MEMOIRS

of the

LITERARY

and

PHILOSOPHICAL SOCIETY

of

Manchester.

Ser I

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1798

LAWS.

I. That the Ordinary Members only shall be invested with the privilege of voting and electing Members; and that the whole expences of the Society shall devolve upon them.

II. That Gentlemen residing at a distance from Manchester shall be eligible into this Society, under the title of Honorary Members; provided no one be recommended who has not distinguished himself by his literary or philosophical publications.

III. That Gentlemen at a distance, who have favoured the Society with important communications, or from whom such contributions may be expected, shall be eligible, under the title of Corresponding Members.

IV. That every Candidate for admission into the Society, whether as an Ordinary, Honorary, or Corresponding Member, shall be proposed by at least three Ordinary Members, who shall sign a certificate of his being, from their knowledge of him, of his character, or of his writings, a fit person to be admitted into it; which certificate shall be read at not fewer than two successive meetings of the Society, previous to the evening of election.

V. That no election shall be made, either of Ordinary, Honorary, or Corresponding Members, except at the Quarterly Meetings; and that notice shall be given to each Member, whenever a Candidate is nominated.

VI. That every election shall be conducted by ballot, and that the majority of votes shall decide; and that the President shall have the determining voice, if the number of votes be equal.

VII. That when an Ordinary Member removes to a greater distance than twenty miles from Manchester, he may be entitled to the continuance of the privileges of the Society, by paying five guineas to the Treasurer, in lieu of his annual subscription.

VIII. That a President, four Vice-Presidents, two Secretaries, a Treasurer, and a Librarian be elected annually by the majority of Members present, on the last Friday in the month of April. The election to be determined by ballot.

IX. That a Committee of Papers shall be appointed by ballot, at the same time, which shall consist of the President, Vice-Presidents, Secretaries, Treasurer, and Librarian, together with six other Members of the Society; and that this Committee shall decide by ballot concerning the publication of any paper which shall have been read before the Society; and shall select, with the consent of the Author, detached parts of any paper, the whole of which may not be deemed proper for publication; but that the presence of seven Members of the Committee shall be necessary for such discussion or decision.

X. That Visitors may be introduced by any Member to the Meetings of the Society, with the permission of the Chairman.

XI. That every Member who shall favour the Society with communications, shall send them to one of the Secretaries, the Monday before the meeting of the Society.

XII. That the Secretary to whom the paper shall be delivered, shall, with the approbation of the President, or two Vice-Presidents, have the power of suspending the reading of it until it be referred to a meeting of the Committee of Papers, whose decision shall be final.

XIII. That all papers judged admissible shall be read by one of the Secretaries, or by the author, in their order.

XIV. That no more than half an hour shall be allowed for the reading of any paper; and if the whole cannot be read within that time, the remainder (except the Society determine otherwise) shall be deferred till the succeeding evening. No paper however shall engage more than two evenings, without the consent of the Society, expressed by ballot, if required.

XV. That every Ordinary Member who produces a paper, shall therewith deliver a summary of its leading contents, which shall be read, paragraph by paragraph, after the paper to regulate its discussion.

XVI. That the Speakers shall direct to the Chair any observations they may make; and, if it be difficult to command immediate attention, it is desirable that they should stand up when they address the President.

XVII. That authors be requested to furnish the Society with an epitome of their papers, which may be read at the meeting succeeding the reading of each paper, and the discussion renewed.

XVIII. That each Ordinary Member shall pay one guinea annually, by half yearly payments, into the hands of the Treasurer, to defray incidental expences, and to establish a fund for the benefit of the Society. Each Member on his election to pay his subscription for the current half year, together with two guineas as an admission fee.

XIX. That each of the Vice-Presidents, in rotation, undertake his office, for one month; during which term he shall take the chair, in the absence of the President, at seven o'clock precisely: it is hoped that he will furnish articles of intelligence; and when no paper is before the Society, it is expected that he provide a subject for discussion.

XX. That no laws shall be enacted, rescinded, or altered, but at the quarterly meetings, on the last Fridays in the months of January, April, and October; and that notice shall be given, at least, fourteen days previous to those meetings.

XXI. That the Society shall publish a volume of miscellaneous papers, at least, every two years. And that, at stated times, the Committee shall select from the papers which have been read to the Society, such as shall appear to be most worthy of publication, but that no paper shall be published without the consent of the author. That every paper, voted for publication by the Committee of Papers, shall be sent to the press without delay; that notice of the printing shall be given to the author, and that he be entitled to thirty separate copies, on paying the extraordinary expence attending them.

XXII. That a Library be formed for the use of the Members of this Society, and that the Librarian be authorized to purchase such books as shall be ordered at the quarterly meetings of the Society; but that no book shall be taken out of the library, without leave of the Librarian, and that the time of keeping it be limited to seven days.

XXIII. That the resolution to establish a library be announced to the Honorary and Corresponding Members of the Society; and that it be intimated to them by the Secretaries, that donations of their past and future publications will be *highly acceptable*.

XXIV. That a GOLD MEDAL shall be given to the author of the most valuable experimental paper, containing some important discovery relative to the arts and manufactures of Manchester, which shall have been delivered to the Secretaries, and read at the ordinary meeting of the Society, before the last Friday in March, 1798.

XXV. That the adjudication of this premium be referred to the Committee of Papers; that their decision shall be made by ballot; and that the medal shall be delivered by the President to the person to whom it shall have been adjudged, or to his representative, at the first meeting of the Society in October, 1798.

XXVI. That two SILVER MEDALS shall be given *annually*, one to the author of the best Essay on a Literary, and another to the best on a Philosophical Subject, which shall have been read at the Society during the course of the season; to be determined by the Committee of Papers.

A

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MEMOIRS

of the

LITERARY & PHILOSOPHICAL SOCIETY

of

Manchester.

CURSORY REMARKS, MORAL AND POLITICAL,

on

Party-Prejudice.

BY SAMUEL ARGENT BARDSLEY, M.D.

READ APRIL 19TH, 1794.

Cuivis enim patet consuescere homines, eos, qui suarum partium sunt, immodicis efferre laudibus; qui autem contrarii sunt, infra meritum deprimere. BACON.

Among the variety of prejudices which have tyrannized over mankind, no one has ruled with more uncontrouled sway, than the prejudice of Party. Prejudices founded on ignorance may be removed, by solely enlightening the mind. But party-prejudice, commonly associated with inveterate habits, and strengthened by the influence of passion and interest, is with difficulty conquered, even with the assistance of knowledge, reason, and truth. The learned therefore, as well as the ignorant, are subject to its influence. For the mind engaged in political subjects, connected with interest, passion, or habit, exercises its faculties with partiality; draws erroneous conclusions; and thus blinds, confounds, and leads the judgment captive to its perverse inclinations. If ignorance* be the parent, passion and self-love may be considered as the nurses of prejudice.

Nor does virtue protect her votary from its attacks. It is, certainly, a plant of quick growth in a vicious and ignorant bosom; but, too frequently, takes deep root, and flourishes in the breasts of the wise and virtuous. Then, indeed, it becomes the source of great moral and political evil. For when in matters even of small importance to the welfare of a state, the characters, stamped by public opinion as great and good, swerve from the line of rectitude, and yield to the voice of interest, or the clamours of faction, our ideas of right and wrong are confounded, and public virtue receives a dangerous wound: and their guilt acquires an importance, in

* The influence of prejudice, operating on honest but ignorant minds, is curiously exemplified in the following form of an old presentment by an inquest. "We say," observe the jury, "that I. Stevens is a man we cannot tell what to make of him; and he hath books we cannot understand them." Hakewell, Mod. Tenend. Parl. page 172.

proportion as the influence of example is extended to others: *Plusque exemplo, quam peccato nocent.*

The evil of party-prejudice is not confined to the state. It invades the peace of individuals, friends, and neighbours. The tender charities of blood and kindred are frequently dissolved. Detraction is the bitter, but detested foe of human happiness. Party-malice, however, acting under the mask of patriotism, instead of exciting detestation of its malignity, too often meets with the applause of the zealous partizan. Characters are thus blasted with impunity; and the atrocity of the crime is concealed by the influence of prejudice.

There appears a natural bias in the human mind towards prejudice. "It often arises," according to the observation of an eloquent French writer,* "from that unhappy tendency in the human mind towards a perplexity in the employment of its powers, which plunges it into error, in spite of opposition: for the human mind, so far from resembling a faithful mirror (whose equal surface admits and reflects the rays of light with unaltered fidelity) may rather be considered as a kind of magical glass, which presents only disfigured and monstrous objects." If the soil be so rank, no wonder that the

* D'Alembert: Encyclopédie.

weeds of party-spirit and detraction spring up to luxuriance, when watered and cultivated by the parent's or tutor's care. Example and authority often conspire to chain down the mind to illiberal and ridiculous prejudices. The pupil is early impressed by some appellation, descriptive of a party or sect. This term generally embraces a *complex* idea. But the tender mind is suffered to entertain *only the single idea* of contempt, or disgrace, affixed to the obnoxious party. Thus the real grounds on which such a party-distinction is built, are concealed from the view: perhaps they are little understood by those, whose duty it is to unfold their nature and designs. For to analyse the various interests of contending parties in a free government, requires a considerable attention to its history; and such a degree of reflection, as seldom falls to the share of a party-bigot. Youth are thus led to form unjust associations of ideas; which are frequently never eradicated during a life of study and information; even when assisted by the most brilliant talents.

Locke observes, in his chapter on association, that some independent ideas of no alliance to one another, are by education, custom, and the constant din of party, so coupled in the mind that they always appear there together; and can no more be separated in thought, than if they were but one idea; and they operate as if they were so.

The history of many great characters in free states affords numerous examples, to prove the danger and folly of communicating and cherishing party-prejudice. Its tenacious hold on the most powerful intellects is truly astonishing! Is there a breast so steeled by party-spirit, as not to lament for human infirmity, when the political bigotry of a Milton and a Johnson appear to view? Milton, who strenuously opposed the re-establishment* of *limited* monarchy, and became the champion of republicanism, sunk so deeply under the power of prejudice, as to glory in being united both in praise†

* See Milton's "Ready and easy Way to establish a Commonwealth." An. 1659.

† Tu mihi sic perge maledicere, *ut Cromuello pejor tibi sim*, quâ nullâ majore me laude afficere potuisti."

"Cum presertim non reipublicæ solum, sed & mei quoque intersit, ut, qui eâdem infamiâ tam prope sim conjunctus quam optimum eum (viz. Cromwell) atque omni laude dignissimum, gentibus, quoàd possum, omnibus atque sæculis, demonstrarem." Milton. Defensio 2da. pag. 90 & 108. Ed. Toland. Amstelodam.

It may, indeed, justly be conceded to the apologists of Milton, that his general conduct was influenced by the purest motives of patriotism; and that his sincerity ought not to be questioned, when, in a solemn appeal to the Deity, he affirms, that a sense of duty, justice, and sincere regard to the best interests of his country, solely guided the pen in all his political writings.

"Testor itidem Deum, me nihil istiusmodi scripsisse, quod non rectum, & verum, deoque gratum esse & per-and dispraise, in danger and in triumph, with the fanatical usurper Cromwell. Johnson was educated a party-bigot. His father who excited his re-

suaserim tum mihi etiamnum persuasus sim, idque nullâ ambitione, lucro aut gloriâ ductus; sed officii, sed honesti, sed pietatis in patriam ratione solâ." Denfens. 2da. pag. 88. But that the mist of prejudice had concealed from him the ambitious and traitorous designs of Cromwell, cannot, in truth, be denied by his warmest panegyrists. Indeed, it requires a large portion of enthusiastic admiration of his character and talents, not to treat with indignation, his flagrant partiality to Cromwell. What incense has Milton offered up to him in his "Second Defence," for not assuming the title of King, but modestly contenting himself with the less pompous and less invidious appellation of Lord-Protector; or, as Milton terms it, "Pater Patriæ!" Father of his Country! This panegyrical address to Cromwell, although seasoned with the most eloquent and spirited advice, recommending moderation and the establishment of a well-ordered commonwealth, was written at the time when the author was Latin-Secretary to the TYRANT, who had, in person, ignominiously expelled one parliament, by whose authority he had acquired power, and soon after dissolved another, packed by himself; but which he considered as not sufficiently devoted to his interest. Indeed, such was Milton's over-heated zeal and prejudice, as to lead him to bestow extravagant praise on the Usurper, who had dismissed a parliament with indecent violence, and of his own authority; and yet to condemn, with unrelenting asperity, the similar *arbitrary*, but, certainly, far less atrocious conduct, in the deceased—ill-fated monarch!

verence, and his mother whose indulgence won his affections, inculcated a set of opinions, which "grew with his growth, and strengthened with his strength." He was not satisfied with taking copious draughts of the spirit of party, but drank up the very dregs and lees of national and personal prejudice!

It forms, then, a most important part of education, to keep the mind free from injurious party-prejudice; and to prevent those early associations, which, by imparting an obliquity to the moral faculty, impress on the mind, ever afterwards, a kind of necessity to deviate from the line of rectitude.* This ought to be the parent's peculiar care; for, as Montesquieu † finely remarks, "The parent is generally capable of communicating his knowledge to his offspring: he is still more able to impress them with his passions."

* "Let any man, who hath been deeply engaged in the contests and views of party, ask himself, on cool reflection, whether prejudices concerning men and things have not *grown* up and *strengthened* with him; and obtained an uncontroulable influence over his conduct? We dare appeal to the sentiments of every such person."

Bolingbroke's Remarks on the History of England: Letter 23d.

† "On est ordinarement le maitre de donner à ses enfans ses connoissances: on l' est encore plus de leur donner ses passions."

L'Esprit des Loix, chap. 5. p. 55.

If the question be asked: whether it be not the indispensable duty of those, who have the charge of education, in free states which partake of a mixed form, to impress the minds of youth not only with general views of the fundamental principles of the constitution, and to inspire them with a rational zeal for the preservation of its liberties and blessings; but also to make them acquainted with the history and transactions of its party divisions? There can be no hesitation in making a reply in the affirmative.

For the necessity of imparting this information, upon just and liberal views of the subject, arises from a sacred obligation due to their country, as well as from a bounden duty to promote the happiness of their children and posterity. Yet, in performing this necessary task, great caution and judgment must be used. Let them beware of chaining down their minds to the opinions of party, instead of binding their attachments to the principles of the constitution. To magnify the patriotism, virtue, and talents of one party; and to exaggerate the faults and depress the merits of their opponents, would be to corrupt the candour of young and ingenuous minds. Indeed, to fix their attention *chiefly* upon the transactions of parties, would be to lead them from the pure streams of constitutional information, to drink of the muddy and bitter waters of rancour and party-strife.

It cannot be denied, that diversities of opinion will arise among the members of free states; and those too of such a nature, as may involve questions of the highest importance to the welfare of the community. But amidst this clashing of opinions and principles, we should never lose sight of the just prerogatives, privileges, and rights of the different branches of the government. To preserve and defend these with zeal and firmness, ought to be our sole aim and endeavour. To maintain an equilibrium between the power and interests of the distinct orders of a mixed government, has been the ostensible motive for the formation of all parties; and, when founded upon this pure and rational principle, they deserve our zeal and affection. For, if we consult the instructive pages of Grecian and Roman history, we shall find, that the preservation of the liberties, and even the existence of the state, frequently depended on the exertion of parties, composed of virtuous and brave citizens. But on this, as well as on most other occasions of human life, we must beware of deviating from the path of moderation. For this spirit of party admits of certain definite limitations. — *quos ultra citraque nequit consistere rectum.*

A want of attention to the boundaries of attachment, has not only led to the destruction of many parties, founded on just grounds, but also proved fatal to the liberties and happiness of the state. Men of the most splendid talents in the ancient republics, have been so powerfully influenced by ambition, party-zeal, and prejudice, as to become the leaders of a guilty faction. Prompted by these motives, they have sacrificed on the altar of their mistaken zeal, interest, or revenge, the dearest rights and happiness of their fellow citizens. Hence arose proscriptions, massacres, anarchy, and all the train of evils which lawless usurpers introduced into countries divided by party-feuds.

Unfortunately, in times of political dissention, moderation becomes branded with the name of cowardice or treachery; and none but violent measures and counsels meet with approbation. Perhaps the surest test of the rectitude and pure intentions of any party formed in a state, is the conduct of its leaders towards the moderate and peaceable class of citizens. For if these contending parties have degenerated into factions, actuated by ambition or false zeal, they will alike mark with detestation the moderate men of the community, who may have refused to inlist under their respective banners. Amidst the horrors and confusion of a revolution or a sedition, the voice of moderation and humanity will have little chance of being heard. In those turbulent periods, the most settled habitudes and affections undergo a total transformation. The admirable description, by Thucydides, of the sedition at Corcyra, affords a melancholy but instructive lesson of the change wrought in men's minds by the spirit of party:—“ Even words,” says the historian, “ now lost their former significance; since to palliate actions they were quite distorted. For truly, what was before a brutal courage, began to be esteemed that fortitude which becomes a human and sociable creature; prudent consideration, to be specious cowardice; modesty, the disguise of effeminacy; and being wise in every thing, to be good for nothing. The hot and fiery temper was adjudged to be the exertion of true manly valour; cautious and calm deliberation, to be a plausible pretext for intended knavery. He, who boiled with indignation, was undoubtedly trusty; who presumed to contradict, was ever suspected. He who succeeded in a dishonest scheme, was wise; and he, who suspected such practices in others, was still a more able genius. But was he provident enough, so as never to be in need of such base expedients, he was one that would not stand to his engagements, and most shamefully awed by his foes. In short, he who could step before another in executing villany, or could persuade a well designing person to it, was sure to be applauded.*”

“ Yet, all this while, the moderate members of such communities (either hated because they would not meddle, or envied for such obnoxious conduct) fell victims to both parties.†

* Thucydides, Ed. Duk. lib. iii, sect. 82, p. 217.
† Sect. 82, page 219.

If these mischiefs arising from party-prejudice and bigotted zeal, have been invariably connected with the operation of the passions and prejudices of mankind, when directed to political objects, we may as justly infer the continuance of similar effects from correspondent causes, as, that any of what are called the laws of nature, will remain inviolate. Both ancient and modern history afford too many proofs of the truth of this remark. In Greece and Rome, political intrigues, cabals, and dissentions incessantly succeeded each other; and, as an elegant writer observes, "the spirit of liberty fled away from a people, devoted to party-prejudices and faction."

If we enquire more particularly into the general causes of the evils afflicting these states, we shall find that they derived their origin from the avarice, profligacy, and ambition of the heads of different parties; assisted by the blind admiration, or supine inattention of their respective partizans.

An inordinate lust of power, joined to great talents, when possessed by any individual, render him a just object of suspicion, both to the government and the people. If he should fail in subverting the principles of the constitution, he may yet involve his country in misery, by splitting it into parties and factions. The admiration of his talents, and that ascendency which strong minds ever possess over the weak and ignorant, will strengthen the prejudices of his party. If he add craft and a command of temper to his other qualities, the danger is more to be dreaded.

Cæsar was daringly and artfully ambitious. Sprung from an illustrious line of ancestors, and endowed with extraordinary talents, he became a candidate for popular favour. He was eminently successful. His eloquence was employed in the impeachment of the guilty; and his compassion exerted in defending the innocent and oppressed. He riveted the affections of the people in pronouncing (contrary to an express law) a funeral oration, on the loss of his young wife. Plutarch remarks: "They sympathized with him as a man of great good nature, and one who had the social duties at heart." He was munificent in his bounty to distressed citizens; and the manner of bestowing greatly enhanced the value of the gift: *Cæsar dando, sublevando, gloriam adeptus est.**

These fascinating qualities served but as a cloak to conceal his dangerous ambition. His apparent moderation, talents, and great military skill attracted a powerful support from the virtuous and well-meaning patriots. Their prejudices blinded their judgment. In vain did Cato declare his suspicions of the purity of Cæsar's motives. Cicero also failed in tearing away the mask. But the union of Cæsar and Pompey, at last, too plainly discovered to the deluded but honest patriots, that a pretended shew

* Sallust.

of patriotism was the stalking horse, which had concealed his destructive machinations against the safety of the republic. The discovery was made at too late a period; as he finally triumphed over their credulity, and the liberties of his country.

