrice, 23ſt December thereafter. 2. Margaret Cleghorn, hectic, weak, and emaciated with the bones of the foot and leg carious, was admitted 2d March 1732, underwent the ſame operation, and was perfectly cured. 3. Iſabel Blackader, whoſe hiſtory I have already related. And, analogous to theſe caſes, I have ſeen people in much the ſame circumſtances from very large ulcerated bleeding cancers, large internal abſceſſes of the liver, kidney, &c. who recovered daily after the amputation of the cancer, or opening the abſceſſes.

XXII. *Remarks on the* Amputations *of the larger* Extremities; *by* the ſame.

IN the operations of ſurgery, there are a number of little circumſtances, ſeveral of which ſeem at firſt view to be of no great conſequence; but, when their obſervation or neglect comes to be attended to in practice, they are found to contribute conſiderably to a ſpeedy or tedious cure, to bring on or prevent bad ſymptoms, to keep the patient eaſy, and preſerve him, or to put him to pain, and bring him into danger; and therefore their good or bad effects ought to be duly conſidered, and the proper cautions concerning them ought to be given by thoſe who write for the public upon ſuch ſubjects. In which particular our chirurgical authors are for moſt part very negligent; as an example of this, I ſhall make ſome remarks on the amputations of the larger extremities, an operation, than which there is none of those that are called the greater or more dangerous, more frequently performed, and about which the directions seem to be very particular, and confirmed by the frequently repeated experience of authors.

This operation of amputation consists in a proper precaution to prevent any hæmorrhage during the operation; cutting all the soft parts, which cover the bone or bones; sawing it or them through; securing the cut vessels from bleeding afterwards; and dressing, so as to promote a safe and easy cure. In this order then I shall make my remarks.

The precaution taken to prevent a hæmorrhage during the operation, is by applying Petit's, or the common tourniquet. I shall refer to what the contriver Mr Petit says * of the advantages and conveniencies of his instrument, and shall only consider the common one, which surgeons, who generally have not, or do not know the other, do commonly make use of. It consists of a thick substance, to be placed on the large common artery of the member, for compressing it; a circular compress to be put round the limb, to defend the teguments; a strong strap that is to be twisted; a small stick with which the twisting of the strap is to be made; and a small piece of paste board or horn, to allow the twisting to be made more easily, and to defend the teguments below the twisted part from being hurt by it.

Several

* Memoirs de l'Acad. des sciences, 1718.

Several French authors order a thick compress to be placed over the artery for its compression; but a roller, which our surgeons generally use, is much perferable; because, as the arteries ly deep in the interstices of muscles, between which the compressing substance must sink before it can affect the arteries, this cannot be done near so well by the plain surface of a compress, as by a convex roller.

The size of this roller must be proportioned to the distance between the muscles, and to the depth of the situation of the artery. If the roller is too thick, it will be born off from the artery by the muscles; and if it is too small, the muscles will hinder the twisted ligature to press it sufficiently on the artery.

Not only must the size of the roller be regarded, but care must be taken to roll it up of a due firmness: If it is too soft, it will have the fault mentioned of the compress: If it is too hard, it presses with too narrow a surface, from below which the artery may slide, and the circulation may therefore be continued in it, after the ligature or strap is fully twisted. One must easily judge that such a firmness is required in the roller as allows it to retain its cylindrical form, till a considerable force is applied to make it a little flat.

Authors in their writings, and surgeons in performing this operation, are uncertain in the application of the circular compress of the tourniquet; some putting this compress round the member, before they place the roller on the artery, which may be attended with very bad consequences; for, if it is wrapped loosely round,

round, it comes to be wrinkled and doubled by the twifted ftrap, which both hurts the fkin, and hinders the twifting. If it is put tight round, it keeps off the roller from finking between the mufcles, and the hæmorrhage is not prevented, as I have feen happen both from this caufe and too hard a roller. The roller ought therefore always to be applied firft upon the artery, and then the circular comprefs is to be drawn tight over both roller and member.

If the roller and comprefs are not fewed to each other, the roller may fometimes be in hazard of fhuffling out from below the comprefs in the time of the operation, and for moft part will do it when the twifting ftrap is left loofe after the operation, and all the dreffings are applied, which may be attended with danger of hæmorrhage during the operation, and the furgeon is difappointed of ufing the tourniquet fo quickly as he would wifh, if any bleeding happens after the operation. Not only therefore ought the roller and comprefs to be fewed together, but the ends of the comprefs ought to be fo fecured that it may not fall off.

I have nothing to remark on the common directions for the ftrap, pafteboard, twifting-ftick, and the twifting, unlefs to take care that the ftrap is ftrong enough, and no way worn, left it break, and the veffels are let loofe upon the operator in the middle of the operation. If fuch an accident fhould happen, the furgeon had need to keep his prefence of mind, which if he does, there will be no great danger; for an affiftant may fupply the want of the tourinquet, by gripping the roller firmly, till either a new ftrap ſtrap is provided, or rather till the ſurgeon has finiſhed the operation, which it is his buſineſs to do, in ſuch circumſtances, as quickly as he can. Nay, though there is no ſuch aſſiſtant, the ſurgeon, by ſawing the bones through very quickly, and then putting his fingers on the large arteries, till he has brought the ſtitches round them with the other hand, may prevent too great a loſs of blood.

In cutting the ſoft parts which cover the bones, all care ſhould be taken to have the ſkin and bone as equal with the ſurface of the wound in the muſcles as poſſible; for, if the ſkin is retracted much in the circumference of the wound, and the bone jets out far in the middle, a tedious cure is to be expected. For this purpoſe the ſkin is not only to be drawn firmly up, while the fillet, which is put immediately above where the circular inciſion is to be made, is applied tightly, but the aſſiſtant who holds the upper extremity of the member, is to draw the ſkin, and, if he can, the muſcles too, as tightly as poſſible, both to ſave them, and to keep them tenſe, by which they cut much more eaſily. And the operator is not only previouſly to cut the ſkin round, and then to make the circular inciſion of the muſcles cloſe by its upper cut edge in the thigh, and other places where a ſtrong retraction of the ſoft part is expected, as is recommended by ſome late French writers; but, after cutting the perioſteum round as near to the fleſh as poſſible, he is to ſcrape it upwards with the edge of his knife, by which the ſide of the blade muſt puſh upwards the muſcles which are next to the bone, and which retract leaſt, becauſe of their con-

connexion to the bone; ſo that, the bone being ſawed near to the fleſh, the whole ſurface of the ſtump may be plain, without any pyramydal prominence in the middle, which not only protracts the cure, by its larger ſurface and diſtance of the ſkin from the bone, but is a great inconvenience to the patient ever after, by the prominence being perpetually galled with every thing that preſſes on it.

Before the ſaw is to be applied, a piece of ſlit linen is always ordered to be put round the bone, wherewith the ſoft parts may be drawn up and defended from the teeth of the ſaw. I have almoſt always ſeen one of two inconveniencies happen from this piece of linen; either the ſurgeon applied his ſaw ſo cloſe to it, that the linen was engaged in the teeth of the ſaw, which made it impracticable for the ſurgeon to go on in ſawing, till it was diſengaged; or elſe, to ſhun this, he left too much of the bone without the fleſh, with a greater chance of a tedious exfoliation, and a certainty of a pyramidal ſtump. This linen ought either not to be applied, from the want of which I never ſaw any inconvenience, or it ought not to be allowed to touch the bone, that the ſurgeon may be at liberty to apply his ſaw upon the bone cloſe enough to the fleſh.

The common directions are ſufficient for the ſawing.

To ſecure the cut veſſels from bleeding, aſtringents and other ſtyptics are found altogether inſufficient in ſuch amputations as I now treat of. Cauſtics are both uncertain and deſtroy more than is neceſſary. Compreſſion by common

mon bandages cannot reftrain the haemorrhage, and Mr Petit's new compreffing machine *, if it is to be depended on, to ftop the bleeding of arteries in the mufcular part of the thigh, at a diftance from any bone; or if it can be applied to that artery of the leg which lies clofe to the fide of the fibula, where it pierces through the ligament between the bones, will be long before it is in the poffeffion of moft furgeons. The artery forceps is generally neglected now as an inconvenient inftrument, with which a furgeon may tear the artery, or may make a ligature which is too eafily pufhed off the ends of the veffels:—— Stitching with a needle and thread has been found by numberlefs trials to be fuch a fafe and fure method of ftopping the bleeding of large arteries, that it is now univerfally practifed among us, and therefore is what I fhall only here confider.

The form of the needles employed here, and the way of making a thin flat ribband, by waxing a number of fmall threads together for tying the veffels, inftead of the common round threads formerly ufed, are now too well known to be infifted on.

In pufhing the needle round the artery, the furgeon fhould be careful to carry it, within the fubftance he pierces, two thirds or three fourths of the circumference of the artery; for, if the thread is only lodged within the flefh of one half or lefs of that circumference, the artery may be miffed altogether in drawing the ligature, or fuch a fmall part of one fide of the extremity

* Memoires de l'acad. des fciences, 1731.

extremity of the artery may be taken into the noose of the knot, that it will easily slide off; and though the bleeding appears sufficiently guarded against at first, yet a fresh haemorrhage begins soon after. I remember once to have seen this accident occasioned in the manner now described.

In passing the needle thus, as few muscular fibres, tendons, or ligaments ought to be taken within the noose as possible, but the surgeon should attempt to thrust his needle only through the cellular substance in which the arteries of the extremities lie; for the threads when drawn have greater effect in bringing the sides of the artery together, when the substance comprehended in the noose is soft and thin, than when it is firm and thick; less pain is given by shunning the nervous parts, less substance is lost when the tied parts fall off, and there is no such danger of the stitches being so long in casting off, and consequently of the new flesh growing over the knots so far, that they are scarce to be come at to cut them away, without danger of opening the artery again; or by leaving the stitches, sinous ulcers are formed in the stump, and no cure can be made. I have more than once seen all those inconveniencies, from more than was necessary being taken into the noose of the thread in stitching arteries. This, which I look on as a hurtful practice, has some reasons to support it, which has brought people into the exercise of it, such is the fear they have of the thread's cutting the coats of the artery in tying, unless some other firm substance is taken in. But this none who makes use of such flat thread as

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I mentioned, and has been the leaſt accuſtomed to make ſuch ligatures, is in any danger of, nay, it is not in any one's power to cut the coats of an artery with ſuch thread, by the ſole force of tying; indeed, by pulling outwards at the ſame time he makes the ligature, the ſurgeon may tear the artery; but this every ſurgeon guards againſt. Next it may be ſaid, in defence of comprehending the firmer ſurrounding parts within the nooſe, that otherwiſe the ligature may be puſhed by the force of the blood over the extremity of the artery: But this will be found to be without foundation too; for, as ſoon as the ligature is made, the cellular ſubſtance beyond the ſtitch, having ſtill a communication with the ſurrounding cells, ſwells and turns firmer and harder, ſo as to prevent the thread from ſliding.

That fear of cutting the coats of arteries in tying the threads makes ſurgeons frequently tye them too looſe; if the blood is ſtopped, they require no more. But they ought to conſider, that threads tying arteries only come away afterwards, by the tied parts mortifying or ſuppurating away, and that the ſooner ſuch corruption is brought on (which will be exactly in proportion to the tightneſs of the ligatures) the ſeparation of the threads will be the more ſpeedy. The rule therefore will be, that, where the artery is very large, and conſequently where the plug of coagulated blood obſtructing its orifice, the firm concretion of its ſides, the new ſprouting fleſh, or whatever elſe it is that blocks up its orifice, muſt be longer in forming, the ligature is not to be ſo very tight that its ſeparation may be longer in making, and all hazard of hæmorrhage may be ſhunned. But, where the artery is not large, the tighter the threads are drawn, ſo much the better, that they may ſooner fall off, and the cure may be more ſpeedy.

It may be eaſily judged, from what has been ſaid, that the compreſs of linen recommended by ſome authors to be put between one ſide of the artery and the nooſe of the thread, cannot be approved; the effects of it being to prevent the tight enough ligature of the veſſel; and if it ſhould ſhuffle out ſoon, an hæmorrhage muſt be expected; or, if it remains, the pus which it imbibes will become too acrid.

After the two knots are made on the ligatures of the veſſels, ſeveral writers recommend the thread's being left of ſuch a length, as to turn over on the ſide of the ſtump; but, when this is done, the blood or matter which comes from the wound never fails to glue thoſe threads ſo firmly to the other dreſſings, that theſe can ſcarce be brought off without the threads being pulled more or leſs, which endangers the tearing the extremities of the arteries, or making the threads ſlide over them, to occaſion an hæmorrhage; whereas, by leaving the threads ſo ſhort that they can ſcarce reach to the edge of the wound, they are always kept moiſt, and ſo cannot adhere to the dreſſings, to run that riſk.

In amputations, the ſurgeon ought not to content himſelf with tying only ſuch veſſels as he obſerves throwing out blood, while the patient is faint with the pain, but he ſhould endeavour

deavour to rouſe him from that faintiſh ſtate by a cordial, and then, wiping off the coagulated blood with a ſponge wet in warm water, he ſhould examine narrowly all the ſurface of the ſtump, to diſcover the bubbling ſtreams, to ſecure them before the dreſſings are put on, otherwiſe he may expect to be obliged, by a freſh hæmorrhage, to undo all.

At firſt dreſſing, ſurgeons uſe to be very anxious about the bleeding, and for that reaſon, applied great quantities of aſtringent powders: but theſe were obſerved to purſe up the ſmall veſſels too much, and thereby to retard the ſuppuration; while, by the hard cake which they form, they gall the wound, and cannot be taken off without much difficulty and pain, and therefore they have been long diſuſed in this country. In place of which, pledgits wet with hot oil of turpentine were applied: This gave very ſharp pain, and by it ſometimes there is hazard of bringing on an hæmorrhage at its firſt application, afterwards it hardens the veſſels and reſiſts the ſuppuration, and never miſſes to ſcald and bliſter the ſkin round the ſtump, and thus creates ſuch pain as the patient complains more of than he does of the wound. If the larger veſſels are well tied, and no fault is committed in applying the other dreſſings, there is occaſion for no other application to the wound than the threads of ſoft half worn linen, which the French call *charpie*, the Fngliſh *lint*, and we *caddiſs*, which is a gentle abſorbent, is ſoft and eaſy to the wound, and, by the corrupting liquors it imbibes, proves

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one of the ſtrongeſt, moſt mild and ſafe ſuppuratives.

Great pains were commonly taken to form the lint into neat pledgits before it was applied, but it is impoſſible to make pledgits without folding the extremities of the threads where it becomes thicker and harder, and ſo makes an unequal preſſure, which produces ſeveral ill effects. I have often ſeen wounds changed to the worſe, by the unequal compreſſion of pledgits, compreſſes, and bandages of one dreſſing. In order to ſhun theſe inconveniencies, the lint needs only to be laid into thin parcels, as is done when pledgits are to be made; or rather the ſtump is to be covered with pieces of the new invented ſcraped cottony lint in ſheets, cut of a proper ſhape and ſize; for with theſe the inequalities between bones or elſewhere can be perfectly well filled up, and an equal ſoft compreſſion can be made on the whole ſurface of any broad wound or ulcer; in all which the lint ought always to be applied in the form juſt now mentioned.

This way of dreſſing makes, you ſee, the compreſſes that are commonly deſired to be put on the extremities of the arteries and the particular pledgits for the bones altogether unneceſſary.

The bladder, which ſome yet recommend to be put upon the lint, is of no uſe, and only hinders the ſurgeon to diſcover ſoon enough any ouzing of blood from the ſtump, and therefore ought not to be applied.

The malta compreſs is pretty well contrived, though it would be better to have a cap of woollen,

woollen, or some fuch fubftance, that would contain the ftump, without any part being doubled or folding over another, as muft be done with the angles of the malta compref, which therefore make an unequal preffure on the parts of the fkin they are applied to. The two long compreffes that are ordered to be applied crofs the ftump upon the malta, feem unneceffary, if not hurtful; for, being made to crofs on the middlemoft prominent part of the dreffing, they prefs only the bone, which anfwers no purpofe, and they hinder the equal compreffion which ought to be made by the bandage on the other parts of the wound. At the fame time, the ends of thefe compreffes which are laid upwards on the member, bruife and gall the fkin when the bandage is applied tight upon them. I have feen them funk their whole thicknefs into the fkin.

The circular comprefs commonly placed round the edge of the ftump, with its ends folded over each other, is alfo of no fervice, and contributes to the unequal compreffion of the parts on which it is applied. The thick narrow comprefs ordered to be put on the courfe of the large artery of the member, and the turns of the bandage directed to be made round the limb, both which are faid to be intended to moderate the courfe of the blood in the artery, and thereby to prevent an hæmorrhage, are effectual means of bringing the hæmorrhage becaufe they have much greater effect in ftopping the blood returning in the veins, than they can have in preventing the flow of it through the arteries, on which account all the arterious canals correſponding to theſe compreſſed veins muſt be diſtended, and among the reſt the cut veſſels of the ſtump muſt be greatly enlarged.

Notwithſtang all the effectual methods we now have for preventing an hæmorrhage in amputations, ſurgeons ſtill continue to act as if they were as much afraid of it as they had reaſon before either tourniquet or ſtitching were known, and by this do ſeveral hurtful things; among the reſt, the too tight application of the bandage upon the other dreſſings is one: If the circulation is not entirely ſtopped, and a mortification brought on by it, they think it cannot be too tight. But, beſides this hazard of mortification, there are ſeveral diſadvantages which they are expoſed to by this practice, whereof the very thing they are afraid of, the hæmorrhage, is one, as I ſhall endeavour to demonſtrate, in conſidering the effects of the different turns of the amputation-bandage applied tightly.

The longitudinal turns of the bandage which are made to paſs over the middle of the ſtump in different directions, to cover it all over, exert their greateſt power againſt that middle part where the bone is, which bears over their preſſure from the other parts, and the large arteries which ſhrink up farther than the extremity of the bone cannot be affected by their preſſure. If this middle part is ſhunned in making the longitudinal turns, the fleſh only is preſſed, and therefore will be thruſt upwards from the bone, which is left prominent and bare, to occaſion a tedious cure, and at laſt a pyramidal

pyramidal ſtump, which is always after uneaſy to the patient. The immediate effect of too great preſſure on the ſoft parts, is to hinder the ſmall veſſels to diſcharge themſelves, which creates pain and inflammation, and does not allow the ſuppuration to come on. I had occaſion to ſee this prettily confirmed in the caſe of one James Spence, who had the amputation performed in the middle of the fore-arm, in the Infirmary here. By changing the poſture of the fore-arm ſoon after the operation, he had made the longitudinal turns, which went alſo round the elbow, tighter than they were at firſt applied. Three days after, there was not the leaſt appearance of ichor coming through the dreſſings, nor any ſuppurating ſmell, his pulſe became quick, and he complained of pain, throbbing, and girding in the ſtump. I judged what was the cauſe, and cut all the longitudinal turns at the elbow; in a few hours after, his complaints were all gone, the exterior dreſſings were ſtained with the liquor ouzing through them; next day all the ſymptoms of a mild plentiful ſuppuration were ſeen, and the cure was ſoon completed.

The circular turns of the bandage, when tight, muſt ſtop the return of the blood in the cutaneous veins, and by making thus a greater reſiſtance to the blood in the arteries which anaſtomoſe with them, will occaſion the contracting power of the heart and arteries to dilate and force more blood into their other branches; but theſe, being cut in the amputation, will pour out their blood, and ſo an hæmorrhage is brought on. Analogous to this it is, that, when a ligature is put round the arm or leg, it becomes all red below, the lateral branches having much more blood thrown then into them than they had when the circulation was free. It can be to this caufe only, that a phænomenon, which furprifes many furgeons, is owing, to wit, after dreffing a wound according to art, it bleeds; upon taking off all the dreffings, not a drop comes out: If the furgeon wifely thinks to prevent any further blooding, by ftill a tighter bandage, the hæmorrhage is greater, unlefs he will chufe to rifk a mortification. To fatisfy fome gentlemen fully of the truth I argue for, I took the management of a tourniquet while the amputation of a thigh was performing; after the large arteries were all ftitched, I let loofe the tourniquet, fcarce any drops of blood fell from the ftump. I then gradually twifted the tourniquet; whenever it became a little tight, the whole furface of the wound feemed ouzing orifices of veffels. I twifted it again fully, and ftopt them all; then untwifting gradually, fhewed them the fame bubbling fcene, till the tourniquet was quite loofe, when no more blood came.

From the whole I would conclude, that no more is required of the bandage than to prefs the other dreffings very gently to the wound. If a furgeon is to fall into any of the extremes of too loofe or too tight bandage, the former will, in my opinion, do much lefs harm than the latter.

Our Britifh furgeons would do well not to be fo free in blood-letting as the French operators direct. I ſhall not now examine whether the French conſtitutions require this evacuation more than we do, or whether ſo frequent and plentiful evacuations of blood is a faulty practice among them, introduced at firſt by a miſtaken theory, and prevailing afterwards by cuſtom: But this is certain, that, though blooding is exceeding neceſſary in plethoric habits that undergo the amputation, and is the grand remedy when fever and inflammation ſeize a patient after this operation, it is by no means a general rule, that all who ſuffer amputations ſhould be let blood of either before or after the operation; for I have in many inſtances ſeen the cure performed without one bad accident, when the patient has ſcarce loſt two ounces of blood in the operation, and was neither blooded before nor after it: And, on the contrary, I have obſerved, in the hoſpitals at Paris and elſewhere, people ſink under the loſs of blood, dying with oedematous ſwellings in ſeveral parts.

What I find has induced practiſers to imagine, that in amputations there was a greater neceſſity of letting blood than in other wounds of equal extent with the ſtump, is their ſuppoſing that, immediately upon a limb's being taken off, the remaining arteries of the body are obliged to circulate the quantity of blood they contained before, with the addition of what was ſent to the amputated member; which additional quantity they pretend to relieve them of by venæſection: But, when it is conſidered that the amputated member takes away its proportion of the liquors of the body with

with it, and therefore leaves no more in the other vessels than they contained before, the reason for this practice must cease. For some days after the operation, the patient is always kept on a spare low diet, to prevent any fulness, and consequently there is no occasion for blooding in the first days after an amputation, on account only of any plethora the loss of a limb can be supposed to bring on. Afterwards indeed, when the patient comes to recover his appetite, and a fuller diet is allowed, it is reasonable to think a plethora may be brought on by the chylopoietic viscera preparing a great quantity of chyle to be mixed with the blood, whose vessels will be too much crowded, because of the want of those that have been cut off. For which reason it is necessary, for all who have lost a larger member, after their recovery, to use a spare diet, or to make frequent evacuations, otherwise they will probably be subject to the plethoric diseases.

The cases then in which blood-letting is required after amputation are, when the patient is of a full habit of body, and has lost little blood before or in the time of the operation, or when there is violent pain or swelling in the member, without being occasioned by any application made to the stump; or when the pulse becomes very quick and strong, with heat, thirst, and other feverish symptoms; then indeed blood-letting, suited in quantity and repetition to the symptoms and strength of the patient, is absolutely necessary. But if, in the first three or four days after the operation, the pulse is only

only a little more frequent than ordinary, without violent pain or other bad fymptom, a low diet, with cooling drinks and laxative clyfters, if the patient is coftive, will be fufficient.

You will perhaps be furprifed, that I have not mentioned haemorrhage as one of the fymptoms which require venaefection, the remedy univerfally employed for checking or ftopping hæmorrhagies. My reafon for this omiffion, is the opinion I have, that hæmorrhage after amputation feldom requires blood-letting; nay, that the common practice is pernicious in moft fuch cafes.——Vigorous plethoric patients have hæmorrhage, either from neglecting to tie fome of the large veffels, for which ligature is proper; or from too tight bandage, which muft be taken away or cut; or from fever, for which I have directed blood-letting: But, by far the greater number of thofe who undergo amputation of the larger extremities are weak, emaciated, and more or lefs hectic from tumors or ulcers of long continuance. Thefe people's veffels are fo lax, and their blood is fo thin, that their ftumps often bleed, during the time of the cure, from a number of imperceptible orifices. Venæfection exhaufts the fmall remains of blood and ftrength of fuch patients, and increafes both caufes of the hæmorrhage. To fuch therefore I order *pulv. cort. Peruvian.* with *pulv. ftyptic.* feveral times a-day, claret warmed with cinnamon, mace, or nutmeg, as a cordial to be taken frequently; and I caufe thefe fpiceries and wine to be mixed with their food. The records of the Infirmary contain feveral hiftories of poor patients

patients who were brought from the brink of the grave by this method.

The phyſicians and ſurgeons of the Royal Infirmary here have always followed the method above-mentioned, and have not loſt one patient of fourteen who have had amputations of the larger extremities performed.

Since theſe fourteen, there have been eighty-five more patients in the Infirmary, who had the like operations performed on them, of whom eight died. Two of theſe eight had violent contuſions on their bodies, and the limbs had a mortification in them. The other ſix were all emaciated with hectic ſymptoms before the operation, and ſurvived it ſeveral weeks or months; ſo that the death of none of them can reaſonably be imputed to the operation.