Catiline was ambitious like Cæsar: yet he failed in his attempts to subvert the constitution. To ambition, he joined avarice, profligacy, and unblushing villany. He endeavoured to involve, in one extended scene of ruin, all that were illustrious in Rome for talents, virtue, and patriotism. He soon became the chief of a party; but this party was composed of the needy, the profligate, and the guilty: *Postremo omnes, quos flagitium, egestas conscius animus exagitabat, Catilinæ proximi familiaresque erant.** This detestable conspiracy was detected by the superior vigilance and sagacity of Cicero; and finally overwhelmed, by the united efforts of the brave and virtuous defenders of the commonwealth.

Catiline and Cæsar both aimed at the destruction of the liberties of their country; but they differed materially in the means to accomplish that end. Catiline, by availing himself of the ready assistance of the wicked and necessitous, openly attempted the subversion of the government. He was opposed and defeated. Cæsar allured to his party the

* Sallust. Bell. Catilinar.

most virtuous, eloquent, and patriotic citizens. Rendered powerful by their confidence and his own military atchievements, he, at length, became the destroyer of that cause, under the banners of which he had pretended to fight.

The Gracchi were born plebeians, though ennobled by their fathers' honours, and their mothers' illustrious descent. Bold, eloquent, and ambitious, Tiberius Gracchus was well qualified to engage the affections of the people. Ardent in the cause of liberty, he beheld, in the unbridled luxury of the rich and the encroaching arrogance of the nobles, sufficient reasons to attempt the increase of democratic influence. With this view, he endeavoured to revive the *agrarian law*, for the division of conquered and bequeathed lands among the people. Inflamed by the spirit of party and the desire of humbling their superiors, the people listened greedily to the flattering proposal. The rich and the patricians as strongly resented the measure. The contention of parties became truly alarming. At this period, Tiberius Gracchus solicited the office of the Tribuneship. He was elected along with Octavius. His passions and ambition now arose to a dangerous height. Hurried on by the spirit of party and a sanguine disposition, he violated a fundamental principle of the constitution, by degrading his colleague Octavius; who opposed his measures, as destructive innovations on the long established forms of the state. By turns he alarmed, soothed, and inflamed his party. Rome, now, became divided into factions; and none but violent measures were adopted on any side. Tiberius offered a *second* time for the tribuneship. This daring innovation on the laws of his country, in attempting to establish a perpetuity of the tribunitian power in one person, excited the utmost indignation of the violent,—alarmed the moderate,—and brought contention to a dreadful crisis!—The senate opposing force to force, the contest terminated in the destruction of Tiberius and his adherents. After this disgraceful violation of the laws of the state by both parties, tranquillity was for a time restored.

Caius Gracchus, not long after, renewed this scene of anarchy and injustice. Impelled by that ardent zeal for liberty, and aided by the same popular talents which distinguished his brother; and, perhaps, further instigated by a spirit of revenge, he trod in the same unhappy path which had led to the destruction of Tiberius. He aspired to the tribuneship, and was elected. Then grown giddy by the applause, and relying with fatal security on the protection of the people, he became a candidate, a *second* time, for tribunitian honours. He was opposed by the nobles; and, the people abandoning his cause, he miserably perished in the attempt.

If we trace the history of parties in free states, from the earliest periods of regular government, down to modern times, we shall find,—that, so far as those parties were governed by the superior ascendency of any distinguished individual; and from that cause, proved fatal to the tranquillity and liberties of a state, the examples just recited, will exhibit the leading features which have characterized all party-chiefs, and their followers. An important lesson may, hence, be taught to the adherents of party, in all free states. The danger arising, from a prejudiced and blind attachment to these "Gods of their idolatry," ought to be forcibly impressed on their minds. That state must be sunk to the lowest ebb of profligacy, in which a Catiline and his associates, should succeed in their attempts against its liberties. Fear and dismay are capable of producing a *temporary* dereliction of the support of order and freedom, in a government composed, for the most part, of able and virtuous citizens; but (as happened at Rome) they will, at length, rally around the constitution of their country, and annihilate the daring disturbers of public tranquillity. The example of an open and criminal attack against the constitution of any government, by a party notoriously infamous; and whose avowed purpose is the destruction of social order, and of respected and established forms, requires only to be held up to view to be detested. But when dissentions among the separate orders of a mixed state are founded upon principle, and supported by leaders of character and abilities, then the danger arising from party-prejudice becomes truly alarming. It is probable, that Tiberius Gracchus was, at first, influenced by a patriotic desire of maintaining an equipoise between the democratical and aristocratical branches of the state. Actuated by the same motive, many moderate members of the aristocracy joined his party. Legal and constitutional means of redress were for a while adopted. But these were too slow for the heated passions and encreasing ambition of Tiberius. He displayed the *genuine character* of an ambitious, popular, party-chief.—*Spurning at a moderate degree of success, and neglecting the salutary lessons of experience, he attempted to overleap, at one bound, the interval which separated him from the attainment of his object; and thus, by adopting summary and violent, instead of slow and moderate means, he risked the success of his enterprise.* Happy would it have been for his country had the evil stopped here! But the dæmon of discord once let loose, the abettors of the rights of the people and their opponents equally assisted in staining their cause with crimes and factious outrage. For a recourse to party-violence for redress, upon a violation of the laws of justice and the constitution, having once been established, contributed to corrupt the morals of the people, and destroy that relish for rational liberty, which had ever distinguished the republic. It is easy to plunge into faction; but difficult to restore order and tranquillity. Had it not been for party-prejudice, there can be little doubt, but such regulations of the agrarian law would have taken place, as to have raised to a due degree the democratical influence. By these means, the preponderating scale of patrician power would have been properly balanced, and the constitution brought back to those *first* principles, which had given it stability and splendour. The Gracchi then, instead of meriting the appellation of factious and ambitious demagogues, would have deserved the glorious title of *defenders* of the liberties of their country. It is the remark of Plutarch: "With such citizens as the Gracchi, Marius, Cinna, &c. it was difficult to preserve a republic; but with such as Cæsar or Pompey impossible." This observation will not apply to the most flourishing periods of the perfection and republican grandeur of the Roman commonwealth. The liberties of a people under such circumstances, are more in danger from the party-zeal and ambition of a Tiberius Gracchus, than from the splendid atchievements and artful address of a Cæsar. For, in all mixed governments, while the people possess sufficient patriotism, to be more anxious in preserving the fundamental principles of the constitution, than attending to the narrow and selfish views of party, they will reject bribes with indignation; look upon victories with jealousy; and carefully watch over the conduct of an aspiring and exalted character. In order to secure their countenance and support, it will be necessary to assume the appearance of a patriotic zeal, in supporting the privileges of, and regulating the balance of power between the separate orders of the state. Self-denial, and austerity in conduct and manners, must be practised to lull suspicion. By these arts Tiberius Gracchus exalted the power of one branch of the state upon the ruins of the other; and thus introduced anarchy, party-virulence, and a habit of insurrection. The foundations of the republic, by these means, were secretly undermined; and the whole edifice, soon after, tumbled into pieces, when assaulted by the vigorous and well-timed attack of Cæsar. Tiberius Gracchus, Marius, Cinna, and other factious leaders, may be considered merely as caterers for Cæsar. Like the jackal, they hunted down the prey, to be devoured by the lion!

If we direct our attention to the Grecian democracies, we shall find ample matter for reflection in the evils introduced by party-prejudice. The same causes, which corrupted the integrity of the citizens and destroyed the liberties of Rome, produced similar fatal effects among the Grecian states. Ambition, avarice, and party-malice reigned every where triumphant.

These are the causes which Thucydides* assigns

* Thucydid. p. 218. Ed. Duk.

for all the dreadful factions that ravaged Greece during the Peloponesian war. "The source of all these evils," he remarks, "is a thirst of power, in consequence of either rapacious or ambitious passions. The mind, when thus actuated, is ready to engage in party-feuds. For the men of large influence in communities, avowing on both sides a specious cause;—some standing up for the just equality of the popular;—others for the fair decorum of the aristocratical government;—by artful sounds embarrassed those communities for their own private ends."

In proportion, however, to the pure democracy,* which prevailed in any state, did the spirit of party arise to a dangerous height. Lycurgus had given to Sparta a mixed form of government. Its tranquillity therefore was better preserved, and its duration extended to a longer period than that of any of the pure democratical states. It is the observation of Aristotle,† that in mixed governments, like Sparta, party prevailed less (i. e. in comparison with the other Grecian states), though the power of the state was divided into separate branches. For

* "Dans les états extrêmement libres, ils trahissent la liberté à cause de leur liberté même, qui produissent toujours des divisions;—chacun deviendroit aussi esclave des préjugés de sa faction, qu'il le seroit d'un despote."—
MONTESQUIEU.

† Aristot. Polit. lib. v. cap. 9.

Theopompus acted with great moderation, as, among many other regulations, he instituted the *Ephori*; and thus, by depriving the royal authority of some of its weight, added to its stability. Instead of lessening, he exalted himself. It is reported, that, when his wife asked, 'if he did not blush to bequeath to his children an authority more crippled than that he had received from his ancestor?' he replied 'no: for I leave it greater, because *more durable*.'

In despotic governments, where the will of the prince is the supreme law, party-prejudices on political subjects seldom arise. The people may revolt against the cruelties of an unfeeling tyrant; or they may be induced by a factious chief to depose the despot, and raise their leader to his throne. But, though their ruler be changed, their principles remain. They hug willingly the chains imposed by themselves; and, if they should prove highly galling, they may be again induced to break them, but will not fail to call in the aid of another tyrant to rivet them faster than ever!

A monarchical government, among a highly civilized people, may admit of some diversities of opinion, but party-prejudice can have little influence in destroying private or public tranquillity. Remove a minister—kindle an external war—promote a taste for frivolous amusements, and the petty murmurs of opposition are generally silenced.

Indeed if the monarchy be feebly ruled, parties may arise threatening, in appearance, the subversion of the state; but as they depend, for the most part, on the contemptible struggles for places and profit among their respective leaders; they become ridiculous in the eyes of the people, and terminate in disgrace and ruin. In the civil war of France, nicknamed the Fronde,* during the minority of Louis XIV, the ladies are well known to have directed the political intrigues and conduct of the different parties.

The frequent union of religious with political

* See "l'Histoire de Fronde," and the "Memoirs de Cardinal Retz," *passim.*

The French seem to have been forced into sedition through the mere effect of caprice and sport. The women ruled every faction. Love formed as well as destroyed cabals. The Dutchess of Longueville prevailed on Turenne (who had just been created a mareschal) to endeavour to corrupt the army he commanded for the king. But he quitted as a *fugitive* the army he had commanded as a *general*, to please a capricious woman, who made a jest of his passion.

Even the philosophic Rochefoucault acknowledged no other leader but the god of love (or rather of gallantry), as his senseless (although celebrated) verses, addressed to the Dutchess of Longueville (written immediately after having nearly lost his sight by a musquet ball, during the war of the Fronde) sufficiently prove.—

"Pour mériter son cœur, pour plaire à ses beaux yeux,
J'ai faite la guerre aux roix, je l'aurois faite aux dieux."

prejudice, among the members of free states, and the mischiefs resulting therefrom are much to be deplored. Religious bigotry may so far inflame the passions of subjects, even of despotical and monarchical governments, as to establish parties, filled with the most rancourous prejudices against each other; and also induce them to depose the lawful monarch from the throne.*

If we consult the pages of Davila, and other writers on the war of the League (a war carried on for the avowed purpose of extirpating liberty of conscience, and establishing a bigotted and persecuting system of religion throughout France), we shall find as many instances of party-prejudices destroying the happiness of individuals, and distracting the state with factions, as ever disgraced the annals of the most licentious democracies. Indeed when we contemplate the fanatical bigotry, refined malice, and blood-thirsty dispositions of the priests who applauded, and the prince who executed the mas-

* The fate of Henry the 4th of France, and the transactions of religious parties during the civil war, previous to his being crowned, are sufficiently known. Sully, in his memoirs, relates an anecdote of his aunt, Madame de Mastin, which strongly exemplifies the power of religious prejudice. The reason she gave for disinheriting her nephew, was—"because he neither believed in God nor his saints, but worshiped the Devil." This was the notion she had received of protestants from her confessor.

sacres and assassinations of a St. Bartholomew's day, we are compelled to cry out with the poet—

"Tantum religio potuit suadere malorum!"

If religious bigotry do not rage so fiercely in free states, where toleration is established, yet religious and political prejudices are frequently united. When this union takes place, political prejudice acquires a tenfold malignancy. A spirit of rancour and persecuting zeal will infallibly widen the breach, which ambition or interest has created. Toleration does not, indeed, permit one sect to cram down the throats of another, with the point of the sword, its religious faith and discipline! Nor does it think fit, that orthodox zeal should attempt to illuminate the minds of heretics, by fires kindled with the bodies of their brethren! Such methods have been tried; but the experiment often proved dangerous and unsuccessful. There is scarcely a bigot of the present day, in Spain or Portugal, that would not detest the barbarous absurdity of such practices. Yet such is the weakness of the human mind, and the strength of prejudice, that, notwithstanding its own full enjoyment of freedom of opinion and the blessings of liberty, it will often survey, with mingled scorn and hatred, the followers of another faith. And from hence it follows, that calumny, acrimonious controversies, and odious names, descriptive of a sect, have been productive of greater mischief to the people and government of tolerant and free states, than ever was accomplished by a display of auto-da-fès, dragooning, or any other mode of persecution practised by arbitrary and intolerant governments! The latter evils occur only at distant intervals; but the former never cease to exist.

If a contrariety of conduct were necessarily connected with a difference in theological tenets, the malice and uncharitableness of the followers of different sects might readily be explained; "but," as a celebrated writer* observes, "where the difference of opinion is not accompanied with a contrariety of action, each being allowed to follow his own way, as happens in religious controversy, what madness to create divisions!"

Religious prejudices ought carefully to be guarded against in the education of youth. For, if they have been suffered to spring up in alliance with political party-prejudice, it is with the utmost difficulty they can be weeded out of the mind. If these associations be not destroyed, the purest benevolence of disposition will be likely to degenerate into a hatred of man, as a mistaken zeal for the honour of the Deity† will be joined to a concern

* Hume's "Essay on Parties in general."

† Montesquieu relates the following instance of a mistaken and blasphemous idea of avenging the Deity, as having happened in France during the 16th century: "a Jew, accused of blasphemy against the holy Virgin, was condemned to be flayed: several *gentlemen* armed with

for the interests of a party. In a moral point of view, the cherishing of such unworthy sentiments is highly pernicious. Detraction and calumny are the sure product of religious bigotry; and, as a keen observer* of human nature has remarked, "a disposition to calumny is too bad a thing to be the only thing that is bad in us. It is too splendid a vice not to be accompanied with a large train of attendants." In a political view, religious prejudices are very injurious; they tend to keep alive a spirit of faction; and the evils arising from disunion among the members of a free state, are sufficiently proclaimed by history.

Lord Shaftesbury has observed, "that a public spirit can only come from a social feeling, or sense of partnership with human kind." If this assertion be well founded, it proves the necessity of being guarded against religious prejudices. For to sour the "milk of human kindness"—to break a powerful link in the chain of social feeling, which binds man to man; and to sow distrust, hatred, and all uncharitableness among human beings, is the peculiar operation of religious bigotry and prejudice!

knives mounted the scaffold, and drove away the executioner, in order that they might themselves avenge the honour of the blessed Virgin!!"

* Dr. Ogden.

EXTRAORDINARY FACTS
relating to the
Vision of Colours:
WITH OBSERVATIONS.

BY MR. JOHN DALTON.

READ OCT. 31ST, 1794.

It has been observed, that our ideas of colours, sounds, tastes, &c. excited by the same object may be very different in themselves, without our being aware of it; and that we may nevertheless converse intelligibly concerning such objects, as if we were certain the impressions made by them on our minds were exactly similar. All, indeed, that is required for this purpose, is, that the same object should uniformly make the same impression on each mind; and that objects which appear different to one should be equally so to others. It will, however, scarcely be supposed, that any two objects, which are every day before us, should appear hardly distinguishable to one person, and very different to another, without the circumstance immediately suggesting a difference in their faculties of vision; yet such is the fact, not only with regard to myself, but to many others also, as will appear in the following account.

I was always of opinion, though I might not often mention it, that several colours were injudiciously named. The term *pink*, in reference to the flower of that name, seemed proper enough; but when the term *red* was substituted for pink, I thought it highly improper; it should have been *blue*, in my apprehension, as pink and blue appear to me very nearly allied; whilst pink and red have scarcely any relation.

In the course of my application to the sciences, that of optics necessarily claimed attention; and I became pretty well acquainted with the theory of light and colours before I was apprized of any peculiarity in my vision. I had not, however, attended much to the practical discrimination of colours, owing, in some degree, to what I conceived to be a perplexity in their nomenclature. Since the year 1790, the occasional study of botany obliged me to attend more to colours than before. With respect to colours that were *white*, *yellow*, or *green*, I readily assented to the appropriate term. *Blue*, *purple*, *pink*, and *crimson* appeared rather less distinguishable; being, according to my idea, all referable to *blue*. I have often seriously asked a person whether a flower was blue or pink, but was generally considered to be in jest. Notwithstanding this, I was never convinced of a peculiarity in my vision, till I accidentally observed the colour of the flower of the *Geranium zonale* by candle-light, in the Autumn of 1792. The flower was pink, but it appeared to me almost an exact sky-blue by day; in candle-light, however, it was astonishingly changed, not having then any blue in it, but being what I called red, a colour which forms a striking contrast to blue. Not then doubting but that the change of colour would be equal to all, I requested some of my friends to observe the phænomenon; when I was surprised to find they all agreed, that the colour was not materially different from what it was by day-light, except my brother who saw it in the same light as myself. This observation clearly proved, that my vision was not like that of other persons;—and, at the same time, that the difference between day-light and candle-light, on some colours, was indefinitely more perceptible to me than to others. It was nearly two years after that time, when I entered upon an investigation of the subject, having procured the assistance of a friend, who, to his acquaintance with the theory of colours, joins a practical knowledge of their names and constitutions. I shall now proceed to state the facts ascertained under the three following heads:

I. An account of my own vision.

II. An account of others whose vision has been found similar to mine.

III. Observations on the probable cause of our anomalous vision.

I. OF MY OWN VISION.

It may be proper to observe, that I am short-sighted. Concave glasses of about five inches focus suit me best. I can see distinctly at a proper distance; and am seldom hurt by too much or too little light; nor yet with long application.

My observations began with the solar *spectrum*, or coloured image of the sun, exhibited in a dark room by means of a glass prism. I found that persons in general distinguish six kinds of colour in the solar image; namely, *red*, *orange*, *yellow*, *green*, *blue*, and *purple*. Newton, indeed, divides the purple into *indigo* and *violet*; but the difference between him and others is merely nominal. To me it is quite otherwise:—I see only *two* or at most *three* distinctions. These I should call *yellow* and *blue*; or *yellow*, *blue*, and *purple*. My yellow comprehends the *red*, *orange*, *yellow*, and *green* of others; and my *blue* and *purple* coincide with theirs. That part of the image which others call red, appears to me little more than a shade, or defect of light; after that the orange, yellow, and green seem *one* colour, which descends pretty uniformly from an intense to a rare yellow, making what I should call different shades of yellow. The difference between the green part and the ~~yellow~~ blue part is very striking to my eye: they seem to be strongly contrasted. That between the blue and purple is much less so. The purple appears to be blue much darkened and condensed. In viewing the flame of a candle by night through the prism, the appearances are pretty much the same, except that the red extremity of the image appears more vivid than that of the solar image.

I now proceed to state the results of my observations on the colours of bodies in general, whether natural or artificial, both by day-light and candle-light. I mostly used ribbands for the artificial colours.

RED.

(*By day-light.*)

Under this head I include *crimson*, *scarlet*, *red*, and *pink*. All crimsons appear to me to consist chiefly of dark blue; but many of them seem to have a strong tinge of dark brown. I have seen specimens of *crimson*, *claret*, and *mud*, which were very nearly alike. Crimson has a *grave* appearance, being the reverse of every shewy and splendid colour. Woollen yarn dyed crimson or dark blue is the same to me. *Pink* seems to be composed of nine parts of light blue, and one of red, or some colour which has no other effect than to make the light blue appear dull and faded a little. Pink and light blue therefore compared together, are to be distinguished no otherwise than as a splendid colour from one that has lost a little of its splendour. Besides the pinks, roses, &c. of the gardens, the following British *flora* appear to me blue; namely, *Statice Armeria*, *Trifolium pratense*, *Lychnis Flos-cuculi*, *Lychnis dioica*, and many of the *Gerania*. The colour of a florid complexion appears to me that of a dull, opake, blackish blue, upon a white ground. A solution of sulphate of iron in the tincture of galls (that is, dilute black ink) upon white paper, gives a colour much resembling that of a florid complexion. It has no resemblance of the colour of blood. *Red* and *scarlet* form a genus with me totally different from pink. My idea of red I obtain from *vermilion, minium, sealing wax, wafers, a soldier's uniform,* &c. These seem to have no blue whatever in them. Scarlet has a more splendid appearance than red. Blood appears to me red; but it differs much from the articles mentioned above. It is much more dull, and to me is not unlike that colour called *bottle-green.* Stockings spotted with blood or with dirt would scarcely be distinguishable.

RED.

(By candle-light.)

Red and scarlet appear much more vivid than by day. Crimson loses its blue and becomes yellowish red. Pink is by far the most changed; indeed it forms an excellent contrast to what it is by day. No blue now appears; yellow has taken its place. Pink by candle-light seems to be three parts yellow and one red, or a reddish yellow. The blue, however, is less mixed by day than the yellow by night. Red, and particularly scarlet, is a superb colour by candle-light; but by day some reds are the least shewy imaginable: I should call them dark drabs.

ORANGE & YELLOW.

(By day-light and candle-light.)

I do not find that I differ materially from other persons in regard to these colours. I have sometimes seen persons hesitate whether a thing was white or yellow by candle-light, when to me there was no doubt at all.

GREEN.

(By day-light.)

I take my standard idea from grass. This apears to me very little different from red. The face of a laurel-leaf *(Prunus Lauro-cerasus)* is a good match to a stick of red sealing-wax; and the back of the leaf answers to the lighter red of wafers. Hence it will be immediately concluded, that I see either red or green, or both, different from other people. The fact is, I believe that they both appear different to me from what they do to others. Green and orange have much affinity also. Apple green is the most pleasing kind to me; and any other that has a tinge of yellow appears to advantage. I can distinguish the different vegetable greens one from another as well as most people; and those which are nearly alike or very unlike to others are so to me. A decoction of bohea tea, a solution of liver of sulphur, ale, &c. &c. which others call brown, appear to me green. Green woollen cloth, such as is used to cover tables, appears to me a dull, dark, brownish red colour. A mixture of two parts mud and one red would come near it. It resembles a red soil just turned up by the plough. When this kind of cloth loses its colour, as other people say, and turns yellow, then it appears to me a pleasant green. Very light green paper, silk, &c. is white to me.

GREEN.

(By candle-light.)

I agree with others, that it is difficult to distinguish greens from blues by candle-light; but, with me, the greens only are altered and made to approach the blues. It is the real greens only that are altered in my eye; and not such as I confound with them by day-light, as the brown liquids abovementioned, which are not at all tinged with blue by candle-light, but are the same as by day, except that they are paler.

BLUE.

(By day-light and candle-light.)

I apprehend this colour appears very nearly the same to me as to other people, both by day-light and candle-light.

PURPLE.

(By day-light and candle-light.)

This seems to me a slight modification of blue.

I seldom fail to distinguish purple from blue; but should hardly suspect purple to be a compound of blue and red. The difference between day-light and candle-light is not material.

MISCELLANEOUS OBSERVATIONS.

Colours appear to me much the same by moon-light as they do by candle-light.*

Colours viewed by lightning appear the same as by day-light; but whether exactly so, I have not ascertained.

Colours seen by electric light appear to me the same as by day-light. That is, pink appears blue, &c.

Colours viewed through a transparent sky-blue liquid, by candle-light, appear to me as well as to others the same as by day-light.

Most of the colours called drabs appear to me the same by day-light and candle-light.

A light drab woollen cloth seems to me to resemble a light green by day. These colours are, however, easily distinguished by candle-light, as the latter becomes tinged with blue, which the former does not. I have frequently seen colours of the drab kind, said to be nearly alike, which appeared to me very different.

* Mr. Boyle observed colours by moon-light to differ from those by day-light. Priestley on Vision, p. 145.

My idea of *brown* I obtain from a piece of white paper heated almost to ignition. This colour by day-light seems to have a great affinity to green, as may be imagined from what I have said of greens. Browns seem to me very diversified; some I should call red:—dark brown woollen cloth I should call black.

The light of the rising or setting sun has no particular effect; neither has a strong or weak light. Pink appears rather duller, all other circumstances alike, in a cloudy day.

All common combustible substances exhibit colours to me in the same light; namely, *tallow*, *oil*, *wax*, *pit-coal*.

My vision has always been as it is now.

II. AN ACCOUNT OF OTHERS WHOSE VISION HAS BEEN FOUND SIMILAR TO MINE.

It has been already observed that my brother perceived the change in the colour of the geranium such as myself. Since that time having made a great number of observations on colours, by comparing their similarities, &c. by day-light and candle-light, in conjunction with him, I find that we see as nearly alike as any other persons do. He is shorter sighted than myself.

As soon as these facts were ascertained, I conceived the design of laying our case of vision before the public, apprehending it to be a singular one. I remembered, indeed, to have read in the Philosophical Transactions for 1777, an account of *Mr. Harris* of Maryport in Cumberland,* who, it was said, "could not distinguish colours;" but his case appeared to be different from ours. Considering, however, that one anomaly in vision may tend to illustrate another, I reperused the account; when it appeared extremely probable that if his vision had been fully investigated, and a relation of it given in the first person, he would have agreed with me. There were four brothers in the same predicament, one of whom is now living. Having an acquaintance in Maryport, I solicited him to propose a few queries to the survivor, which he readily did (in conjunction with another brother, whose vision has nothing peculiar), and from the answers transmitted to me, I could no longer doubt of the similarity of our cases. To render it still more circumstantial, I sent about twenty specimens of different coloured ribbands, with directions to make observations upon them by day-light and candle-light: the result was exactly conformable to my expectation.

It then appeared to me probable, that a considerable number of individuals might be found whose

* A translation of this account, to which is annexed the extraordinary case of M. Colardeau, is inserted in Rozier: *Observations sur la Physique, &c.* p. 87. E. H.

vision differed from that of the generality, but at the same time agreed with my own. Accordingly I have since taken every opportunity to explain the circumstances amongst my acquaintance, and have found several in the same predicament. Only one or two I have heard of who differ from the generality and from us also. It is remarkable that, out of twenty-five pupils I once had, to whom I explained this subject, two were found to agree with me; and, on another similar occasion, one. Like myself, they could see no material difference betwixt pink and light blue by day, but a striking contrast by candle-light. And, on a fuller investigation, I could not perceive they differed from me materially in other colours. They, like all the rest of us, were not aware of their actually seeing colours different from other people; but imagined there was great perplexity in the *names* ascribed to particular colours. I think I have been informed already of nearly twenty persons whose vision is like mine. The family at Maryport consisted of six sons and one daughter; four of the sons were in the predicament in question. Our family consisted of three sons and one daughter who arrived at maturity; of whom two sons are circumstanced as I have described. The others are mostly individuals in families, some of which are numerous. I do not find that the parents or children in any of the instances have been so, unless in one case. Nor have I been able to discover any physical cause whatever for it. Our vision, except as to colours, is as clear and distinct as that of other persons. Only two or three are short sighted. It is remarkable that I have not heard of one female subject to this peculiarity.

From a great variety of observations made with many of the abovementioned persons, it does not appear to me that we differ more from one another than persons in general do. We certainly agree in the principal facts which characterize our vision, and which I have attempted to point out below. It is but justice to observe here, that several of the resemblances and comparisons mentioned in the preceding part of this paper were first suggested to me by one or other of the parties, and found to accord with my own ideas.

CHARACTERISTIC FACTS OF OUR VISION.

1. In the solar spectrum three colours appear, yellow, blue, and purple. The two former make a contrast; the two latter seem to differ more in degree than in kind.

2. *Pink* appears, by day-light, to be sky-blue a little faded; by candle-light it assumes an orange or yellowish appearance, which forms a strong contrast to blue.

3. *Crimson* appears a muddy blue by day; and crimson woollen yarn is much the same as dark blue.

4. *Red* and *Scarlet* have a more vivid and flaming appearance by candle-light than by day-ligh

5. There is not much difference in colour between a stick of red sealing wax and grass, by day.

6. Dark green woollen cloth seems a muddy red, much darker than grass, and of a very different colour.

7. The colour of a florid complexion is dusky blue.

8. Coats, gowns, &c. appear to us frequently to be badly matched with linings, when others say they are not. On the other hand, we should match crimsons with claret or mud; pinks with light blues; browns with reds; and drabs with greens.

9. In all points where we differ from other persons, the difference is much less by candle-light than by day-light.

III. OBSERVATIONS TENDING TO POINT OUT THE CAUSE OF OUR ANOMALOUS VISION.

The first time I was enabled to form a plausible idea of the cause of our vision, was after observing that a sky-blue transparent liquid modified the light of a candle so as to make it similar to day-light; and, of course, restored to pink its proper colour by day, namely, light blue. This was an important observation. At the same time that it exhibited the effect of a transparent coloured medium in the modification of colours, it seemed to indicate the analogy of solar light to that resulting from combustion; and that the former is modified by the transparent blue atmosphere, as the latter is by the transparent blue liquid. Now the effect of a transparent coloured medium, as Mr. Delaval has proved, is to transmit more, and consequently imbibe fewer of the rays of its own colour, than of those of other colours. Reflecting upon these facts, I was led to conjecture that one of the humours of my eye must be a transparent, but *coloured*, medium, so constituted as to absorb *red* and *green* rays principally, because I obtain no proper ideas of these in the solar spectrum; and to transmit blue and other colours more perfectly. What seemed to make against this opinion however was, that I thought red bodies, such as vermilion, should appear black to me, which was contrary to fact. How this difficulty was obviated will be understood from what follows.

Newton has sufficiently ascertained, that opake bodies are of a particular colour from their reflecting the rays of light of that colour more copiously than those of the other colours; the unreflected rays being absorbed by the bodies. Adopting this fact, we are insensibly led to conclude, that the more rays of any one colour a body reflects, and the fewer of every other colour, the more perfect will be the colour. This conclusion, however, is certainly erroneous. Splendid coloured bodies reflect light of every colour copiously; but that of their own most so. Accordingly we find, that bodies of all colours, when placed in homogeneal light of any colour, appear of that particular colour. Hence a body that is red may appear of any other colour to an eye that does not transmit red, according as those other colours are more copiously reflected from the body, or transmitted through the humours of the eye.

It appears therefore almost beyond a doubt, that one of the humours of my eye, and of the eyes of my fellows, is a *coloured* medium, probably some modification of blue. I suppose it must be the vitreous humour; otherwise I apprehend it might be discovered by inspection, which has not been done. It is the province of physiologists to explain in what manner the humours of the eye may be coloured, and to them I shall leave it; and proceed to shew that the hypothesis will explain the facts stated in the conclusion of the second part.

1. This needs no further illustration.

2. *Pink* is known to be a mixture of red and blue; that is, these two colours are reflected in excess. Our eyes only transmit the blue excess, which causes it to appear blue; a few red rays pervading the eye may serve to give the colour that faded appearance. In candle-light, red and orange, or some other of the higher colours, are known to abound more proportionably than in day-light. The orange light reflected may therefore exceed the blue, and the compound colour consist of red and orange.

Now, the red being most copiously reflected, the colour will be recognized by a common eye under this small modification; but the red not appearing to us, we see chiefly the orange excess: it is consequently to us not a modification but a new colour.

3. By a similar method of reasoning, *crimson*, being compounded of red and dark blue, must assume the appearances I have described.

4. Bodies that are red and scarlet probably reflect orange and yellow in greatest plenty, next after red. The orange and yellow, mixed with a few red rays, will give us our idea of red, which is heightened by candle-light, because the orange is then more abundant.

5. Grass-green is probably compounded of green, yellow, and orange, with more or less blue. Our idea of it will then be obtained principally from the yellow and orange mixed with a few green rays. It appears, therefore, that red and green to us will be nearly alike. I do not, however, understand, why the greens should assume a bluish appearance to us and to every body else, by candle-light, when it should seem that candle-light is deficient in blue.

6. The green rays not being perceived by us, the remaining rays may, for aught that is known, compound a muddy red.

7. The observations upon the phænomena of pink and crimson, will explain this fact.

8. Suppose a body to reflect red rays as the number 8, orange rays as the number 6, and blue as 5; and another body red 8, orange 6, and blue 6: then it is evident that a common eye, attending principally to the red, would see little difference in those colours; but we, who form our ideas of the colours from the orange and blue, should perceive the latter to be bluer than the former.

9. From the whole of this paper it is evident, that our eyes admit blue rays in greater proportion than those of other people; therefore when any kind of light is less abundant in blue, as is the case with candle-light compared to day-light, our eyes serve in some degree to temper that light, so as to reduce it nearly to the common standard. This seems to be the reason why colours appear to us by candle-light, almost as they do to others by day-light.

I shall conclude this paper by observing, that it appears to me extremely probable, that the sun's light and candle-light, or that which we commonly obtain from combustion, are originally constituted alike; and that the earth's atmosphere is properly a *blue fluid*, and modifies the sun's light so as to occasion the commonly perceived difference.

An ENQUIRY *into the* Name *of the* Founder *of* HULN ABBEY, *Northumberland, the first in England of the Order of Carmelites: with* REMARKS *on* Dr. Ferriar's Account *of the* Monument *in the Church of that Monastery. By* ROBERT UVEDALE, *B. A. of Trinity College, Cambridge, Corresponding Member of the Literary and Philosophical Society, Manchester.*

Addressed to Dr. PERCIVAL.

READ APRIL 10TH, 1795.

Sir,

I have taken the liberty to send you some observations on Dr. Ferriar's account of the monument in Huln Abbey, published in vol. III of the Memoirs of the society over which you preside; relying on you to communicate them to the society, if they should be thought in any degree to merit such honour. Not doubting Dr. Ferriar's impartiality, I hope he will not be displeased at any thing which may seem to militate against him.

Dr. Ferriar informs us, that an ancient monument has been discovered in the church of Huln Abbey, which, he supposes, was intended to commemorate the founder of that monastery; and farther observes, that, "in the old plan of the abbey, first published by Mr. Grose, it is marked as the founder's tomb." He gives a circumstantial account of the founding of the abbey; and apprehends, that the title of founder could only belong to William de Vescy.

Nevertheless, as it seems yet undecided who was the founder (a point on which some of the best antiquaries disagree) this memoir will be, not improperly, divided into two parts: the FIRST containing

AN ENQUIRY INTO THE NAME OF THE FOUNDER OF HULN ABBEY.

Huln in Northumberland is generally supposed to have been the first monastery in this kingdom of the Carmelites:* an order of mendicant friars, deriving their name and origin from Mount Carmel in Syria.

Camden is of opinion, that John, Lord Vescy, founded Huln Abbey: *Joannes autem Vescy, e bello sacro rediens, Carmelitas secum primus in Angliam adduxit; illisque hic, in Holne solitudine non dissimili Carmelo Monti in Syria, conventum extruxit.*†

* Leland de Script. Britan. p. 293. Stevens, II. 157, 158.

† Britannia, p. 669, edit. 1607. According to Sir William Dugdale (Warwickshire, p. 117), John de Vescy, of Alnwick, brought the Carmelites into England; and built

On which Bishop Gibson observes, that there never was any convent or monastery founded at Alnwick or near it by John Vescy; and that the first convent of Carmelites was founded at Huln, near Alnwick, by Ralph Fresburn*. The editor of the last edition of the Britannia seems likewise to think, that Huln Abbey was founded by Fresburn. †

You will readily allow, sir, that in endeavouring to illustrate obscure remains of antiquity, every circumstance should be minutely as well as deliberately examined; and that reason and truth ought, as much as possible, to predominate over partiality and prejudice. Had this been altogether the case in the instance before us, Fresburn would never have been

for them the Monastery at Huln, on his return from the Holy Land, 1250, 34 Hen. III. Here it is observable, that Dugdale differs from Leland and most other authors, who assign 1240 as the year in which that abbey was founded. Dugdale refers to Matt. Westm. who, I find, only says, "Ordines multiplicabantur in Anglia, præter ordines prædicatorum et minorum, videlicet fratres de Monte Carmeli." p. 348, in an. 1250. By which we are not to understand, that the Carmelites came into England in the year 1250; for M. Westm. mentions also the Augustines *et multi alii*. And that the Augustines, with many other orders of friars, all came into this country A. D. 1250, is not very credible, even if such a supposition had not been contradictory to the best historical information.

* Gibson's Camden, II. 1094. 2d edit.

† Gough's Camden, III. 258.

esteemed the founder of Huln Abbey; or, at least, the learned antiquaries, who assert that he was the founder, would have expressed their doubts as to the circumstance.

With all proper deference then towards those who differ from me in opinion, I shall venture to affirm, that Fresburn has no just claim to the appellation of founder of Huln monastery.

First: Because Bishop Gibson and others have asserted Fresburn to have been the founder merely on the authority of John Bale.

Secondly: Because, from Leland and Camden and other authorities, it would appear that John de Vescy was the founder.

I. The account of the founding of Huln abbey in Gibson, Fuller, &c. and a considerable part of Mr. Grose's account, are wholly built on the authority of John Bale. Mr. Grose adds, that Fresburn erected the buildings himself:* a circumstance of which Bale, &c. makes not the least mention. But errors will multiply if suffered to take root.

Let us hear what Bale himself says. In his fourth century he tells us, that Fresburn laid the first foundation in this kingdom of the order of Carmelites, A. D. 1240; and that he died 1254.† In support

* Antiq. vol. III.

† Gibson, Grose, &c. say 1274; but Leland *De obitu ejus recte computare non possum.*

of this account, he cites Mantuanus.* But it seems Bale has, in great measure, "huddled up the centuries of English writers from Leland; and, with most prodigious slanders, has defiled the truth of chronology, and of many histories received from that diligent antiquary."†

Leland's account is in many respects different: he expressly says, that John Vescy founded Huln Abbey;‡ and he mentions Fresburn merely because *scripsit non contemnenda, ut fama est, opuscula.*|| Fresburn and some other Englishmen lived at Mount Carmel; and John Vescy and Richard Grey finding them there *præsidem loci enixissime rogabant, ut liceret illos abducere; tandemque exorabant, sed non alio nomine, quam ut fundamenta tam claræ religionis in Anglia jacerent.*§

Amongst the Englishmen thus brought over into England, Fresburn was placed at the head of the society at Huln, and Radulph Ivo at the head of Grey's foundation at Ailsford: but neither Fresburn nor Ivo were the *founders* of those monasteries.

* Bapt. Mantuanus wrote 1498. He is quoted by Leland.

† Stevens, II. 158.

‡ Collect. I. p. 103.

|| Com. de Script. Brit. p. 292.

§ Com. &c. p. 292.

II. 1. With respect to John de Vescy, the testimony of Camden and of Leland has been given before: that he was in early times considered as the founder is, I think, clear from Pat. 4, Ed. 2, p. 1, m. 3, pro confirmatione donationum *Joannis de Vesci* et aliorum. And Bishop Tanner refers us to MSS. Bibl. Bodl. Oxon. Dodsworth, vol. XIV, f. 15, excerpta e Cartulario Carmelitarum de Alnwyke.*

2. But Dr. Ferriar is of opinion, that "the title of founder could only belong to *William* de Vescy." I apprehend Dr. Ferriar asserts this on the authority of Mr. Grose.

William de Vescy certainly lived at the time Huln Monastery was founded; but still he has no just claim to the title of founder. By the *charter* of John Lord Vescy we find, that the said John did *grant* to the White Friars, all the buildings, &c. which William de Vescy his father permitted them to inhabit. Hence, it should seem, that John de Vescy brought those friars from the Holy Land; that, at his intercession, they were permitted to inhabit Huln Abbey; and that he afterwards granted, &c. And therefore, to *John* de Vescy the appellation of founder properly belongs.

The same disposition which induced John de Vescy to bring the Carmelites into England, and to

* Not. Monast. p. 398.

build them a monastery, is observable in other instances; he being known to have *brought over* a great number of Gascoignes to serve King Edward in his Welsh wars,* and to have given to the monks of Rufford, Nottinghamshire, the whole lordship of Roderham, &c.† He seems to have made two pilgrimages to the Holy Land—one, about the year 1240; the other, after he had been taken prisoner at the battle of Evesham; and had been, by *Dictum de Kenilworth*, admitted to composition.‡

I now proceed, sir, to the SECOND part of this memoir, which will consist of

REMARKS ON DR. FERRIAR'S ACCOUNT OF THE MONUMENT IN THE CHURCH OF HULN ABBEY.