It is generally too ſoon to take off the firſt dreſſings on the ſecond, third, or even fourth day after an amputation; for they ſtill adhere to the wound, and cannot be brought away without pain and bleeding: And there being no ſuch effectual ſuppurative as the liquor ſent out from the wound, the fifth, ſixth, or ſeventh day is generally ſoon enough for removing the dreſſings. If the ſmell of the ichor of the wound becomes in the mean time very ſtrong and offenſive to the patient, it may be neceſſary to cut the band, and with the aſſiſtance of ſciſſars to take off the compreſſes and exterior part of the lint, on the third or fourth day, and to apply clean things in their place; but the lint next to the wound ought not to be removed, till the ſuppuration moiſtens and ſeparates it.

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At the second dressing there is no occasion for any other suppurant, than not to be too anxious in cleaning off the pus that adheres to the stump, all moisture upon the skin being carefully dried, to prevent excoriation.

Nothing contributes afterwards more to a speedy cure than dressing seldom: The rule that might be taken from nature is, to wait till the patient is sensible of an uneasy itching in the wound; which shews the pus is beginning to turn acrid, which commonly happens every second or third day. And as I hinted formerly, an equal gentle compression is of great use in keeping up a right suppuration, and preventing the growth of spungy flesh.

If the threads with which the arteries were tied should remain too long, (that is, three weeks or a month, according to the largeness of the artery) and the new sprouting flesh covers the ligatures, they had best be cut out, lest, by the growing of the flesh, they should become so much covered, that they can scarce be come at, and sinous ulcers should be formed, to prevent a cure. The best method of making this excision is, to take hold of the depending threads, and to introduce a probe or small directory along them, till it enters the noose, which is easily known, by drawing the noose very cautiously outwards with them; for the resistance which the thread makes, will very plainly be felt. Upon the probe or directory, slide in one blade of a pair of scissars a little opened, till the point of it is where the other instrument was, and the point of the other blade is consequently on the outside of the noose when it is ſniped in two, and is eaſily drawn out. In bringing the threads away in this cautious manner, there is no danger of bringing on an haemorrhage from the artery round which the thread had been tied; for long before this time, ſo much of what was taken at firſt into the nooſe muſt have fallen off, to make it quite looſe, and without any effect upon the artery.

If the patient is of a tolerable habit of body, and is managed in the manner above deſcribed, dry lint, and ſometimes touching the ſprouting fleſh with lunar cauſtic, are all the medicines neceſſary towards a compleat cure, without any exfoliation of the bone; which the ſurgeon ought to be ſo far from endeavouring to promote, unleſs he finds the bone corrupted by ſome accident or miſmanagement, that, on the contrary, it ſhould be his ſtudy how to prevent it. The common methods for which, as alſo for procuring an exfoliation, are, in my opinion, very faulty: I may poſſibly inform you hereafter of my reaſons for thinking ſo, and ſhall only juſt now remark, that, of the fourteen who ſuffered amputation in the INFIRMARY, there was no exfoliation from any of their bones, except from the thigh-bone of Alexander Sheppard, whoſe hiſtory I have already ſent you. In two others, Iſabel Blackader and John M'Millan, who had the amputation alſo performed above the knee, towards the end of their cure there was a very ſmall piece or two of bone obſerved among the pus; but in no other was there any thing like bone ſeen to come off, though in ſome of them it was thought altogether improbable to prevent ſome corruption and conſequent ſeparation of

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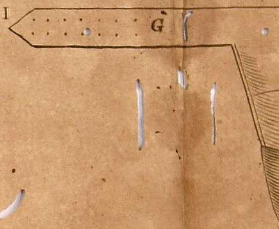
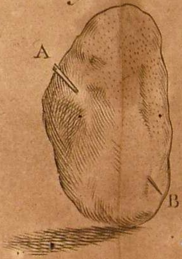
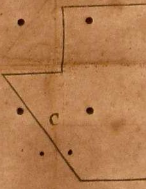



Fig. 1



a part of the bone, particularly in Margaret Cleghorn, whoſe tibia and fibula were ſo ſpongy at the place of amputation below the knee, that, had their condition been known before, the knife might have been made to cut them through, as well as the teguments and muſcles. Notwithſtanding which, and a bad hectic habit of body, both bones were quite covered with fleſh at the third or fourth dreſſing, the wound healed ſoon, and the cicatrice has remained now firm ſeveral years.

In theſe remarks on amputations, I have blended the rules of management ſo with reaſoning, that ſeveral readers will not at firſt have a full orderly view of all the ſteps to be taken in performing this operation after the manner I have propoſed; nor do I deſign to ſum up the agenda, on purpoſe that thoſe who have moſt need of rules, I mean the young ſurgeons, may have them more firmly fixed in their memories, by taking the trouble to compare what they read in the books of chirurgical operations with what has been ſaid here, and then to form for themſelves a compleat deſcription of the whole operation and method of cure, by following the order I have done, but ſupplying from their books what is here ſuperficially paſſed over as being common, and changing the ordinary directions for ſuch of mine as they ſhall think reaſonable and confirmed by practice.

XXIII. *An Eſſay on nervous Fevers: In a Letter to Dr* JOHN STEVENSON *Phyſician in* Edinburgh, *from Dr* EBENEZER GILCHRIST *Phyſician at* Dumfries.

HAVING had a good deal of practice of late *, in fevers of the low kind, commonly called nervous, by ſome, internal, influent, depreſſed, I venture to offer you ſome conjectures upon the nature and cure of them; minding as little as I can what authors have ſaid, I ſhall give you the thoughts juſt as they aroſe.

I have never yet been able to follow out ſome in their almoſt endleſs diviſions of fevers, nor in the cauſes they aſſign for them. As little can I be ſatiſfied with thoſe who would allow but of one general cauſe of fevers. Tho' obſtruction may be frequently the cauſe, I believe it is ſometimes more an effect than a cauſe. But obſtruction does not give a compleat notion of the diſeaſe; nor by it alone will we be able to conceive juſtly the method of cure. The part principally obſtructed, the nature of the obſtructing matter, and ſeveral other circumſtances muſt be well conſidered. It is not eaſy to ſay, whether it be certain miaſmata, diſproportion of parts, particular acrimony, exaltation of ſome of the principles of the blood, or undue attraction of them, that occaſions this diſorderly motion. And as amongſt ſo many different opinions we are left in uncertainty, in order to be ſatiſfied as to

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the cause of this, or any other distemper, we are obliged at last to trust to our own senses and reason.

This kind of fever I would speak of has been these many years fatal in Britain. I shall not account for its frequency, how far it may be owing to the manner of living, and a long course of warm and rainy seasons. It would be but an unsatisfactory piece of theory perhaps, to show how these causes bring on such a state of the fluids as is observed, or may be justly suspected in this fever.

For a history of the disease, I incline to give a description of it, as it had different appearances in different persons. Young people generally complained of pains and stitches the first days; by which I have been deceived, taking the case sometimes for a slight rheumatism. Their complaints otherwise were but few. The pulse was not much different from natural. Their sleep was pretty natural, and the sickness was rather heaviness, with some degree of faintness. Under such moderate symptoms I have been secure, till a delirium, or signs of it, shewed the danger: And those who seemed to be in no danger the first days, for most part died.

In others, the seisure and symptoms, the first days, were more violent. They had vomiting or nausea, head-ach, full, strong, or hard pulse, heat and thirst, redness of the eyes. The case then having a good deal of inflammation in it, it was necessary to bleed once and again; and the symptoms were considerably lessened by it. This did not always happen: But by the time that a delirium came on, the ſigns of inflammation were much abated, the pulſe was low and contracted, the heat moderate, and they were altogether as thoſe who in the ſeiſure had but ſmall ſigns of inflammation, were not bleeded, nor indeed would bear it.

Sometimes they would languiſh two or three weeks before the diſeaſe formed into a fever; and it ſtole on ſo inſenſibly, that they were in danger before we were aware. In this caſe, there was no reckoning of days, for it was not known when they were ſeized. In ſhort, as to the manner of ſeizure, there was great difference; but the diſeaſe in the progreſs and height was always the ſame.

When the firſt ſymptoms were over, an obſtinate delirium came on, ſooner or later, for moſt part very early. Sometimes there was looſeneſs, with pains of the belly; partial ſweats, which gave no relief; tickling cough, and more or leſs of faintneſs. Soon after a delirium came on, the mortal ſymptoms appeared. In ſome the dilirium did not come to ſuch a height as in others, nor was it conſtant; but at times they would talk reaſonably, even when a ſubſultus was upon them. Theſe lay much diſpirited, and waſted faſt, without any, or but very little increaſed, evacuation. In ſome ſuch I have obſerved a fatty pellicle upon the urine. Continual, cold, clammy ſweats are ſometimes obſerved: At other times they, as it were, melt with profuſe ſweats, as if water were ſprinkled upon them, and the ſkin feels death-cold.

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I have only taken notice of the more obvious appearances and remarkable differences of ſymptoms; not needing to make a nice enumeration of them to one who has ſo often obſerved them.

It was the poorer ſort, and thoſe a degree above them, who were ſubject to this fever. I knew but few inſtances of it amongſt thoſe who lived well; and of wine-drinkers, I do not remember that there was one ſeized.

Though I am not for multiplying cauſes in this, or any other diſtemper, I would here ſuppoſe two very different ſtates of the fluids: A ſizineſs or rheumatic lentor of the blood, tending more or leſs to inflammation; and a too great diſſolution of it: Of which we have ſome notion, when it is ſaid the blood is poor, vapid, effœte. It was from the firſt of theſe that the fever with us generally proceeded, and it is in this view that I deſign to conſider it: For, when bleeding was neceſſary, we almoſt always found the cruſt at the top more or leſs tough and compact; and when we durſt not bleed, the ſymptoms gave ſigns of this lentor. The tongue was for moſt part only white and moiſt, ſeldom very dry, black, or chapt; which argues neither ardency nor defect of the fluids. The urine was much like that in health, ſometimes redder; a thin, meally, duſky ſediment was moſtly obſerved, or a thick cloud falling to the bottom. A criſis was not to be judged of by the urine.

This want of a natural ſediment may be owing to different cauſes, depending upon the different thickneſs and velocities of the blood.

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In ardent, ſanguineous, inflammatory fevers, where the blood's motion is much increaſed, the attrition is great, and the ſeveral parts of the blood are beat down, and ſo blended together, that in the urine the different principles remain confuſed, and never attract or ſeparate. So likewiſe when the fluids are thick, and there is but a ſmall increaſe of the velocity, the watery parts will be ſeparated by urine; while the more ſolid of oil, ſalt, and earth, will be wrapt up and cloſely retained: Hence we do not meet with a ſediment. And this makes a large flux of urine, profuſe ſweats, or increaſed looſeneſs, of bad prognoſtic in this fever.

This ſizy ſtate of the blood is attended with more or leſs of inflammation, as appears from the different manners of ſeizure. The more tendency there is to inflammation, the greater is the danger. On the contrary, the hazard is leſs as the diſeaſe is farther removed from it, till there is reaſon to think, that the lentor is in ſo ſmall a degree as not to be the cauſe of the fever; but that the oppoſite ſtate of the blood, viz. ſome degree of diſſolution, is to be ſuſpected; and then the danger grows in proportion on the other hand. You ſee that, to maintain the ſuppoſition of two different ſtates of the blood, and that one or other of them is always the cauſe of the fever, I have fancied different degrees of the morbific cauſe, alledging that, when we come to the laſt or ſmalleſt degree of viſcidity, the firſt or ſmalleſt degree of diſſolution begins.

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Theſe different degrees of ſizineſs cannot be better conceived, than by obſerving them in other diſeaſes. In pleuriſies of the inflammatory kind, we find it in the higheſt degree; and the moſt cooling, diluting, attenuating method is little enough to diſſolve it. We meet with it again of a middle degree, in pleuriſies of the cold kind, that have ſomething in them a-kin to *peripneumonia notha*: A method very different from what is uſed in the former muſt be followed here. The more we evacuate, the diſeaſe continues longer, and the ſtitch is more fixed. So that the very vulgar rule, to bleed till the buff diſappear, or the blood turn better, as they ſay, is very hurtful. But once bleeding, more or leſs, according to ſymptoms; and giving volatile attenuating things, with proper diluents, and ſometimes bliſtering, anſwer all intentions. This lentor is found in chronical caſes, where it has little or no tendency to inflammation, and therefore is leſs apt to produce obſtruction or fever.

How a lentor, which of itſelf appears to be unactive, ſhould excite a fever, I ſhall not take up your time to explain; but I am of opinion that it is this which, according to the ſeaſon, manner of living, and conſtitution of the perſon, produces fevers continual and intermittent, pleuriſies, rheumatiſms, and other diſeaſes pertaining to thoſe of the inflammatory kind, but that do not come fully up to their nature. If we conſider the likeneſs and complication of ſymptoms in theſe diſtempers, and how readily one of them is changed into another; for inſtance, continual fevers into intermittent,

mittent, *et vice versa*; as also, those of a good kind into malignant; if we consider these, I say, we will have some reason to think that the cause is much the same in them all.

I have said that this lentor is unactive; but then it is easily put into motion, or so disposed as to produce any of these diseases. By an error in the non-naturals it may be thrown upon some particular part, or so fixed as to excite the fever. This gives us the most simple idea of the disease, and is the most favourable kind of it, which will have several degrees, as the lentor is more or less compact. We have another idea of it, when it acquires an inflammatory disposition, and the disease will have different appearances. When it is joined to a particular acrimony, we have still another and very different notion of it.

To determine the several kinds of this fever, the degrees of it, and the malignancy with which it may be attended, a scheme might be made out in this manner: Fever from a lentor——lentor and inflammation—— lentor and acrimony——lentor, inflammation and acrimony——1, 2, 3, degree of lentor, inflammation, acrimony; and so on in the division and subdivision, if you please, of these; which, according to the various modifications of matter in fevers, and combinations of causes, might be run out to a good length, more for amusement, I own, than any real advantage in practice.

What happens in other fevers, deserves to be particularly taken notice of in this. I do not know how to call it, a muscular tension, or universal

verſal ſpaſm; which does not appear ſo evidently as in fevers that have more of inflammation in them, but we may be very ſenſible of it from effects. This lentor and a ſpaſm conſequent of it, eſſentially conſtitute the fever: And all that is done in the method of cure has, or ought to have a relation to one or other, or both of theſe.

As I make frequent uſe of the word *ſpaſm*, it ſhould be explained. Every effort of nature to free herſelf of what is hurtful, is really a ſpaſm; which will be more or leſs violent, according to the nature or force of the ſtimulating cauſe; and with which more or fewer parts, according to the nature of the diſeaſe, are obſerved to labour. What are tremblings, horrors, rigors in the attack of fevers, but a ſpaſm of the whole body? What are headach, vomiting, looſeneſs, and all diſorderly ſecretions and excretions, but a ſpaſm of ſome particular parts, or effects of more univerſal ſpaſm? The ſame are all theſe miſchiefs that follow upon a wrong adminiſtration of medicines; ſuch as, increaſe of the fever, anxiety, contracted or irregular pulſe, which happen frequently from bliſtering, hot ſtimulating medicines, &c. Whatever therefore, to ſpeak yet more properly, increaſes too much the oſcillations of the ſolids, will be the cauſe of a ſpaſm.

One general obſervation, taken from the pulſe before and after the height or criſis, will further ſhew what I mean by a ſpaſm, and what ſhare it has in this fever; whether it be reckoned a joint cauſe or a chief ſymptom; for this we know ſometimes requires our greateſt attention. The

The pulſe before the height is felt low, weak, ſmall, hard, irregular, contracted, being always below the ſtandard. There is a certain increaſed degree of circulation neceſſary for reſolution, preparation, and expulſion of the matter of diſeaſes. As the fever goes off, or a criſis ſucceeds, the pulſe becomes full, firm, ſoft, and ſtrong; and, if it has not theſe conditions, the patient hardly recovers, or he ſuffers a relapſe. This remarkable change of the pulſe cannot be well accounted for, but upon the ſuppoſition of a ſpaſm, which, abating at the height of the diſeaſe, gives room to the blood to flow equally into all the veſſels. The quite contrary happens in ſanguineous and inflammatory fevers, where, before the ſtate, the pulſe is full, hard, and ſtrong; but after, it becomes ſmall, weak, and languid. This makes me think that the criſis muſt be explained in a different manner. If we would ſtill have a clearer apprehenſion of the nature of this diſeaſe, we ſhould ſeparate the fever from the delirium, and conſider them ſingly. Let us imagine a fever of this kind performing its courſe, and no delirium attending it. We may ſuppoſe one will bear up a good while under it, even when there are conſiderable degrees of malignity, while the ſeveral functions are performed, or not much leſed, and the matter is free in the veſſels, which, by repeated circulations, will be at laſt concocted, and the fever determined.

But, when a delirium comes on, there is really a new diſeaſe formed, not neceſſarily depending upon the fever, but from a particular diſpoſition of the matter of this fever, rather than

than of any other to take to the head. The analogous ſymptoms of the fever and delirium conjunctly will ſerve now to increaſe the cauſe, or heighten the appearances of the fever, beſides the ſymptoms peculiar to the delirium itſelf.

The ſymptoms in a head-ach, for ordinary the forerunner of a delirium, and which we ſhall ſuppoſe to be in the membranes of the brain only, are coldneſs and trembling, nauſea and vomiting, ſtraitening of the breaſt and præcordia, involuntary motions of head and neck, contracted, irregular, and ſometimes intermitting pulſe. Theſe are all the effects of a ſpaſm. When an obſtruction is formed in the brain itſelf, another ſet of ſymptoms appears. Beſide theſe mentioned, the functions are not performed, the faculties are impaired or loſt, the ſecretion of a fluid in the brain is in ſome meaſure hindered: This occaſions an irregular diſtribution of ſpirits; for, while they are not ſecreted into ſome places at all, they are violently or unequally ſent into others. Hence proceeds all that variety of unnatural actions and motions obſerved in delirious people; ſtartings, ſubſultus, convulſions, which are all greater degrees of a ſpaſm. A fever with a delirium muſt be conſidered as a complicated diſeaſe.

It will help a good deal too, in forming a judgment of this fever, if we examine what proportion the fluids bear to the containing veſſels. The quantity is ſeldom more than the natural, I mean as it is found in a well-conſtituted body, ſometimes leſs. The appearance of of fulneſs in the beginning of the fever is rather from ſome degree of inflammation than a plethora. Here then is a ſtricture of the veſſels in a collapſed ſtate, or where their ſides are brought nearer together, by which the intervening fluids are ſtrongly compreſſed. This gives a notion of it, very different from what we have of a fever from fulneſs, where the ſides of the veſſels are diſtended. The removal of the ſtricture in one caſe is by plentiful bleeding and cooling; in the other, by relaxing the veſſels and attenuating, that the fluids may be made to occupy a larger ſpace.

Theſe things conſidered, we come to know, how a fever, that in the ſeizure appears mild and favourable, may, when a delirium comes on, be equally malignant, as that which has more acute ſigns in the beginning.

Why this diſeaſe is ſo dangerons, for the ſame reaſon that diſeaſes from inanition are more difficult of cure than thoſe from repletion.

That a delirium is not to be regarded as merely a ſymptom, and the removal of it attempted by means that in general only reſpect the fever.

Hence likewiſe we account for a weak, low, ſmall, contracted, irregular pulſe, ſhrinking of the ſolids, and ſudden appearance of waſting, when there is no encreaſed evacuation.

And laſtly, we fix the ſenſe of *malignity*, which ſhould not ſtill be left under the ſcandal of being a myſterous or inſignificant term.

As I take this fever to be very different its nature and changes from other fevers, it is lefs fubject to the rules in prognoftics. Particular hiftories fhould be adduced for proof and illuftration of thefe things: This might be done, but it would be too tedious.

The ordinary evacuations in the beginning are bleeding and vomiting: I do not know that purging has had a place here, nor for what reafon.

We frequently find the patient under a feeming plethora: Though we do bleed, the fymptoms are not always much abated by it; and if we bleed freely, being deceived by this appearance of a plethora, we do harm. Indeed, in general, I imagine bleeding feldom did much good; and if great caution was not ufed, I fufpect it was hurtful: But, as I was not often called in the beginning, I am unwilling to pronounce pofitively about it.

We are generally pretty fure what will be the effects of blood-letting, but we are not fo with refpect to vomiting; and I am in fome doubt whether it be always ufeful here. In this fever, we may expect a delirium pretty foon. By a mechanifm in vomiting, the force of the circulation is ftrongly determined to the brain; which at this time fhould be diverted from it, left a thick fizy blood impacted into the brain bring on a delirium fooner than might otherwife happen. There is feldom great danger, where this fymptom does not come on before the ninth day.

Vomiting has been of a long time ufeful in fevers. It is faid, that by it nature is affifted to throw out every thing hurtful, from the centre to the circumference, as the phrafe is. I do not well underſtand this; for, however true it may be in eruptive fevers, where vomiting is of great ſervice, I do not ſee what it has to do, upon this ſuppoſition, in other fevers where we know not what is to be expelled, nor when.

In fevers, we ſhould have a regard to the ſtate of the *primæ viæ*, whether or not the cauſe of the diſeaſe be lodged there. For, if it ſhould be ſo, by vomiting we remove part of this cauſe, and ſo cut off from the ſupply that might be made to the blood. But, though the fomes ſhould not be in the *primæ viæ*, we yet diſcharge all ſuperfluities, and bring thoſe paſſages into a right condition to do their office during the courſe of the diſeaſe, which they could not do, were they left charged with a great deal of groſs humours and recrement. Vomiting will be proper upon another account, as in ſome low caſes it gives a greater ſpring to the ſolids, ſtraitens the veſſels, and keeps the blood, where it tends to diſſolution, more compact; and ſo prevents its ſtagnation, and hinders the fluids from running off at a wrong time and by wrong outlets, as ſometimes happens in profuſe ſweats, looſeneſſes, &c.

I only make it a queſtion, whether vomiting be proper in fevers *caput petentes*: For, though by it the patient may be relieved for ſome time, by ſuch an agitation a greater quantity of ſpirits being forced; yet, if the obſtructing lentor be not, in a good meaſure, broken and diſſolved, it will only be driven farther into the veſſels, or into ſome ſeries of veſſels it has not reached, by which a delirium muſt be hurried on.

Notwithſtanding this, what you obſerved to me ſhall have its full force, that vomiting is perhaps the quickeſt mean in our power of attenuating a lentor, before it be caſt upon any part. Perhaps it may do hurt after it is impacted, by driving it farther; though it is poſſible, even then, it may contribute to its attenuation, that is, coction. I do not pretend to determine in the affair: But we know that the coction or preparation of humours, to be recirculated with the blood, or evacuated by ſome common outlet, is the work of nature, to be performed in a determinate time, and under certain conditions: And to aſſiſt her at a wrong time, or by too forcible means, would be to diſconcert her in her more regular and ſafer operations. If vomiting is judged abſolutely and conſtantly neceſſary, it ſhould not be delayed beyond the firſt or ſecond day; for after this I think it hurtful.

But, as we are to have regard to the ſtate of the *primæ viæ*, if vomiting is not proper, a purgative will perhaps anſwer all that is intended by it, and do ſomething more than can be expected from a vomit. When a purging medicine is doing its part in cleanſing the *primæ viæ*, its effect ſeems to reach farther. Purgatives excite ſome degree of a fever; and, from what frequently happens, we muſt believe that ſome part of them mixes with the blood. In rheumatic caſes, whether acute or chronical, they are of great ſervice. Sydenham lays a ſtreſs upon them in a *peripneu-*

*monia notha.* Some fevers in the beginning are of no certain type, which, after other evacuations, if they were neceſſary, have turned out of a diſtinct ſpecies, upon purging. Thus agues are every day brought into form, and ſometimes carried off. I know it is ſaid, that purgatives cure agues, by removing the cauſe in the *primæ viæ*, where ſome are pleaſed to place it: But this does not hinder the effect of a purgative to reach the blood, where the cauſe of the ague may be ſtill, notwithſtanding any thing that has been ſaid to the contrary. And I hope they will not place the cauſes of ſome other diſeaſes in the firſt paſſages, where the effects of purgatives are as remarkable as in agues. Nay, ſome tell us, their effect goes ſtill farther, to cleanſe the cuticular ducts, ſo as to favour eruptions: And it is well known that ſome kinds of eruptions inflame and turn worſe, upon taking of purgatives, I mean during their operation. Nor will all this appear ſtrange, if we conſider, that the ſame medicine, differently managed, will vomit, purge, paſs off by urine or ſweat. I need not mention that they are known to complete an imperfect criſis, and to promote or aſcertain it, where there are no evident, or but very doubtful ſigns of it.

From all this I would alledge, that, in many caſes, purgatives are more proper in the beginning of fevers than vomits. For, while they cleanſe the ſtomach and inteſtines, they ſeem to have a peculiar virtue to attenuate a ſizy blood: Beſide that, they make a notable revulſion from the nobler parts, upon which the

the force of the diseafe fo readily falls; and all this without that hurry and difturbance, fo often occafioned by vomiting. It is probable that, after bleeding to a due quantity when neceffary, purgatives timely given would either break the force of the difeafe, or difpofe it to take fome more favourable form, as of remittent or intermittent, or perhaps deftroy it. I fhall not affure you of this from practice; it requires more time than I have had to bring conjectures to a certainty.

One thing I would not mifs to take notice of here: The diftemper fo mortal amongft the cattle in this country, is a fever of a particular kind. I know of no medicine that has been of much fervice, either to prevent or cure it.