There are, it seems, armorial bearings about the monument; viz. a bend; a chevron, &c. Dr. Ferriar infers, that the bend is the ancient arms of Vescy; but in regard to the chevron, he says, "To whom the shield, charged with a chevron on the left, belonged, I have attempted in vain to determine."

I. As to the bend, Mr. Grose's supposition seems

* Dugdale's Baronage, vol. 1. p. 94.
† Dugd. *loc. cit.* & Monast. Anglic. 1, 849.
‡ H. Knyghton, 2438, n. 30.

the most probable: namely, that it is the arms of Tyson, proprietor of Alnwick castle in the Saxon times.* For a shield charged with a bend is placed, *first*, among the armorial bearings sculptured on the towers of Alnwick; and, *immediately after*, is placed the shield of Vescy.

That the original arms of the Vescy family were *quarterly, or and gules*, admits not of a doubt; for, in Pine's plate of King John's Great Charter, are the arms of the twenty-five barons (who were to decide any dispute between the king and his subjects), as preserved in Coll. Armor. and among them, quarterly, or and gules, *Eustace de Vescy.* But these arms were changed by William, son of Eustace, into *gules, a cross argent*, as Camden hath observed. —*Eustachii ex Beatrice filius Gulielmus, e materno utero cæsus, Vescy nomen sibi assumpsit, et insignia, videlicet crucem argenteam in rubeo scuto.*† And the William de Vescy, who was famous for his exploits in Ireland, again changed the arms of the family, *in auream parmam, cum nigra cruce.*‡

II. With respect to another device upon the monument, something like a catharine wheel, Dr. Ferriar supposes it may allude to Vescy's travels; and quotes Gerard Leigh to shew that the wheel

* Antiq. vol. III.

† Britannia, p. 588. edit. 1607.

‡ Britannia, *loc. cit.*

"is proper to the most honourable persons only." But that author says nothing farther, than that some kings having had the catharine wheel for arms, therefore "the bearer honoureth the thing that is borne."*

Perhaps what Dr. Ferriar thinks a catharine wheel, is merely an arbitrary embellishment; such as is the device upon the chanfrin fig. 7 and 8, pl. xxiv, of Grose's Ancient Armour.

III. Having shewn that the bend was never the arms of the family of Vescy, I shall endeavour to determine to whom the chevron probably belongs.

It may not be unnecessary to premise, that, from the manner in which the monument is executed, it can by no means be reasonably supposed of much higher antiquity than the reign of Henry the third.

Though the arms about the monument did never appertain to the Vescy family, it is probable that the person to whom the chevron belonged was possessor of Alnwick castle; and that the bend was either intended to allude to the castle, of which *Tyson* was the first possessor; or to express that the deceased held it on forfeiture of the descendants of Tyson. †

* Accedens of Armorye, fol. 102.

† In Mr. Foxlow's Horn, the coat of Ferrers is impaled with that of Lancaster: "because (says Mr. Pegge), it signifies and expresses to us the title by which the house of Lancaster, proprietors of the honour of Tutbury, came by that honour, namely, by the forfeiture of Robert de Ferrers, Earl of Derby, temp. Henry III, on which occasion

The sword on the monument seems to indicate, that the person interred was of the degree of a knight;* and as to the horn, may it not denote the barony or honour of Alnwick?—possibly many of the lands within the honour of Alnwick were held by cornage; for, by that service, lands were frequently holden on the borders of England; or, possibly the horn may have a relation to the Fitz-nigels, of which family the Lords of Alnwick inherited all the privileges, &c.†

Now, Dugdale in his Baronage thus speaks of William de Vescy, the son of Eustace;—"Which William being in the tuition of the earl of Salisbury, with purpose that he should marry Isabel his daughter, as he did, in 10 Hen. III. obtained livery of all his lands (the earl of Salisbury being then

the king gave the earl's estate to his second son Edmund." Archæologia, vol. III, p. 7. Ferrers preceeds the coat of Lancaster; and they are in two separate escutcheons in the church window of Merevale, Warwickshire. Dugd. Warw. p. 783.

* Compare it with the sword on the tomb-stone of Urian de St. Pere, Archæologia, vol. v, pl. 2. Gent. Mag. vol. xxxv, p. 73, and plate there.

† Dugdale's Baronage, vol. I, p. 91.

Nigel, in the reign of King Edward the Confessor, held the custody of the Forest of Bernwood, *per unum cornu, quod est charta prædictæ forestæ;* and his successors, by the name of Fitznigel, did bear for arms, argent, a fess gules between two crescents, and a *horn* vert.

deceased). So likewise of his castle at Alnwick, which then was in the hands of Everard de Tyes."*

The arms of Tyes are, argent, *a chevron* gules.† Whence, all circumstances considered, I think it extremely probable that the monument in question was intended to commemorate Everard de Tyes.‡

I find no other mention of Everard de Tyes; nor any traces of the connexion of his family with the Vescys of Alnwick, except that in 30th Hen. II, Adam de Carduis rendered an account to the Exchequer, of the land or honour of William de Vescy; viz. of the ferms of the manors belonging to the honour, of the pleas and perquisites of it, &c. and a fine made by Randolph *de Teis.*||

I am, sir,

with great respect,

your most obedient humble servant,

ROBERT UVEDALE.

LANGTON, near SPILSBY,
Feb. 24, 1795.

* Baronage, I, p. 93.

† Glover's ordinary of arms in Edmondson's Heraldry, p. 96.

‡ Query.—Whether of the same family with Henry de Tyes, who, 4 and 7 Edw. II, was summoned to be at Newcastle upon Tyne, with horse and arms, to restrain the hostilities of the Scots; and who, having been summoned to parliament among the barons, from 28 Edw. I till 14 Edw. II, at length suffered death for engaging in an insurrec-

THE PEDIGREE OF THE VESCY FAMILY, here annexed, may, it is apprehended, contribute to render some passages in the foregoing dissertation more perfectly understood.

Eustace, a Norman. | William Tyson, Lord of Alnwick.

John Monoculus. | Ivo de Vescy, a Norman ‿ Alda.

Agnes, sister and heiress of William Fitznigel ‿ Eustace Fitzjohn, died A. D. 1157 ‿ Beatrice.

William de Vescy, d. 1185.

Eustace de Vescy, d. 1216.

William de Vescy, 1253.

John, Lord de Vescy, summoned to parliament among the barons, A. D. 1264; died 1289. | William.

tion? The name of that family is very variously written —de Tieès (Camden), Tyeys (H. Knyghton), Teyes (Sandford), le Tyes (Dugdale), Tyes, Tyeis, de Tyeys, de Tyes, &c. (Summons to Parliament).

|| Mag. Rot. 30 Hen. II. Rot. II. b. Madox's Baronia Anglica, p. 74.

On the Variety of Voices.

BY MR. JOHN GOUGH.

Communicated by DR. HOLME.

READ JAN. 8, 1796.

The variety of voices is perhaps as great as the variety of features: and, like the countenance, it serves as a personal distinction, to which all men have recourse under certain circumstances; and those that are deprived of sight, by cultivating a more delicate sense of the modification of sound under consideration, acquire a facility in discriminating between man and man, in their intercourse with the world. This wonderful diversity* does not stand in need of a formal proof of its existence to be admitted as true; for no one who can hear is

* The property of voice, which is the subject of the present paper, does not include the hoarse croaking method of articulating, that occurs not unfrequently. This may be referred, in certain cases, to a natural or accidental imperfection in the larynx; but the defect appears to arise more commonly from an ungraceful habit of speaking, which is acquired by imitation, and confirmed by negligence. This being premised, it will be easily understood, that the tone heard in the smooth uninterrupted tenor of the voice will constitute the subject of the present enquiry.

ignorant of its effects and extent. But the cause of the great difference in the tone of the vocal organs, is but badly understood; or, to speak more properly, has perhaps never been examined in a philosophical manner: and, as it is the intention of this essay to enquire into the subject with more care and strictness, it will not be improper to begin by reviewing the commonly received notion of the nature of sound. Sound is defined* to be a sensation excited in the ear by a quick succession of aerial pulses corresponding to the vibrations of an elastic substance; for a body of this kind, upon receiving a tremulous motion, immediately communicates it to the portion of air in contact with itself; and it is, in like manner, successively propagated through the whole of the air extending from the vibrating surface to the auditory organs, by which means men acquire a notion of sound, together with the whole class of ideas depending on the sense of hearing.

The preceding definition is sufficient to account for all the phænomena of the musical scale, but it may be easily proved to be too simple to explain the present difficulty; for, were the sensations produced in the ear only modified by the cause assigned above, it is evident that the variety of voices could consist in nothing besides comparative loudness and acuteness; because the effect produced on this organ by a

* Smith's Harmonics, sect. 1.

single vibrating body being determined by the force of the pulses of air, and the celerity with which they follow each other, the only modifications that can be inferred from any conjunction of these properties are the two specified above. But every man's experience will convince him how inadequate such a combination is to elucidate the subject of the present essay; for an acquaintance is easily recognized by his speech, whether he speak vehemently or softly, in a high or low key; and the voice of two singers may be made to sound in unison, though they be in other respects very dissimilar; on which account it is certain that some circumstances, not comprised in the definition of sound, enables men to identify persons by the ear, without the assistance of the eye. The diversity of sound so remarkable in the human voice and vocal organs of animals, prevails also in sonorous bodies of almost every description; for a musician can single out, from a number of instruments of the same kind, one that is familiar to him merely by hearing them separately; and a flute will play in concert with a violin, yet their notes, considered apart, are as distinct to sense as any two things can be. The effect appearing (from the preceding appeals to common sense) to be general, must be referred to a general cause; and, as it has been already proved that the diversity in question is not produced by any modification of a simple sound, it follows that some combination of sounds must constitute the cause of that indefinite variety observable in the voices of men and the tones of sonorous bodies. The sounds which are constantly striking our ears, and with which we are alone acquainted, being proved to be compounded of simple or elementary sounds, it may be safely concluded, that the vast variety of tones which prevails in the world, is solely occasioned by an union of simple sounds differing among themselves in acuteness, which, according to what has been shewn before, is the only distinguishing character they can possess, excepting loudness. It is evident from the preceding consideration, that every natural or ordinary tone consists of what is called in harmonics, an interval of sound; which is defined,* by writers on the science, to be a quantity of a certain kind, terminated by a graver and an acuter sound. For, if tones be supposed to consist invariably of perfect simple unisons, the effect of any one aggregate on the ear would vary from that of any other in loudness and acuteness only, which being the characteristic of elementary sounds, it is clear that men are indebted to some other combination of these elements for their idea of the diversity in question. On the other hand, it is equally manifest that these intervals are not great; for, on the supposition of their being considerable, we should find no more difficulty in perceiving the terminating sounds pro-

* Smith's Harmonics, sect 1, art. 9.

duced by the vocal organs, than we do in distinguishing the same in the concords and discords of a piece of composite music. The contrary is therefore true, viz. the intervals that enter into the composition of the human voice, and the tones of sonorous bodies, are too small to have their terminating sounds accurately discriminated by the ear, but sufficiently large* to affect it with distinct sensations corresponding to their relative affections. The certainty of the last conclusion can hardly be supected when the grounds on which it stands are properly attended to. The common idea of tone has been considered in conjunction with the received definition of sound; a definition which easily explains the phænomenon abstractedly taken, but is incapable of accounting for its various modifications. But, as sound can only be modified † by sound, the three

* The truth of this assumption rests on the supposition that sounds cease to be distinct to sense before they are in perfect unison; an opinion that will be proved in the sequel of the essay. After which it must appear evident, that the required effect will take place in intervals, the ratios of whose terminating sounds do not exceed a given ratio.

† That sound can be modified by nothing but sound must be admitted as an axiom in phonics; for, if the contrary be maintained, an absurd consequence will ensue; viz. that sonorous bodies can produce in the ear sensible impressions, arising from their specific or chemical qualities. But this is a doctrine repugnant to the common theory, which

cases which alone can be supposed adequate to the effect, have been separately examined, viz. a combination of perfect unisons, and of great and small intervals. The two first of these being rejected on sufficient reasons, the last is admitted of necessity to be the true cause of the subject under examination.

The principles of the theory being now established, it is proper to say something, in the next place, on that part of the mechanism of sonorous bodies, by which the combination of elementary sounds is formed. It would be superfluous to treat this branch of the enquiry in a diffuse manner, a concise statement of the cause appearing sufficient to prove the coincidence of facts and the preceding conclusions.

The mechanism in question which is capable of producing effects so diversified to sense, though so slightly discriminated in nature, depends on a principle that is easily understood. It is purely this: if a vibratory motion be imparted to any one of a system of elastic bodies that are connected together, the same is immediately communicated in a less degree to every body of the system, whose time of vibrating

ascribes the whole effect to the force and celerity of the pulses of air striking the auditory organs, no regard being paid to the qualities in question; excepting that a greater degree of elasticity renders a body capable of sounding for a longer time than one possessed of a less degree: hence a vessel of brass is more sonorous than one of wood both in in point of loudness and duration.

agrees nearly with that of the body first put in motion. For instance, let two equal strings be stretched on a frame, with degrees of tension that are nearly equal but not perfectly so; then, if either of them be made to vibrate, the other will accompany it in so distinct a manner, that their joint tone is easily known from the sound of either of them taken singly. This plain experiment reconciles the theory to common observation, as it points out the method followed by nature in compounding ordinary tones from elementary sounds; for not only all musical instruments, but also the vocal organs of men and animals are complex machines, consisting of one particular part intended for the production of sound, which is connected with many others necessary to render the whole perfect. Now, it is evident that such of these secondary members as are nearly in unison with the principal, must participate of all its motions, forming in conjunction with it a number of simple sounds, all of them contained in a narrow interval,* which is terminated by one of the number

* A circumstance that must be known to the most superficial observer may be brought forward to corroborate what is here advanced. We frequently find ourselves at a loss to identify a voice at a distance, which is in other respects familiar. The reason of this difficulty appears to be, that the feebler sounds that enter into its composition are perceived by a person standing near the speaker, but being lost to the distant ear, the tone becomes simpler, and the hearer's judgment is perplexed.

that is graver, and one that is acuter than the rest. The relative affections of these combinations, or the mutual ratios of their constituent imperfect unisons, may be varied indefinitely even in instruments or vocal organs of the same description, from the numberless slight variations that take place of necessity in the elasticity and tension of their respective similar parts; the obvious consequence of which is, that the cycles of their joint beats or pulses will be diversified in a manner equally unlimited. Now it is very well known, that the different sensations produced by several musical intervals, arise from the comparative properties of their respective cycles: but what is proved of larger intervals will hold good in respect to smaller; and is equally applicable to their effects on the ear, which are therefore shewn to be susceptible of unlimited modifications in the common course of things. The conclusions that have already appeared in this paper, may incline the reader to imagine, that either the theory is false, or music is a very imperfect art; because, according to the preceding scheme, our ideas of symphony and harmony result from sensations excited by the use of small intervals, constantly substituted in the room of elementary sounds, of which our only knowledge seems to be that they exist, but are never perceived alone, or uncombined with others of the same kind. To this plausible objection, the following answer (grounded on observation and experience) may be given. The ear, though it judges with wonderful exactness, falls short of mathematical accuracy in its discriminations: a defect to which it is liable in common with the other senses. The truth of this proposition may be exemplified by an instance familiar to every scientific musician.*

The smoothness or agreeableness of a musical consonance depends on its simplicity, those consonances being called the simplest in which the sum of the lowest terms expressing the ratio of the single vibrations of the terminating sounds, is least; or, in case several such sums are the same, that consonance is said to be simpler than the rest, in which

* The proof here exhibited of the ear's inability to distinguish sounds, that are nearly the same or equal among themselves, is the best I am able to advance, after consulting various authors.

The accounts given by experimental philosophers relative to the least sensible interval are very discordant: Dr. Keill observes in his Anatomy (edition 3d, page 167) that a good ear can perceive the disagreement of two strings, differing only by the $\frac{1}{116}$ part of a note. M. Muschenbroek says, that a good ear can distinguish no more than forty-three tones in an octave. Mr. Atwood has recorded an experiment (at page 99 of his Treatise on Rectilinear Motion) wherein two strings appeared to be in unison with a fixed sound; the tones of which I have found, by a calculation drawn from his data, to disagree by seven tenths of a comma. Such contradictory statements prove, indeed, the existence of the interval in question; but shew, at the same time, its magnitude to be undetermined.

the least term is smallest. Whence it follows, that the smoothness of a consonance arises from the simplicity of its cycle; because the terms of the ratio of the single vibrations of the terminating sounds are proportionate to the numbers of their times of vibrating in the course of one cycle. This is the reason why the fifth major makes the best concord in the diatonic scale below the octave. But the fifth in the tempered scale being diminished by a quarter of a comma, the terms of the preceding ratio are one and the fourth root of five; consequently it has no cycle, as the ratio of incommensurable magnitudes is not that of number to number. Nevertheless this concord is used with advantage, though it would be inadmissible in music, on the supposition of the ear discriminating with mathematical precision. The truth is, an interval so tempered, consists of an infinite succession of periods, so like the simple cycles of the diatonic fifth as to supply its place, without doing much violence to the nicest sense. The various concords and discords may, for the same reason, be produced on a musical ring of bells, which can never be made perfect in the strict sense of the word; for if any one bell, in the best set, be struck separately, its note will be heard to undulate or tremble, being manifestly disturbed by cycles, which result from a slight inequality in the times of vibrations in different sections of the vessel parallel to its mouth.

A good approximation towards perfection being shewn sufficient in the present, as well as in all other cases of a similar kind, to satisfy our limited powers of perception, it will not be difficult to prove the small intervals of natural tones to be fit for all the purposes of music, being calculated to afford the most correct ideas of which the sense of hearing is susceptible. For the imperfect unisons, that unite to produce the human voice or the sound of a musical instrument, preserve the same ratio among the times of their respective vibrations, in various degrees of acuteness. This is manifest, because the identity of a given voice or instrument is discoverable by the sameness of its tone at any point of the scale to which it can arrive with ease, consequently the consonances that enter into the composition of such sounds are similar at all degrees of acuteness; or, in other words, the ratios of the imperfect unisons composing a tone, which is capable of being raised or depressed, are consonant among themselves under every possible variation.* The last consideration makes it clear, that what has been inferred from a ring of bells (namely, that their notes, though manifestly compounded, serve all the purposes of music) applies with equal, if not greater, force and certainty to those minute intervals, the cycles of which are not so easily perceived.

* Smith's Harmonics, sec. 111, art. 3.

The subject having been already considered, perhaps too circumstantially, this essay may be properly concluded by observing, that the ear of a muscian receives the compound tones of his art for elementary sounds, in the same manner that the eye of a geometer contemplates his schemes as perfect, though points and lines are represented in them by dots and strokes of ink. The existence of an error is certain in both cases; but the deviation from truth is too small to be estimated by the senses; on which account the practical parts of both sciences are sufficiently exact for our limited capacities.

On the Benefits *and* Duties *resulting from the* Institution *of* Societies *for the Advancement of* LITERATURE *and* PHILOSOPHY. *By the* REV. THOMAS GISBORNE, A. M.

Communicated by Dr. PERCIVAL.

READ FEBRUARY 19TH, 1796.

Every situation and circumstance of life brings its attendant duties. This position was recognized, and apparently in its full extent, by some of the moralists of antiquity. Cicero says expressly: *Nulla vitæ pars, neque publicis, neque privatis, neque forensibus, neque domesticis in rebus, neque si tecum agas quid, neque si cum altero contrahas, vacare officio potest.* Revelation teaches the same lesson: illustrating with new light the momentous truth; and enforcing it partly by motives unknown to the heathen world, and partly by others which, though previously discovered and avowed in a greater or a less degree, had failed of their due effect on human conduct, in consequence of neither being grounded on sufficient authority, nor supported by adequate sanctions.

If the unceasing and universal recurrence of duty be a truth clear and obvious to the understanding, resting on immoveable foundations, and involving consequences of the highest importance; it is a truth of which we ought never to lose sight. If it be a truth at all, it is a truth to be applied by every man, and under all circumstances. It is to be applied not only as affecting our behaviour in points immediately connected with our station and profession, and with the relative obligations which result from the ties of kindred and of friendship; but as equally extending to our conduct in any special undertaking in which we engage, singly or collectively, for the purpose of promoting a particular object.

It may therefore be neither unseasonable nor wholly unprofitable to enquire, what are the general duties incumbent on individuals in consequence of their associating themselves for the encouragement of Literature and Philosophy.

To consider distinctly the advantages which may be expected from such an association, and also the disadvantages by which its beneficial effect may be liable to be more or less impaired, will, perhaps, be the most perspicuous method of prosecuting the enquiry. For in the improvement of those advantages, and the counteraction of those disadvantages, the duties to which an individual subjects himself by entering into the society may be stated to consist.

It is manifest that the advantages in question, whatever they may be, cannot be attained, unless the situation where the society is established be such as to afford the aids necessary for the growth and welfare of the institution. The society cannot flourish, unless the place of its establishment, and the vicinity of that place, shall together furnish such a number of persons respectable for their talents, and attached to literary and scientific pursuits, as is of itself sufficient to uphold the institution in vigour and activity; or, at least, such a number of persons of this description as may be likely, by the effect of their characters and of their exertions, speedily to kindle in the breasts of others that love of learning and philosophy which animates themselves. Of all situations the metropolis is unquestionably the most favourable. It comprehends within its precincts a far greater assemblage of abilities and of knowledge than is to be found collected in any other part of the kingdom; and it supplies to abilities and knowledge those means of facilitating research, and of perfecting discoveries, which it would be in vain to seek for elsewhere. There the literary man, whether he devotes himself to the investigations depending on profound erudition, or to the elegant pursuits of ingenuity and taste, is all times within the reach of libraries stored with the information, ancient or modern, of which he stands in need; and in the neighbourhood of eminent persons engaged in kindred enquiries, and ready by communication of knowledge and by friendly discussion to throw light on the subject on which he is employed. The philosopher enjoys similar advantages both with respect to books, and to the assistance of others. He stands at the conflux of scientific intelligence pouring in from every quarter of the globe. He is roused to exertion by the accounts continually reaching his ears from foreign countries, and by witnessing the successes of his coadjutors at home. And he is surrounded by artificers equally prepared by quickness of invention to contrive instruments, by the aid of which he may complete new and difficult experiments; and by skilfulness of workmanship to execute mechanism which he may have planned himself. Next to the metropolis, those situations which bear the nearest resemblance to it in the circumstances recently stated are most congenial to the institutions of which we are speaking. Such, for example, are cities abounding with persons whose minds a liberal education has polished and enlarged: opulent provincial towns, in which the prosperous state of manufactures evinces the benefits of mechanical and chemical knowledge, and thus disposes men to science and observation: and even smaller towns which are fortunate enough to contain some inhabitants of distinguished talents and acquisitions; or which may serve as a central point of union to able men scattered in their neighbourhood, yet not so widely dispersed as to be in danger of failing in that regular intercourse, which is essential to the utility and to the permanence of the association. When the propriety of establishing such an association at any particular place is agitated, the view ought to be extended beyond the existing moment. The prospect of the durability and energy of the establishment is a matter which ought to be carefully and impartially considered. For more is involved in the event than the success of the individual undertaking. If the society, once instituted, should fall into speedy decay; or be doomed with laborious efforts to struggle for a lingering and unprofitable existence; the injudicious attempt will throw discredit on all similar institutions; and may ultimately prevent the foundation of societies in other places, where they might have flourished, not merely with honour to themselves, but with advantage to the community.

The benefits which may be expected to result from the establishment of a society for the promotion of Literature and Philosophy, when the situation where it is fixed is selected with discernment, are by no means small. They include, on the one hand, the light which may be reflected on general learning, on the fine arts, and on the different branches of natural and experimental philosophy, by the united efforts of the members of the society; and on the other, the various happy effects that accrue from the institution to the individual members, and directly or indirectly through the medium of those members extend their influence to others. These benefits, though in strictness of speech belonging to two separate classes, are yet so nearly allied and so firmly connected together, that it may scarcely be practicable to speak of those pertaining to one class without, at the same time, touching on observations, referring immediately to those of the other. Perspicuity however seems to require that the benefits of the latter class should be stated in the first place. And if that circumstance were insufficient to turn the balance, I know not whether the weight of the claim arising from superiority in point of importance might not also be thrown into the same scale.

Writers, who have discussed the merits of the several parts of the British constitution, have not scrupled, when describing the advantages which the existence and the powers of the two houses of parliament diffuse over the community, to rank the following very high among the number: namely, that by the admission of men respectively eminent in different lines and professions into each of these branches of the legislature, and by the importance, the animation, and the publicity of the parliamentary debates, a useful and manly direction is given to the public mind; the attention of the higher and middle ranks of society is continually invited to rational and interesting objects; and topics connected with political science and the general welfare are rendered familiar and attractive to multitudes, who have no prospect or expectation of ever being raised to the dignity of peerage, or enrolled among the popular representatives. Reasoning similar to this in principle, though differing from it as to the particulars to which it relates, may be alleged respecting societies of the description now under consideration. They bring literature and philosophy from the college and the closet into public view, into the walks of common life, into scenes which would otherwise have been merely the haunts of business or of dissipation; and subject numbers to the influence and enrich them with the treasures of learning and science, to whom little was previously known of either but the name. When once such an institution is planted in a soil congenial to its growth, the studious member, whatever be his line of research, has an additional security against the danger of employing himself in a barren or indiscriminate perusal of bulky volumes: he feels himself admonished to be judicious in the selection of the authors and of the topics on which he bestows his time and attention; to direct his thoughts into some regular and promising train, to point them to some predetermined and beneficial end; and, perhaps, to read less than formerly, that he may weigh, digest, and reflect the more. He is excited to a persevering exercise of his talents by the laudable example of his associates; and by the desire (a desire far from reprehensible when kept under the control of proper motives) to be qualified to bear his part in the discussion of the various subjects which will be submitted to the society at its periodical meetings, and to contribute his due proportion to the general stock of instruction. Similar wishes, in the mean time, are gradually felt and cherished by many who do not belong to the society: by some, who are already no strangers to learning and science; by others, to whom study of any kind has hitherto conveyed no impression but that of irksomeness. The former are affected by the institution in a manner resembling that recently stated. The latter have before their eyes an association consisting principally of neighbours of their own, and, possibly, comprehending nearly all those of their neighbours who have given strong proofs of abilities and attainments; and are struck by the respectability and weight of such an assemblage of talents and intelligence. They perceive in that association some of their own daily companions in the intercourse and occupations of common life, who appear raised above them by the only honourable distinction, that of merit, and moving in another sphere. They are conscious of a secret regret at finding themselves apparently sunk below their former level. And though they perhaps do not yet look to be admitted into that body in which their friends and acquaintances are stationed; they are at least anxious not to be under the necessity of either remaining silent in their presence, or of speaking but to expose themselves, if at incidental meetings in a private circle some topic connected with literature or science should chance to become the subject of discourse. They imagine, in short, that a broad line is now drawn between ignorance and knowledge; and they perceive themselves on the wrong side. Hence they insensibly view things with other eyes. In proportion as the value of mental improvement advances in their estimation, their repugnance to books and reflection diminishes. Persuaded of the worth of the object, they no longer refuse to employ the means necessary for its accomplishment. They begin to discern that to be proficients in the science of dress, to be connoisseurs in the dainties of the table, to run the never-ceasing round of frivolous amusements, to vie in habits of luxury and in the ostentatious display of wealth, do not render a man quite so respectable as they formerly believed: and discover ere long, at first with some surprise, but with a surprise of short continuance, that such characteristics render him deservedly contemptible. This change of sentiments is followed by its natural effects. Information, taste, a love of literature, of liberal arts, of philosophical enquiry, are qualifications which grow more and more alluring; and are counted among the most desirable ingredients in contracting intimacies and friendships. Some of the individuals, in whom the establishment of the society has wrought so fortunate an alteration, become, in process of time, both members and ornaments of the institution itself. And those who in consequence of a limit prescribed to its number, or through other circumstances, do not happen to be received within its pale, regard it with grateful esteem, as the source of those habits and attainments in themselves to which, next to the conscientious discharge of duty, they owe many of the happiest hours of their life.

The degree in which these advantages may be expected to result from the rise of literary and philosophical associations, will be in some measure regulated in each instance by incidental causes. But to a greater or less extent they are always likely to ensue. And to the beneficial consequences which have been already mentioned others of a similar nature are yet to be added. When the society is numerous, and respectable in the public eye, the tone of general conversation throughout the place and its vicinity, whether in domestic intercourse or in mixed company, becomes manifestly raised. He, who is accustomed to allot a due portion of his retirement to instructive reading and improving thought, will naturally have his discourse tinctured, when evening draws the family circle round him, with the subjects to which the studies of the morning were devoted. He, to whose mind the productions of literature and science are familiar, will be prone to enliven the tedious langour, and to enrich the vapid talkativeness of the afternoon visit by the easy and well-timed introduction of facts or observations derived from literature, philosophy, or the fine arts, such as are adapted to the occasion, and interesting without being abstruse: and will frequently be seen, perhaps unconsciously, to fix attention and communicate pleasure, while the neglected card-table stands vacant or is pushed aside. Even the noisy and the dissipated, when in company with such men, will gradually be led, partly by a sense of shame, partly by a desire of rendering themselves agreeable, partly by the influence of example, to adopt habits and sentiments better than their own. They will cease to confine their discourse to the tale of scandal of the hour, the adventures of a fox-chase, the history of a dinner, the determination of a gaming-bet; and to publish their own shame and their own guilt by detailing their feats of riot and intemperance. Folly will learn some degree of caution, and silence will appear the only refuge of vice.

Such are the advantages derived to individuals from the institutions which form the subject of the present enquiry. Other benefits which flow from them relate to the general interests of literature and science. That benefits of the latter description may reasonably be expected to follow, is a truth which the observations already made will unavoidably have suggested. For when a number of persons, antecedently addicted to literary or philosophical investigations, are stimulated, by the accession of some new motive, to additional industry in their several pursuits, and invited to direct their talents to some specific and important ends; and when perhaps an equal or larger number of men previously unaccustomed or disinclined to such investigations become habituated and attached to them; is it probable, is it possible, that literature and science can fail to profit by these events? The field, on which so much labour of cultivation could be employed without producing any fruit, must have been doomed to more than common sterility. From the encreased stock of zealous and persevering exertion, it may unquestionably be hoped that much light will be thrown on some of the various departments of literature, especially on its more popular and elegant branches. And science has perhaps reason to look forward to still greater assistance from the same cause. New mines of philosophical treasure will be opened; new discoveries will be made; new analogies traced; erroneous hypotheses exploded; rational theories corroborated; and conclusions, hitherto resting on speculative conjecture or dubious experiments, will be established on the basis of fact and demonstration. The laws of nature and the properties of bodies will be developed more and more; new processes applicable to the polite arts will be made known; and new inventions disclosed to facilitate the labours of the practical mechanic, and improve the workmanship of the manufacturer. By its publications, the society will preserve and spread abroad much important knowledge, which otherwise would never have been committed to writing. By honorary premiums, if they are comprised within its plan, or by the more honourable reward of admission among its members, it will add vigour to exertion and credit to success.

From the wise purpose of Providence (that every period of life and every plan which we undertake shall form a part of the grand scheme of moral probation) it results, that every good, attainable on the present stage of being, is liable to be accompanied by evil. Hence every human institution, from the political arrangement framed for the government of an empire to the humblest local association, of whatever benefits it may be productive, will also bring a train of attendant disadvantages. And however inferior in magnitude the latter may be to the former, they will yet be such as to render conscientious vigilance to obviate them, a strict and important duty. It is therefore material to enquire, what are the disadvantages not unlikely to be annexed to the valuable institutions now before us. I do not speak of disadvantages which may be nearly or altogether precluded by the fundamental rules of the society properly executed. If political contention and the spirit of ministerial or antiministerial attachment be suffered to embroil the periodical meeting; if local disputes and private animosities, instead of being mitigated by the concurrence of all the adverse parties in literary and philosophical pursuits, make use of the evening dedicated to those pursuits as an opportunity to vent their bitterness; if the hours assigned to rational enquiry and debate be trifled away in discourse foreign to the avowed object of the association; if the discussion of subjects corresponding to its design be disgraced by acrimony and taunts, by scurrility and invective; these are not evils attached to the nature of the institution. They result from some defect in its code of internal regulations; or from the want of care and honest steadiness in the members to enforce the observance of the existing laws. If men of depraved morals and profligate conduct be admitted into the society; the fault rests wholly with those who, from a respect to abilities and attainments, are led to receive among them persons destitute of qualifications infinitely more respectable. On evils so prominent, and so easy to be remedied, it is unnecessary to enlarge. The subsequent remarks are meant to be appropriated to such as seem occasionally to spring from sources, from which some of the benefits of the institution flow; to those failings, namely, which are sometimes found to be produced or aggravated in an individual, by the very circumstance of his being admitted into the society.

Self-conceit will commonly be observed to prevail the most in those minds, which are not distinguished by soundness of judgment, or deeply imbued with learning and science. But the most powerful understanding and the highest attainments afford no security against its influence. When once a member of the society is seized by this malady, he speedily shews symptoms of its attack; and if it be not vigorously checked in the outset, continues from time to time to manifest encreasing marks of its progress. He conceives himself an object of general attention; and is ever on the watch to strengthen the opinion of his talents and importance. His manner becomes studied, his tone dogmatical, his praises qualified, his censures peremptory. Accordingly as he aims at the character of erudition, or of taste, his conversation is pedantic, or affected. He talks that he may be admired: he reads for the purpose of display. His compositions are loaded with technical terms; or glitter with false and superabundant ornament. Whatever be the subject which he treats, his desire to shine is equally apparent and prejudicial. It pervades the researches of learning; it even infects the simplicity of philosophical demonstration. He proves himself ambitious to evince the knowledge which he possesses rather than earnest to acquire more. The information which he has accumulated, the discoveries which he makes, are degraded by the cumbrous and tawdry garb in which they are enveloped. The interests of literature have more to fear from his defects than to hope from his labours. The great stream of science rolls on beside him, and scarce accounts his turbid and frothy effusions among the number of its tributary rills. In the mean time he grows more and more proud of what he knows, more and more forgetful of the extent of his ignorance. He is athirst for compliment and applause; and seeks to magnify the credit of the society to which he belongs, in proportion as his own reputation in it is recognized. And if at length he approaches the top of the detached eminence on which he is stationed, he seems to fancy himself arrived at the pinnacle of knowledge; unobservant of the distant hills which rise higher and higher around him, and lead on to mountains concealing their summits in the clouds.

When vanity has thus taken possession of the head, it usually happens that darker passions become inmates of the heart. He who is puffed up with overweening ideas of his own merit easily proceeds, if it can indeed be called an additional step, to think contemptuously of others. The natural consequences of these dispositions are partiality, jealousy, envy, impatience of contradiction, unkind sentiments towards the person differing in opinion, and a secret desire to detract from the credit of successful fellow-labourers in the field of literature and philosophy.

The vanity by which, proportionally to the degree in which it exists, these most criminal tempers of the mind are fomented, is to be extinguished only by the active prevalence of those motives (very different from a passion for human praise) which Christianity prescribes for the regulation of the heart and conduct in every station and circumstance of life.

There yet remains one subject, on which (in treating of disadvantages not unlikely to be annexed to Literary and Philosophical institutions) it would be improper to be wholly silent. I allude to the charge not unfrequently alleged against philosophy; and against the institutions in question as encouraging philosophy:—namely, that the philosopher is sometimes found to advance in the road to infidelity in proportion as he devotes himself to scientific researches. This charge is, I trust, an imputation, which has derived an unmerited degree of credit in consequence of being unhappily supported by some particular examples, to enlarge on which would be unsuitable to the present occasion. With respect therefore to those examples I shall only observe, that if the effect of philosophical pursuits on faith in Christianity were to be estimated by the authority of the names of believing and unbelieving philosophers; the venerable Newton would be found stationed at the head of a phalanx, whose intellectual penetration and scientific attainments would in vain, I apprehend, be sought on the adverse side. No supposition however can be more unreasonable than to imagine that a sedulous enquiry into the works of God, when conducted with that frame of mind, the fitness of which in such enquiries will not be disputed; an honest desire to discover truth; an impartial investigation of evidence; humility suited to the shortsightedness of man; a willingness to renounce erroneous preconceptions; and a reverence and love for the glorious Creator, can tend to subvert the belief of a revelation, which, in order to be received, requires only to be studied with those dispositions. But if men are precipitate in judgment, self-sufficient, and presumptuous; if they investigate, not with a solicitude to establish truth, but to substantiate their own previous decisions; if, after gleaning together some facts and conjectures in natural knowledge, they deem themselves competent to pronounce on the mysteries of the universe, and I had almost said, on the duties of its Governor; if they construct high-sounding theories, and instead of trying the truth of revelation on its proper evidence, estimate it by its agreement or disagreement with those theories; if the object of their researches be to glorify not God, but themselves;—such men may easily be unbelievers, but let them not usurp the name of philosophers. If to be a philosopher, is to be lover of wisdom; he alone may hope to deserve the appellation, who cherishes the dispositions which wisdom enjoins. If to be a philosopher, is to be a lover of wisdom, he is the true philosopher, who loves the wisdom revealed from above.

The important point to be borne in mind is this: That the institution of a society for the advancement of literature and science brings with it, like every other human institution, its peculiar moral obligations: that each member of such a society has, in that capacity, as in every other, talents to employ for the glory of God and the benefit of man: that he has in that capacity, as in every other, duties to discharge and temptations to withstand: and that the degree in which he performs or disregards those duties, and encourages or resists those temptations, will compose one of the subjects of the great account to be given hereafter; and will be attended with consequences to be experienced in another stage of existence.

T. GISBORNE.

YOXALL LODGE,
January 28th, 1796.

On an UNIVERSAL CHARACTER: *in a Letter from* JAMES ANDERSON, L.L.D. F.R.S. F.A.S.S. &c. &c. *to* EDWARD HOLME, M.D.

READ NOVEMBER 4TH, 1796.

COTFIELD, near Leith, Feb. 20th, 1795.

Dear Sir,

Since I had last the pleasure of seeing you in Manchester, I have had more leisure to turn my attention towards literary investigations than for some years past. My time indeed has been *chiefly* employed in facilitating the communication between different places within land, by means of roads and canals: in regard to both which great undertakings, I find we are as yet not a great deal farther advanced than children beginning to walk. AQUEDUCT BRIDGES, which are at present erected at so enormous an expence, as materially to obstruct the extension of canals, may certainly be constructed in such a way as to cost less than the bare *carriage* of the materials will in many cases amount to. But, without explanations, these assertions must appear mere impossibilities suggested at random: and, as explanations would require more time and room than can be spared, I shall pass from this to another subject, that is more in the line of your pursuits.

Every literary person knows the difficulties to which he is subjected by reason of the multiplicity of languages, that prevail over the globe. The time that is lost, in the acquisition of these languages, will not, I think, be over rated, if it be stated at one half of the whole time that literary men can apply to study, taking the chance of deaths at an early period; and, after all, very few persons can acquire the perfect knowledge of more than three or four languages; and every other person is stopped in his enquiries the moment he extends his views in the smallest degree beyond the limits of his native tongue.

I have at last hit upon a device by which this difficulty can be totally removed; which is so perfectly simple, that it is inconceivable why it should not have been adopted many ages ago. This may be called a *new art of writing*. It is of such a nature, that two persons instructed in this art, though they use each a language that is totally unknown to the other, may correspond with each other with much more facility than I can correspond with you; and though each uses his own language in *writing*, the other *reads* it in his own language. In short, the same writing, were it shewed to a multitude who used five hundred different languages totally unknown to each other, would be equally intelligible to the whole; and every individual would read it, and express it readily in his own native tongue, provided he had been previously acquainted with this art. Nor would it be a matter of greater difficulty to learn this, than it is at present to learn to read and write one's native language.