The moft fuccefsful method to prevent it is, when the cattle are thought to be infected, or the infection near, to change the grafs, by which they are purged; and this is the ordinary effect of new grafs. We cannot think that it is owing to the particular qualities of the grafs as a proper antidote, that they are preferved, grafs being much the fame every where; but it muft be from its purging quality: For if this vifible effect does not follow, I am afraid they will not efcape. This fuggefts to us the ufe of purgatives in this difeafe of the cattle, which, amongft the many remedies handed about, and faid to do wonders, is fcarce ever thought of.

Having mentioned this difeafe of the cattle, a comparifon might be made betwixt it and fome fevers that have affected human bodies; fo far as they may be found to proceed from the fame ſame firſt cauſe, viz. the air and weather. For ſome years the ſeaſons have not been orderly. They have been unkindly, as they ſay. Warm open winters without froſt, rainy ſummers and harveſts, have been generally complained of. If by theſe a diſtemperature of the fluids is brought on, it will be kept up, ſo long as the general courſe of the weather is the ſame. We with the beaſts are under the ſame external influences from the air and ſeaſons; and the ſame diſeaſes, near, will be found in human bodies as brutes, though ſomewhat different in appearances; which is not ſtrange, if we conſider that the beaſts are conſtantly and more immediately expoſed to theſe influences, their food being always the ſame, and very different from ours. Some have imagined this diſeaſe of the cattle to proceed from the great ſwarms of inſects, of the clock-kind, that come in ſummer. I ſhall not enter into a diſpute about this. But the ſame external cauſes, that favour the increaſe of theſe inſects, will produce the diſeaſe amongſt the cattle, and diſeaſes of the ſame kind amongſt men too. I have juſt taken notice of this to oppoſe a general miſtake of taking, for cauſes of diſeaſes, things that are obvious to ſenſe, and becauſe they have ſomething uncommon in them; while air and changes of weather are neglected, which are cauſes much more powerful and conſtant, and certainly productive of the greateſt alterations in bodies, tho' in an imperceptible way.

But let us ſuppoſe the diſeaſe nothing leſſened, nor altered in its ſhape, and now a continual fever; I am afraid there is a trite way

too

too much insisted in, in treating it, without distinguishing the cause from whence it may proceed. You know the common method, which I have some time been scrupulously exact in following, without the least variation, except where a very evident difference of symptoms obliged me to alter something in my way, which yet was not perhaps very material in itself, nor well judged as to time.

The first thing in the method of cure, I take notice of, is blistering. As soon as a fever is known to be of the nervous kind, a blister is laid to the back, then to the arms, next to the legs, last of all to the head, and at the same time cataplasms are applied to the feet: Which last I have seen so ill-timed, that they have been but an hour or two applied, when the patient, after long watching and raving, has seemed to fall asleep, but never awaked again. All this appears very methodical. And every one is now so well acquainted with blisters, that every body knows when they are to be applied, how many at a time, to what places, and which is, by custom, to have the preference of being blistered first. So that he who will blister, or do any thing else out of the fashion, is hardy indeed, and runs no small risk.

I am persuaded that mistakes are frequently committed, both as to the times of application and the places to which blisters are applied. This fever I have observed to be attended with a muscular tension or universal spasm; and this owing to a lentor in the blood, having more or less tendency to inflammation.

Blisters

Bliſters are abſolutely neceſſary to attenuate this lentor, and the good ſucceſs of them every day convinces us of their efficacy this way. But then they very much increaſe the ſpaſm that attends the fever, eſpecially if applied to parts more irritable, as back and arms. I have been much diſappointed, and at my wits end what to do, when bliſtering, which I moſt truſted to, has heightened all the ſymptoms; and this was moſt obſervable from the pulſe; which, in the intervals betwixt the different bliſterings, was pretty full and ſoft, upon every new application becoming ſmaller and more contracted, other bad ſymptoms increaſing in proportion. This contractedneſs of the pulſe I could attribute to nothing but a general ſpaſm, from a ſtimulus applied to a nervous part, as is the ſkin, which, by conſent, will bring every part of the body, capable of it, into a ſtate of contraction; and this is a property every particular fibre is endued with.

To increaſe the contractile power of the veſſels, is, in ſome caſes, a very good intention, whereby to attenuate the viſcidity of the blood; and that is, where the veſſels are relaxed beyond their juſt dimenſions. But, in the preſent caſe, the veſſels ſeem to be too much contracted, perhaps within their natural diameters; and to increaſe their force now, would be to render yet more compact their ſizy contents: Which poſſibly might be diſſolved and rendered fit for circulation, were the ſtricture of the ſolids taken off, and room given to the particles of the blood, now ſtrongly in contact, to ſecede from one another; proper

attenuants

attenuants of the blood and diluents being given to aſſiſt herein.

Bliſters are likewiſe hurtful, as they draw off a great quantity of ſerum, and leave the blood thicker. It is true the good effect of a bliſter in attenuating the blood, may make amends for all loſs of ſerum by the bliſtered part. But I mention this inconveniency, becauſe I think bliſters are not always intended to evacuate. And we ſhould be very careful not to make any inconſiderable drain of ſerum from the blood; becauſe it is ſo neceſſary for the dilution of its thicker parts, and ſo hinders obſtructions from being formed. But, which is worſe, the loſs of ſerum by bliſters, is the ſame as taking away the ſame quantity of blood, which the patient is not in a condition to bear.

I know it is abſolutely neceſſary to do ſomething, the ſooneſt we can, in fevers, that we may be before-hand with the diſeaſe; for an opportunity loſt at firſt may not afterwards offer. And, from what I have been ſaying, you muſt not think that I am againſt the uſe of bliſters; but I would gladly fall upon ſuch a way of applying them, as they ſhall anſwer all that is propoſed by them, without the hazard and inconveniency that attends the application of them the ordinary way. If I durſt propoſe a method, it would be this:

Upon the firſt appearance of the head being affected, as when the urine turns pale, when they ſigh and have great anxiety, are deaf, or the eyes ſparkle, or look ſtaring, &c. I would have a bliſter applied to the whole head. This

I have seldom seen done, till the delirium was come a great height, and seemed then to be done as a push for the patient's life, when, indeed, it is more likely it did mischief; but might have done great good, had it been applied sooner.

We have instances where, by blistering the head, giving the strongest alexipharmics, and every thing that could quicken nature, the mortal symptoms have been commanded after they were come on. But I am apt to think, that this happened in very low cases, where the strongest spur was necessary, and could do no harm. And singular instances should not determine us to stick close to a method, as if the first symptoms of a delirium were too inconsiderable to require this Herculean remedy; but the symptoms are suffered to go on to their greatest height before it is applied, lest we should be thought to do too much where less might have done. Besides this very bad reason, if ever used, the appearance it has of severity makes people afraid. But blistering the head does not put such a force upon nature as is thought, nay, not so much as blistering other parts. A blister on the head gives far less pain than when applied to any other part; which shews that this part is less irritable, and consequently all the bad effects from too great irritation will be prevented; which I said before, were an increase of the spasm, and a further stricture of the vessels. A blister betwixt the shoulders has been known to bring on a delirium, which has not gone off till the blister

was

was removed. This could be occaſioned by nothing but the irritation.

When a delirium comes on, there is then a beginning obſtruction of the brain. By applying a bliſter to the head, we endeavour to attenuate and diſlodge this obſtruction; which we have a good chance to do, while it is but ſmall, and the veſſels have not loſt their action by being over-diſtended. If we can reſolve the obſtruction at this time, the ſame cauſe that reſolved it will alſo ſtimulate the veſſels, and give them a firmneſs able to reſiſt the viſcidity being forced into them any more. Beſides, the blood will be more determined to flow by the external carotids, by which the preſſure will be conſiderably taken off the brain. And will not a ſtimulus affecting the muſcles and membranes of the head externally, accelerate the blood's motion in the external jugulars, and ſo give ſome relief to the brain this way: Thoſe acquainted with the anatomy of this part will eaſily find how bliſtering anſwers the purpoſes both of revulſion and derivation, and how the active parts of cantharides may paſs into the brain, ſo as to reach the ſmalleſt veſſels that are obſtructed.

Where-ever there is an obſtruction, we incline to make our applications as near the affected part as we can. By bliſtering the back, then, in a delirium, we fail in this rule, this part being very remote from the ſeat of the obſtruction; and all the good that is obtained by it, is attenuating the blood in general; when, in the other way, the whole force of the bliſter is immediately exerted upon the part obſtructed.

But let us ſuppoſe the head not to be bliſtered till late in the diſeaſe, when the obſtruction is great, or the veſſels of the brain being overdiſtended, have loſt the power to recover themſelves; the violent ſtimulus will tear and deſtroy theſe delicate veſſels; or the matter will be farther impacted into the brain, and the obſtruction rendered irreſolvable: And, if the veſſels have loſt their elater, from an overdiſtenſion, we do nothing at all. It is for theſe reaſons, I fancy, that we are forbid to bliſter the head when the eyes are inflamed, which is a ſign there is conſiderable inflammation of the brain too; and if we dare not bliſter in this caſe, the only time we have left us to do it in, is while the obſtruction is forming, or at moſt has occupied but a ſmall number of veſſels.

I ſhall not mention the good ſucceſs of bliſtering the head in ſome caſes where it might have been doubtful, other things having been adminiſtered. But it was remarkable in a young man, ill, as was thought, of a rheumatic fever: A delirium came on very ſoon; a bliſter was applied to the head, and, a few hours after, it went off. The ſurgeon coming next day, took off the bliſter, and very ſoon the delirium returned. The bliſter was again applied, and with the ſame ſucceſs as before. It was from this inſtance, and obſerving ſymptoms to increaſe upon bliſtering other parts, that I took the hint to bliſter the head firſt in a delirium; and, having tried it ſeveral times ſince, I flatter myſelf that I was not diſappointed.

When

When a blifter is applied to the head, it is to be minded, that it muft be kept on three or four days, becaufe it will not do its office to any purpofe, in a fhorter time. This is another reafon for applying it foon, that it may have fome effect, before the delirium comes to a height, or has continued above three days.

I do not imagine that bliftering the head will prevent, or always carry off a begun delirium. It is feldom we are fo lucky in any cafe, and I do not wonder to fee the delirium come to a confiderable height, notwithftanding this precaution. But if by it we can fo difpofe the veffels of the brain, or the obftructing matter in them, as that the obftruction fhall be refolved in a proper time, which happens when the delirium does not continue above four days, then I think we do a great deal.

While we are thus endeavouring by bliftering the head, to refolve a beginning obftruction, and render the brain fome way able to refift being farther obftructed, the legs fhould be bliftered, that the force of the circulation may be determined downwards, and the head relieved. I once imagined that blifters at the legs gave little pain, having had fome inftances where the patient made but little complaint of them: But I am now convinced they are the moft painful of all blifters It does not feem agreeable to the fcheme I am propofing to blifter the legs; becaufe of the great pain and irritation, and lofs of ferum that happens by it, and which I alledge fhould be prevented. I own there is a dif-

difficulty here, and every thing has its advantages and disadvantages. Though there is a considerable loss of serum by blistering the legs, a discharge may be more safely promoted here than from any other part; it strongly diverts the humours from the head. The sense of irritation will be much abated by the frequent bathing of the legs, which may be done some time before the blisters are applied. The hair should be shaved off the legs, because of the exquisite pain the pulling of it occasions in dressing the blisters.

The feet and legs should be warm-bathed two or three times a day. By this, not only the parts to which the bath is immediately applied will be relaxed, which will a good deal allay the spasm through the whole body, but a great many aqueous particles will get into the blood, which, mixing with it in the extremest vessels, will cool and attenuate, and be more effectual than drinking plentifully, to dilute it. Such things may be put into the bath as will best answer these ends.

Your observation comes in very properly, as a caution not to be too free in bathing the feet, however harmless it appears to be: I was not aware of the danger of it, which makes me now the more pleased with what you say of it. In nervous fevers, a delirium is sometimes hurried on, and much hurt done by a pediluvium. The mischief it does, seems to be owing to the withdrawing too much from the head. As they cannot bear bleeding, but faint from lowness, so neither can they bear the simple revulsion, by putting the feet in warm water,

water, with the head elevated, (as is common, to get them into the water), without fainting, raving, and bad nervous ſymptoms.

Acrid cataplaſms, applied to the feet at this time, might divert from the head; but they ſtimulate too much, and ſo increaſe the ſpaſm, giving as much pain for ordinary as bliſtering, and the patient is thrown into a rage by them. I cannot conceive of what benefit they can be when applied the third or fourth day of the delirium, being in no ſenſe ſuitable in that ſtate of the diſeaſe. And, inſtead of acrid cataplaſms, thoſe of an anodyne relaxing virtue, and that ſome way attenuate the blood, are more proper; ſuch as, *capita papaveris*, *ſal ammoniacum*, *ſtercus bovinum*.

Epithems will be of good ſervice here, and the beſt I know is one you mentioned to me of wine, camphire, and *acetum roſatum*. This applied to the temples and forehead, armpits, wriſts, and other nervous and glandular parts, will very much ſoothe and allay the ſpaſm; and being grateful to the ſmell, and penetrating, will refreſh, attenuate, and reſolve. Something anſwering the ſame intention may be frequently ſmelled at, and ſnuffed up into the noſe.

If, by bliſtering in this manner, and other aſſiſtances, we can hinder a delirium to come on, or ſo provide againſt it when it does come on, as there ſhall not be a confirmed obſtruction; we may then proceed to bliſter other parts, as the diſeaſe ſhall require: Nor need we fear that the irritation, or a large evacuation, which ſometimes happens, will be so hurtful now as they would have been sooner.

You'll certainly be thinking by this time, that I have said enough about increasing a spasm, and loss of serum by blistering. I have observed that this fever was attended with a notable orgasm, so great, that, upon every little disturbance or irritation, the patient was thrown into heats, anxiety, and disorder, which necessarily increased the delirium. It is of the greatest advantage to patients, that they be kept in a dark room, free from noise, or any thing that may disturb them: And, if we are thus careful, by a proper regimen, to procure them quiet, ought we not to be as much so in all the applications we make to them? If we are not, it is just like one with sore eyes, who finds great relief, by having them covered from the light, but has a candle held to his skin till he is burnt; though he be free of pain one way, he feels the smart another. All the quiet one may enjoy in darkness, silence, and by other good management, is soon at an end, when nature is fretted by the painful stimulus of a blister. This way of reasoning will seem to bar the way to blistering altogether, because pain and irritation are inseparable from it. But I think I have shewn how they may be in a good measure prevented; and it will not be alledged, that blisters are useful only as they stimulate and give pain. I have said before, that they are not always intended to evacuate; and I say now, that they would do more good in many cases, if they do not irritate at all, or but very little. In our fever, I cannot allow that they are otherwise useful than by

attenuating

attenuating the ſizy blood, which they do powerfully, by means of a volatile alcaline ſalt. So that the conſequences of bliſtering, a painful ſtimulus, and great evacuation, ought as much as poſſible to be prevented. In other caſes, where there is great laxity and diſſolution of the blood, they will be uſeful, both as they are a briſk ſtimulus, and promote a plentiful diſcharge of acrid or ſuperfluous ſerum.

I do not know whether it be for fear of increaſing this ſpaſm, or making too ſudden a drain of ſerum from the blood, or both, that we are adviſed by ſome to apply but a few bliſters at a time, and to make as great diſtance of time betwixt the applications of them as the caſe will allow; but then they muſt be kept running a good while. This way of turning the bliſters into iſſues, will have a very good effect, as it makes a moderate diſcharge from the blood, as it determines the circulation to ſome particular parts, and by a gentle continued ſtimulus keeps it up, and prevents ſtagnations in the viſcera and organs. I have obſerved it to do very well when the fever runs out beyond the fourteenth day, and the patient, through weakneſs, or that a ſenſible criſis has not given a turn to the diſeaſe, ſtill labours under it, and the event is doubtful. The only hope we have ſometimes is, that things are at a ſtand, and the ſymptoms do not increaſe. In this caſe I am always loth to harraſs nature by a new application of bliſters, leſt the ſtrength ſhould ſink. But if ſhe be well managed, ſhe will at laſt do the buſineſs. For I judge the diſeaſe is for moſt part at a height, the time

I mentioned (they ſeldom die when they get over the fourteenth day) and muſt decreaſe, though ſlowly: And all that ſeems neceſſary to be done is to keep the bliſters running, to give ſuch things as may inſenſibly waſte the diſeaſe, as diaphoretics and gentle purgatives, (I give ſmall doſes of *pil. rufi*, and frequently); not forgetting to nouriſh according to the ſtrength, and to give proper cordials, leſt they languiſh into a hectic, and go off that way. This hectic is of the intermittent kind, partly from inanition, partly from the matter of the fever not fully carried off. Bliſtering can be of no ſervice, and the ſucceſs of the cortex I very much doubt of.

Bliſtering is reckoned uſeful, as it determines the circulation to the outward parts, and ſo preventing internal obſtruction and inflammation. I agree to it, but not in the caſe of a ſpaſm; which indeed is the reaſon that the blood is forced from without inwards, upon theſe parts where there is the leaſt reſiſtance.

It ſometimes happens, that the patient is fatigued with continual or partial ſweets, and the loſs of the more watery parts of the blood, ſo neceſſary to cool and dilute it, ought carefully to be prevented. Theſe ſweats are ſymptomatical only, and give no relief, but frequently the ſick grow worſe upon them: And they are owing to a ſtricture upon the veſſels, by which the watery parts are expreſſed and poured out by the ſkin; which is relaxed, and eaſily allows the expreſſed ſerum to paſs through, for want of a due ſecretion of ſpirits from a thick blood, or that this ſizy blood cannot be circulated

circulated into its veſſels, to give it a tightneſs. That there is but a ſmall force of circulation towards the ſurface, appears from the degree of heat felt upon the ſkin, which is ſeldom more than natural. I have ſeen a bliſter ſtopping theſe ſweats, no doubt, by giving a greater firmneſs to the ſkin. But, as I make a ſcruple to apply a bliſter too ſoon, unleſs to the head, becauſe it increaſes the ſtricture, ſome other method may be tried to prevent the ſweats, cauſing the patient to ſit up in bed, if he is able, and ordering the bed-cloaths in a proper manner.

There is one thing more I would obſerve, it is with reſpect to the time in which bliſters ought to be applied; which ought never to be in the acceſs of a fever. The diſeaſe has generally exacerbations towards night, which is the ordinary time of applying them. I am ſure a bliſter of itſelf raiſes no ſmall degree of a fever: Now, betwixt this and the paroxyſm of a fever already upon him, we cannot but pity the poor patient, who moſt undergo a pretty ſevere trial. But this would be little minded, if the diſeaſe were not really increaſed by it. For if, in the acceſs of a fever, which we would gladly prevent if we could, nature is under a violent ſpaſm, it would ſeem needleſs to put a greater force upon her, by a ſuperadded ſtimulus. The fitteſt time then to apply bliſters is, when there is the greateſt abſence of the fever. And indeed the management of bliſters in this fever, and from the cauſe I have all along ſuppoſed, is not ſo eaſy as is thought. To apply them at ſuch times, and

to

to ſuch places as moſt favours nature, to obtain all that we wiſh for from them, and do no harm by them, when we intend by them to do the greateſt good; to do all this, I ſay, will require ſome little niceneſs.

In paroxyſms of fevers, we are not at liberty to give the cortex, not in convulſive caſes, which are always attended with pain and tenſion. This medicine, if given in the fits of theſe diſeaſes, while nature is under a violent ſpaſm, might, by its great ſtimulous or aſtringent quality, ſo ſtraiten the veſſels as to occaſion a ſtrangulation in them; ſo that, however uſeful it may be out of the fit, we find by experience, that it is hurtful and dangerous in it. The ſame may be ſaid of all theſe things which act by a ſtrong ſtimulus, which we are careful not to apply in the acceſs of fevers, but rather ſuch things as ſoothe and relax, and in ſhort have an effect quite oppoſite to that of a ſtimulus. And thus I have told you my opinion about bliſtering, and the time, and manner in which I would have it done: You ſee I have only taken the liberty to invert the order, doing that firſt which is generally deferred to the laſt. When I reaſon upon the nature of an obſtruction, together with the time and methods proper for reſolution, I cannot think but this way of bliſtering is more agreeable to the notion we have of theſe things than that which is commonly followed.

But bliſtering will not of itſelf do all. I have ſuppoſed, as the cauſe of the fever, a lentor of the blood, and a ſpaſm conſequent of it; and theſe depend ſo much each upon the other, that,

that, without ſome caution, we may, providing againſt the one, eaſily increaſe the other. The intention of the cure then will be double, to attenuate the viſcidity, and allay the ſpaſm. Bliſtering is very well fitted to anſwer the firſt, but with this inconveniency, that it increaſes the other, unleſs managed in ſome ſuch manner as I have hinted; and at the ſame time medicines be given that may anſwer the other intention, or both.

Common practice bids us, without making proper diſtinction, give warm, generous medicines, alexipharmics, and all of that tribe that heats, ſtimulates, and forces ſweat. But, if what I have ſaid about bliſtering be true, we will ſee that medicines of this kind are ill-ſuited to the nature of this diſeaſe, at leaſt in many caſes. For theſe things that ſtimulate, and ſo increaſe the circulation, will but farther increaſe the ſpaſm and obſtruction. And if ſweats are forced, this will render the ſizy blood ſtill thicker, and leſs fit for circulation.

It will be ſaid, however, that this fever is attended but with a ſmall degree of inflammation, and little increaſe of the circulation; which ſeems to indicate medicines of the warm kind. This at firſt view will appear true; but I am convinced from experience, that nature is here as much affected with a ſpaſm, as in ſome caſes where there is greater inflammation; and that this ſpaſm would not produce the bad effects it does, if nature were not forced on by indiſcreet methods, to exert herſelf in the cure of the diſeaſe more than is neceſſary. The many inſtances of recovery amongſt

mongſt the poorer ſort, who have little attendance and leſs medicine, may be a proof of this. The lowneſs of the pulſe, fainting, and moderate heat impoſe upon us, making us believe that the blood is poor or defective, or that there is ſomething of malignity, (which is not very well underſtood), and that upon theſe accounts the vital functions are not performed: And upon this ſuppoſition the indication is taken for warm ſtimulating medicines. But it may be eaſily made to appear, I think, that theſe ſymptoms proceed from a very different cauſe, viz. a lentor of the blood, and a ſpaſm depending upon it; which is the reaſon that there is a leſs ſecretion of ſpirits for the uſe of the ſeveral organs, and a more difficult circulation through the whole ſyſtem of veſſels. From the ſame cauſes, in other diſeaſes, we obſerve the ſame effects. This is plain in the caſe of vapours or hyſteric fits, which moſt frequently are thought to proceed from a viſcidity of the fluids, and ſuch a conſtitution of the ſolids, as I chuſe to call a *genus irritabile*. A rational practice has found, that in this diſtemper, I mean in the fits of it, warm, ſtimulating, or highly attenuating things, given with a deſign to raiſe the ſpirits, are not the ſafeſt.

But the connexion betwixt vapours and a fever will not be eaſily perceived; nor is it neceſſary, in reaſonings of this kind, that things ſhould anſwer ſo exactly in all circumſtances. I ſhall own my want of invention, in not being able to contrive a different cauſe for every different diſeaſe that may fall in the way; but I endeavour

endeavour to pursue the same cause through as many distempers, and degrees of a distemper, as can be done with any probability: Though, in all appearance, the difference is very great betwixt an acute and a chronical disease; perhaps it will not be found so, when the thing is more nearly considered. For do we not see acute diseases ending in chronical, when the *materia febrilis* is not fully exterminated? There seems to be no other difference, but that the cause, not existing in the same degree or force, cannot produce suddenly a train of violent symptoms, as it did, when, in a greater degree, it was the cause of a fever. On the other hand, many chronical diseases are not cured, but by a supervening acute one. Thus palsies, epilepsies, and several other diseases we have no name for, have been carried off by a fever continual or intermittent. Now, whether is this fever accidental, and in its cause different from that of the former disease, or is it the same cause increased, or acquiring some new quality, so as to be able to excite a fever? The more obstinate chronical diseases are said to have been cured by a quartan. I shall therefore, with leave, call this the mid-way betwixt acute and chronical, unless a quintan or sextan be more properly so; but these rarely happen. In chronical diseases, the cause of them seems to be seated in the smaller vessels, and has not such properties as to produce any effect in the larger vessels, which I take to be the scene of fevers. But if this matter, from its nature, or a course of external causes, changes its seat, and in the larger vessels forms larger moleculæ, acquires new qualities, and is put into motion, we may conceive how at length it will produce a fever: And, proportionally to the degrees of increafe, it will at firft, leaving the form of a chronical difeafe, appear in an acute form of the longeft period, and fo on till it becomes a fever without periods, or continual. When I have faid this, it will look ftrange if I fay again, that the acceffes or fits of chronical difeafes, many of which are periodical, fome regularly, fome irregularly, are efforts of nature, to put on an acute form: And, when all our art has been baffled in the cure of them, we have been glad to leave them to the chance of an acute diftemper, by which they may be carried off. Though I fay that this change of a chronical difeafe into acute, is owing to an increafe of the caufe, I do not mean it as bad; for this increafe is a gradual tendency of the matter to coction, to be affimulated again to the mafs of fluids, or wholly expelled the body.