This, you will say is a great *discovery*; and I shall not be surprised if you should think it a great *gasconade* at the same time: yet it is perhaps neither the one nor the other in the strict meaning of the words. It can lay no claim to the name of a *discovery*, if it be only the application of a principle that has been long known, to some very obvious particulars that have been hitherto disregarded. Nor will it be deemed a *gasconade*, if I can shew that every literary man is in the practice of doing this very thing every day without paying attention to it.—For example:

Supposing a stone were to fall from the clouds, with the characters 1795 delineated upon it; and that stone were to be exhibited to a convention of people, consisting of one of each of the nations of Europe, they would all read it with equal ease, and understand it perfectly. If you asked an Englishman what it was, he would answer, *one thousand seven hundred and ninety-five*, and that it denoted the present year of the Christian æra. Ask a Frenchman, he would as readily answer, *mil-sept-cent-quatre-vingt-quinze*. A Spaniard—a German—a Russ, &c. would each read it in the same manner in his own language. Here then is all that I propose to do:—it is merely to extend to words in general, what we now apply only to those words that denote *numbers.*

That this *may* be done, is obvious from the example just given. That it actually has been done for upwards of three thousand years past, by the Chinese nation, admits of the clearest proof. And that the powerful nations of Japan, Siam, and Tonquin (all of which use languages very different from each other and from the Chinese) read the writings of each other*—in short, make use of the same written characters in all respects, is likewise an undeniable fact. The discovery then is re-

* Thus it is remarked by Sir George Staunton, that a missionary, who was of his party, could not, in any degree, understand the conversation of the inhabitants of Condore; "but when the words were written, they instantly became intelligible to him. Though their colloquial language was altogether different from what is spoken in China, yet the characters were all Chinese: and the fact was clearly ascertained on this occasion, that those characters have an equal advantage with Arabic numbers, of which the figures convey the same meaning wherever known; whereas the letters of other languages denote not things but elementary sounds, which, combined variously together, form words or more complicated sounds, conveying different ideas in different languages, though the form of their alphabet be the same." Authentic Account of an Embassy to the Emperor of China, vol. 1. page 312.

E. H.

duced to no discovery at all; and is only the effect of a very moderate stretch of reasoning.

This fact, respecting the Chinese language, has been long known in Europe; but as mankind always depreciate the acquirements of others who exceed them in knowledge, so our European philosophers have been liberal in their abuse of the Chinese mode of writing; representing it as an unwieldy chaos of crude materials, which the life of a man is not sufficient to unravel. I have found only *one* European (M. de Guignes) who had made himself perfectly master of the Chinese writing, in Europe; and another (M. Freret), who had made very considerable progress in it before he died; but, so far were these men from complaining of the defects alleged against that mode of writing, that they admired it exceedingly.

Still, however, when I contemplated the subject *at a distance*, it appeared to be environed with dreadful difficulties. The number of characters that would be wanted seemed to be so great, that there would be difficulty in forming them so distinct from each other, as to be in no danger of being confounded; —and to retain all these in the mind so as to use them readily, appeared to be a still more difficult task. But provided these two difficulties were surmounted, how would it be possible to reduce such characters to the form of a dictionary of easy reference? or how could printers acquire a facility in using such a variety of types as seemed to be indispensibly necessary? I own these appeared to be absolute impossibilities; and these considerations had well nigh stopped me from going farther, as I presume has been the case with many others.

It chanced, however, that I was not quite disheartened. Upon a little investigation I found, that, on account of derivatives and compounds, the *number* of characters would be prodigiously diminished. Diminutives, augmentatives, opposites, inflections, &c. would furnish the means of an infinitely greater reduction in that respect; so that the first objection vanished in a moment.

As to the second:—Is it not as easy to recollect the meaning of an obvious distinct mark placed before the eye, as to recollect the meaning of a distinct sound pronounced, by means of the ear? Yet it is well known that this must be done, by every one who learns a foreign language, with regard to *every word* in that language; so that, if the signs were as much multiplied as the words are, they must be as easily recollected. And it is well known that it does not exceed the human powers to acquire ten or twelve languages.

As to the difficulty of devising signs, this was so very easy, that at the very first trial I made, I found, that taking a perpendicular straight stroke as the basis, thus |, it might be easily so varied, by slight but distinct marks, without a probability of one being mistaken for another in any case, as would give a greater number of characters than *all* the words in the English language; and perhaps, a *hundred* times more than can be wanted for our purpose: that there could, in fact, be no difficulty in forming a hundred millions of distinct characters, no two of which could be mistaken for each other, were it necessary: but there will not be wanted perhaps above five hundred characters for all the purposes of language.

These characters too can be formed in such an analytical way, as to be of even more easy reference in a dictionary than the alphabetical arrangement now in use; and printing might be practised with half the number of types that are now required. I was perfectly astonished at the facility with which all these things could be done; and not less pleased on contemplating the benefits that would result from this mode of writing, were it introduced into general practice.

The first advantage would be the opening of a free literary intercourse among all nations; as the writings and books of every nation would be equally intelligible to all other nations as to those to whom they originally belonged.

The second would be facilitating the art of writing—for any man could then write as fast as another can speak; and the discourse would be taken down, not as it now is (by those who write it in short-hand) by half words and mutilated sentences, liable to be mistaken; but completely and entirely, with as much accuracy as if the orator had written it word for word with his own hand.

A third advantage would be a diminution of space in writing, and a still greater diminution in printing; so that a single page might be made to contain nearly half a volume. This would greatly diminish the price of books, and consequently augment their circulation.

To these advantages I may add, that it would give a precision and accuracy of expression to written language that it never yet has attained, without necessarily affecting the spoken language of any country. But I am sensible that till it can be shewn *how* all this can be done, it is like putting down a parcel of enigmas to state them, though they will be perfectly obvious when explained. What would our forefathers, before the knowledge of the Arabic numerals, have thought of a man who should have said: that, by means of ten trifling characters, he could perform the different operations in arithmetic, which we know can be done with the utmost ease? He would have been nearly as much credited if he had said: that, by means of ten little sticks, he could make a ladder on which he could ascend to the moon.

I speak *now* with some degree of certainty on subject; for, after having committed a few thoughts to paper respecting it, I put this essay into the hands of a gentleman, who I know to be much more capable of such investigations than I am, and desired him to turn his attention to it in a particular manner, which I assured him he would find to be much less difficult than he would at first sight apprehend, and left it with him, without giving any explanation of the mode of notation that had occurred to myself. When I next met with him, about a fortnight afterwards, I found that he had entered into the subject with great ardour, and had made a progress in it much greater than I believed to be possible. He had devised a mode of notation totally different from mine; and, I believe, equally comprehensive and simple. He had formed the outlines nearly of a complete grammar, arranged with the most beautiful simplicity, and with such peculiarities as give this written language such precision above all spoken languages as must tend greatly to improve, without deranging, the spoken language of all who shall use it. I have requested him to proceed till he completes the whole: for, as he advances, he finds that the *signs* may not only be more simplified than he first thought of; but also that, by this simplification, the *ideas* may be rendered more distinct and precise. He thinks at present, that about five hundred characters are all that can be wanted for any purpose in language. These are to be varied by moveable signs denoting general ideas, which become particular when connected with individual signs.

In order to give you a slight notion of what is meant, and the manner in which it *may* be done, I send you inclosed a small schedule containing the personal pronouns only, and the manner in which they *might* be denoted, according to the mode of notation I had thought of.* In these you will observe, that this mark ^, always denotes the masculine,—this mark ˅, the feminine,—and no mark at all, the neuter gender;—that a point above the character, denotes the plural;—that this mark ' at the right hand side denotes the definitive, commonly called the genitive or possessive case;—and that the same mark with the cross in the middle (which is the sign of the accusative case) denotes the possessive, properly so called.

You will now see somewhat of what is meant by the accuracy of ideas that would be thus conveyed, in comparison of what can be done by spoken language. For instance, the plural of *he*, *she*, and *it*, are all equally *they*; but it is certain that in language, to speak with precision, there is as much want of a plural for the different genders as of a singular. A writer then, when he had occasion to discriminate these ideas would do it; and when *they* denoted males, he would write it thus -[̂]; when the same

* See the annexed Scheme, page 101.

word denoted females, it would be -▯̈; and when neuters, simply -▯̇, without gender. A reader who saw these characters would understand them perfectly, though in the English language he must read them all alike *they*; and the same in regard to all its derivatives, *them*, *their*, and *theirs*. In like manner, the words that are in italics in the schedule (all of which are deficient in our language, and must have their place supplied by other words forced from their natural meaning, by means of the discriminative characters) have their precise meaning ascertained with the most perfect accuracy, though we are compelled to express ourselves in the same inaccurate manner as formerly in speaking. In this way of proceeding, it is inconceivable what a number of inaccuracies would be discovered and corrected in every language. For, if the work be carried on in the manner it is begun, all this will be done, not only without difficulty, but the characters will be formed even with greater ease than if these anomalies were not to be at all corrected. Thus will the character become universal, so that no language will ever miss its own excellencies in it, though those who use it must put up with its deficiences, and supply them in the best way they can in reading.

I shall give another example.—The Greek and some other modern languages have a definite plural of two, called the dual number; I see no reason why we might not have a plural of three, or even more. To denote that dual plural, instead of the usual plural ǀ, let it be put ˙ǀ˙ ; and if a plural of three were wanted, ·ǀ˙·. Wherever a writer wishes to discriminate this idea he has it in his power; and, though I cannot put it into English, it can be just as well understood as if I were reading the Greek dual number.

I have carried the distinctions of gender above no farther than the English language admits of; but the character needs by no means to be so restricted, as there would be no difficulty in making as many discriminations in that respect, as I have marked in the *Essay on Pronouns*, in the 11th vol. of the BEE. But I have already, I fear, tired you, with this partly unintelligible letter; and shall not proceed farther than barely to assure you, that I am convinced, if the gentleman who has begun this investigation can be induced to continue it till he completes a grammar and a dictionary (which I am persuaded he will do, if he meet with proper encouragement) this will prove to be, if not one of the greatest *discoveries*, at least one of the most useful literary *improvements* of the present age. I thought you would be well pleased to hear of the first beginnings of an undertaking that may prove so extensively useful to society; and remain, with much esteem,

sir,

your most humble servant,

JAMES ANDERSON.

PERSONAL PRONOUNS, WITH THEIR DERIVATIVES.

	Of the first person.		Of the second person.	
Nominative.	I	WE	THOU	YE
Accusative.	ME	US	THEE	YOU
Definitive.	MY	OUR	THY	YOUR
Possessive.	MINE	OURS	THINE	YOURS

Of the third person.

	Masculine.		Feminine.		Neuter.	
Nominative.	HE	*they*	SHE	*they*	IT	*they*
Accusative.	HIM	*them*	*her*	*them*	*it*	*them*
Definitive.	*his*	*their*	HER	*their*	*it*	*their*
Possessive.	HIS	*theirs*	HERS	*theirs*	ITS	*theirs*

The Inverse Method of Central Forces.

Communicated by Dr. HOLME.

The following Scholium and Proposition are here introduced by way of *Addenda* to the paper on *the Inverse Method of Central Forces*, given vol. iv. page 369, of these MEMOIRS, with which they have an intimate connection; as the proposition leads, in a different manner, to a conclusion given in the 9th section of the PRINCIPIA, and as both may yield some amusement to those who are versed in these abstract enquiries.

SCHOLIUM TO PROP. III.

If the force which varies as the q power of the distance reciprocally, act *from* the centre; or, which is the same thing, c be negative, then $p^2 =$

$$\frac{m\,P^2\,y^{n-1}}{r^{n-1} - c \times \frac{n-1}{q-1} \times r^{q-1}\,y^{n-q} + \overline{m-1+c \times \frac{n-n}{q-1}} \times \overline{y^{n-1}}}.$$

At the distance y from the centre, the two forces become $\frac{r^n}{y^n}$, and $\frac{cr^q}{y^q}$ which being made e-qual to each other, the value of y will be determined, where the curve will have a point of contrary flexure, viz. $y = \frac{1}{c^{\frac{1}{n-q}}} \times r = c^{\frac{1}{q-n}} \times r$; which will be greater or less than r, according as n is greater or less than q; c being less than unity. Substitute this value of y in the above equation, and, at the same time, suppose $y = p$; then the point of contrary flexure will be at an apse: therefore a similar and equal curve will be described on the other side the apse that was described before: but this is impossible, as the curve is now convex towards the centre of force; wherefore the body, upon this supposition, can *never* arrive at an apse, but will continually approach nearer and nearer to a circle described at the distance y determined above; which circle will therefore be an asymptote to the orbit, or spiral described. By substitution

$$c^{\frac{2}{q-n}} \times r^2 = \frac{m P^2 c^{\frac{n-1}{q-n}} r^{n-1}}{r^{n-1} - c \times \frac{n-1}{q-1} r^{q-1} \times c^{\frac{n-q}{q-n}} \times r^{n-q} + \left(m - 1 + c . \frac{n-1}{q-1}\right) \times c^{\frac{n-1}{q-n}} \times r^{n-1}} =$$

$$\frac{m P^2 c^{\frac{n-1}{q-n}}}{\left(m - 1 + c \times \frac{n-1}{q-1}\right) \times c^{\frac{n-1}{q-n}} - \frac{n-q}{q-1}}, \text{ or } r^2 =$$

$$\frac{m P^2 c^{\frac{n-3}{q-n}}}{\left(m - 1 + c . \frac{n-1}{q-1}\right) \times c^{\frac{n-1}{q-n}} - \frac{n-q}{q-1}}.$$ But $m =$

$\frac{s^2 \times \overline{n-1}\,.\,\overline{1-c}}{2}$; whence, by substitution and reduction, $s^2 = \frac{2\,r^2 \times \left(c^{\frac{n-1}{q-n}} \times \overline{1-c \times \frac{n-1}{q-1} + \frac{n-q}{q-1}}\right)}{\overline{n-1}\,.\,\overline{1-c} \times \left(r^2\, c^{\frac{n-1}{q-n}} - P^2 \times c^{\frac{n-3}{q-n}}\right)}$

Hence the velocity is determined with which a body must be projected, so as never to arrive at an apse, nor ascend beyond, or descend to a circle, described at the distance $c^{\frac{1}{q-n}} \times r$ from the centre of force.

If $n = 2$, $q = -1$, and $P = r$, then $s^2 =$

$$\frac{2 + c - 3\,c^{\frac{1}{3}}}{\overline{1-c} \times \overline{1 - c^{\frac{2}{3}}}} = \frac{2 + c - 3\,c^{\frac{1}{3}}}{\overline{1-c} \times \overline{1 + c^{\frac{1}{3}}} \times \overline{1 - c^{\frac{1}{3}}}}$$

Wherefore, if the velocity were known with which a body (as the moon) would move in a circle round another (as the earth) at the distance r of the lower apse, with the compound force $1 - c$; then the velocity is determined with which it must be projected from that apse, to move in the manner described in this Scholium; or in such a manner as never to arrive at another in any finite number of revolutions.

Hence it will easily appear, that a *very small* variation in the value of s, will occasion a very large one in the excentricity of the orbit, and in the motion of the apsides: it is therefore evident that the two last will increase or decrease at the same time.

PROP. V.

If a body revolve round a centre, and be acted upon by a force tending to that centre which varies as the *n*th power of the distance inversely, and whose quantity at an apse, the distance of which apse being r, is $= 1$: likewise; if another body, besides being subject to this force, be acted upon by an additional one, which varies inversely as the *q*th power of the distance, its value being c at the same distance r; it is required to investigate a general expression for the ratio of the angular velocities of the two bodies when at the *same distance* from the common centre of force.

Let the required ratio be that of $F : G$. Put y = common distance, p = perpendicular upon the tangent to the orbit described by the single force, and p' = perpendicular upon the tangent to the other orbit, drawn from the common centre.

It is evident by Prop. 1st, that $F^2 : G^2 ::$

$$\frac{p^2 y^2}{\overline{y^2 - p^2} \times y^2} : \frac{p'^2 y^2}{\overline{y^2 - p'^2} \times y^2} :: \frac{p^2}{y^2 - p^2} : \frac{p'^2}{y^2 - p'^2}.$$

But (by Prop. 1st and 3d) $p^2 = \dfrac{\dfrac{m}{1-m} \times r^2 y^{n-1}}{\dfrac{r^{n-1}}{1-m} - y^{n-1}}$, and $p^2 = \dfrac{M r^2 y^{n-1}}{r^{n-1} + c \times \frac{n-1}{q-1} r^{q-1} y^{n-q} + \overline{M - 1 - c \times \frac{n-1}{q-1}} \times y^{n-1}}$, writing M here instead of m in Prop. 3d. Hence, by substitution and reduction,

$$F^2 : G^2 :: \frac{m}{\overline{m-1} \cdot y^2 - m r^2 + \not{m}\, r^{n-1} y^{3-n}}$$

$$: \frac{M}{\overline{M - 1 - c \cdot \frac{n-1}{q-1}} \times y^2 + c \cdot \frac{n-1}{q-1} \cdot r^{q-1} y^{3-q} + r^{n-1} y^{3-n} - m r^2}.$$

But $m = \frac{n-1}{2} \times s^2$ and $M = \frac{\overline{n-1} \cdot \overline{1+c}}{2} \cdot S^2$ (writing S, for s, in Prop. 3d), therefore $F^2 : G^2 ::$

$$\frac{s^2}{(n-1 \cdot s^2 - 2) \times y^2 - \overline{n-1} \cdot s^2 r^2 + 2 r^{n-1} y^{3-n}}$$

$$: \frac{\overline{1+c} \cdot S^2}{\left(\overline{n-1} \cdot \overline{1+c} \cdot S^2 - 2 - 2c \cdot \frac{n-1}{q-1}\right) y^2 + 2c \cdot \frac{n-1}{q-1} r^{q-1} y^{3-q} + 2 r^{n-1} y^{3-n} - \overline{n-1} \cdot \overline{1+c} \cdot S^2 r^2},$$

which is general, whatever be the values of n and q, s and S.

Cor. 1. If $q = 3$, then $F^2 : G^2 ::$

$$\frac{s^2}{\overline{\overline{n-1} \cdot s^2 - 2} \cdot y^2 - \overline{n-1} \cdot s^2 r^2 + 2 r^{n-1} y^{3-n}}$$

$$:\ \frac{\overline{1+c}\cdot S^2}{(\overline{n-1}\cdot\overline{1+c}\cdot S^2-\overline{2-c}\cdot\overline{n-1})\,y^2-(\overline{n-1}\cdot\overline{1+c}\cdot S^2-c\cdot\overline{n-1})\,r^2+2\,r^{n-1}\,y^{3-n}};$$

where it is evident that if we make $\overline{n-1}\cdot\overline{1+c}\cdot S^2-c\cdot\overline{n-1}=\overline{n-1}\cdot s^2$, then the denominators will be equal, and therefore $F^2 : G^2 :: s^2 : \overline{1+c}\cdot S^2$, which is a constant ratio; and therefore when $q=3$, or when the additional force varies as the cube of the distance reciprocally, the angles, which lines drawn from the bodies to the centre of force, make with the line passing through the apse, are in a constant ratio to each other, whatever be their common distance (the condition mentioned being observed).

Cor. 2. Because $\overline{n-1}\cdot\overline{1+c}\cdot S^2-c\cdot\overline{n-1}=\overline{n-1}\cdot s^2$, therefore $S^2=\frac{s^2+c}{1+c}$. Let $R=$ rad. of curvature and $v=$ velocity at the apse in the orbit described by one force; then the centripetal force at the distance r, $=\frac{v^2}{R}=\frac{v^2}{s^2 r}$, therefore $s^2=\frac{R}{r}$, and $S^2=\frac{s^2+c}{1+c}=\frac{R+rc}{r\cdot\overline{1+c}}$; hence $F^2 : G^2 :: \frac{R}{r} : \frac{R+rc}{r} :: R : R+rc$, or $F^2 : \frac{G^2-F^2}{r} :: R : c=\frac{G^2-F^2}{rF^2}\times R=$ additional force at the apse. But because $q=3$, $y^3 : r^3 :: \frac{G^2-F^2}{rF^2}\times R :$

$\frac{G^2 - F^2}{F^2} \times \frac{R r^2}{y^3}$ = additional force at the distance y, and the force in the first orbit at the same distance $= \frac{r^n}{y^n}$, therefore the whole compound force $= \frac{r^n}{y^n} + \frac{G^2 - F^2}{F^2} \times \frac{R r^2}{y^3}$.

Cor. 3. If instead of (1), $\frac{v^2}{R}$ be supposed = force in the first orbit at the distance r, it will easily appear that the compound force in the other orbit, at the distance y, will $= \frac{r^n v^2}{R y^n} + \frac{G^2 - F^2}{F^2} \times \frac{v^2 r^2}{y^3}$; the same expression that is found after a very different manner, PRINCIP. book 1st, sect. 9th.

Observations on Iron and Steel.

BY JOSEPH COLLIER.

READ NOVEMBER 18TH, 1796.

After examining the works of different authors, who have written on the subject of making iron and steel, I am persuaded that the accounts given by them of the necessary processes and operations are extremely imperfect. Chemists have examined and described the various compound minerals containing iron with great accuracy, but have been less attentive to their reduction. This observation more particularly applies to steel, of the making of which I have not seen any correct account.

It is singular to observe, how very imperfectly the cementation of iron has been described by men of great eminence in the science of chemistry. Cit. Fourcroy states the length of time necessary for the cementation of iron to be about twelve hours; but it is difficult to discover whether he alludes to cast or to bar steel: for he says, that short bars of iron are to be put into an earthen box with a cement, and closed up. Now steel is made from bars of iron of the usual length and thickness; but cast steel is made according to the process described by Cit. Fourcroy, with this essential difference: the operation is begun upon bar steel and not bar iron.

Mr. Nicholson is equally unfortunate in the account given in his Chemical Dictionary. He says, that the usual time required for the cementation of iron is from six to ten hours, and cautions us against continuing the cementation too long; whereas the operation, from the beginning to the end, requires sixteen days at least. In other parts of the operation he is equally defective, confounding the making of bar with that of cast steel, and not fully describing either. In speaking of the uses of steel, or rather of what constitutes its superiority, Mr. Nicholson is also deficient. He observes, that "its most useful and advantageous property is that of becoming extremely hard when plunged into water." He has here forgotten every thing respecting the temper, and tempering of steel instruments, of which however he takes some notice in the same page. "Plunging into water" requires a little explanation: for if very hot steel be immersed in cold water without great caution, it will crack, nay, sometimes break to pieces. It is however necessary to be done, in order to prevent the steel from growing soft, and returning to the state of malleable iron; for, were it permitted to cool in the open air, the carbon which it holds in combination would be dissipated.*

* It is the opinion of some metallurgists, that a partial abstraction of oxygen takes place, by plunging hot metal into cold water.

I shall, at present, confine my remarks to the operations performed on iron in Sheffield and its neighbourhood: from whence various communications have been transmitted to me by resident friends, and where I have myself seen the operations repeatedly performed.

The iron made in that part of Yorkshire is procured from ores found in the neighbourhood, which are of the argillaceous kind, but intermixed with a large proportion of foreign matter. These however are frequently combined with richer ores from Cumberland and other places. The ore is first roasted with cinders for three days in the open air, in order to expel the sulphureous or arsenical parts, and afterwards taken to the furnaces: some of which are constructed so that their internal cavity has the form of two four-sided pyramids joined base to base; but those most commonly used are of a conical form, from forty to fifty feet high. The furnace is charged at the top with equal parts of coal-cinder and lime-stone. The lime-stone acts as a flux, at the same time that it supplies a sufficient quantity of earthy matter to be converted into scoriæ, which are necessary to defend the reduced metal from calcination, when it comes near the lower part of the furnace. The fire is lighted at the bottom; and the heat is excited by means of two pair of large bellows blowing alternately. The quantity of air generally thrown into the furnace is from a thousand to twelve hundred square feet in a minute. The air passes through a pipe, the diameter of which is from two inches and a quarter, to two and three quarters, wide. The compression of air which is necessary is equal to a column of water four feet and half high. The ore melts as it passes through the fire and is collected at the bottom, where it is maintained in a liquid state. The slagg, which falls down with the fused metal, is let off, by means of an opening in the side of the furnace, at the discretion of the workmen.

When a sufficient quantity of regulus, or imperfectly reduced metal, is accumulated at the bottom of the furnace (which usually happens every eight hours), it is let off into moulds; to form it for the purposes intended, such as cannon or pig iron.

Crude iron is distinguished into white, black, and grey. The white is the least reduced, and more brittle than the other two. The black is that with which a large quantity of fuel has been used; and the grey is that which has been reduced with a sufficient quantity of fuel, of which it contains a part in solution.

The operation of refining crude iron consists in burning the combustible matter which it holds in solution; at the same time that the remaining iron is more perfectly reduced, and acquires a fibrous texture. For this purpose, the pigs of cast iron are taken to the forge; where they are first put into what is called the refinery: which is an open charcoal-fire, urged by a pair of bellows, worked by water or a steam engine; but the compression of air, in the refinery, ought to be less than that in the blast furnace. After the metal is melted, it is let out of the fire by the workmen, to discharge the scoriæ; and then returned and subjected to the blast as before. This operation is sometimes repeated two or three times before any appearance of malleability (or what the workmen call coming into nature) takes place; this they know by the metal's first assuming a granular appearance, the particles appearing to repel each other, or at least to have no signs of attraction. Soon afterwards they begin to adhere, the attraction increases very rapidly, and it is with great difficulty that the whole is prevented from running into one mass, which it is desirable to avoid, it being more convenient to stamp small pieces into thin cakes: this is done by putting the iron immediately under the forge hammer and beating it into pieces about an inch thick, which easily break from the rest during the operation. These small pieces are then collected and piled upon circular stones, which are an inch thick, nine inches in diameter, and about ten inches high. They are afterwards put into a furnace, in which the fire is reverberated upon them until they are in a semi-fluid state. The workmen then take one out of the furnace and draw it into a bar under the hammer; which being finished, they apply the bar to another of the piles of semi-fluid metal, to which it quickly cements, is taken again to the hammer, the bar first drawn serving as a handle, and drawn down as before. The imperfections in the bars are remedied by putting them into another fire called the chafery, and again subjecting them to the action of the forge hammer.

The above method is now most in use, and is called flourishing; but the iron made by this process is in no respect superior to that which I am going to describe. It is, however, not so expensive, and requires less labour.

The process for refining crude iron, which was most common previously to the introduction of flourishing, is as follows.

The pigs of cast iron are put into the refinery, as above, where they remain until they have acquired a consistence resembling paste, which happens in about two hours and a half. The iron is then taken out of the refinery and laid upon a cast iron plate on the floor, and beaten by the workmen with hand hammers, to knock off the cinders and other extraneous matters which adhere to the metal. It is afterwards taken to the forge hammer and beaten, first gently, till it has obtained a little tenacity; then the middle part of the piece is drawn into a bar, about half an inch thick, three inches broad, and four feet long; leaving at each end a thick square lump of imperfect iron. It this form it is called ancony. It is now taken to the fire called the chafery, made of common coal; after which the two ends are drawn out into the form of the middle, and the operation is finished.

There is also a third method of rendering crude iron malleable, which, I think, promises to be abundantly more advantageous than either of the two former, as it will dispense both with the refinery and chafery; and nothing more will be necessary than a reverberating furnace, and a furnace to give the metal a malleable heat, about the middle of the operation. The large forge hammer will also fall into disrepute, but in its place must be substituted metal rollers of different capacities, which, like the forge hammer, must be worked either by a water wheel or a steam engine.

It is by the operation of the forge hammer or metal rollers, that the iron is deprived of the remaining portion of impurity, and acquires a fibrous texture.

The iron made by the three foregoing processes is equally valuable, for by any of them the metal is rendered pure; but after those different operations are finished, it is the opinion of many of the most judicious workers in iron, that laying it in a damp place, for some time, improves its quality; and to this alone, some attribute the superiority of foreign iron, more time elapsing between making and using the metal. To the latter part of this opinion I can by no means accede, as it is well known that the Swedish* ores contain much less heterogeneous matter than ours, and are generally much richer, as they usually yield about seventy *per quintal* of pure iron, whereas the average of ours is not more than thirty or forty:† add to this, that the Swedish ores are smelted in wood fires, which gives the iron an additional superiority.

Iron instruments are case-hardened by heating them in a cinder or charcoal fire; but if the first be used, a quantity of old leather, or bones, must be burnt in the fire to supply the metal with carbon. The fire must be urged by a pair of bellows to a sufficient degree of heat; and the whole operation is usually completed in an hour.

The process for case-hardening iron, is in fact the same as for converting iron into steel, but not continued so long, as the surface only of the article is to be impregnated with carbon.

Some attempts have been made to give cast iron, by case hardening, the texture and ductility of steel, but they have not been very successful. Table and pen-knife blades have been made of it, and, when ground,

* Steel is commonly made of Swedish iron.

† The iron made from the ore found in the neighbourhood of Sheffield, contains a great deal of phosphate of iron, or siderite, which renders the metal brittle when cold.

have had a pretty good appearance; but the edges are not firm, and they soon lose their polish. Common table knives are frequently made of this metal.

The cementation of iron converts it into steel:— a substance intermediate between crude and malleable iron.

The furnaces for making steel are conical buildings; about the middle of which are two troughs of brick or fire-stone, which will hold about four tons of iron in the bar. At the bottom is a long grate for fire. The steel furnace, however, is not well adapted for description. I shall therefore avail myself of an accurate drawing, which was communicated to me by a gentleman conversant with the manufacture, and which is copied in plate I, page 122.

A layer of charcoal-dust is put upon the bottom of the trough; and, upon that, a layer of bar iron, and so on alternately until the trough is full. It is then covered over with clay to keep out the air; which, if admitted, would effectually prevent the cementation. When the fire is put into the grate, the heat passes round by means of flues, made at intervals, by the sides of the trough. The fire is continued until the conversion is complete, which generally happens in about eight or ten days. There is a hole in the side by which the workmen draw out a bar occasionally, to see how far the transmutation has proceeded. This they determine by the blisters upon the surface of the bars. If they be not sufficiently changed, the hole is again closed carefully to exclude the air; but if, on the contrary, the change be complete, the fire is extinguished, and the steel is left to cool for about eight days more, when the process for making blistered steel is finished.

For small wares, the bars are drawn under the tilt hammer, to about half an inch broad and three sixteenths of an inch thick.

The change wrought on blistered steel by the tilt hammer, is nearly similar to that effected on iron from the refinery by the forge hammer. It is made of a more firm texture, and drawn into convenient forms for use.

German steel is made by breaking the bars of blistered steel into small pieces, and then putting a number of them into a furnace; after which they are welded together and drawn to about eighteen inches long; then doubled and welded again, and finally drawn to the size and shape required for use. This is also called shear steel, and is superior in quality to the common tilted steel.

Cast steel is also made from the common blistered steel. The bars are broken and put into large crucibles with a flux. The crucible is then closed up with a lid of the same ware, and placed in a wind furnace. By the introduction of a greater or smaller quantity of flux, the metal is made harder or softer. When the fusion is complete, the metal is cast into ingots, and then called ingot steel; and that which afterwards undergoes the operation of tilting, is called tilted cast-steel.

The cast steel is the most valuable, as its texture is the most compact and it admits of the finest polish.

Sir T. Frankland has communicated a process, in the Transactions of the Royal Society,* for welding cast steel and malleable iron together; which, he says, is done, by giving the iron a malleable, and the steel a white heat; but, from the experiments which have been made at my request, it appears, that it is only soft cast steel, little better than common steel, that will weld to iron: pure steel will not; for, at the heat described by Sir T. the best cast steel either melts or will not bear the hammer.

It may here be observed, as was mentioned before, that steel is an intermediate state between crude and malleable iron, except in the circumstance of its reduction being complete; for, according to the experiments of Reaumur and Bergman, steel contains more hydrogen gas than cast iron, but less than malleable iron;—less plumbago than the first, but more than the latter;—an equal portion of manganese with each;—less siliceous earth than either—more iron than the first, but less than the second. Its fusibility is likewise intermediate, between the bar iron and the crude. When steel has been gradually cooled from a state of ignition, it is malleable and soft, like bar iron; but when ignited and plunged into

* Phil. Trans. 1795.

cold water, it has the hardness and brittleness of crude iron.

From the foregoing facts, we are justified in drawing the same conclusions with Reaumur and Bergman, but which have been more perfectly explained by Vandermonde, Berthollet, and Monge, that crude iron is a regulus, the reduction of which is not complete; and which consequently will differ according as it approaches more or less to the metallic state. Forged iron, when previously well refined, is the purest metal; for it is then the most malleable and the most ductile, its power of welding is the greatest, and it acquires the magnetic quality soonest. Steel consists of iron perfectly reduced and combined with charcoal; and the various differences in blistered steel, made of the same metal, consist in the greater or less proportion of charcoal imbibed.

Iron gains, by being converted into steel, about the hundred and eightieth part of its weight.

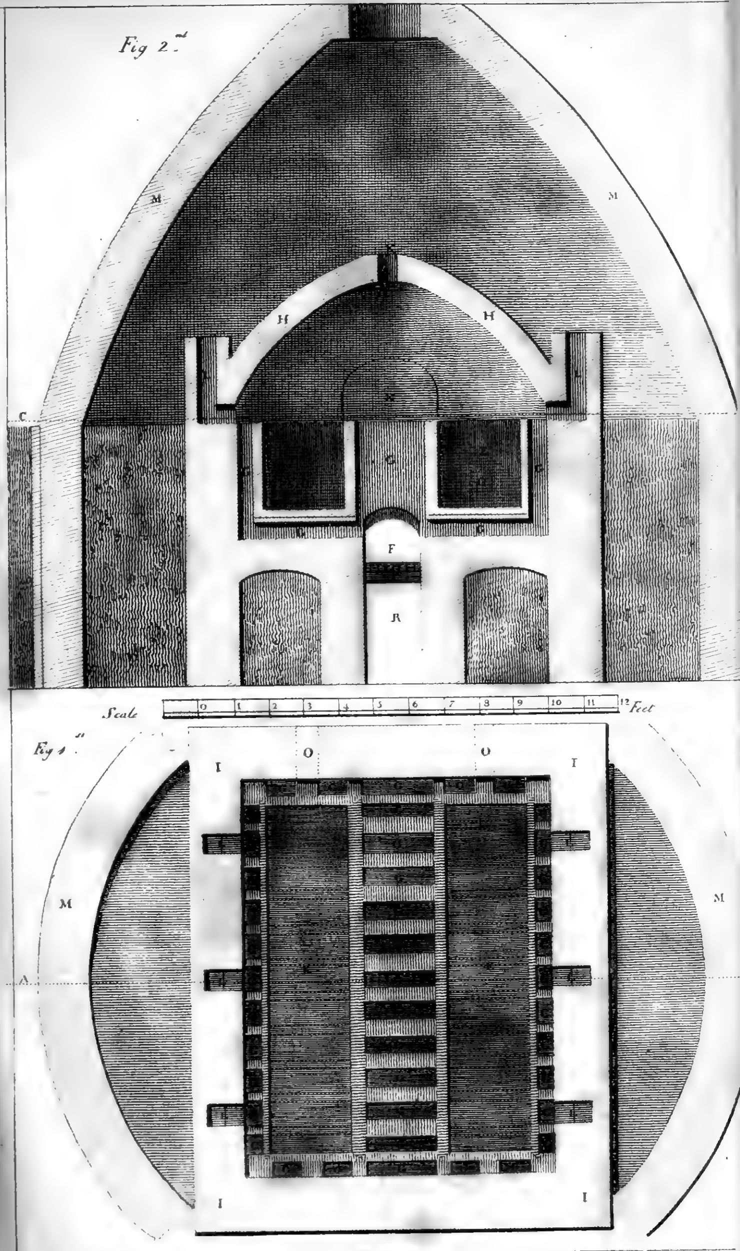
In order to harden steel, it must be put into a clean charcoal, coal, or cinder-fire, blown to a sufficient degree of heat by bellows. The workmen say, that neither iron nor steel will harden properly without a blast. When the fire is sufficiently hot, the instrument intended to be hardened must be put in, and a gradual blast from the bellows continued until the metal has acquired a regular red heat; it is then to be carefully quenched in cold water. If the steel be too hot when immersed in water, the grain will be of a rough and coarse texture; but if of a proper degree of heat, it will be perfectly fine. Saws and some other articles are quenched in oil.

Steel is tempered by again subjecting it to the action of the fire. The instrument to be tempered we will suppose to be a razor made of cast steel. First rub it upon a grit stone until it is bright; then put the back upon the fire, and in a short time the edge will become of a light straw colour, whilst the back is blue. The straw colour denotes a proper temper either for a razor, graver, or penknife. Spring knives require a dark brown; scissars, a light brown, or straw, colour; forks or table knives, a blue. The blue colour marks the proper temper for swords, watch-springs, or any thing requiring elasticity. The springs for penknives are covered over with oil before they are exposed to the fire to temper.

EXPLANATION OF THE PLATE.

Fig. 1 is a plan of the furnace, and *fig.* 2d is a section of it taken at the line A B. The plan is taken at the line C D. The same parts of the furnace are marked with the same letters in the plan and in the section. E E are the pots or troughs into which the bars of iron are laid to be converted. F is the fire-place; P, the fire bars; and R, the ash-pit. G G, &c. are the flues. H H is an arch, the inside of the bottom of which corresponds with the line I I I I, *fig.* 1, and the top of it is made in the form of a dome, having a hole in the centre at K, *fig.* 2. L L, &c. are six chimneys. MM is a dome, similar to that of a glass-house, covering the whole. At N there is an arched opening, at which the materials are taken in and out of the furnace, and which is closely built up when the furnace is charged. At OO there are holes in each pot, through which the ends of three or four of the bars are made to project quite out of the furnace. These are called tasting bars, one of them being drawn out occasionally to see if the iron be sufficiently converted.

The pots are made of fire-tiles, or fire-stone. The bottoms of them are made of two courses, each course being about the thickness of the single course which forms the outsides of the pots. The insides of the pots are of one course, about double the thickness of the outside. The partitions of the flues are made of fire-brick, which are of different thicknesses, as represented in the plan, and by dotted lines in the bottom of the pots. These are for supporting the sides of the pots, and for directing the flame equally round them. The great object is to communicate to the whole an equal degree of heat in every part. The fuel is put in at each end of the furnace, and the fire is made the whole length of the pots and kept up as equally as possible.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Remarks on Dr. Priestley's *Experiments and Observations relating to the Analysis of Atmospherical Air, and his Considerations on the Doctrine of Phlogiston and the Decomposition of Water. By* Theophilus Lewis Rupp.

The high rank of Dr. Priestley in the philosophical world entitles his writings to the greatest attention; and, it is with much diffidence I venture to lay before this society a few observations on a late publication of this gentleman, entitled "Experiments and Observations relating to the Analysis of Atmospherical Air; and Considerations on the Doctrine of Phlogiston and the Decomposition of Water."

The general arguments in support of the new chemical theory, and against the doctrine of phlogiston are so well known; and (to judge by their effect) so well understood, as to require neither repetition nor elucidation. I shall therefore confine myself to the consideration of the particular arguments contained in that publication.

The intention of the first part of it is to shew, that in every case of the diminution of atmospherical air, which the antiphlogistians ascribe to simple absorption, there is emitted some substance; and that this substance is the same which has been called phlogiston.

That a mixture of iron-filings and sulphur with a little water (if it be continued in the air after the diminution has advanced to its maximum) occasions an increase of the quantity, by an addition of inflammable air, is easily accounted for. In this instance, as in many others of the same nature,* the attraction of the iron to sulphuric acid increases the attraction of the sulphur to oxygen. That of the atmospherical air, which surrounds the mixture, is absorbed; and when this is effected, the attraction of the sulphur and iron still continuing, the water contained in the mixture is decomposed; its oxygen combines with the sulphur and iron; and its hydrogen is set at liberty. Hence the increase of bulk after the diminution has advanced to its maximum. When this mixture is confined with pure oxygenous gas, the whole of this is absorbed, and no azote presents itself after the operation. But if something were emitted, which has the property of phlogisticating† pure air, there should be found,

* To these belong the production of alum from the *schistus aluminaris*, of sulphate of iron from martial pyrites, of nitrate of lime from lime mixed with substances containing the basis of nitrous acid, &c.

† I ought to make an apology for the confusion of terms which occur in this paper; but I was not able to avoid making use of the terms of my antagonist, without producing greater confusion.

at the end of the process, azotic gas or phlogisticated air, exceeding in quantity the pure air, in as much as the substance emitted amounted to.