Nor, with refpect to the cure of both, are the intentions different. The moft general are, to evacuate, or fupprefs an evacuation; to attenuate the blood, or preferve its confiftency; to refolve an obftruction; to correct acrimony; to reftrain the irregular and increafed motion of the fluids, or raife it when too languid. All thefe ends are obtained by the fame means. The only difference then is this: In acute cafes, the difeafe finifhes its courfe in a fhort time; the fymptoms during this time are all upon the patient at once, and very urgent. The matter of the fever is either very moveable, and fo may eafily be thrown upon fome part

part neceſſary for life; or it is firmly inherent in the veſſels, and by a too forcible trial to remove it, may form a mortal obſtruction. This obliges us to be cautious both in the choice and application of medicines, and very obſervant of the times that are moſt proper for ſuch application. On the other hand, it is eaſy to ſee, that in chronical diſeaſes we are more at liberty, and may ſometimes make a bold attempt for the patient's recovery; nor need we be ſo exact, either in the choice of remedies, or in the time and manner of applying them. It would be taking up your time too much, to advance all that might be ſaid upon this head, the deſign of which is to ſhew, that we ſhould not put ſuch a difference betwixt acute and chronical diſeaſes, as not to admit of reaſoning from the one to the other, or think that the methods of cure are as oppoſite as ſome imagine the cauſes of them to be. I do not think I am ſtraining the thing, when I plead the practice in ſome nervous chronical diſtempers, as an argument for the ſame practice in this fever, which, becauſe of the near reſemblance it has, in many things, to theſe diſeaſes, is properly enough called *nervous* too.

The medicines I would chuſe, as beſt ſuited to the diſeaſe, are ſuch as do not ſtimulate, or but very little, nor increaſe inflammation: *Oculi et chelæ cancrorum, ſperma ceti, rad. ſerpentariæ virg. valerianæ ſylv. caſtoreum, ſal prunellæ, ſal abſinthii, ſp. nitri dulcis, ſp. ſalinus aromat. ſp. cornu cerv.* and the like. *Antimonium diaphoret.* is highly commended by ſome in a delirium. Saffron in ſmall quantity is anody... If we examine into theſe, the moſt of them will be found to enter into the compoſition of the moſt celebrated antiſpaſmodic remedies. As ſome of them allay the ſpaſm, others attenuate the blood; both which contribute to keep up a free perſpiration; which is always a good ſign, forced or continual ſweats being generally hurtful. They may be mixed and proportioned as there is a greater or leſs tendency to inflammation. They may be given with more advantage in ſmall doſes, and every hour or two, than every fourth or ſixth hour, as is commonly done; when the doſe being larger, the patient finds himſelf heated, ſweats, and is uneaſy: Whereas, by giving them in ſmall doſes and frequently, we put no force upon nature, and have a conſtant, equal, and gentle effect from the medicine. They may be given conveniently in a julep made up of cordial and analeptic waters, as they are called, which may be drank at pleaſure, and will not fail to give relief under lowneſs and oppreſſion. We are not, upon every change or appearance of a ſymptom, to ſtop the giving of theſe things, or give more forcible medicines upon an increaſe of ſymptoms. Theſe perſiſted in, even when they ſeem to be doing but ſmall ſervice, will perhaps in the event anſwer our expectation; for it is not the giving of a medicine for a day or two that will do the buſineſs; they are very unreaſonable who look for any conſiderable effects from ſuch ſlight adminiſtrations. I ſhould have mentioned camphire, which has this great advantage, that it may be given in

any

any case of this kind, without fear of increasing inflammation; and, whether the case have more or less of it, it is very proper, and may be conveniently joined with other medicines, whether intended to warm or cool. Camphire is really an antispasmodic, as by immediately affecting the solids, it procures a relaxation of them when too much contracted. How it becomes useful in hysteric and maniac cases, as a diaphoretic, antaphrosyniac, &c. may be accounted for from this. The particular manner of its operation will be understood from what shall be said, when I speak of the use of wine in fevers.

It is well known, that dilution has a large share in the cure of fevers, and drink must be given plentifully, not only as a vehicle to convey medicines into the blood, but as it cools, attenuates, relaxes, and keeps up the liquid secretions in a natural order. Lenient, aperient decoctions, somewhat saponaceous, are best suited to the present case. These, as they are easily miscible with the blood, do not run off so soon as drinks that are thinner, or vinous, and force sweat, which is not to be encouraged beyond a moisture. Though sack whey is the common drink, and very good, I sometimes prefer common whey. When I do not, or but seldom, favour a drink that has wine in it, you'll readily guess that I forbid all ardent spirits, as they stimulate too much, and may in some degree coagulate the blood. Nor do I see that spirituous juleps are proper either as vehicles, or to be taken when faintish. When I allow any thing by way of cordial, it it is two or three ſpoonfuls of malaga or ſack by itſelf, more or leſs as the caſe requires. Theſe wines being ſtocked with rich oil, when applied to the veſſels, adhere, and give a kindly heat and gentle ſtimulus. When they are drank to any quantity, they do more harm than the lighter and more ſprightly wines; but taken in the ſmalleſt quantity, are a much better cordial, and are preferable to ardent ſpirits, which indeed have a ſudden effect, but of no continuance. Thoſe who have uſed any freedom in drinking theſe liquors, will know the difference.

The neceſſity and uſefulneſs of wine, together with the manner of its operation, will appear from what follows. It is a known property of heat, that it leſſens the power of coheſion in all bodies, and in a proportionate degree deſtroys it in the hardeſt. This power of coheſion in different bodies, when we conſider the manner of it, is matter of curious ſpeculation. But it is wonderfully adapted to uſeful purpoſes in the flexible canals of animal bodies, which can be ſtraitned or relaxed from various accidents, and as the occaſions of nature may require. The effect of heat upon the human body is ſufficiently felt in hot weather; when all the ſolids are relaxed, even to weakneſs and loſs of ſpirits. In the firſt attacks of acute diſeaſes, attended with horror and rigor, or the cold fits of an ague, the patient ſtrives to divert the uneaſy ſenſation of cold and the ſtruggle and pain he is in, by drinking ſomething warm, or ſitting over the fire; which gives ſome relief for the time:

And

And the beſt way to pr[illegible] the ſeverity of theſe coldneſſes, is to put him to bed, and give plentifully of diluting, aperient liquors, warm. If then a certain portion of elementary fire, applied outwardly, gives ſo much relief, any thing that will have the ſame effect, and applied to the inſides of the veſſels, promiſes to anſwer the end much better, of procuring an agreeable relaxation to the ſolids, under that ſtate of rigidity they are in, when affected with a ſpaſm. This effect I ſuppoſe wine will have, if given diſcreetly.

Three ſorts of medicines operate in this manner, and differ only as this warming penetrating oil is more or leſs involved. Theſe are camphire, wine, and natural balſams, with their oils. I had almoſt placed opium at the head of them, but I was afraid of a debate. Camphire is extremely volatile, having nothing of phlegm, gluten, or impurer oil to hinder it from flying off. Its volatility renders it in ſome caſes more uſeful than wine, particularly in inflammations, where, did this oil adhere to the veſſels a long time, it would increaſe it. But, for this reaſon, it is leſs uſeful in other caſes than wine, which, applied to the veſſels, adheres more firmly, and ſo has a more laſting effect, which ſeems neceſſary in fevers, where the inflammation is ſmall, and the tenſion great. Theſe things, beſides relaxing the ſolids by their kindly heat, which ſome, fond of words, would call *congenial*), they likewiſe attenuate the fluids, and blunt the acrimony, and ſo remove the cauſes of tenſion and inflammation. Natural balſams are more fit for external application, their thickn... ...o great cohesion render-ing them unfit, in cases where the other are proper. When they are given internally, it is chiefly with a design to deterge, prevent putrefaction, and increase heat, where it is below the standard. Oil of turpentine applied externally to the spine, before the fit of a quartan, is said to have been a cure. I cannot account for this, but from the heat it occasions. This heat diffuses an agreeable warmth through the whole body, by which the tension is taken off, and the resistance which was given to the blood's motion, which now flows easily into the extremest vessels.

I commonly give a small glass of wine five or six times a-day, and never observed any bad effect from it. I have known it taken to a much greater quantity for several days, beside a reasonable quantity of common julep, which contributed not a little, I believe, to the patient's recovery. I do not think, however, that wine is to be given at all times of the disease, particularly in the beginning, when the inflammatory symptoms are any thing considerable, but, for several days before the height, it may be given with great advantage, as well as after it.

I sometimes meet with opposition in thus prescribing wine. The very mention of wine in fevers, and where there is a delirium too, is apt to give people bad impressions of rashness or want of skill. And because such will not receive any thing they are not used to, without the sanction of antiquity or great experience, I can tell them, that this practice of gi-ving

ving wine, fometimes in a ... cafe, has the grave authority of Hippocrates to fupport it.

Wine promifes to be ftill more ufeful, where the blood is poor and much diffolved, in which cafe it naturally acquires fome kind of acrimony. For it will reftore the relaxed veffels to their former tone, invigorate the blood's texture and motion, by ftoring it with warm balfamic parts, exhale the vapid ferum, refift putrefaction, and correct the acrimony. Upon all which accounts it becomes a fovereign remedy in all fevers of this kind, that have not a greater degree of inflammation in them, and, in fome particular kinds of fmall-pox, there does not feem to be a better, provided it be given in a right manner and fufficient quantity.

When the patient is faint and oppreffed, wearied and anxious, we are obliged to prefcribe fomething by way of cordial, when perhaps the cafe will not allow of any thing that is heating. I am of opinion, that the beft way to procure relief under fuch loweffes and oppreffions, would be by gentle anodynes. Thefe, as they allay the fpafm, would give greater freedom to the blood in its motion, and have fome effect to attenuate it, and fo would anfwer the intention of a cordial much better than what is commonly given. and which acts no other way than as a ftimulus. When the difeafe feizes with greater figns of acutenefs or inflammation, as vomiting, loofenefs, heat, thrift, fighing, and ftrong depreffion, (as this hurry of feeming inflammatory fymptoms is more or lefs, fo will the faintnefs and anxiety be): When this is the cafe, I fay, a gentle opiate,

piate, given in some refreshing julep, will have a happy effect to allay these overbearing symptoms. These symptoms then happen only the first days; for, by the time the patient is delirious, they are pretty much gone, or he is not sensible of them: This is the proper time to try opiates, lest, by the urgency of such symptoms, greater be brought on. A prudent use of them might hinder a delirium from coming so soon as otherwise would happen. This would be no small advantage, for the patient's life depends upon the delirium its coming sooner or latter. When the seizure is with moderate symptoms, there is little occasion for them, till the disease is further advanced, and a delirium comes on with watching, raving, and a dreadful train of nervous spasmodic symptoms, which will not be commanded by a less powerful remedy than opium.

But perhaps it will be thought, that there is yet no great necessity for opiates, as no considerable symptom has appeared to require them. If the symptoms depended upon any other cause than that I mentioned, I should think so too. I still insist upon it, that there is in this fever a violent tension of the solids, even when we cannot be sensible of it from more evident appearances. And of how much advantage it would be to soothe and compose nature, under this tension and proneness to be irritated, one may very easily conceive. The known effects of opiates, their being without danger when rightly managed, and their great usefulness, in cases so like to this I am speaking of, should encourage us to try them here too. We are informed by some authors of the incredible success of opiates in fevers of a bad kind. You have told me, that an expected crisis may be safely promoted by giving an opiate; this I suppose is when there is some fear that nature may fail, if not well assisted in it. This has a great deal of reason in it, and it cannot be useful this way, but as it allays the violent spasm, and frees nature from the wild hurry and struggle she must be in, in the instant of a crisis: And by this means all impediments being taken away, the humours already concocted and fitted for separation, fall off of themselves almost, by some common outlet fitted, according to the exact laws of the oeconomy, to receive them. Dr Boerhaave * has an antipyreticon, which, in agues, he calls *rarofallens*. From my own trial of it, I know that it will prevent the fit for the time, and, if it does not remove the disease, it paves the way for more successfully exhibiting the cortex, which is given frequently without effect. As the great stress must be laid upon the large dose of opium in it, this may be referred to what is said. In hysteric cases, we can do little without opiates to allay these sudden and violent affections of the body in that disease, which I have frequently observed to grow worse upon the smallest irritation. In convulsive disorders, I think they might be more frequently used, and with more success than what is common. All things administered here, externally as well as internally, are warm, stimulating, spirituous, and aromatic; and therefore are

* Libello de mater. medic. § 761.

are ſaid to be good and comforting for the head. I am very certain the ſymptoms are frequently increaſed by theſe. But I have ſeen bathing from the middle down, dry cupping, and whatever will make revulſion, without loſs or irritation, and theſe things allay a ſpaſm by their anodyne quality, (properly) having a ſurpriſing effect, to procure a remiſſion of the fit, when the moſt noted cephalics have been hurtful or uſeleſs.

In adviſing the uſe of opiates, I ſhall be very much under correction; and believe that it is only in ſome caſes of fevers they can be given with ſafety and advantage. Where there is any conſiderable degree of inflammation, they are thought to be hurtful, eſpecially if there be obſtruction of a particular part: Yet the papaveracea are given here. And, where there is great relaxation and diſſolution of the blood, they are plainly out of the queſtion. It is in a mixed kind of fever that they can be uſeful, ſuch as ours was, where there were degrees of inflammation, but that would not admit of the methods of cure in inflammation, and had in it beſide a remarkable ſpaſm. I know it will be taken for a putrid fever of the rheumatic kind I have been ſpeaking of: But there was conſiderable difference in many things from that fever, as we have it deſcribed: And therefore I have retained the common name of nervous, being more careful to explain the diſeaſe as it is in itſelf, than fix it to a particular claſs: *Putrid* is a term, which, till defined, gives me no idea of the nature of it.

I do not think [illegible] are to be given to any considerable degree, but in ſuch a manner, that though their effect can hardly be obſerved, we may be ſure they have ſome. And, by mixing them with other things, we may prevent their bad conſequences, ſo often obſerved and juſtly feared. The bad effects of opiates are not from their being abſolutely hurtful in themſelves. There is a great deal in the time, the manner, the doſe in which they are given; not to ſpeak of the patient or the diſeaſe. Were theſe circumſtances duly minded, opiates might be applied ſucceſsfully to many more purpoſes than they have yet been. I have known an aſthma increaſed by what was only thought a reaſonable doſe of opium: But the ſame quantity, or a little more, given at times, in ſuch manner as the whole ſhould not be conſumed in leſs than twelve hours, has had the deſired effect, and the patient has been greatly relieved. Frequently we are obliged to give an opiate in the morning, the effect of which is not wanted till night, for ſome are wakeful after taking it.

I am favoured in the opinion of opiates being uſeful in this fever, from the contrivance of a medicine now pretty much in uſe, the tincture and decoction of ſerpentaria of the Edinburgh diſpenſatory, which are gently ſudorific and anodyne. The tincture is certainly a fine medicine, and the only objection is, that it will be too warm in ſome caſes, and that the opium is there joined to the other ingredients, and muſt be always given, whether neceſſary or not. I ſtill like to have it in my own hand.

And now, to put [illegible] long letter, you will easily find from whom several hints and observations here are taken: Any thing of my own will be as easily discovered, as having less to support it, perhaps. I have taken the pains, however, to bring these things into one view, and endeavoured to accommodate them to a general scheme; in which I have kept as close by nature as I was able, having had all along a strict regard to the genius of this distemper. There is a great deal more to do upon the subject: I have only attempted to set one kind of the fever in a clearer light. I wish much to see some, whose greater judgment and practice might better enable them, undertaking something more full and distinct upon it, than we have yet been favoured with.

## XXIV. *Remarks on the Cure of* Agues; *by Dr* Alex. Thomson *Physician at* Montrose.

AGues having been endemic in this place and neighbourhood these many years, I have had good occasion of experience in this disease, and shall mention some remarks I have made in the cure of it.

I went on some years in the ordinary way of vomiting on the days of intermission, as preparative for curing by the cortex, till, reading the old physicians books, I found they recommended vomiting in the beginning of the paroxysm, thinking the morbid matter was then the[illegible] and [illegible] fluxion, particularly a-bout [illegible] dia, which they called its con-coction, [illegible] erefore was then fit to be pump-ed up from the ſtomach, agreably to that aphoriſm of Hippocrates, Sect. 1. Aphor. 22. Πεπονα φαρμακευειν και κινειν μη ωμα, &c. *Cocta non cruda eſſe movenda* [illegible] *medicanda.* Which [illegible]ay of reaſoning is alſo agree[illegible]ble to the account g[illegible]ven of the periodical returns of the paroxyſms of intermitting fevers by Bellini, and all who, ſince him, have wrote on this ſubject in the mechanical way.

Another advantage ſeemed likewiſe to ariſe naturally from the operation of emetics in the beginning paroxyſm, to wit, that, by the vigorous ſhock given to all the parts in vomit-ing, the morbid matter might be ſooner diſen-gaged, and the fit made ſhorter, if not pre-vented.

This method appeared to me ſo reaſonable and natural, that I began to give emetics up-on the firſt appearance of the aguiſh fit, and have found ſo good ſucceſs by this way, that, I have now continued in it theſe twenty years paſt. The only alteration I have made is, that if the coldneſs of the fit go ſoon of itſelf into a vigorous ſhaking, without the ſickneſs of the ſtomach, I poſtpone vomiting till the ſickneſs begins in the hot fit.

[illegible] is eaſy to ſee that in this ſickneſs, from the flow of the morbid matter towards the ſto-mach, one half or two thirds of an emetic medicine will do more and more effectually than the full doſe could do by ſtraining nature when otherwiſe at eaſe. And indeed it would appear evident, [illegible] can be well able to reach the di[illegible] morbid matter is so blended wi[illegible] [illegible]ing mea[illegible] liquors of our body in the intermi[illegible]ease, while

I have frequently seen one vo[illegible] the thus given put away the disease, or, if another paroxysm came on, it was so bro[illegible] by a second dose, that the progress of it co[illegible]d scarce be observed. An[illegible] I have always remarked, that, when patients were treated in this manner, a third, fourth, or less quantity of the bark which was necessary to others, was sufficient to confirm and accomplish thier cure, or to prevent any relapse.

The success I had by giving vomits in this manner in agues, encouraged me to try them also in the analogous circumstances of other fevers; and I have found, that, by catching the times when the horror or shivering and sickness came on, to give a vomit, the reli[illegible]f and consequent benefit were incomparably greater than when taken at any other time.

It is with pleasure I have observed our physicians of greatest practice very cautious in giving the bark for agues; they seem to follow the directions of the wiser antients, in allowing the morbid matter to be concocted, and then to throw it out of the body, before they pretend to amuse their patients with the hopes of a cure, by suppressing for a little the uneasy but ordinary symptoms of their disease. N[illegible] withstanding such good example, and the many unhappy metastases attended with such direful consequences, which the too hasty and preposterous giving the bark in great quantities brings on, yet still t[illegible]e are many, who no soon[illegible]r ſooner can deter[illegible] [illegible]e their patient's diſeaſe to be a[illegible] ague, than th[illegible]y [illegible] down as much bark in the f[illegible] interm[illegible]ſſion as they think may make ſure to prevent another paroxyſm; and, if that does not ſucceed, they repeat the bark as ſoon as the fit is over.

To deter all from ſuch dangerous practice, I could bring many inſtances of jaundice, drop[illegible], aſthma, and all the train of nervous diſorders brought on in a ſurpriſingly ſhort time, after ſuch prepoſterous uſe of the bark, which otherwiſe, when given judiciouſly after proper evacuations, is a noble and ſafe medicine in this diſeaſe. At preſent I ſhall confine myſelf to two or three, where the ſymptoms were very uncommon.

1. A young man had taken five drachms of the cortex in each interval of three fits of a quotidian ague. Inſtead of the fourth paroxyſm, he had only a little horror or ſhivering. Next day, after ſome minutes ſhivering, his ancles were vehemently racked, as if twiſted and cut at once. This agony laſted about five minutes, when the ancles being ſuddenly relieved, his knees were as long affected in the ſame way. Next the joints of the thighs were ſeized; to theſe ſucceeded a hardneſs, ſwelling, and pains of the belly. His thorax being next ſeized, he appeared as one ſtrangled, then he fell down as a[illegible]plectic, and laſtly turned altogether deliri-ous. When that ceaſed after five or ſix minutes, he ſeemed well, about as long as from his beginning to be attacked to his recovery, and then underwent the ſame ſymptoms in the ſame

order and time. He was cured by strong large blisters, emetics, and nervous medicines.

2. A young gentlewoman, labouring under a quotidian ague, with some hysterical symptoms, had a trial made of the bark in small quantities and slowly given; but, upon observing the nervous symptoms rather increase, I discharged the further use of it. Notwithstanding this, it was given till the ague ceased, when regularly at the time of the paroxysm, after a little shivering, she became speechless, her breathing alternately interrupted about half a minute, she inspired with a sibilus through her nose, had contractions of the hypochondria, and her belly was drawn in, with heavings and fallings of the shoulders, contractions of her neck, stretching of the arms, and grippings of the fingers: She remained thus twenty five minutes; recovered then as long as to take a little sack whey, and relapsed into the former circumstances. She remained thus four months: After which she began to have longer intermissions, and some more variety in the symptoms, but has now continued ill these nine months.

3. A gentleman long subject to the ague, resolved to keep it off by a constant diet of bark; he got quite free of his ague, but fell into violent lowness of spirits, and all the train of nervous symptoms.

XXV. Ano-

XV. *Anomalous Shakings after an Ague,* ...; *in a Letter to Mr* MONRO *Professor of Anatomy at* Edinburgh, *from Dr* ANDREW WILLISON *Physician at* Dundee.

SIR,

YOUR design in concluding the history of anomalous shakings after an ill-managed ...ue, (See art. XIX. of Vol. II.) with a gene... observation of the methods of cure in all you ...d seen or heard of, being unsuccessful, was, ...m persuaded, to engage any who had the ...od fortune to cure such patients, to communi...e their method to the public for the benefit ... mankind; and therefore I hope the follow...g history of a woman, whose case was very like the one related by you, will not be unac...ptable.

In July 1733, an unmarried woman, about ...rty years of age, of a plethoric habit, who ...d laboured under a regular tertian ague three ...nths, for which she had got some herbs from ...ardener, which had stopped the fits, came to ... my advice. She was then frequently seized ...th an universal shaking and trembling over ... her body, which continued long. Some...es these shakings seized her head so violent... that two men could not hold it, at other times ... or both arms were thus also affected. Her ...lse was soft and languid, but her veins ap...ared turgid. She had no drought. Her ap...tite was lost. No menses had appeared for ... three

three months. She knew w[illegible]en the shakings were a-coming, and what par[illegible] or parts would be affected; for, she said, she [illegible]lt a [illegible] coming into them. In the intervals from shaking, [illegible]e was drowsy, and inclined so much to sleep, that she would have fallen from the seat she sat on, unless she was supported.

I ordered her to [illegible] blood of at the ancles, and to take two mustard vomits. Being little relieved by these, I desired her to try the cold bath, and to rub her extremities strongly when she came out of it.

After using this method daily two weeks, she came to return me thanks, telling me she was perfectly recovered from all her symptoms.

XXVI. *A* Mania, *from a callous* Pia Mater; *by* Dr EDWARD BARRY *Physician at* Cork, *and* F. R. S.

A Gentleman twenty five years of age, naturally of a dark melancholy aspect and temper, complained, about four years ago, of a weight increasing over his head, which sometimes was attended with a swimming and giddiness, which threw him into fainting fits, in which he often remained for a considerable time deprived of his senses. He said that he often escaped these fits, by keeping his eyes shut when that pressure and swimming seized him. About six months before the time I now write, his friends observed his temper much changed, and soon after he became di-[illegible]red,

... to deſtroy himſelf and o- ... ac diſorder returned frequent- ... ne intervals, he converſed and behaved regularly. For ſome time paſt, he had frequently paroxyſms of a fever which laſted three or four days.

His friends hearing ... inſtances of ſucceſs from the operation of the ... in ſuch caſes, reſolved, after many other methods of cure had been attempted, to have this operation performed.

The day after the operation I viſited him, and ſaw him walk about his room. Next day he could not be prevailed on to riſe, his pulſe became feveriſh, a ſlow delirium and ſtupor came upon him, with ſpaſms in his limbs, which increaſing, notwithſtanding bleeding and other medicines, he died on the tenth day after he was trepaned.

Upon removing the ſcull, nothing preternatural was obſerved in the *dura mater*; but, when this membrane was taken off, ſeveral phyſicians and ſurgeons who were preſent concluded, from the appearance of the *pia mater* on both ſides of the brain, that a large ſuppuration was extended under it; for it was of a colour between green and yellow. Upon examining it, I found it hard and callous, and in moſt places twice the thickneſs of the *dura mater*. There was no appearance of veſſels in it, and it cut like ſoft horn. The cortical part of the brain, which this thickened *pia mater* covered, was much whiter than uſual, with few blood-veſſels. On ſeparating the hemiſpheres of the brain, the *pia mater*, contigu-

ous to the falx, appeared in
condition. The ventricles
very large, and distended with

XXVII. *An* Epilepsy, *from an uncommon Cause, by Dr.* THOMAS SHORT *Physician at Sheffield, and* F. R. S.

IN July 1720, a woman about thirty-eight years of age was brought to me; she had laboured twelve years under an epilepsy, which, from one fit a-month, was come to four or five violent ones every day, each continuing an hour, or an hour and a half; by which she was rendered moppish and silly, and incapable to take care of her house and family. Her husband was reduced in his circumstances, from his affection and care for her, having got and followed all the advice he could. Evacuations of all kinds had been tried; the epileptic and cephalic tribe of medicines had been ransacked, and many other medicines had been used in vain, the disease growing more severe. Her fit always began in her leg, toward the lower end of the gastrocnemii muscles, and in a moment reached her head, threw her down, foming at the mouth, with terrible distortions of the mouth, neck, and joints. Whilst I talked with her, she fell down in a fit: I examined the leg, and found no swelling, hardness, laxness, or redness different in that place from what was in the other leg: But suspecting from her fit beginning always at that part, that the cause of her disease lay there, I immediately [illegible]diately plunged a ſcalpel about two inches into [illegible], where I found a ſmall indurated body, [illegible] I ſeparated from the muſcles, and then [illegible]k it up with a forceps; it proved a hard car[illegible]aginous ſubſtance or ganglion, about the ſi[illegible]e of a very large pea, ſeated on a nerve, w[illegible]ch I cut aſunder, and [illegible]k out the tumor. She inſtantly came out of the [illegible], cried out ſhe was well, and never after had a fit, but recovered her former vigour both of body and mind.

XXVIII. *Of the Cure of an* Ulcer *of the* Lungs *by Blood-letting; by*——————

Gentlemen,

IN my preſent circumſtances it would be inconvenient for me to appear openly as an advocate for the cauſe which I here plead: This obliges me to beg you would ſuppreſs my name, if you think fit to publiſh this eſſay; which, though it ſhould be generally condemned, may at leaſt have the good effect of engaging others to contrive a more ſucceſsful method of cure than has hitherto been made uſe of in this frequent and moſt dangerous diſeaſe, the *conſumption*. I flatter myſelf you will more readily allow me to remain concealed, that I have advanced no facts which require a particular voucher; and that you will thereby much oblige your's, &c.

The ulcer of the lungs is a diſeaſe ſo frequently fatal, that ſome of the ableſt phyſicians make it a queſtion whether ever a conſumption from that cauſe has been cured. And indeed,

indeed, whoever confiders the important office of this vifcus, its fpungy texture, ... perpetual motion, and the particular rapidity with which the copious blood pours through its veffels, will not be greatly furprifed that a fuppuration here fhould fo little yield to the efforts of phyfic.

A good many different methods of cure have been tried, but none of them is much celebrated for its fuccefs: A new one has been lately recommended, which, in my humble opinion is by no means abfurd, I mean that of frequent bleeding in fmall quantities.

Young people of plethoric habits, and fuch as have been accuftomed to frequent blood-lettings, are very often fubject to this difeafe; and in thefe it is generally obferved to be moft acute, and to flide on the fafteft to its cataftrophe. In fuch patients I fhould take Dr Dover's method of cure to be extremely reafonable, efpecially if it is ufed with proper cautions, and before the conftitution is much drained of its natural fluids.

I fhall offer the reafons that occur to me in behalf of this doctrine. Any body, I believe, will allow, that to heal the ulcer is to cure the difeafe; and this method, I think, bids as fair to do that as any. 'Tis granted on all fides, that fome degree at leaft of a fever, is neceffary to the making of pus, and that the quantity of pus will always be, *cæteris paribus*, in proportion to the force of the heart. 'Tis likewife undeniable, that the more the circulation is hurried, the conftitution is the more heated, the purulent matter acquires the greater virulency;

virulency, becomes the ſooner thin, and is the faſter reſorbed; while in the mean time the circulating fluids are attenuated, exalted, and expelled the faſter. Thus the whole train of hectic ſymptoms is very remarkably influenced, or rather their degree of virulence is entirely determined by the velocity of the blood. The reſorbed pus occaſions the hectic fever, and that again prepares new pus; that is to ſay, ſupplies freſh fuel to the diſeaſe. And thus the ſtruggle is maintained till the heart ceaſes to beat.

Now, as blood-letting is the moſt effectual way to abate the force of the heart, it muſt of courſe diminiſh the quantity of pus, and alleviate all the bad ſymptoms that owe their origin to this fomes. The mere ſubtracting of acrimonious blood too ſeems to be no deſpicable advantage, ſince this diminution may eaſily be repaired by the addition of more laudable juices from the aliments, which in this caſe ought always to be of a mild kindly nature, eaſily elaborate, and for the moſt part aceſcent, to be adminiſtred frequently, and in ſmall quantities at a time. And beſides, if bleeding takes off, or conſiderably abates the hectic fever, it may come to be of no real expence at all, ſince by this means the great waſte of fluids, by colliquative ſweats, or diarrhoea, will be ſaved. For which reaſons it might perhaps be, with proper cautions, ventured upon, even in patients that are already pretty much exhauſted: Seeing it is certain that their veſſels are ſtill exquiſitely full, and may, in proportion to their contracted ſtate, even ſuffer a plethora; which appears

appears frequently in the weaker ſex, who are frequently viſited with regular returns of their menſes to the very laſt ſtage of the diſeaſe, notwithſtanding all the loſs they undergo by plentiful colliquative diſcharges.

But the good ſucceſs of riding, and other exerciſes, in the cure of conſumptions, may be objected to this reaſoning, ſince theſe concuſſions and agitations of the body ſhould, according to this ſcheme, by accelerating the motion of the blood, rather aggravate the malady, and ſpur it on to its laſt fatal ſtage, than contribute to its cure. This fact at firſt ſight ſeems to ſhake the doctrine here advanced; but, when more narrowly conſidered, I am apt to think it rather ſtrengthens it: For, beſides the advantages of corroborating the flaccid fibres, and compacting the melted fluids into a juſt denſity, there is perhaps a very conſiderable one procured from theſe exerciſes, by their enabling the veſſels to throw out the purulent miaſmata as faſt as they are taken in; and as by this means an accumulation of reſorbed pus is prevented, the hectic paroxyſm, during which the ulcer is moſt ſupplied with new pus, is either quite cut off, or much mitigated.

In a word, the particular violence with which this diſeaſe is obſerved to act, and the uncommon diſpatch with which it proceeds in plethoric habits and warm conſtitutions, plainly ſeems to indicate this practice, at leaſt, in ſuch patients.

Blood-letting will, for the ſame reaſons, be equally juſtifiable in all internal ulcers, though there

there seems to be the greateſt neceſſity for it when the lungs are the ſeat of the diſeaſe, upon account of the abundant torrent of blood that rolls with ſo much fury through their numberleſs veſſels.

Before I put an end to this paper, I muſt take the liberty to propoſe a few queries, concerning the management of conſumptions by this method, which I ſhall ſubmit to the conſideration of proper judges.

1. Whether is it not a reaſonable piece of caution to abſtain from blooding, as long as there are any well grounded ſuſpicions of abſceſſes yet unbroken in the lungs, ſince bleeding in that caſe would only weaken the patient to no purpoſe? And whether all the proper methods of deterging and expectorating ought not to be diligently uſed before bleeding is called in?

2. Whether it is not the moſt proper time to let blood when the patient is pretty much recovered from the fatigue of his laſt paroxyſm, after his having cleared his lungs as much as poſſible by coughing?

3. Whether, in the very time that the blood ſprings, it may not be adviſeable to make him draw in gently aſtringent, drying, and balſamic ſteams, ſuch as of myrrh, maſtic, &c.?

4. Whether the more volatile, detergent, and antiſeptic medicines, ſuch as aloes, myrrh, vinegar, kindled pitch, ſulphur, &c. might not be happily conveyed to the lungs in this ſhape?

5. Whether there may not frequently be harm in inſiſting much upon expectoration?

ſince by this means the ulcer is ſtill kept crude; the plexus of new tender veſſels is broken in forming; the lungs are robbed of that lymphatic and mucous moiſture, which would much contribute to heal the ulcer *; and their nerves are laid bare to every ſtimulating cauſe; whence an inceſſant cough, and all its bad effects. May not antiſeptic ſteams, and a mild aceſcent diet, iſſues, &c. much take off the neceſſity of expectorating medicines? Are mild pacifics (the preparations of our own poppies, eſpecially their ſeeds, in form of emulſion) mixed in ſmall quantities with balſamic, refrigerating, and gently detergent materials, to be allowed only at night? And even during the operation of the paregorics, will not the lungs, (if their tone is not much enervated, and the opiates are not too ſtrong), when their veſſels begin to be overburdened, be ſufficient, of their own accord, in moſt caſes, to expel the offending load, without the aſſiſtance of any more artificial ſtimulus? Does not the ſucceſs of opium in the catarrh, even when the mucus is very thick and hard to be diſcharged, warrant this practice, and even invite us to it?

Laſtly, Would not the bark and other medicines that corroborate, without ſtimulating much, frictions, and gentle exerciſe, be neceſſarily uſed at the ſame time with blooding, to aſſiſt in curing the hectic fever, and to prevent crudities, hydropic collections, and tumors,

* This is in conſequence of an obſervation made in an admirable eſſay on the nutrition of foetuſes, in the ſecond volume of Medical Eſſays, p. 15[illegible].

mors, and other bad ſymptoms that might probably be introduced by loſs of blood?

XXIX. *A Collection of Matter in the* Liver *evacuated by Stool; by Mr* JAMES JAMIESON, *Surgeon in* Kelſo.

ON the 2d May 1729, I was called to ſee the wife of George Tait inkeeper in the town of Yettam, who for ſome years had been afflicted with pains about the region of the liver, a ſhort and dry cough, loſs of appetite, irregular tremors, and feveriſh paroxyſms like theſe of an ague, and a ſuppreſſion of the catamenia. After ſhe had continued in this ſtate about two years, an unequal tumor appeared immediately under and before the *coſtæ nothæ*, proceeding gradually both ways; till croſſing the *linea alba*, ſome inches below the umbilicus, it filled the whole epigaſtrium, to the *cartilago xiphoides*, forcing the muſcles externally to very unequal projections, and hard to the touch. To her former complaints, were added vomitings, and a continual hectic diſpoſition, whereby ſhe became much emaciated, and her belly always coſtive during the whole courſe of her illneſs.

In this caſe I found her the firſt viſit, and adviſed calling a phyſician, which ſhe abſolutely refuſed, from an entire diffidence of her recovery, joined with mean and low circumſtances of life, wanting only eaſe from ſome of the moſt painful ſymptoms, deſiring all the help I could give her thereto, which was endeavoured by the following simple method, viz. I ordered her to take a gentle dose of the *pilul. benedict.* with calomel, at bed-time, and to repeat it every night or second, as she found it agreed with her. To use morning and evening an emollient and aromatic fomentation, the same materials being sometimes used for cataplasms.

From this time, I did not see her till the sixth day thereafter; when, being sent for in haste, I was surprised to find the swellings entirely gone, and the patient chearful in hopes of recovery, and only now complained of a fullness in the hypogastrium, with a little dysuria. Upon striking that fullness with my finger, I found the contents fluctuate; whereupon I not only advised the continuing of the pills, but an infusion of the aromatic diuretics in white wine, with millipedes and some of the *spir. nitr. dulc.* with the above fomentation used only at night, and so left her.

On the fifth night thereafter, an express was sent for me to see her die, from a purging of her intestines, (as the messenger expressed it, by commission from both his master and the patient), which I found was occasioned from a stool she had had in the night, whereby about a pound of a parenchymatous kind of substance, the thickness of brown paper, all cohering, of the toughness of well-boiled tripe, and diaphanous, was discharged, without any other excrement. This I caused to be put into a gallypot with some brandy, brought it home, and shewed it to the physicians and surgeons here, and to that curious observer Mr Monro, the

the professor of anatomy, who happened to be at Kelso in a short time after.

From that time to this, the patient has continued well, and I am informed is in perfect health at writing hereof.

XXX. *An uncommon* Dropsy, *from a steatomatous* Omentum; *by* Alex. Monro, *Professor of Anatomy in the University of* Edinburgh, *and* F. R. S.

Christian Seton was subject to the erysipelas in her legs from her youth. At thirty-one years of age married, but never conceived. Her menses left her when she was thirty-nine years old. Three years after, viz. in July 1727, she perceived her belly turning bigger than naturally it used to be, which she imagined was occasioned by a draught of sour wine which she had taken two years before. In August following, her legs also began to swell, when she asked the advice of some physician, who ordered her several purgatives and other medicines; notwithstanding which, her disease increased very fast.

When I first saw her in the beginning of October, her belly was swelled so big as to reach down beyond the middle of her thighs when she was sitting. The bastard ribs, and *regio epigastrica* were violently protruded outwards, and the point of the xiphoid cartilage was turned directly forwards. Her thighs and legs were oedematous, and double their natural thickness. The upper part of the trunk of her body, her face, and superior extremities, were greatly emaciated.

maciated. She had no appetite for food, but had a thirſt, though not violent. Her tongue was moiſt, but ſlimy. She was coſtive, and paſſed very little urine. The pulſe was weak and frequent. The breathing ſo difficult, ſhe durſt not lie down, but was obliged to ſit always in an erect poſture. A perpetual cough, night and day, kept her from ſleep, and with the cough ſhe ſpit up a thick mucus. She was ſo weak as not to be able to ſtand. On ſtriking the belly, a fluctuation of water was plainly felt. To all theſe ſymptoms was added one of the worſt circumſtances that can happen in ſuch a diſeaſe, want of attendance, and of the common neceſſaries of life.

Several of the moſt preſſing and uneaſy ſymptoms in her caſe depending on the violent diſtenſion of her belly, I reſolved to attempt giving her ſome relief by tapping her, which I performed in preſence of my colleague Dr John Innes profeſſor of medicine, Mr Charles Allan ſurgeon, and ſeveral ſtudents of phyſic, and let out ſix Scots pints, or twenty four pounds of water, which at firſt came off briſkly in a ſtream, but in a little only dropped from the canula of the trocar, and at laſt ſtopped entirely before the ſwelling of the belly was near gone; though I took care, during the operation, to keep the abdomen tightly compreſſed with a belt, and aſſiſtant's hands, and to clean the pipe frequently, by introducing a probe through it. The patient bore the evacuation without the leaſt fainting, and I left her eaſier with the belt I commonly uſe on theſe occaſions, well charged with the fumes of benzoin,

benzoin, mastich, and succinum, applied to her belly, and drawn pretty tight. In the evening she took a cardiac mixture with some drops of liquid laudanum; she slept well all night, and was considerably refreshed in the morning.

The regimen now ordered her was to drink none, or very little; but, when she was thirsty, to put a little bit of tamarinds mixed with crystals of tartar into her mouth; to eat fleshes, or with any other food to mix wine and spiceries; which would appear a ridiculous direction for one in so much poverty: But the honourable Mrs Margaret Balfour of Burleigh, moved with her wonted extensive charity, had promised to supply every thing necessary for diet or attendance; and, as long as my poor patient lived, the promise was most punctually and bountifully performed; for that honourable lady not only gave orders to her servants to execute whatever directions I gave, but carefully inspected the due execution of them. Every day, the patient's belly was well rubbed with flannels strongly charged with the aromatic fumes. Every 4th day, she had a purgative given her of *Decoct. amar. cum dupl. senn. unc.* ii. *Oxymel. scillit. unc. sem. Syrup. de rhamn. unc.* i. *Pulv. jallap. scrup.* i. *Misc.* And four times every intermediate day she took the bulk of a nutmeg of an electuary composed of *Rad. helen. irid. florent. gum. ammon.* and honey, washing each dose down with $\frac{1}{4}$ of the following liquor, *Decoct. amar. unc.* iv. *Sp. succin. drach.* ii. *Misc.*

By

By the use of this sort of diet, and of these medicines, she passed very large quantities of water both by stool and urine; and in a few weeks all her symptoms went off, except the swelling of her belly, which was however greatly diminished, and gave her no uneasiness.

In the month of February 1728, having sat long in church, exposed to the air, which was then very cold, and having neglected to take her medicines some time, the quantity of her urine lessened much, she became costive, and the former bad symptoms returned. Nor could I after this procure the evacuation of water which had been so beneficial to her, by any medicines; so that she in a short time relapsed into as bad circumstances as when I saw her first, with the addition of a very sharp pain under her short ribs, especially of the left side.

On the 2d of July, I again tapped her with a larger trocar than what I had formerly made use of; and drew off first mucus, then pus, which sunk immediately to the bottom of the vessel where the mucus had been put, and had white membranes mixed with it. These two liquors, mucus and pus, ran alternately, till I had evacuated in all sixteen pounds, when no more would run: And in the time of the evacuation, I had frequently been obliged to draw out the tough slimy stuff, which stopt the pipe, with a probe. When I thrust the probe softly into the cavity of the abdomen, it was soon resisted by a firm solid substance, which on the least touch bled. The lower part of the belly ſubſided remarkably on this tapping, but the *regio epigaſtrica* yielded nothing.

The patient being no way relieved by this evacuation, I again tapped her on the 14th July, but was reſolved to prevent the matter contained from being ſtopt by a narrow cannula; and therefore performed the operation with a trocar as large as my little finger, but could bring away only eight pounds of purulent matter. The poor patient, daily turning worſe, died ten days after; and I was allowed to examine the body.

Before I give an account of what I obſerved there, I muſt aſk pardon for pretending to refer to ſuch indifferent figures as I ſend you with this paper; but neceſſity obliging me to commence painter at that time, and being ſenſible of the difficulty there is to repreſent things, eſpecially ſuch as are out of the common order of nature, in words, and knowing my own inſufficiency that way, I was afraid not to be underſtood without them: To prevent obſcurity, therefore, to your readers, or the disfiguring of your book with bad figures, I beg you'll be ſo good as to correct either the deſcription or figures, or both.

The body being laid on a table, had the appearance repreſented Tab. IV. Fig. 1. The Belly A hanging down over the thighs, and monſtrouſly ſwelled every where, but with a depreſſion D croſs the belly about mid-way between the navel B and the ſternum, the under part of which was advanced, and the point of the xiphoid cartilage E was directed ſtraight forwards. The hypochondria were alſo conſiderably

ably raifed. The thighs F and legs were greatly œdematous and the reft of her body was much emaciated.

The *tunica cellulofa* of the abdomen was very thin, without any water contained in it. In cutting through the mufcles and peritoneum, on the left fide of the navel, two pounds of yellow water ran out; and from the cavity G (Fig. 2.) where this was lodged, ten ounces of pus were taken, with white tender membranes fwimming in it. Having cut the containing parts from the navel to the left loin, no more water appeared, but eight ounces of pus were collected. All round the cavity, in which the water and pus were lodged, I obferved a white hard fubftance, feeming to be compofed of conjoined veficles, and adhering ftrongly to the peritoneum. This I diffected away from the peritoneum, till in cutting upwards I had reached half-way between the navel and fternum, when it adhered fo very clofe, and became gradually fo thin, that I was of opinion I would fooner and better difcover what it was, and how far it reached, by cutting it through at H, and opening into the cavity; which therefore being done, I raifed the containing parts ftrongly, and looking behind them faw this knotty fubftance BB adhering to the peritoneum three inches further up, and then feparating from the peritoneum, it was extended backwards, and fupported a confiderable quantity of water D lodged above it. I next with my fciffars cut clofe off from the peritoneum that fubftance, which being extended from one fide of the abdomen to the other, ferved

ferved as a fort of tranfverfe mediaftinum or diaphragm, to divide that cavity into two. After this, feeing nothing more above the navel in danger of being deftroyed, I divided the containing parts in the common way of diffection, and turned up the two quarters of the teguments now raifed on the ribs, to have a better view of the veficular fubftance BB adhering to their internal furface. It confifted of a fatty membrane befet with veficular bodies, full either of mucus, or of a white hard fteatomatous fubftance. The *regio epigaftrica* C, fig. 1. was exceedingly enlarged; and out of the cavity D, fig 2. and 3. formed in it, I took with a fponge thirty pounds of water, which had preffed out the furrounding bones, and had thruft the diaphragm confiderably upwards. I next diffected the teguments from the veficular body F, fig. 2. as far as it reached, which was to the *offa pubis*; and then divided them in the ordinary way.

All the teguments being thus removed, I viewed carefully the contents of the abdomen, yet in fight: At the upper part, I faw the great cavity of the epigaftrium D. Below this appeared a very large membranous fac C, of a brownifh black colour, diftended with air, and reaching from the extremity of the cartilages of one fide to thofe of the other, when it funk down under both hypochondria; the middle part of it was much the largeft, and no divifion into cells, or longitudinal ligaments, were to be feen on it; though I found evidently afterwards it was the great arch of the colon. The part of the fatty membranous body left, when I cut this in raising the two superior quarters of the teguments, mounted over this sac; and then sinking backwards adhered to another viscus, which I could not yet discover, without putting some parts out of their situation; and from the under part of this same sac depended a white vesicular fatty substance, with large blood vessels, I. spread on it: This at first was thin, but gradually as it descended became thicker, till it was hid by the anterior lamella which had been dissected off from the peritoneum. When this last was raised, I softly thrust my hand into a large bag formed between the two, as far down as the *ossa pubis*, where they made one continued substance. Besides these, I could observe no other viscus, except the cæcum or great sac of the colon K, lodged in the cavity of the right ilia. Wherefore I dissected that vesicular body away from all the parts it was contiguous or adhered to; and, in cutting, remarked, that all the little cells, opened in this dissection, poured out water. When this body was wholly removed, (see fig. 3.) I could not at first discover any thing in the cavity below, except a flat circular protuberance L, lying on the vertebræ, covered with a dirty black-coloured membrane, and this, in a great many places, had a white tender membrane, resembling the pellicle formed on boiled milk when it cools, lying upon it. The cavities of the loins were very large, and filled with water as well as the pelvis, and under the water, pus was every where found. When these liquors were taken out with a sponge, the surface of the cavities

Fig. 1

cavities had much the ſame appearance with that of the large middle protuberance. The cellular ſubſtance ſurrounding the pelvis was two inches thick, and diſtended with water and mucus.

Looking at laſt narrowly to the great protuberant body, I perceived ſome faint reſemblance of the convolutions of the ſmall guts appearing through the black membrane, and diſcovered the colon MN in the ſame way in the loins. When the membrane was diſſected off, all the inteſtines came in view, of the colour and ſize of which they are commonly found in hydropic emaciated bodies; but the ſmall guts and folds of the meſentery where they were contiguous, ſlightly adhered to each other by a weak ſort of membrane, which, when tore by gently drawing the viſcera, yielded water out of its cells.

The great guts were diſtended with air, and in a natural enough condition.

The meſentery was ſhorter than ordinary, but otherwiſe ſound.

The ſtomach, hid in the great epigaſtric cavity, was very little larger in its tranſverſe diameter than the ſmall guts, but of a natural colour, without any black membrane over it. From its fundus, the remains of the cut omentum depended, which was the veſicular ſubſtance I at firſt divided in opening the abdomen.

The liver was quite hid under the baſtard ribs, and adhered ſo firmly to the livid peritoneum by its membrane, which was about a quarter of an inch thick, that I could not obſerve

ſerve its colour or ſubſtance, till I had taken off the membrane, when this great bowel looked pale, and had very little blood in its veſſels.

I found no concretion in the gall-blader.

The ſpleen was much in the ſame condition with the liver.

The pancreas was pale and hard.

The kidneys were in a natural condition, but with little blood in them.

The ureters were of the ordinary ſize.

The bladder, ovaria, *tubæ Fallopianæ*, and uterus, were all ſound, only covered with a thick livid membrane.

The water and pus taken out with ſponges amounted in all to forty pounds.

I cauſed a ſervant to carry home the large veſicular body which occupied all the anterior part of the abdomen, and there examined it. It conſiſted of two lamellæ, which were thin above, but gradually turned thicker as they deſcended, till, at the loweſt part where they united, they were ſix inches thick. Each was covered all over with a ſmooth membrane, only the anterior was ulcerated at its ſuperior external part. Several ounces of purulent matter, with pellicles ſwimming in it, were taken out from the cavity formed between the laminæ. When the external membrane was ſeparated, each ſeemed to conſiſt of veſicles of different ſizes, ſome of which were diſtended with water, others with mucus, and a third ſort with a ſteatomatous ſtuff. So far as could be diſcovered, there were no communicating paſſages immediately from one veſicle to another.

From

From the whole, there is reaſon to conclude this body to have been the omentum diſeaſed, which very probably might be the cauſe of the dropſy, as well as tumours of other parts frequently are, of which I have ſeen ſeveral examples.

Whether is it peculiar to the membranous parts, when ſuppurated, to have pellicles, like to that which gathers on boiled milk, mixed with the pus? I have ſeen them after inflammations of the guts, pleura, and in the foregoing hiſtory of the omentum.

Whether are thoſe pellicles, the membranes ſeparated and turned tender by ſoaking, or the particles of the pus adhering and preſſed firm?

XXXI. *A total Obſtruction of the* Valve *of the* Colon; *by* Dr Thomas Short, *Phyſician at* Sheffield, *and* F. R. S

APril 24th 1726, I was called to Griffin Lee, who ſold mild-beer in Coventry, aged about ſeventy, of a phlegmatic conſtitution and groſs habit of body, and had a *hernia inteſtinalis* for many years. He had become conſtipate twelve days before. The firſt week, he took no medicines; the next he was attended, and had ſeveral things from a very honeſt ingenious ſurgeon; during which he was not wholly deprived of ſtools, but the fœces were very inconſiderable, ſmall, and hard, and he had vomited what he ſwallowed, with foetid excrements. From the Thurſday, when I was called, to Monday after, he had ſeveral motions, but no ſtools. He complained of a ſharp pain in his belly, eſpecially in that part where the ilium ends and colon begins, and indeed all over the ilium, with a ſenſe of weight and fulneſs. His hernia was not then down; his ſickneſs was little; he was able to riſe, walk about, eat and drink, if it could have found paſſage. I preſcribed a great variety of laxatives in different forms, to no purpoſe. I ordered air to be forced *per anum* by a bellows; it returned immediately without fœtor. He lived till May 2d, with the ſwelling of his belly and other ſymptoms increaſing.

After his death, the paſſage at the valve of the colon was found ſhut up, and about two fingers breadth of the gut there was degenerated into a hard ſolid ſubſtance, below which there were no fœces. The ſmall guts were much inflamed and livid.

XXXII. *The Caſe of a Male Child born without an* anus *or* Inteſtinum Rectum; *by Mr* JAMES JAMIESON, *Surgeon in* Kelſo.

SOme years ago, Mrs Hannah, midwife in this town, was called to one Mrs Stevenſon in Plowland, five miles diſtant from this place, whom ſhe delivered of twins, the one female, the other male; and diſcovering in the latter no appearance of an anus, came home, and ſent me to ſee the child, whom I found otherwiſe ſprightly, and ſeemingly in perfect health, but not the leaſt veſtige of an anus to be ſeen or felt, but equally firm and ſolid from the coccix to the ſcrotum: Whereupon I told the grandmother,

grandmother, who only was acquainted therewith by the midwife, that it was preternatural, and that, though I had twice ſeen the anus covered by a membrane, which was eaſily cured, I could not propoſe to do the like in this; but, if ſhe pleaſed, I ſhould try to reach the gut by inciſion, which ſhe, with the mother's conſent, fondly agreed to: Whereupon I made an inciſion pretty deep in the moſt reaſonable part, then introduced my little finger into the wound, to find the gut, but in vain.

I afterwards tried the trocar, which penetrated, but nothing followed but ſome gutts of blood; ſo was obliged to leave the patient without proſpect of further help from me, only deſired, that when he died, I might be allowed to open the body, which I did next day.

Upon opening the child, I ſaw the rectum entirely wanting, and the colon was a perfect *inteſtinum cæcum*, ſuſpended looſely in the abdomen, and full of meconium; all the other parts being in a natural ſtate.

XXXIII. *Coagulated Blood extravaſated upon the* Uterus, *and the thickneſs of the* Womb, *in a laborious Birth; by Mr* JOHN PAISLEY, *Surgeon in* Glaſgow.

AUthors having differed very much as to the thickneſs or thinneſs of the uterus of a woman with child; ſome, with Mauriceau and Dionis *, aſſerting that it turns always thinner as

* Mauriceau maladies des femmes groſſes, chap. 4. *de la matrice.* Dionis, chap. 1. lib. 1. Midwifery.

as it extends, whilſt others, I may ſay almoſt all anatomiſts, affirm that it turns thicker as the woman advances in her pregnancy, and draws nearer to the time of her labour; or, to ſpeak more properly, that, in the ſeveral ſtages, the thickneſs of the ſides of the womb keeps the ſame proportion to its cavity as in a natural ſtate, the ſinuſes and veſſels being proportionally enlarged as the uterus is extended. I ſay, this having occaſioned ſome diſputes amongſt anatomiſts, I thought proper to ſend you the following hiſtory of a woman who died in child-labour, where I had an opportunity of obſerving the thickneſs of it, and at the ſame time of diſcovering a fatal miſtake in the midwife who attended her, who, by delaying to call for aſſiſtance in due time, was the unhappy occaſion of the death both of mother and child.

Upon the 9th of June 1730, I was called to a woman in labour, about a middle age, of a low ſtature, and pretty fat, who had born ſeveral children; and found her in an exceeding low condition, with cold ſweats, ſevere faintings, her extremities cold, without any pulſe, and unable to utter one word, though ſhe ſhewed ſome ſigns of her being deſirous to ſpeak with me. The midwife that attended her had gone off upon my being ſent for, and left a young practitioner whom ſhe was training up in that buſineſs, who gave me the following account of the poor woman's caſe, viz. That ſhe had been ſeveral days in labour, and that all along the midwife imagined affairs were in a very good way, and the child, as ſhe thought, in a very right poſture; though, after the wa-

ters

ters broke, the child's head had never advanced by the ſtrongeſt pains. Hence the midwife either blamed the mother for not bearing down ſtrong enough when the pains came upon her, or elſe pretended that the pains were too faint and languid; and, as there was no flooding, ſhe never apprehended any danger, and therefore cheared up the mother and friends with the hopes of a good iſſue by a little patience: And as ſhe had a good deal of other buſineſs upon her hand, ſhe frequently left the poor woman for half a day together, and, upon her return, ſtill found all things in the ſame ſituation as ſhe left them. From the firſt day the woman was taken with her pains, ſhe ſcarce made one drop of water; wherefore on the fifth, the midwife ſuſpecting that to be the cauſe of the birth's being retarded, ſhe ſent to an apothecary's ſhop for a ſtrong ſtimulating diuretic mixture, to increaſe her pains and provoke urine, being aſſured all things were right, only the pains were too faint; as no doubt they were, when the woman had been ſo long fatigued with her labour. This having no effect, a ſtronger one was called for, which proved likewiſe unſucceſsful; and all things continued in the ſame ſtate, only that the woman's ſtrength was continually decaying, till the ſixth day at midnight, when I was ſent for, and found her in the ſituation above mentioned. It is evident, when matters were brought to this paſs, that the poor woman had not ſo much ſtrength left her, as to bear the fatigue of being put into a poſture for being delivered, and that it was impoſſible to afford her any relief. I acquainted, ed the friends with it, aſſuring them that it would be madneſs to attempt it in theſe circumſtances, being perſuaded ſhe could not live above a quarter of an hour; which accordingly happened, ſhe dying in a few minutes.

Next day, I prevailed with the friends to have her opened, and, after I had cut the integuments and laid them back, I was ſurpriſed to meet with a black membranous body like coagulated blood (which it in reality was) covering all the fore part of the uterus, though diſtended ſo much with the child. This I eaſily ſeparated in one cake from the uterus, and when it was ſpread upon the table, it was about a foot and a quarter long, a foot broad, and a quarter of an inch thick. Whether this proceeded from the ouzing out of blood from the ſubſtance of the uterus, by the ſtrong preſſure when the pains were violent, or from the rupture of ſome ſmall veſſels, either of the uterus, or ſome other part of the abdomen, I do not determine; for I could not obſerve the leaſt appearance of any ruptured veſſels in either, after the moſt accurate ſearch I could make; nor was there one drop of blood in any other part of the cavity of the abdomen. I know not if this is a thing that is always obſerved in ſuch caſes, having had no opportunity before that time, or ſince, to examine any ſuch ſubject; though no doubt it is a thing may readily happen in very laborious births, and then it is no wonder that violent after-pains, fever, inflammations, and their conſequences follow; for in ſuch a bad habit of body, as women in theſe circumſtances are

generally,

generally allowed to be in, it is ſcarce to be ſuppoſed that coagulated blood can be eaſily diſſolved, and again abſorbed by the veſſels in ſo large a cavity as that of the abdomen; wherefore, by its ſtagnation and putrefaction, it may bring on a train of bad ſymptoms; the cauſe of which lying entirely out of the phyſician's power to know, it need be no ſurpriſe though he fail in his attempt to remove them; and I do not know but this may be one of the chief cauſes of theſe many diſorders and frequent deaths that happened after very violent and laborious births, though there are many other cauſes well enough known which are capable of producing ſuch like effects.

This phaenomenon being what had never occurred to me either in reading or practice, I thought it would not be unuſeful to acquaint the world therewith, to prompt thoſe of greater abilities, or who have more leiſure and more opportunities of meeting with proper ſubjects, to inquire, if ſuch a caſe often happens; how far the cauſes hinted at are juſt; or what other cauſes may probably be aſſigned for it; what ſigns it may be diſcovered by; what method of cure might be proper in ſuch an event, and the like.

When I had removed this coagulated blood, I obſerved a large ſac or bag full of water, lying along the ſide of the uterus, above the inteſtines, and reaching as high as the kidney of the right ſide. Upon feeling it all round with my hands, I found it was looſe at its ſuperior part, and appeared to come out from the pubis, where only it had any attachment: This,

upon

upon examination, proved to be the urinary bladder, thus distended to a vast bigness, and thrust to one side by the pressure of the uterus on the fore-part of the abdomen; I opened it, and measured the urine; it contained no less than eight English pints, or a Scots quart.

The uterus was pretty closely contracted up the child, and, in opening it from the fundus to the cervix, I found it at least half an inch thick in the thinnest part of it, though a good deal more at its fundus, where I observed the sinuses so large, as easily to admit the end of my little finger into them. The placenta adhered to the fore-part of the fundus. The waters having been broke so long before, I could not expect to find the allantois.

The child had fallen down into the passage, much in the natural way, only with its head a little obliquely to one side, so that part of the frontal and parietal bones of the right side rested upon the *os pubis* and neck of the bladder; and, by the violence of the pains, these bones had been pushed so strongly against the pubis as to make a considerable indentation in them, and raised an inflammation for an inch or two round the contused part.

I believe I need scarce add, that, if assistance had been called in time, the swelling of the bladder might have been prevented, by drawing off the urine with a catheter. And if the child's head could not be easily stirred, then the child might have been turned, and brought away by the feet, as is usual in such cases.

Hence midwives ought to be advised to call for assistance in due time, especially in a case of this nature, where both the mother and child's life is in so great danger, though there be no flooding, since it is one of the most difficult cases can well happen in midwifery, and thereby they may save two lives, and secure their own reputation.

Hence all physicians and surgeons may take warning not to trust too much to the report of midwives, who too often pretend all things are in a fair way, and that there wants only some medicine to promote the pains that they suppose are too faint and languid, because the head does not fall any lower by the pains, while it may be owing to the above cause, as well as others mentioned by practical writers, when the giving of such medicines may be of the worst consequence.

XXXIV. Books *omitted in the former Volumes of this Collection.*

MIchaelis Bernhardi Valentini Archiatri Hassiaci & prof. med. Gisseni historia simplicium reformata, seu Musei Museorum titulo antehac in vernacula edita, jam autem in gratiam exterorum sub directione, emendatione, et locupletatione autoris, a D. Joh. Conrado Beckero medico Alsfeldensi, Latio restituta. Accedit India literata elingua Belgica primum in Germanicam translata, nunc vero ad desiderium exterorum latinitate donata, longe auctior reddita, novisque figuris illustrata a Christophoro Bernhardo Valentini, M. B. filio, fol. Offenbaci ad Maenum 1733.

De terra Tokayenſi, a chymicis quibuſdam pro ſolari habita, tractatus medico-phyſicus; autore D. Daniele Fiſchero Hungaro, 4to, Vratiſlav. 1732.

Remedios de deplorados, probados en la piedra lidio de la experienſia. Su autor D. Franciſco Suarez de Ribera medico, Madrid 1732.

Nouve ed crudite oſſervazioni, ſtoriche mediche e naturale di Hieronymi Caſpari, M. D. 8vo, Venetiis 1731.

Alberti Haller de muſculis diaphragmatis diſſertatio anatomica, 4to, Bernæ 1733.

Cartilla fiſiologica, Galenico eſpagirica mathematico-medica, eſcrita par el D. Don Iuan Gimenez de Molina. Madrid 1733.

De urinis tractatus duo, autore H. I. Rega in celeberrima Lovanienſium univerſitate med. P. P. 12mo, Lovanii 1733.

Examen de cirurgia nuevamente annadido con las operaciones ſu autor el D. Don Martin Martinez. Madrid 1732

Cæſaris Magati de rara medicatione vulnerum, libri duo. Acceſſit Joh. Bapt. Magati tractatus quo rara vulnerum curatio contra Sennertum defenditur, cum praefatione Frid. Chriſtian. Cregut. M. D. 2 vol. 4to, Amſtelod. 1733.

Clave medico-cirurgica ſu autor D. Franciſco Suarez de Ribera medico. Madrid 1731.

Reſtauracion de la medicina antiqua ſu autor D. Fraciſco Suarez de Rebera medico. Madrid 1731

Saggio di medicina teorico-practica di Carolo Gianelli phil & med. D. 8vo, Venetiis 1732.

Miſcellanea phyſico-medica mathematica Vratiſlavienſia ann. 1728, 4to, Erford, 1732.

XXXV.

XXXV. *An Account of the most remarkable* Improvements *and* Discoveries *in Physic, made or proposed since the Beginning of the Year* 173[illegible]

FAther Regnault (in his *origine ancienne de la physique nouvelle*) has revived the dispute concerning the discovery of the circulation of the blood, alledging that Harvey was taught it by *Fabricius ab Aquapendenté*, to whom it was communicated by father Paul, who again was, according to our author, prevented by Cæsalpinus. Father Regnault also follows the example of the other writers, who endeavour to rob our countrymen of the honour of this discovery, in quoting passages from Hippocrates, Plato, and Seneca, in which, he thinks, the circulation is described; nor does he omit the knowledge which the Chinese are said to have had of it 4000 years ago.

Dr Tronchin (in his dissertation *de clitoride*) quotes several antient authors who knew the clitoris, and proves Bonaciolus to have mentioned it before Columbus or Fallopius.

Mr Morand gives a short history of the lateral operation for the stone, and argues that the Methods of Celsus, Frere Jacques, Rau, and Chefelden are in the main the same. *Memoires de l' Acad. des sciences* 1731.

In the end of the year 1724, and beginning of 1725, the royal society at London published accounts they had received from New-England concerning Ambergris. In the first, (See transact.

act. num. 385. § 11.) Dr Boylston of Boston writes the society the information he had from the whale-fishers. One of these fishers having accidentally found a large piece of ambergris in a bull sperma-ceti whale, the other fishers always afterwards searched carefully for ambergris in the bodies of these creatures. They affirmed to the doctor, that they never met with any except in the male of the sperma-ceti whale, and scarce in one of a hundred of these. When they found it, it was, they said, contained in a cyst or bag near the genital parts. The bag having no inlet or outlet, was sometimes found empty, though entire. According to the report of these men, the ambergris, when first taken out, is moist, and of an exceeding strong and offensive smell.

The other account of the ambergris, published by the society, num. 387. § 2. was sent by the honourable Paul Dudley, on the faith of one Mr Atkins. It is more particular than the former, but agrees with it in this principal part, viz. that ambergris is sometimes found in a bag situated at the root of the penis of whales, and therefore is an animal substance.

The papers now mentioned have given rise to a long treatise on ambergris, by Dr Neuman professor of chemie at Berlin, published in the Philos. Trans. Num. 433. § 5. num. 434. § 1. num. 435. § 1. in which the learned professor relates all the opinions concerning ambergris, with the reasons for and against those of them that are not altogether absurd, and examines in a critical way these accounts sent from

from New-England; from which he endeavours to prove, that what the fiſhers took for ambergris, was no other than calculi contained in the urinary bladders of whales, at leaſt that it was not ambergris. The chemical analyſis of which ſhews it not to be an animal ſubſtance; for it affords no urinous volatile ſpirit or ſalt, but, on the contrary, a ſmall quantity of an acid ſalt, exactly like to ſalt of amber, is got from it. Near the concluſion of this treatiſe, he communicates his method of making a right tincture of ambergris in ſpirit of wine, which, he ſays, other chemiſts could not do. The proceſs is very ſimple, it is only to put a twelfth part of ambergris broke into ſmall pieces, among well dephlegmated ſpirit of wine, and then to expoſe them to ſuch a heat in a glaſs as makes the ſpirit begin to boil.

The quantity of ambergris uſed by Dr Neuman in the chemical analyſis above mentioned, having been very ſmall, ſome London chemiſts analyſed larger quantities, and their experiments are publiſhed in tranſact. num. 435. § 2. The principles it yielded were very like to thoſe got from ſuccinum, only Mr Brown could obtain no acid ſalt. Mr Godefrey obtained twice a ſub-acid phlegm like weak vinegar, and, in his third trial, the phlegm rather appeared to be impregnated with a neutral ſalt.

Mr Petit the phyſician concludes, from a great many experiments he made in covering pieces of fleſh with the different ſorts of aſtringents employed in haemorrhagies, that ſome act only as abſorbents, ſuch are earthy ſubſtances, moſt of the aſtringent plants, ſome gums, refines, and animal ſubſtances. Other aſtringents abſorb, and at the ſame time their ſaline and ſulphureous particles inſinuating themſelves into the fleſh, preſerve it from corruption. Vitriol and allum, which are acknowledged to be among the ſtrongeſt aſtringents, appeared by Mr Petit's experiments to abſorb moſt humidity. *Memoires de l'Acad. des ſciences* 1732.

Mr de Maupertuis, having cauſed ſcorpions to bite ſeveral animals, of which very few died, or ſuffered any more than the pain of the ſting, is of opinion, that oil of ſcorpions, and other vulgar antidotes to the poiſon of theſe animals, have rather got their reputation from the innocence of the ſting of theſe creatures, than from any conſiderable virtue in the medicines. *Memoires de l'Acad. des ſciences* 1731.

Mr Vincent Bacon relates what he obſerved in a man who had eat monkſhood, napellus, or *aconita ſpica florum pyramidali Moriſon. Prælud. Bot.* in a ſallad dreſt with oil and vinegar, after a ſupper of pork Immediately after eating the ſallad, the man felt a tingling heat, which did not only affect his tongue, but his jaws, ſo that the teeth ſeemed looſe; and his cheeks were ſo much irritated, that the people about him, nay, even his looking glaſs, could ſcarce perſuade him that his face was not ſwelled to twice its proper ſize. This tingling ſenſation ſpread itſelf farther, till it had taken hold of his whole body, eſpecially the extremities. He had an unſteadineſs in the joints, eſpecially of the knees and ancles, with twitchings upon the tendons, ſo that he could ſcarce walk

acroſs

across the room: and he thought, that in all his limbs he felt a sensible stop or interruption in the circulation of his blood, and that, from the wrist to the fingers ends, and from the ancles to the toes, there was no circulation at all: But he had no sickness, or disposition to vomit, till, suspecting himself to be poisoned, he drank a large quantity of oil, not less than a pint in all, and, after that, he loaded his stomach with carduus-tea till he vomited; and, though he threw up the greatest part of his supper, yet his symptoms still increased. His head grew giddy, and his eyes shifty and wandering. Next a kind of humming or tinging noise seemed continually to sound in his ears, which was followed by syncopes. Some spirit of hartshorn, being poured into his mouth, rouzed him a little, and set him first a coughing, and next a vomiting. Being plied with carduus-tea, he vomited several times more, but swooned often between the times of reaching, notwithstanding that forty or fifty drops of *sal volatile* and *tinctura croci* were given in a glass of wine after each time of reaching. At length he began to find a working downwards, which was followed by a stool; after which he vomited two or three times more, and then said, his head was so heavy, and his strength and spirits so exhausted, that he must needs lie down. His pulse was then a little returned, though very much interrupted and irregular, sometimes beating two or three strokes very quick together, and then making a stop of as long or a longer time than the preceeding strokes altogether took up. Having observed that what he had last vomited,

was

was little more than the pure carduus-tea, Mr Bacon gave him a draught made of *aq. epidem. theriac. androm. conf. alkerm. &c.* and gave orders to make him ſome ſack-whey to drink between whiles, ſometimes alone, and in caſe of great faintneſs, with ſome of the above-named drops. He lay awake, though ſtill, an hour or two; but being very cold and chilly, had a great deal of covering laid on him, and then found a kindly warmth come over his limbs, which was ſucceeded by a moderate ſweat, and then a quiet ſleep of four or five hours, from which he awaked very much refreſhed. Next day in the forenoon, he was much amended, and was capable of anſwering queſtions with regard to ſtrength, his ſenſes never failing him but during the ſwoonings. In three days, he was quite well.

A woman, who had eat a little of this ſallad, felt and complained of the ſame ſymptoms, but in a leſs degree than the man. She would not be prevailed on to vomit, and remained longer out of order. *Philoſ. Tranſact. Num.* 432.

§ III.

Mr Queſnay, in his *Eſſai phyſique ſur l'occonomie animale*, p. 87. infers, from the effects of heat upon oils, how much the nature of oily medicines may be changed in preparing them, according to their being longer or ſhorter heated or boiled; and therefore that ſurgeons ſhould have a particular care to adapt them by this means to the various caſes for which they are to be applied.

Dr Morgan, (Mechan. Practice of Phyſic, prop. vii.) gives it as his opinion, that the me-

chanical

chanical effections of medicines may be reduced to these six. 1. Repletion and depletion. 2. Rarefaction and condensation, or heating and cooling. 3. Solution or fluxilization, and olefaction or inspissation. 4. Derivation and revulsion. 5. Constriction and relaxation. 6. Stimulation and pacification.

Dr Neuman, professor of chemy at Berlin, having related to the Royal Society of London his remarks on the hard crystalliform substance found in the oil of thyme, and other plants, affirmed it to be so like to camphire as to deserve that name (Vid. Transf. numb. 389. § 2.) Mr Brown chemist of London, very soon after (Num. 390. § 2.) made remarks on Dr Neuman's paper, and mentioned several experiments, by which it appeared that common or oriental camphire differed considerably from that crystalliform substance, which Mr Brown chuses to call *coagulated oil of thyme*, and thinks the name of *camphire* improper. Dr Neuman (in Num. 431. § 2) acknowledges the differences between these substances, but still argues, that it is as proper to give the name of *camphire* to those crystalline bodies in oils, as it is to reduce metalls, salts, &c. under the same classes.

Mr Boulduc has given much the same description of the manner of making Epsom-salt as Mr Brown had done in Philos. Transf. num. 377. § 10. and num. 378. § 11. to wit, that it is the salt which crystallizes after boiling to a due consistence the bitterns, which is the liquor remaining in the salt-pans after the sea

salt

falt for common ufe is all feparated. *Memoires de l'Acad. des fciences* 1731

Mr. Boulduc alfo defcribes Signette's fal polychreft, which has been long efteemed in France; it is a foluble tartar made with *fal kali*, inftead of falt of tartar. Ibid.

The method hitherto employed for making tartar or its cryftals foluble, has been by faturating them with an alcali falt; but now Meffrs Groffe and Du Hamel have fhewn, that all lime, chalk, or earth that is diffolvable by vinegar will ferve for making *tartarus folubilis. Hiftoire et memoires de l'acad. des fciences* 1732

Mr Homberg, by diffolving borax in water, into which he poured oil of vitriol, and then diftilling this mixture, obtained his quieting falt. Mr Geoffroy has lately taught us an eafier way of preparing it; for, inftead of fublimating, he evaporates the liquor to the proper confiftence, and then allows it to cryftallize; which cryftals anfwer all the characters of the fublimed falt. Ibid.

The common opinion is, that all the animal liquors, excepting chyle and milk, are of an alcalefcent nature; but Dr Quefnay (*fur l'oecon. anim.* p. 144.) affirms, that "our gelatinous liquors contain a very acefcent falt capable of refifting a heat of 200 degrees. The proof of which, fays he, offers itfelf daily to every one. Who is it who has not remarked, that broth made with flefh well freed from fat, when corrupted, becomes as four as verjuice?"

The foundation on which Mr Quefnay builds his doctrine concerning animal liquors, is the

feparation

separation of milk into its oily, cheesy, and watery substances, by which, he says, p. 165. the genealogy of our humours begins. In the blood he remarks, 1. The albuminous juices; 2. The fatty; 3. The gelatinous; 4. The bilious; And 5. the watery liquors. Which, according to him, comprehend the four predominant humours of the antients. Their blood being the oily or fat part of the chyle. Their bile is the same with the salino-sulphureous part of that oil. Their melancholia or black bile takes in his albuminous and gelatinous liquors, which is the caseous part of the chyle, and the pituita is the watery.

Mr Hunald has the following observations on the fat of the body: 1. That tho' fœtuses and children have much fat under the skin, yet they have only a small piece or two (*pelotons*) at the base of the heart; whereas even lean adult bodies have fat all round the base, on the vessels that go out of the heart, and accompanying the larger coronary vessels, and at the point of the heart. 2. That the omentum of very young children has no fat, and their mesentery has very little. 3. That in many people the fat under the skin is exhausted, while the bowels are overcharged with it. 4. According to him, the exterior part of the *tunica cellularis* is the first filled with fat and the last emptied of it. From which, and seeing aponeuroses and membranes spread over so many muscles, he concludes the common opinion of muscles being lubricated by the fat to be ill founded. *Histoire de l' Acad. des sciences* 1732.

The common opinion concerning the ossification

cation of bones is, that they are first cartilages, which, by pressure, and the addition of an ossifying juice, are gradually hardened into bone. Dr Nisbet, in his human osteogeny, undertakes to demonstrate, " that the notion of all, or any " bones being originally cartilaginous, is without " foundation in nature."

1. He observes, That several bones are formed between membranes, without the appearance of any cartilage. He is so just however to those who differ in opinion from him, as to allow (p. 15.) that some of these which he calls membranes are so like to cartilages, that no less an anatomist than Kerckringius affirms them positively to be cartilages; and, p. 39. says, " We find most of these bones (formed in membranes) even when their ossifications are far " advanced, to be either so exceeding thin, or " very small and slender, that a cartilaginous " substance of their size could not have much " more solidity, than the membranes between " which those bones are produced."

2. The doctor, supposing the favourers of the common opinion to know nothing of liquors circulating in cartilages before and while they ossify, and consequently to believe that no other solid particles form bones than what were in the cartilages formerly, refutes them, p. 30. from the bulk of the part not having been greatly diminished in ossifying, the bulk and weight of all animal substances, except bone, depending indisputably much more on their fluid than solid parts: And, p. 33. from bones, when burnt, leaving a greater quantity of earth than cartilages do.

3. Cartilages

3. Cartilages are often harder, and bones are ſofter than ordinary; but our author, (p. 25.) never found any particles or fibres in a middle ſtate between bone and cartilage, and therefore concludes the ſofter ſubſtance not to be gradually tranſmuted into the harder.

4. In anſwer to the experiment of ſteeping bones in vinegar, whereby, it has been ſaid, they may be brought to a cartilaginous ſtate; D. Niſbet tells us, (p. 31.) that, if bones are ſteeped long enough, and the vinegar is often enough changed, bones may be reduced to a ſpongy ſubſtance, which is very different from cartilages.

The doctor's own opinion of oſſification is, (p. 27.) that in the blood, or a fluid ſecreted from it, there is an oſſifying juice conſiſting of particles which are not apparent; that (p. 17. 25.) whenever nature deſigns an oſſification between membranes, or within a cartilage, ſhe, (ſome cauſe or other) occaſions a more than uſual afflux of fluid, which diſtends ſo much the veſſels which were before inviſible, as to make them capable of receiving the red globules of blood, which is always to be ſeen near to where oſſification is begun. In this blood (p. 18.) gritty bony particles are to be felt by the point of a knife, which (p. 28.) have been formed by the attraction and coheſion of the particles of the oſſifying juice obſtructed, along with the other groſſer fluids in the beginning of the veſſels, prepared to receive refluent juices. "The blood being "capable of forming fine membranes, the "membranous parts of a bone, which act as

" a gluten to keep theſe particles and fibres " together, if there be any ſuch, that do not " ariſe from the coats of its veſſels, are pro- " duced by a coheſion round the cretaceous " particles of a part of the fluid, in which " they were generated and contained." Thus, (p. 10. 38) the membranes or cartilages ſerve as a bed between, or within which the bony particles are depoſited or ſhoot; but (p. 21) without any intermixture of the particles of the bone and cartilage, or continuation of the fibres of one ſubſtance to thoſe of the other, as, ſays he, is evident in cartilages containing bones kept long enough in water, and then ſlit; for the bone will, as ſoon as the large veſſels that enter its ſubſtance are divided, ſlip as eaſily, if not eaſier, from it, than an acorn does out of its cup; and there is a ſmoothneſs and poliſh of the parts of both cartilage and bone, which ſhew there is no conjunction or union of the fibres of the two ſubſtances. While the bones are increaſing within cartilages, (p. 34 35.) the cartilages are extended and ſpread out, by which, with the preſſure which they ſuffer, and the great influx of various fluids, and the nutritious matter being hindered to flow freely into them, they decreaſe continually, and at laſt may truly be ſaid to be entirely deſtroyed.

Mr. Weitbrecht has deſcribed and delineated a ligament which had not been obſerved by anatomical writers, ſtretched from the poſterior part of the anterior extremity of each clavicle behind the ſternum, to the ſame part of the other clavicle, which makes the articulation

culation of the sternum and clavicles stronger. *Comment. Acad scient. Petropolitan. tom. IV. p. 255*

Mr Weitbrecht has also described and painted some bones and muscles of the hand more accurately than is to be found in other books: What corrections he has made will not admit of being told in fewer words than the author has used. Ibid. p. 234.

The improvements made by Mr Albinus, in his *Historia Musculorum*, will as little allow of an abridgment, and therefore we must refer to the book itself. We cannot but regret that he has given us no more plates than four, representing the muscles, ligaments, and bones of the hand, which are most accurate and elegant.

Dr Morgan, in his mechanical Practice of Physic, prop. XII. repeats the objections he had made in his philosophical Principles of Medicine, to muscular contraction being owing to the influx of a fluid of the nerves into the muscular fibres. The principal of these are, 1. The vesicles in an animal body being all distractile, such a fluid would distend the vesicles of the muscles in every direction alike, and consequently, by such influx, muscles would be made longer, instead of being shortened, as they really are. 2. In accounting for the motion of the heart, those who would have it to depend on the animal spirits, are, says he, brought into the absurdity of making the secretion of the brain to depend on the contraction of the heart, and the contraction of the heart to depend on the secretion of the brain, which is running things into a circle without any cauſe. Or, ſuppoſe theſe two to be ſet at firſt miraculouſly in motion, yet the reſiſtances, that would be met with, would neceſſarily put ſoon an end to that motion. Weights and ſprings, which act by their conſtant uniform force of gravity and elaſticity, are the only cauſes of continuing motion in a reſiſting medium. To elaſticity then it is he aſcribes the motion of muſcles, as is more fully explained in his principles. We wiſh the doctor would impartially try the application of the objections he has made to the common opinion, likewiſe to his own doctrine.

In ſchol. 1. of this prop. XII. he particularly attacks Boerhaave's arguments in favour of animal ſpirits, which, he thinks, the profeſſor gives up, by acknowledging that they cannot be exhibited to any of our ſenſes.

In ſchol. 2. he laughs at the opinion of ſecretion being only owing to the laws of circulation, and thinks ſome concoction or fermentation neceſſary. Nor will he allow that all the liquors ſecerned have firſt entered the blood-veſſels, inſiſting ſtill, as he had done formerly, that the urine paſſes from the ſtomach to the bladder, without entering into the common courſe of the circulation.

Dr Morgan (Mechan. Pract. prop. VI.) endeavours to ſhew that Dr Juryn (*Diſſert. de motu aquarum fluentium*) "has confounded three "perfectly different and diſtinct laws of mo-"tion, namely, the law of communicating "motion by impulſe, the law of gravity in "in general, and the particular law of preſſure

"in

"in fluids;" and then examines the paradox, concerning the circulation of liquors in animals, advanced by Dr Juryn in that differtation, to wit, that the momentum or impetus of the blood is greater at the extremities of the evanefcent arteries than at the heart. The contrary of which, Dr Morgan thinks, is clearly demonftrated by the greater thinnefs of the coats of the fmall arteries than of the large ones, and from the capacities of all the branches of every artery being fo much greater than of the trunk itfelf, while the fame quantity of blood paffes through the trunk and branches in the fame time.

In fchol. of prop. V. he roughly criticizes Dr Robinfon's laws of motion in fluids, and afterwards attacks the doctrine which Dr Robinfon has borrowed from Sir Ifaac Newton concerning the acid in the air, which they fuppofed neceffary for the life of animals, and to preferve the action of fire and flame. (See our vol. I. p. 340.) Dr Morgan's principal objection to this doctrine is, that acids extinguifh fire, and acid vapours are more fuffocating and deftructive than any common watery fume or vapour; and acids cannot effervefce with the blood; nor do they increafe, but rather diminifh heat.

Our author's opinion of the ufe of air to fire and animals is, that it ferves as a proper exhaling medium to receive and carry off thofe copious difcharges of humid effluvia or moift vapours, which all living creatures, and all combuftible matter under the action of fire, are inceffantly emitting and throwing out, and consequently, for want of such an exhaling medium as that of the air, those vast quantities of humid vapour being thrown back, and not discharged or carried off, the life of animals, and the action of fire, must soon be suffocated and extinguished, after the same manner, and by the same sort of mechanical necessity.

Anatomists, in their figures and descriptions of the brain, only represent the cavities that are seen upon separating the hemispheres, and taking away the *corpus callosum*, without observing that the *crura fornicis* sink down, and then turn forwards on each side of the *medulla oblongata*, in cavities which are extended far forwards, under the commonly-known anterior ventricles. In these inferior cavities, the *crura fornicis* are of a beautiful form, resembling a white silk-worm, or sea-horse; on which account they were called *hippocampi* by Julius Cæsar Arantius, who is the only author who has given any description of them, till lately Mr Du Vernoy has revived them, by an exact description and delineation.

Mr Du Vernoy also observes, that the *septum lucidum* between the anterior ventricles has a cavity between the two lamellæ of which it is composed, in which he has often found water, and that the internal side of the septum is made rough, by a great many small grains and papillulæ. *Comment. Acad. Petropol. tom.* IV. p. 130.

The French anatomists have of late disputed, whether or not the heart is shortened in its systole, though all allow that it is then straitened.

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——In diffections of living creatures, the motions of the heart are fo quick, convulfive, and irregular, that it is fcarce poffible to determine this queftion by them.——If it is faid, that the longitudinal fibres contracting will make the heart fhorter, it may be anfwered, that the tranfverfe fibres, being much ftronger than the longitudinal, may prevent their action.——The valves at the orifices of the ventricles of the heart being evidently ftretched towards the point of the heart in its diaftole, when the blood rufhes into the ventricles, and thefe valves being raifed towards the bafe of the heart in its fyftole, to prevent the blood from returning into the auricles; which motions of the valves feem to depend on the relaxation, and on the ftretching of the tendinous cords, by which the valves are connected to the fides of the ventricles, appears to be one of the ftrongeft arguments for the heart being fhortened in its fyftole, efpecially that thefe motions of the valves can be feen, by alternately raifing and letting fall the point of a heart filled with water, and held with the bafe upwards. But even this is not conclufive; for, in making the laft-mentioned experiment, the motions of the valves are the fame, when the fides of the ventricles only are preffed without the point being raifed or depreffed.——Nor is there any confequence to be drawn in favour of the heart's being lengthened in its fyftole, from its pulfation felt at that time on the ribs; becaufe that pulfation may as well be owing to the heart's being raifed by the ftretched, diftended auricles and arteries, as to the increafed length of of the ventricles. *Hist. de l'Acad. des sciences*, 1731.

According to Mr Quesnay, (*l'oecon. animale* p. 227.), the motions of the heart and the circulation of the liquors depend on the motion of the lungs, which sending the blood forcibly into the left auricle, revive and increase its elasticity and contraction by this shock, which the auricle communicates to the ventricle, which affects the arteries in the same way; and these do the same to the veins which act upon the right auricle, and that upon its ventricle; and thus the circulation is continued.

Mr Lindern, physician at Strasburg, relates two observations, which contradict the doctrine of violent trituration, said to be performed by the stomach in digestion. 1. A dog having swallowed a dice, vomited it eleven or twelve hours after; when the bony part of the dice was much diminished, but the pins of wood on which the spots are marked were entire, and stood out a considerable way from the bone. 2. Three stomachs of swine were crusted so thick over their interior surface with a stony substance, that all their cavity was filled except a canal in the middle, of about an inch diameter; notwithstanding this, the flesh of the creatures was fair and sound, and sold well. *Hist. de l'Acad. des scien.* 1732.

Ruysch, (Adverſ Dec. 3. Tab. I. Fig. 4. 5. 6. 7.) painted the meseraic arteries and veins as having different courses in the distribution of their branches in the intestines. Dr

Albinus

Albinus (*Differt de arter. et ven. inteftin.*) attributes this miftake to Ruyfch's having injected the arteries at one part of the inteftines, and the veins at another; and, by a figure reprefenting the internal cellular membrane of the ilium with both arteries and veins injected, fhews their courses to be the fame.

Mr Du Vernoy is of opinion, that the *valvulæ conniventes* of the inteftines are formed by the arched veffels and fat in the internal cellular coat, and covered by the villous or nervous coat. *Comment. Acad. Petropol. tom.* IV. *p.* 192.

Every body knows the many opinions which have been given concerning the ufe of the fpleen. Mr Du Vernoy has added one more to the number. From obferving a large empty fpace near the fpleen, in the abdomen of a dead body, the proportional largenefs of its blood-veffels, and the ftructure of the fpleen analagous to that of the penis, he concludes the fpleen in a living perfon to be fubject to inflations like a bellows: But, how it is thus to be moved, or to what purpofe, he does not tell us. *Ibid. p.* 156.

Dr Rega, in his fecond treatife on urine, mentions the opinion of fome modern authors, who imagine that our drink paffes through the coats of the ftomach and bladder, and not in the ordinary courfe of the guts, lacteals, &c. when it is fo quickly evacuated by urine, as it is obferved to be after drinking feveral mineral waters and other liquors. He fhews this to be no new opinion, it having been mentioned by Hippocrates and Afclepiades; and then he endeavours to prove, by an eafy calcul

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of the quantity of urine secerned in the kidneys, that they are capable of furnishing all the quantity observed at any time. And lastly, he mentions the fulness and greater frequency of the pulse after drinking these diuretic liquors, as a proof of their being mixed with the blood.

Dr Morgan (Mechan. Pract. p. 246) after several arguments taken from the chlorosis, and other symptoms which appear after obstructions of the menses, and observing that blood-letting does not supply this natural evacuation, concludes that the menses are not designed only for evacuating a superfluous quantity of arterious blood, but that there is then a very different and very independent secretion, or a derivation of a certain excrementitious and redundant lymph or serum, from the membranous cells and ventricles of the glands in general, but chiefly from those parts of the *membrana cellulosa*, which are more directly and immediately connected with the kidneys, uterus and ovarium.

Dr Neufville, in his dissertation on the allantois, § 10. affirms, that a liquor injected by the human urethra, after the bladder is blown up, will come out at the urachus; as it will, he says, likewise do, upon gently pressing a bladder filled with liquor, while the urethra is tied. He also informs us, that Mr Albinus, professor of anatomy at Leyden, shewed his students the urachus of an adult, which was pervious, and allowed the urine to pass an inch from the bladder. And (§ 24.) that Mr Albinus, in 1730, shewed the allantois of a human fœtus about seven weeks old, loosely connected by small fibres, and placed betwixt the amnios and chorion

rion, exactly where the placenta adheres to the chorion; it was like an oblong bladder, and much more capacious than the bladder of urine. The urachus likewise appeared distinctly in the umbilical rope towards its side, like a small thread, and terminated in the allantois.

Mr Hunauld, royal professor of anatomy of Paris, has communicated some thoughts on the operation of the *fistula lacrymalis*, and proposes that no tent should be put into the perforation of the *os unguis*, because the tears will of themselves keep it open, and the tent, by its pressure and irritation, may occasion inconveniencies. Philosoph. Transact. numb. 437. § 5.

The itinerant oculist Dr Taylor (in his treatise on the diseases of the crystalline humour of the human eye) seems to aim at something new in the operations he describes, but expresses himself in a manner that makes us suspect we may mistake his meaning. We shall, however, for once try our skill in explaining profoundly obscure authors.

In couching a cataract or opaque crystalline, he makes a small puncture with a lancet, through the coats of the eye, in the ordinary place of piercing with a needle in this operation; then introducing his needle at this puncture, he directs its point to the lower edge of the cataract, and raising the cataract a little with the side of the needle, he observes whether it moves directly upwards, without being turned forwards or backwards. If the cataract thus moved is neither pressed nearer to, nor farther from the iris, he draws back the needle some way; and, as soon as the cataract descends to its former situation,

ſituation, he puſhes the needle quickly into it, and breaks its lower edge, by forcing the needle through it, and ſome little way into the ſubſtance of the vitrous humour. When this is done, he draws the needle back again, and then directs its point to the upper part of the cataract, upon which he preſſes in different directions, till he ſees, by the perpendicular motion of the cataract, that the needle is right placed, when he uſes more force to thruſt the altered cryſtalline out at the aperture already mentioned to have been made in the lower part of its capſula. When ever he ſees the cataract paſſing through this opening, he brings back his needle to it, and puſhes the cataract into the divided part of the vitrous humour, and then takes his needle out of the eye.

In what he calls the ſhaking cataract, or where the altered cryſtalline has got out of its capſula, and floats in the aqueous humour, the operation, according to him, is much the ſame as in the *true cataract;* only that the part of the former operation, whereby the capſula, of the cryſtalline was opened at its lower edge, is omitted, and the needle muſt be placed farther forward in the eye, in depreſſing the cataract.

The falſe cataract, according to Dr Taylor, is the cryſtalline reduced to a fluid ſtate with an opaque capſula, in which he adviſes firſt to perform the ſame operation as in the true cataract, and afterwards to ſeparate the capſula all round from its adheſions with the *ligamentum ciliare,* that it may alſo be depreſſed into the vitrous humour.

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The glaucoma is said by our author to be the cryſtalline, with its capſula become opaque and enlarged in its volume; for removing which, he propoſes the ſeparation of the capſula, with its included cryſtalline, and their depreſſion into the vitrous humour, in the ſame manner as was propoſed for the capſula of the falſe cataract.

After theſe operations, Dr. Taylor drops a mixture of the tincture of *balſ. peruvian.* with warm water, into the eye; and then applies, upon the eye, a cataplaſm made of ſome drops of the mixture, with pulp of caſſia. This cataplaſm is to be renewed every four hours of the firſt two days, each time fomenting the eye, half an hour, with a ſpirituous fomentation in which there is camphire. He then gives free motion to the eye-lids, but keeps a ſhade over the eyes for ſome time.

The Cæſarean operation is recommended by ſeveral authors, and ſome examples have been recorded, of the mother having been ſaved by it; notwithſtanding which, women are frequently allowed to die with their children buried in their womb, without this attempt being made for ſaving either. To encourage practiſers to do their duty, by performing what art directs for ſaving patients in deſperate circumſtances, Mr Helvetius communicates to the academy of ſciences at Paris a well atteſted hiſtory of a woman recovering after the Cæſarean operation had been performed upon her by a midwife. *Hiſt. de l' Acad. des ſcien.* 1731.

Mr Petit the ſurgeon, in examining all the different methods employed by ſurgeons for ſtopping

ſtopping hæmorrhagies, aſſures us, that in each of them a piece of clotted blood is contained in the veſſel, which ſerves to keep the blood from eſcaping after the effect of the medicine firſt employed ceaſes. Where abſorbents or aſtringents are uſed, there is, he ſays, not only a cylindrical piece of clotted blood in the veſſel, but there is a covering of it on and round the orifice of the cut veſſel; which cylindrical form makes it eaſily puſhed out at the extremity of the veſſel; and therefore the hæmorrhage is in danger of returning, when it is ſtopped by ſuch medicines.——Cauſtics have much the ſame effects, only the covering on the extremity of the veſſel is firmer by the ſolid parts being confounded with the clotted extravaſation.——When a ligature is uſed, there is no clotted covering, and the internal plug is of a pyramidal figure, the ſmaller extremity being neareſt to the ligature; therefore it is with difficulty that ſuch a clotted piece can be puſhed out. Beſides this advantage, the ſides of the veſſel, which are brought to be contiguous by the ligature, will probably grow together; and on both theſe accounts, there is much leſs riſk of a freſh hæmorrhage, after tying veſſels, than in the other methods above mentioned.——Compreſſion rightly applied, to wit, on the ſides of the veſſel, renders the clotted plug of ſuch a form as cannot eaſily eſcape at the narrow orifice; and a large ſurface of the ſides of the veſſel being made contiguous, they will grow ſooner and more firmly together than in any other way; for which reaſon, and the ſaving both pain to the patient, and loſs of ſubſtance of of the ſtump, he prefers compreſſion to all the other methods. *Memoires de l'Acad. des ſcien.* 1731. In proof of the effects of coagulated blood ſtopping the efflux of blood from large arteries, he relates two hiſtories, in one, the artery was oſſified; and in the other, it lay in a bony groove, ſo that in both, it muſt have been a plug which prevented the hæmorrhage. He endeavours to prove, that the caillot or plug is ſtronger and firmer when formed of coagulated lymph, than when it is compoſed of red blood. Ib. 1732.

For executing the compreſſion right, after amputations of the larger extremities, he has contrived a machine, compoſed of a large circular belt, to be put round the trunk of the body, or ſuperior part of the member, which is to ſuſtain the bandage, by ſtraps going from it, which are faſtened at the other end to a leſſer circular belt that is put round the part of the member where the tourniquet is commonly applied. This leſſer belt is to have two plates with ſcrews, &c. in the form of his ſcrew-tourniquet, deſcribed, *Memoires de l'Acad. des ſciences*, 1718, and now generally known, and is to ſerve the ſame purpoſes of ſtopping the blood during the operation, and in the time of each dreſſing, and to moderate its courſe at all other times. From this leſſer belt, four ſtraps go out, to croſs over two plates with their ſcrews, as in the tourniquet placed on the ſtump. When the operation is performed, he applies thick bolſters of lint on the ſide of each large artery fartheſt from the bone, and placing the interior convex plate of

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the small tourniquet on them, he secures it in the right situation with the four straps, and screws down the plate on the bolsters and vessel, which being pressed between the instrument and the bone, can allow no blood to pass. Ibid. 1731.

Dr Desaguliers has contrived a machine for changing the air of the chamber of sick people in a little time, by either drawing out the foul air, or forcing in fresh air; or doing both successively, without opening doors or windows: A figure, description, and account of which are inserted into the Philosoph. Transact. numb. 437. § 1. 2. 3. which we cannot abridge.

Mr Quesnay (*l'art de guerir, par la saignée*) observes, that the effects of blood-letting must be, 1*st*, To empty the vessels, which he calls depletion. 2*dly*, To take away more of some sorts of liquors than of others, which he calls spoliation. The depletion may soon be supplied by new chyle, but this chyle is not so soon reduced into the nature of the liquors taken away; therefore, though the first effect of blood-letting may cease very soon, the second will continue longer, and is the principal. The red globules, according to our author, must be most affected by the spoliation, because of their small proportion to the other liquors, and their quick circulation in the larger vessels; whence he infers, that the great effect of blood-letting is to render the fluids more serous, and to weaken the solids.

From considering, says Mr. Quesnay, p. 47. all the effects of blood-letting, it must be concluded,

cluded, that there is only place for blooding when the liquids disturb the action of the solids, or when the solids cause disorder in the fluids. For when the solids or the liquids are found defective absolutely, or in themselves, the bad state of neither of them can be repaired by blooding.

It is impossible to make such an abridgement as our design allows, of all the particular cases in which he examines, whether blooding is useful or not; only we may in general observe, that there are very few diseases in which he thinks it may not be of advantage to let blood.

Dr. Langrish (Modern Theory and Practice of Physic) gives us tables of the different proportions of serum and gore, and the different powers of cohesion between the red globules which constitute the crassamentum, as also the proportions of the different principles obtained by a chemical analysis from the blood, and from the urine in different kinds of fevers, and in their several stadia.

P. 66. He tells us the manner in which his statical experiments were made: 1. "He always took care to bleed into a porringer as "near the same shape and size as possible, because a larger surface of blood should not be "exposed to the influence of the air in one "trial than in another. 2. All the blood "was received in one porringer, because he "has found by experience, that a pound of "blood does not separate so much serum, "when divided into several parcels, as when "contained in one vessel. 3. He always set "the blood in a cool place; and after it had

" ſtood twenty-four hours, he very carefully
" weighed the ſerum and cruor ſeparately, in
" order to find their different proportions. (4.)
" He took a very thin glaſs tube, 12 inches
" long, and ⅓ inch diameter, and having her-
" metically ſealed up one end of it, he blowed
" it out to an obtuſe point, about the bigneſs
" of a middling pea. Now this point being ſet
" upon the craſſamentum, the weight of the
" tube was not of itſelf ſufficient to preſs
" through, and but very ſeldom, when filled
" with water; ſo that his way of trying the
" coheſion of the gore, was to pour mercury
" into the tube, till it was juſt heavy enough
" to cut its way through; and as the tube
" was exactly graduated, he could, by this
" means, very nicely determine the power of
" coheſion between the globules which conſti-
" tute the craſſamentum."

By the tables of Dr Langriſh's ſtatical experiments made on the blood of people in acute continual fevers, p. 68. compared with what he ſays of the blood of three young men in perfect health, p. 74.; it appears that in ſuch fevers the ſerum is in leſs proportion, and the craſſamentum is more viſcid and tenacious than in health. The indications of cure from which are plain.

By the chemical analyſis, p. 80. it was proved, that in acute fevers the ſaline and ſulphureous parts did abound more than in health. And, p. 94. that the urine was impregnated more and more with theſe ſaline and ſulphureous principles, as the ſymptoms abated upon a criſis by urine.

Dr

Dr Friend had recommended bleeding at the jugular veins, in a phrenzy coming upon a fever; which Dr. Langriſh, p. 131. endeavours to prove rather to be hurtful: 1. because of the ligature's ſtopping the blood ſome time. 2. Becauſe opening the external jugular cannot make a revulſion from the internal parts; ſince not only the reſiſtance to the blood in the common trunk of the carotid is thereby diminiſhed, but alſo the reſiſtance to that which comes out of the heart is alſo diminiſhed, which will therefore ſend more by that common trunk, and conſequently as much as formerly by the internal carotid. 3. After the orifice of the jugular is ſhut, there is ſome reaſon to believe, that the blood continues to flow more by the ſuperior branches ſome time, which will do harm. 4. He thinks the caſes mentioned by Dr Friend, for proving his opinion, either do it not, or can be turned againſt him.

By our author's experiments in intermitting fevers, p. 229. the craſſamentum of the blood is not ſo viſcid and tenacious; neither is the ſerumſo bilious, ſaline, and acrid, as in acute continual fevers; and the red globules abound more, and the cruor is more viſcid and tough in quotidians than in tertians, and in tertians than in quartans.

The epidemic fever, deſcribed by Dr Douglas, ſeized one half of the inhabitants of New-England, and killed one in thirty-five; in ſome places one ſixth, one fourth, or one third of the ſick died. —The greateſt number of thoſe labouring under this epidemic diſeaſe, after the t... common ſymptoms of a fever, ..ad ſwel-ing, pain, and white ſpecks in the ..vula and tonſils, and a diſtinct, red, miliary eruption over all. (or a breathing ſweat th.. ſ..elled as the eruption did) which was at the height the fourth day; after which it itched and ſcaled off, and the ſpecks ſloughed o.. from the ſub-ſiding fauces. They generally did well.——A worſe kind of this fever was accompani-ed with a low unequal pulſe, proſtration of ſtrength, deſpondency, colliquative vomiting, purging, or ſweats, chop'd tonſils, with brown or livid ſpots; the eruption darker-coloured, or appearing and diſappearing; ichor or pus coming by mouth or noſe from parts out of ſight; mucous exuviæ ſloughing off the tongue, oeſophagus, or bronchia. Many thus affected died the ſixth or ſeventh day.———In the worſt ſort, the pulſe and ſtrength were ſtill lower, the colliquations were greater, and the ſick had a ſinking pain at the ſtomach, ſtupor, deli-rium or convulſions, and an intolerable fœtor. The few thus ſeized died the firſt, ſecond, or third day.

This fever was ſeldom too high, and the pa-tients generally recovered when left to nature, with a temperate regimen. Evacuations, parti-cularly blood-letting, haſtened death, or re-tarded the cure. Hot cordials alſo did hurt. Snakeroot tea; or Sp. C. C. and a little wine ſupported the faint and weak, and promoted the ſalutary breathing ſweats. Profuſe ſweats and diarrhœas were ſtopt by *c. cinam. decoct. alb. elixir. vitriol.* and toaſted *rhubarb. Calo-mel,* which made the evacuation they bore beſt, belt, and gargles of tinct. myrrh and aloes, helped off the sloughs, and kept the throat moist.——This epidemic disease was followed in some with discolourings, hæmorrhagies, &c. like those in the scurvy, which were cured by milk-diet, peruvian bark, and *elixir vitrioli*. In others with tumors, which resolved with mercurial plaisters and purging, but suppurated with cataplasms; and, when suppurated, spread by digestives, but cured with spirituous drying applications. The nervous symptoms, such as hysteric ails, melancholy, fatuity, &c. remaining with others, were soon removed by a restoring diet.

Dr Cohausen, in his book, intitled, *Archeus Febrium Faber et Medicus*, recommends the quinquina or Jesuits bark in intermitting fevers, but mentions several rules to be observed before it is given, while it is used, and after taking it. He condemns the use of purgatives as preparatives for the bark, and recommends emetics, especially in mesenteric fevers, where the *primæ viæ* are stuffed. He recommends the bark to be given immediately after the aguish paroxysm, and to repeat the dose every four hours; and is of opinion it has better effects when taken with a bitter stomachic wine than in pills, and it is still more effectual when some green tea is drank with it. While the bark is used, and in the intervals of paroxysms, he says, exercise is of great use.

Though our author is very fond of the bark in the cure of intermittent fevers, he cautions practisers to be very careful not to give it to all patients, or at improper times; because, though it may put off the fever, it occaſions ſwellings, dropſies, pains of the belly, obſtructions of the bowels, and a great many other diſeaſes, (of which he gives ſeveral ſtrong examples) that are more dangerous and worſe to cure than the ague is. The beſt method, he ſays, to remove theſe bad ſymptoms, is to bring back the fever, for which Etmuller recommends volatile ſpirit of ſal ammoniac; our author thinks a wine in which reſolving diuretic and deterſive medicines are infuſed would be ſafer. He tells us, the people of Weſtphalia ſeldom miſs to bring back the fever when neceſſary, by eating a high-ſmoked ſow's head.

Mr Cohauſen is of opinion, that the continued remitting fevers, which are periodical in their remiſſions, are of the ſame nature with agues, and ought alſo to be cured by the bark, unleſs they are of a malignant kind, accompanied from the beginning to the end with coldneſs and a weak pulſe; in ſuch caſes, diaphoretics are the proper medicines.

The German phyſicians ſeem at preſent to be divided in their opinions concerning blood-letting in the ſmall pox, ſome ſtill adhering to the hot regimen, while others recommend venæſection as the medicine moſt to be depended on. Dr Burghart, phyſician at Breſlaw, in ſupport of his friend Dr Tralles, phyſician in the ſame place, his treatiſe on the uſe and neceſſity of blood-letting in the variolous fever, proves by numerous obſervations of the good effects of hæmorrhagies from the noſe and other parts of the body, and venæſection in the

variolous

variolous fever, that in a great many cases it is the chief or only medicine from which the prevention of the worst symptoms is to be expected, though perhaps it is not necessary to every patient under this disease. *Mantiss. ad Specim.* 1. *Satyr. Medic. Silesiac.*

Dr Calderwood (in his new Method of curing the apoplexy) condemns the common method of letting blood from any vein, giving emetics or sharp clysters, and applying blisters; but insists much on the advantage of arteriotomy; and recommends cordials in the cure of the apoplexy.

Dr Astruc has wrote a regular and complete account of the origin, nature, symptoms, prognostics, and cure of the *lues venerea.* In which, after examining critically all the arguments that have been used in proof of the lues having been known in Europe before the conquest of the West Indies, he thinks them insufficient; and concludes, that it was brought from the island Haiti or Hispaniola by the Spaniards, who being employed in the defence of Naples, attacked by Charles VIII. King of France, communicated this disease to the inhabitants of that country, and to the French army in 1494, when it was first taken notice of; and therefore was called the Neapolitan or French disease.

He thinks the reason why the inhabitants of Hispaniola, and some other hot countries, had the lues endemic among them, was the heat of the climate, and the promiscuous coition of their women even in the time of their menstrua.

After

After having obſerved, that ſeveral diſeaſes have had their progreſs and periods, and that the venereal diſeaſe is gradually become more mild, our author is hopeful that it may alſo wear out.

Dr Aſtruc proves this diſeaſe always to be communicated here in Europe by infection; and modeſtly conjectures, from the effects of its poiſon, that it is of an acid or acido-ſaline, corroſive, and fixed nature.

It is impoſſible for us, in the narrow bounds we are confined to, to follow our author in his aitiology, diagnoſis, prognoſis, and cure of the ſeveral ſtadia of the venereal diſeaſe, which he diſtinguiſhes very accurately, both when it affects the whole body, or any particular part of it, and when it is attended with no other diſeaſe, or complicated with others. We ſhall only remark, that he proves the gonorrhœa to affect the proſtata and *veſiculæ ſeminales* in men, as well as the mucous glands, and Cowper's and Littre's glands, to which it is confined by ſeveral authors: And that he prefers the ſalivation by inunction to every other method for curing the *lues*.

We ſhall conclude this ſuperficial account of Dr Aſtruc's book, with a cenſure he makes, which we wiſh our countrymen would ſhun to deſerve. In giving the character of a particular Engliſh writer, he ſays, *Ordine parum compoſito diſſeritur, ſaltem non eâ methodo quæ lucem afferat, et quam in Anglorum medicorum operibus plerumque deſiderari dolemus.*

*Pudet hæc opprobria, &c.*

Dr

Dr. Morgan (*mechan. pract.*) recommends several uncommon methods of cure in different diseases; the most remarkable of which we shall mention.

The tincture of cantharides is, according to him, p. 114. a medicine that may almost be absolutely depended on for checking, restraining, and stopping the immoderate flow of urine in a diabetes. He chuses to make this tincture by infusing or digesting half an ounce of cantharides upon a pound of the *elixir vitrioli*, of which tincture, from 15 to 30 or 40 drops may be given twice or thrice a day, as the symptoms may indicate; and the best vehicle is the Bristol hot-well water.

Prop. XIII. is employed in inculcating the advantages of curing fevers by sweating, raised by low cooling drinks in the effluent or inflammatory fevers, and by the warmer regimen in influent or nervous fevers. In which last he recommends blisters much, especially when soon applied; and proposes that the blistering plaisters should be left on four or five days, or as long as they will draw off any thing.

The cure proposed by the Doctor, *p.* 179. for intermittents, which he assures us is much preferable to the common practice, is to give a vomit about an hour after the invasion of the cold fit; which being wrought off, the patient goes to bed, and is put as soon as possible into a large and copious sweat, to be continued and succoured by plentiful dilution, for six or eight hours. This method, three or four times repeated, scarce ever fails, he says, of curing a quartan, especially if the person afflicted use the cold bath every day between the fits. But in a tertian, this method, once or twice repeated, makes commonly a cure.

In petechial and malignant fevers there is, in our author's opinion, no hope but from ſweating, which it is impoſſible to raiſe and maintain uniformly and equally in theſe caſes, without the moſt powerful and effectual bliſtering.

He eſteems ſweating and looſe ſtools of the greateſt advantage in the ſmall-pox; and likewiſe propoſes it as a cure for the gout, ſciatica, and rheumatiſm.

Sweating ſuſtained with proper diluters is alſo recommended by him in dry coughs. After the dry cough, and catarrh which follows it, is thoroughly fixed, he never found any thing effectual but giving calomel, or ſome ſuch moderate mercurial, to raiſe a ſlight and gentle ſalivation.

He thinks the *fluor albus* is the lymph corrupted, and aſſures us the tincture of cantharides given in a pretty ſtrong decoction of guajac, has good effects in its cure when recent. But, where this diſeaſe is of long ſtanding and inveterate, recourſe muſt be had to mercurials.

P. 255. He appears to be no friend to bloodletting, making it matter of advice and requeſt to all younger and unexperienced phyſicians to be ſparing of human blood, to ſee an abſolute neceſſity of it before they ſpill it, &c.

P. 271. When opium, ſays the Doctor, affects the head or lungs by its volatile, æthereal oil or ſpirit, acids, eſpecially foſſil acids, are

are the proper correctors. When opium produces ſickneſs, nauſeas, vomitings, ſpaſms, flatulent cholic pains, and ſuch like ſymptoms, by the action of its ponderous, ſtimulating, and adheſive oil, the warmeſt alexipharmics muſt be uſed.

P. 278. According to our author, the moſt effectual medicines in ſcorbutic diſorders are mercurial deobſtruents. He has found the following medicine very ſucceſsful: *R Mercur. viv. unc* ii. *Terebinth. drachm.* ii *vel q. s. ad mercur. fixandum; cui adde rhubarb unc.* i. *Sem. cochinel. unc. ſem. et cum elixir. propriet. q. s. fiat maſſa pilularis.* This commonly proves an effectual, ſafe, and benign diuretic. While it is taking, the patient is now and then to be moderately ſweated. If the pills ever raiſe a ſalivation, the force of the mercury on the ſalival glands may be eaſily prevented or reſtrained at pleaſure, only by rinſing the mouth pretty often in a day with a ſolution of camphire in the oil of olives.

P. 354. For the cure of the ſcurvy, he propoſes that the patient ſhould be put into a warm or hot bath for half an hour or forty minutes, till the pores are all opened, and the ſweats are moderately raiſed and brought out: And then let him be taken out, and immediately immerſed in cold water for half a minute, or juſt time enough to dip the head two or three times, and then taken out again and put to bed, in order to keep up a pretty free flowing ſweat for three or four hours, to be maintained and ſupported with any of the common warm diluters, ſuch as ſage tea, wine-whey, poſſetdrink, &c. At the ſame time the parts affected may be ſlightly touched every other night going to bed, with the *ung. Neapolitan.* or ſomething equivalent, in every ounce of which a drachm of the prepared cantharides has been mixed and incorporated, firſt reduced to the moſt ſubtile powder. And upon this any common ſudorific draught or bolus may be given, to be ſupported with diluters as before, in order to keep up a moderate breathing ſweat for the night.

XXXVI. *A Liſt of Medical Books publiſhed ſince the beginning of the year* 1734.

A Diſſertation on the ſtate of phyſicians among the old Romans, by Dr Conyers Middleton, 8vo, London 1734.

Remarks on Dr Middleton's diſſertation, tranſlated from the Latin of P. W. M. D. 8vo, London 1734.

An account of Alexander Trallian, one of the Greek writers that flouriſhed after Galen; ſhewing that theſe writers are far from deſerving the reputation of mere compilers, by Edward Milward, M. D. 8vo, London 1734.

Oratio Harvæana in ædibus collegii regalis medicorum Londinenſium habita, Oct. 18. A. D. 1735, ab E. Wilmot, 8vo, London 1735.

Diſquiſitio phyſico-medica de natura aquæ, et quænam ſit ſaluberrima, à *Joanne Baptiſta de Malmedie*, M. D. 12mo, *Auguſtæ Eburonum* 1735.

Deſcription des plantes qui naiſſent, où ſe renouvellent aux environs de Paris, avec leurs uſages

usages dans la medecine et dans les arts, le commencement et progres de cette science, et l'histoire des personnes dont il est parlè dans l'ouvrage, par *M. Fabregou*, botaniste et demonstrateur, tome I. 12mo, à Paris 1734.

A treatise of the fossil, vegetable and animal substances made use of in physic, containing the history and description of them, with an account of their several virtues and preparations, by Stephen Francis Geoffroy, M. D. translated by George Douglas, M. D. 8vo, London 1735.

Histoire generale des drogues simples et composées, par *Pomet*; nouvelle edition, corrigée et augmentée, par le Sieur *Pomet* fils, Apothecaire, 2 vols. 4to, à Paris 1735.

Mechanica Medicamentorum; autore *Joanne Baptista Mazino* Brixiano in gymnasio Patavino med. pract. prof. 4to, Brixiæ 1734.

Pilulæ Wardianæ dissectio et examinatio: Ward's pill dissected and examined, and its true composition plainly discovered even to ocular demonstration. In a Latin epistle to the ingenious Dr Boerhaave; now translated into English, 8vo, London 1736.

Dispensatorium regium electorale Borusso-Brandeburgicum variis observationibus locupletatum, ab *Ernesto Fagino* Augustano, fol. Erford. 1734.

Pharmacopœia Collegii Regii Medicorum Edinburgensis. Editio tertia, 8vo, Edinburgi 1735.

Boerhaave's chemistry translated by Timothy Dallowe, M. D with several corrections and emendations by the author's approbation, 2 vols. 4to, London 1735.

Remarques de chymie touchant la preparation de differentes remedes usitées dans la pratique de la medecine, 12mo, à Paris 1735.

Abregé de l'anatomie du corps humain, où l'on donne une description courte et exacte des parties qui le composent, avec leurs usages, par *M. Verdier* chirurgien juré de Paris, 2 vols, 12mo, à Paris 1734.

*Oribasii* anatomica ex Galeni libris, cum versione Latina *J. Bapt. Rasarii.* Curante *Gulielmo Dundas*, cujus notæ accesserunt, 4to, Lugd. Batav. 1735.

Human osteogeny explained in two lectures, illustrated with figures accurately drawn from the life, by Robert Nisbet, M. D. 8vo, London 1736.

*Bernard Siegifried Albini* anat. et chir. in Acad. Batava quæ Leidæ est professoris, historia musculorum hominis, 4to, Leidæ Batav. 1734.

Tractatus quatuor anatomici de aure humana, tribus figurarum tabulis illustrati, autore *Joan. Friderico Cassobhom*, M. D. et prof. P. 4to, Halæ 1734.

De vasis linguæ salivalibus atque sanguiferis epistola *Christoph. Jacobi Trew*, M. D. 4to, Norimberg. 1734.

*Bernard Siegfried Albinus*, anat. et chir. P. de arteriis et venis intestinorum hominis; adjecta icon coloribus distincta, 4to, Leidæ Batav. 1736.

Dissertationes medicæ de membrana allantoide, autore *Ludovico de Newfville*, M. D. de clitoride, autore *Theodoro Tronchin*, M. D. Editio nova emendatior et accuratior, 8vo, Lugd. Batav. 1736.

De

De præcipuis humoribus qui humano in corpore reperiuntur, deque eorum historia, qualitatibus, et officiis exercitatio *Josephi del Papa* med. in Pisana Universitate prof. 4to, Florent. 1733. Recus. in 8vo, Lugd. Batav. 1736.

Fundamenta physiologica, sive positiones, hominis statum sanum ad officia sibi in hoc mundo expediunda necessarium, delineantes. Autore *D. Detharding*, M D. et physic. P. 8vo, Hafniæ 1735.

Dissertatio de graviditate debitum gestationis tempus excedente, seu diuturna, autore *Joanne de Buchwald*, M D. 8vo, Hafniæ 1734.

Essai physique sur l'oeconomie animale, par *Francois Quesnay* chirurgien, 12mo, à Paris 1736.

Chirurgia Theorico practica de vulneribus, autore *Petro Guisard*, M D. Monspeliensi, 12mo, Avenione 1735.

Chapman's treatise of midwifery, second edition with additions, 8vo, London 1735.

A treatise on the immediate organ of sight, by John Taylor, M. D. 8vo, London 1735.

A new treatise on the diseases of the crystalline humour of a human eye; or, of the cataract and glaucoma, by John Taylor, M. D. 8vo, London 1736.

Dr Taylor couched for a cataract: Wherein the absurdity of his new treatise on the diseases of the crystalline humour, as likewise his theory of the causes of cataracts, is fully demonstrated, by J. S. surgeon, 8vo, London 1736.

L'art de guerir par la saignée, où l'on examine en meme tems les autres secours qui doivent concourir avec ce remede, où qui doivent lui être preferées dans la cure des maladies tant medicinales

dicinales que chirurgicales, par *François Quesnay*, chirurgien, 12mo, à Paris 1736.

The fountain of health, by H. Bourdon, 8vo, London 1734.

Elementa diætæ, sive regulæ physico-medicæ ad sanitatem conservandam, vacillantem fulciendam, deperditam verò citò, tutè, et jucundè magis recuperandam, autore *K. Detharding*, M D. Hafniæ 1735.

Traité du bon chyle pour la production du sang, où l'on voit, entre les causes ordinaires qui le corrompent, plusieurs maladies qu'on n'a pas connuës. Il contient aussi les moyens de les prevenir, et les remedes pour les guerir. Par *M. Viridet*, docteur en medecine à Morge, 2 vols. 12mo, à Paris 1735.

*Roberti Welsted* tentamen alterum de propriis naturarum habitibus, remediisque ad singulos accommodandis, 8vo, Londini 1735.

*Pauli Gottlieb Werlhorfii*, M. D. cautiones medicæ de limitandis laudibus et vituperiis morborum et remediorum, 4to, Hanover 1734.

Demonstratio Medico-practica prognosticorum *Hippocrates*, ea conferendo cum ægrotorum historiis in libro primo et tertio epidemiorum Hippocratis, ab *Henrico Cope* medico regio ad statum in Hibernia, 8vo, Dublini 1736.

Hippocrates upon air, water, and situation, upon epidemical diseases, and upon prognostics in acute diseases especially. To this is added, by way of comparison, Thucydides's account of the plague of Athens. The whole translated, methodized, and illustrated with useful and explanatory notes, by Francis Clifton, M. D. 8vo, London 1734.

The

The aphorifms of Hippocrates, and the fentences of Celfus, with explanations and references to the moft confiderable writers in phyfic and philofophy, both antient and modern, by Sir Conrad Sprengel, knight, M. D. The fecond edition enlarged, 8vo, London 1735.

The practical hiftory of an epidemic fever, with an *angina ulcufculofa*, by William Douglas, M. D. 8vo, Bofton in New-England 1736.

An effay on the practice of phyfic: Or an attempt to revive the practice of the antients. With fome general obfervations on animal mechanifm and the *materia medica*, by Andrew Hook, M. D. 8vo, London 1734.

Inftruzione intorno alle febre di *Joanne Dominico Santorino*, proto-medico anatomico, 4to, Venetiis 1734.

A fhort and certain method of curing continued fevers. Second edition, 8vo, London 1735.

Medical practice in curing fevers, by Theophilus Lobb, M. D. and F. R. S. 8vo, London 1735.

The arcanum concerning horfes explained; an introduction to phyfic, with the method of fevers, 8vo, London 1734.

The modern theory and practice of phyfic, by Browne Langrifh, M. D. 8vo, London 1735.

A rational and mechanical effay on the fmall pox, by William Hillary, M. D. 8vo, London 1735.

A new method of curing the apoplexy, by John Calderwood, M. D. 8vo, London 1735.

De catalepfi fchediafma: Unà cum hiftoria mulieris catalepticæ, focietati regiæ communicata

cata à *Richardo Rynell* pharmacopœo Londinenſi, 4to, Londini 1736.

A treatiſe of ſudden deaths, by Nicholas Robinſon, M. D. The ſecond edition, 8vo, London 1735.

Diſſertation ſur la petrification d'un epiploon, par M. *Mongin*, M. D. 12mo, à Paris 1734.

A preſervative againſt the miſerable conſequences of the bite of a mad dog, 8vo, London 1734.

A treatiſe on the rheumatiſm, as well acute as chronical, by John Cheſhire, M. B. 8vo, London 1735.

Obſervationes circa ſcorbutum, ejuſque indolem, cauſas, ſigna, et curam inſtitutæ, autore *Joanne Friderico Bochſtrom*, M. D 8vo, Lugd. Batav. 1734.

Syſteme de M. *Herman Boerhaave* ſur les maladies veneriennes, traduit en François par *M. de la Mettrie*, M. D. avec des notes et une diſſertation du traducteur, ſur l'origine, la nature, et la cure de ces maladies, 12mo, à Paris 1735.

De morbis venereis libri ſex: In quibus diſſeritur de origine, propagatione, et contagione horumce affectuum in genere: Tum de ſingulorum natura, aitiologia, et therapeia; cum brevi analyſi et epicriſi operum plerumque, quæ de eodem argumento ſcripta ſunt. Autore *Joanne Aſtruc*, regi à conſiliis medicis, &c. 4to, Pariſiis 1736.

A new method of curing (without internal medicines) that degree of the venereal diſeaſe called a gonorrhœa or clap, by George Warren, ſurgeon, third edition, 8vo, London 1734.

A

A treatife on the venereal difeafe, in three parts, by Nicolas Robinfon, M. D. 8vo, London 1736.

A fynopfis of the hiftory and cure of venereal difeafes, by J. Armftrong, M. D. 8vo, London 1737.

Aphrodifiacus, containing a fummary of antient writers on the venereal difeafe, by Daniel Turner, M. D. 8vo, London 1736.

Confulti medici del fignor *Dottore Giufeppe del Papa*, 2 tom. 4to, Venetiis 1734.

*Frederici Hoffmanni* confultationum et refponforum medicinalium centuria prima, complectens morbos capitis et pectoris, tom 1.

Earundem centuria fecunda, complectens morbos abdominis et artuum externorum, tom. 2. 4to, Halæ 1734.

The mechanical practice of phyfic. In which the fpecific method is examined and exploded, and the Bellinian hypothefis of animal fecretion and mufcular motion is confidered and refuted, by T. Morgan, M. D. 8vo, London 1735.

Boerhaave's aphorifms tranflated into Englifh, 8vo, London 1735.

Syftema novum mechanico-hippocraticum de morbis fluidorum et folidorum, ac de fingulis ipforum curationibus, opus theorico-practicum *Jofephi Thomæ Rofetti*, phyf. et med. prof. fol. Venetiis 1734.

Confpectus medicinæ theoretico-practicæ tabulis 138, omnes primarios morbos methodo Stahlianâ tractandos exhibens, tertiâ vice editus, correctus et auctus autore D. *Joanne Junkero*, M. D. et prof. 4to, Halæ 1734.

Philofophical

Philoſophical tranſactions for the years 1734 and 1735. 4to, London.

L'hiſtoire et les memoires de l'Acad. des ſciences, années 1731 et 1732, 4to, à Paris, 12mo, Amſterdam.

Commentarii academiæ ſcientiarum imperialis Petropolitanæ, tom. 4. ad annum 1729, 4to, Petropoli 1735.

Miſcellanea Berolinenſia ad incrementum ſcientiarum, ex ſcriptis ſocietati regiæ ſcientiarum exhibitis edita. Continuatio tertia, ſive tom. 4tus, cum figuris et indice materiarum, 4to, Berolini 1734.

Medicorum Sileſiacorum ſatyræ, quæ varias obſervationes, caſus, experimenta, tentamina ex omni medicinæ ambitu petita exhibent, ſpecimen, 1. cum figuris, 8vo, Wratiſlaviæ et Lipſiæ 1736.

## XXXVII. *Books propoſed, and other Medical News.*

MR Fabregou promiſes to publiſh ſeven volumes of his deſcription of plants, to which the one mentioned, p. 399. of this volume is an introduction.

There is now in the preſs at Leyden, Bernhardi Siegfried Albini, anat. prof. oſſium fœtus humani icones 163, cum earum explicationibus, 4to.

Dr Hook, in his eſſay on the practice of phyſic, propoſes to publiſh a new Pharmacopœia.

Dr Edward Milward is to favour us very ſoon with a treatiſe on the Peruvian bark, in which he only propoſed at firſt to conſider it as

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a ſpecific in gangrenes, but as we are now informed, he is to examine all its properties.

A letter has been publiſhed, inviting all the phyſicians living or born in Sileſia, to communicate their obſervations, experiments, &c. any way relating to phyſic, which are to be publiſhed under this title, *Medicorum Sileſiacorum Satyræ, quæ varias obſervationes, caſus, experimenta, tentamina, ex omni medicinæ ambitu petita exhibent.* We ſuſpect Dr Burghart, junior, phyſician at Breſlaw, to be the Collector.

FINIS.

To the BOOKBINDER.

| | Tab. | | Pag. |
|---|---|---|---|
| Place | I. | Fronting | 62. |
| | II. | | 230. |
| | III. | | 256. |
| | IV. | | 350. |