The mode in which the martial sulphuret operates on pure air, is illustrated by the operation of the calcareous and alkaline sulphurets. These also have a very strong smell, and the property of absorbing pure air. But when the diminution has arrived at its maximum, no increase of the bulk of the air ensues.* But if the diminution were effected by the emission of some substance from the sulphuret, that emission would probably go on after the diminution: at least there is as much ground for this supposition as there is for Dr. Priestley's, namely, that it is probable that something is emitted from the martial sulphuret during the diminution of the air, because something is emitted afterwards. In all these cases, the air is diminished both in volume and in weight; and it is surely absurd to say, that this is caused by an addition of something. But the result of the following experiment will remove every doubt on this subject. I kept a mixture of iron-

* I know that the contrary has been asserted by Dr. Priestley and some others; but I am convinced by my own experiments, that no increase of air takes place after the alkaline or calcareous sulphurets have absorbed the oxygen of air exposed to the action of either of them. I constantly make use of liquid sulphuret of pot-ash as a test, but I have never found the least increase of air after the greatest diminution of it had taken place.

filings sulphur and water, in a large glass bell, containing atmospherical air confined by water. The mixture and the glass vessel which contained it (which was purposely chosen large and with a long narrow neck, to prevent the moisture from evaporating), weighed, together, 1324 grains, troy. When the air was diminished thirty cubic inches, the vessel containing the mixture was taken away, made perfectly dry and weighed again. It now weighed 1333¼ grains, consequently nine and one fourth grains more than before the operation, which corresponds exactly enough with the weight of the air which was absorbed.

Dr. Priestley admits (p. 6), that "iron-filings and sulphur, as well as phosphorus, and most of the other substances which have been generally used for the purpose of phlogisticating atmospherical air, do likewise imbibe the dephlogisticated air contained in it, and thereby gain as much weight as the air has lost." This cannot be reconciled with his notion, that something is emitted from the iron-filings and sulphur, which has the property of phlogisticating pure air, unless we take for granted that atmospherical air contains more oxygen than what, by synthesis, we know that it contains; and even then the difficulty will recur, that pure oxygenous gas exposed to this mixture is wholly absorbed, without any residuum of azotic gas.

Dr. Priestley says, that flowers, and especially those which have the strongest smell, phlogisticate air. But it is well known on the other hand, that aromatic plants, in particular, afford very pure oxygenous gas in great quantity. When vegetables vitiate air, it is probably owing to a beginning of putrefaction. This was the case in an experiment which Dr. Priestley relates in his third vol. of Experiments on Air, p. 278. A sprig of mint was introduced into a jar of dephlogisticated air, sometime in April. It was examined the 12th May. "The dephlogisticated air," says he, "was injured, which I attributed to the rotting of some of the leaves of the plant." I recollect only one experiment which has been made to ascertain the effect of flowers on atmospherical air. It is described in Dr. Priestley's 2d vol. of Experiments, p. 247. The air was considerably injured by a rose confined in it. It would have thrown light on the subject, if notice had been taken, whether or not the volume of the air in that experiment was diminished. At all events it is certain, that the effect was not produced by the smell or aroma of the flower, since cloves and musk, which I kept confined for a fortnight in atmospherical air over mercury, neither diminished nor vitiated it in the least.

It is flattering to the antiphlogistians, that their opponents, in the very act of opposition, are obliged to have recourse to their theory in explanation of certain phænomena. Dr. Priestley observing that vitriolic-acid-air has, according to his mode of expression, the power of diminishing and phlogisticating air, says: "that it is owing, *no doubt*, in part, to its imbibing the dephlogisticated part of it, and with it forming vitriolic acid;* but at the same time part of its phlogiston *may* unite with another part of the dephlogisticated air, and with it form phlogisticated air." However, when this vitriolic-acid-air is mixed with pure oxygenous gas in due proportion, the whole is absorbed and no azotic gas is produced. No phlogistication, therefore, takes place in this process.

I shall now consider the experiments which Dr. Priestley has made with black bones and steel needles. In the former, he heated, by means of a burning lens, 140.5 grains of black bones in 23.75 ounce-measures of air, which thereby were reduced to 20 ounce measures. In the other experiment, he heated 200 grains of polished steel needles in 24 ounce measures of air, which were reduced to 19.5 ounce measures. In these experiments the operator avoided to apply a great heat, and a copious precipitation took place, or a thick crust was formed, when they were made over lime water. The bones gained no weight, but rather lost some. The needles and iron gained a little weight, though very incon-

* Vitriolic acid, according to the doctrine of Phlogiston, is sulphur deprived of its phlogiston.

siderable. By these experiments it is intended to prove, not only that something was emitted from the bones and the needles, but also, and particularly that during the calcination of metals, and in combustion, no oxygen is absorbed or imbibed.

"What is most important in these experiments," says Dr. Priestley, "is that, since the diminution of the air was effected by heating those substances, and they did not *gain* any weight in the process, the phlogistication of air is not the absorption of any part of it by the substance which produces that effect, as the antiphlogistic theory supposes."

Before I attempt to explain the result of these experiments, I must request the society to attend a little to their nature, and to the manner in which they were made. It is not only very objectionable that atmospherical air, or a mixture of different gases, was employed in experiments, the result of which was to shew the mutual action of the bones and needles on only one of them, namely, the oxygenous gas; while it would have been easy to have made use of this gas in its greatest purity; but we are not even informed of the degree of purity of the air previous to the operation. This information should not have been withheld, because the purity of atmospherical air varies in different places and circumstances.—These experiments were made over water: a method which always leaves some doubt of the exactness of the result, not only on account of the attraction, which the substances under operation have to moisture, with which they may combine, or which they may decompose, and thereby produce errors; but also on account of the air which water contains, and which may be expelled by the heat of the operation, or attracted by the bodies under examination.—It is known that iron in that first degree of oxidation, formerly called martial æthiops, is capable of combining with so much oxygen, as to increase in weight from 30 to 35 per cent. Why then were there employed 24 ounce-measures only of atmospherical air with 200 grains of needles, though twenty times as much is necessary to saturate the iron and produce a decisive effect?—Bones are not a proper substance for experiments of this nature, because they contain ammoniac and consequently azote.—There should have been an account kept of the carbonic acid gas which was produced.—In a well made experiment nothing is either lost or gained: whenever, therefore, the weight of the product and of the residuum does not correspond with the weight of the substances from which they came, there must be an error. This is the case with these few experiments. They can then surely not be put in competition with the many experiments on which the new theory rests, and which have been made with the most scrupulous exactness, with substances of the greatest purity, and in circumstances which leave no room for any reasonable doubt.—But I proceed to the explanation of the Doctor's experiments.

Since the constituent principles of carbonic acid are demonstrated, both by analysis and synthesis, to be oxygen and pure charcoal, we cannot doubt, that the diminution of the air was occasioned by its oxygen combining with the carbon of the black bones, and forming the carbonic acid which precipitated the lime. These bones therefore could gain no weight, but would on the contrary lose some little: but, 12.0288 parts of carbon saturate 56.687 parts of oxygen,* the loss of weight must have been trifling. This seems to have been the case; for we are informed, that the bones rather lost something. This experiment with calcined bones is, in reality, little else than a combustion of charcoal, the result of which is not to be sought for in the residuum of the bones, but in the carbonic acid which was produced: of which however Dr. Priestley takes no notice.

The excess of azote in the residuum of the air, I ascribe to a decomposition of the ammoniac contained in the bones; and this is rendered more probable by the result of the experiments with iron and needles, in which much less azotic gas remained. A portion of oxygen may also have combined with the bones. Through our ignorance of the original degree of purity of the air and of the quantity

* Chaptal, vol. 1, p. 220.

of carbonic acid which was produced, we can unfortunately do little more than guess.

In the experiments with needles and iron, carbonic acid was likewise formed; which appears by the crust on the lime-water. Iron, and particularly steel, is known to contain carbonaceous matter;* which, combining with some of the oxygen of the air, made the carbonic acid. The iron, by losing carbon, must have lost weight; but, as the production of carbonic acid seems to have been but small, it is probable, that only part of the oxygen, lost in the operation, was saturated with carbon; and that the remainder, combining with the iron, repaired the loss of weight of the iron, occasioned by the loss of its carbonaceous matter, and farther caused the small addition to its original weight. †—But I am aware, that in following these experiments, which have not been made and described with the Doctor's usual care and exactness, I am but groping in the dark; and I hasten to engage the attention of the Society to the following experiments on the same subject, made by the same able philosopher, and recorded in the 3d vol. of his Experiments on Air, page 480. They are unexceptionable and decisive; and I beg leave to recal them to your remembrance, by transcribing every thing material in them.

* Plumbago.

† There would probably have been nothing equivocal in the result of this experiment, if more air or a smaller quantity of needles had been employed in it.

Dr. Priestley introduced into a glass vessel, containing seven ounce-measures of pretty pure dephlogisticated air, a quantity of iron turnings; having previously made them, the air, and the mercury by which it was confined, as dry as possible. Also, to prevent the air from imbibing any moisture, he received it immediately in the vessel in which the experiment was made, by the process for obtaining it from red precipitate, so that it had never been in contact with any water. "I then," says he, "fired the iron by means of a burning lens, and presently reduced the 7 ounce-measures of air to 0.65 of a measure. Examining the residuum of the air, I found one fifth of it to be fixed air; and when I tried the purity of that which remained, by the test of nitrous air, it did not appear that any phlogisticated air had been produced in the process: for, though it was more impure than I suppose the air with which I began the experiment must have been, it was not more so than the phlogisticated air of the 7 ounce-measures, which had not been affected by the process, and which must have been contained in the residuum, would necessarily make it. In this case, one ounce-measure of the residuum and two of nitrous air occupied the space of 0.32 of a measure. In another experiment of this kind, ten ounce-measures of dephlogisticated air were reduced to 0.8 of a measure; and, by washing in lime-water, to 0.38." Sensible that such a quantity of air must have been imbibed by something, to which it must have given a very perceivable addition of weight, he weighed the calx to which the iron had been reduced, and "I presently found," says he, "that *the dephlogisticated air had actually been imbibed by the iron.* In the first instance, about twelve ounce-measures of dephlogisticated air had disappeared, and the iron had gained six grains in weight. Repeating the experiment very frequently, I always found that other quantities of iron, treated in the same manner, gained similar additions of weight, which was always very nearly that of the air which disappeared. This calciform substance I found by various experiments, to be the same thing with the scales that fly from iron, when it is made red-hot, or the substance into which it runs in a very intense heat in an open fire." And, in a note, he informs us, that this calx is the same with finery-cinder.

Here then we have experiments of the same nature as those recited in the pamphlet under examination, but simplified as they ought to be, and made with care and exactness, and in circumstances and by means, which leave little or no room for error. But no azote made its appearance more than was contained in the air before the operation. The iron emitted nothing, but increased in weight as much, or nearly as much, as the oxygenous gas amounted to. About twelve ounce-measures of it disappeared, and the iron increased in weight six grains;

which is nearly the weight of the gas consumed. And if a proper allowance were made for the oxygen contained in the fixed air, and the previous loss of weight of the iron by the combustion of its carbonaceous matter, the increase of weight of the iron would still more nearly correspond with the weight of the oxygenous gas consumed.

The conclusion from these experiments, or (more properly speaking) the fact, that the oxygen combined with the iron, was too obvious to escape the Doctor's observation, as we see by his own declaration: but he soon changed his mind and reverted to his theory: for, in a note to the same article, he declares, that it was not dephlogisticated air that was imbibed by the iron, but only the water, which, he says, is by far the greatest part of it.* But, if that were the case, what, it may be asked, is become of the air? Or, if six grains of water were absorbed in this process, where is the inflammable air that must have been produced? There should have been formed 20 ounce-measures of it at least. For Dr. Priestley says, that in passing steam over red-hot iron, the iron imbibes the water and emits its phlogiston in the shape of inflammable air; and though this is his doctrine and not mine, I may avail myself of the argument. Moreover it has been shewn by M. de

* I shall hereafter have occasion to consider the experiments on which the opinion rests, that water enters into the constitution of gases.

Saussure, that a cubic foot of atmospherical air can contain only 12 grains of water in solution: and supposing that it is only the respirable part of it that can dissolve water (which is however too great a concession) the 12 ounce measures of oxygenous gas consumed could not contain one half grain of water, even if they had been purposely impregnated with as much water as they could take up; though the reverse was the case here.

I ought, in this place also, to refer the Society to Dr. Priestley's experiments in his 3d vol. p. 210. He suspended pieces of lead and tin in atmospherical air, and heated them by means of a burning mirror or lens. The air was diminished 25 per cent. And, in p. 212, he informs us, that, by throwing the focus of a burning lens on iron, the air was diminished and made noxious to as great a degree as in the calcination of lead or tin. And when he heated iron in atmospherical air, till his lens could make no farther impression on it, he always found that the iron gained weight upwards of 30 per cent. (Ibid, p. 483).

The degree of heat, on which Dr. Priestley seems to lay some stress, makes no difference in the result of the experiment. Every one may convince himself of this by the following simple process. Take 100 grains of fine harpsichord-wire cut in small pieces, or clean iron filings; put them into a tobacco-pipe; and expose it to a red heat in a common fire-place for several hours: and the iron shall acquire weight from 25 to 30 grains, though not made to fuse. The cup of the pipe should be slightly covered to prevent loss; but room should be left for the escape of the air, which is continually circulating through the tube. Those who have a furnace with a muffle may make this experiment in the most satisfactory manner.

Dr. Priestley next says, that the phlogistication of nitric acid is owing, in some cases, to its imbibing something; and not always to its parting with something, which the antiphlogistians maintain. He dissents from them, because nitrous air being, as he terms it, imbibed by nitric acid, the latter becomes phlogisticated.—To this we may reply, that part of the oxygen of the nitric acid combines with the nitrous air, and forms nitrous acid with it. The nitric acid, being thus deprived of a portion of its oxygen, thereby becomes nitrous acid, or what has been called phlogisticated nitrous acid. The difference between the nitric and nitrous acids, consists in the proportion between their common basis and the principle of acidity. Hence it is plain, that they may be transformed into one another; that is to say, the proportion of their parts may be altered, either by subtraction of the principle of acidity, or by addition of the basis, and *vice versa*. The Doctor's objection is therefore merely a dispute about words.

This philosopher thinks he has discovered, that atmospherical air contains much more pure air, than has hitherto been found. He makes the proportion of it amount to 46.6 parts in a hundred, instead of 27 parts. He has found, that equal measures of nitrous and atmospherical air mixed together (though, at first, they occupy a space of 1.01 measures) will, in the course of a month, occupy only 0.6 of a measure.—That this fact does not warrant the conclusion he draws from it, will appear by what the same chemist relates in the 1st vol. of his Experiments, p. 361. "Having mixed," says he, a "quantity of air, which I knew to be thoroughly phlogisticated by the putrefaction of fishes, with an equal quantity of nitrous air; I transferred the mixture into my graduated tube, when, instead of occupying two whole measures (as I had expected) they only occupied 1.95 measures. I poured the air back again into a wide jar; and, transferring it once more into a graduated tube, found it to be only 1.8 measures; and pouring it about ten times backwards and forwards, without any unnecessary agitation, it was reduced to 1.6 measures. *Having stood in water all night*, I measured it again the next morning, when *I found it to be* 1.5; and by measuring three times more, it was reduced to 1.4." To whatever, therefore, the farther diminution of a mixture of these two gases, by standing over water for a long time, may be owing, it is certain that it is *not* owing to the presence of pure air; since it takes place when air, thoroughly phlogisticated, is kept with nitrous air in the same circumstances. This is a strong proof, that the nitrous test cannot be depended upon. By all other eudiometers it appears, that atmospherical air contains about one fourth of its bulk of oxygenous gas. And when this and pure azotic gas are mixed in such proportion, they form a fluid exactly similar to that of the atmosphere.

Dr. Priestley's experiments on the composition of azotic gas are certainly very curious and interesting, and deserve great attention.—I therefore regret much, that I have not been able to succeed in any attempts to repeat them. I speak only of those in which he confined rusted iron and inflammable air in glass vessels by mercury or water. Those in which he kept the oxygenous and hydrogenous gases in a wet bladder are not admissible: the azote is, in this case, formed by the decomposition of the bladder; and is produced though no inflammable air is present.* I kept the red oxyd of iron, which I put into a phial with hydrogenous gas, in a glass jar over water for several weeks; but no diminution took place. A large piece of rusted iron was then suspended in a jar with inflammable air confined by water. No diminution took place; and when, in six weeks afterwards, the gas was examined, it appeared as in-

* Priestley's Experiments, vol. 1, p. 179---181.

flammable as ever. Not being able to succeed, I had recourse to the ingenuity of my friend Mr. William Henry. We repeated the experiment over mercury with no better success. I also kept the black oxyd of manganese, and the red oxyd of mercury in hydrogenous gas; expecting, that as the oxygen has a weaker attraction to these metals than to iron, it would more easily combine with the hydrogen; but I was again disappointed. There cannot however be the least doubt of the truth of the facts mentioned by Dr. Priestley; and I mean to make fresh attempts, and to acquaint the Society with their result if successful. In the mean time, is it not probable that the oxygen of the rust combining with the hydrogen, formed water; and that, at the same time, a portion of azote, of which all metallic oxyds contain more or less (particularly those which have been exposed to the atmosphere) was disengaged? This conjecture is so much the more plausible, as it rests on well known phenomena. If it should be discovered that azote is not a simple substance, I cannot see what the doctrine of phlogiston would gain by it. The phænomena of combustion and calcination in pure oxygenous gas, where no azote is produced, will still be inexplicable on that theory.

I come now to the consideration of the last part of the publication in question, entitled, " Considerations on the Doctrine of Phlogiston and the Decomposition of Water."—This part is merely argumentative. I shall pass over several objections to the new theory that have been made and answered before, and confine myself to what appears to me to be new.

After stating that mercury, by exposure to the atmosphere in a certain degree of heat, is converted into the calx, called precipitate *per se*; which becomes running mercury again by exposure to a greater degree of heat (on which experiment the antiphlogistians have built their new theory), Dr. Priestley says, " that all that can be inferred from it is, that, in this particular case, the mercury in becoming that calx imbibed air, without parting with any or very little of its phlogiston." In support of this, he mentions, that the calx which remains, after exposing Turpeth mineral to a red heat, cannot be revived completely by any degree of heat; but may be revived by substances supposed to contain phlogiston.

It will be recollected that Turpeth mineral is made by heating mercury in an equal quantity of sulphuric acid till the mixture becomes dry. This sulphate of mercury is then washed in warm water, by which means a yellow precipitate is obtained, which is the Turpeth mineral. When this is again heated, it becomes of a red colour; and is then the calx mentioned above. It is probable therefore (and it is allowed) that this calx still contains sulphate of mercury; which can hardly be expected to be decomposed but by adding substances which have a greater affinity to the sulphuric acid than mercury, or else a greater attraction to the oxygen of the sulphuric acid than the sulphur. However, Dr. Gren, professor at Halle, who, in matters of fact, may be depended on, informs us (in his Treatise on Chemistry, §. 2262) that this calx may be reduced to its metallic state by mere heat, without any addition whatever; and that, during the process, oxygenous gas and vitriolic-acid-air are produced.

In farther support of his assertion (that mercury, in becoming precipitate *per se*, parts with none or little of its phlogiston) Dr. Priestley says, "if we judge by the air expelled from the calces of metals and other circumstances, there are few, if any, of them but contain more or less of phlogiston."—I imagine he alludes here to the azote, which is mixed with the oxygenous gas obtained from the oxyds of metals. If this be occasioned by a portion of phlogiston retained by the calx, how does it happen that the purest oxygenous gas that can be procured from metallic oxyds, is obtained from precipitate *per se*, though, according to Dr. Priestley, it retains all or nearly all its phlogiston?—This argument, moreover, involves a contradiction to the doctrine of phlogiston; according to which, a calx is reduced to its metallic state by *acquiring* phlogiston: but here it is said to be reduced by *parting* with phlogiston.

When mercury is dissolved in the nitric acid, nitrous air is produced; which the phlogistians ascribe to the phlogiston of the metal. If therefore it were true, that precipitate *per se* retains all or nearly all the phlogiston of the metal from which it comes, it would likewise produce nitrous air during its solution in the nitric acid. But this is not the case. As mercury in dissolving in nitric acid produces nitrous air, it cannot be denied that the red oxyd, obtained by these means, has parted with its phlogiston; and yet it is reduced to its metallic state by mere heat, without any addition whatever. To obviate this objection, he asserts, that it loses only *part* of its phlogiston; and that it has a deficiency of phlogiston when reduced by mere heat: but that, on the contrary, the metal obtained by reducing precipitate *per se* in inflammable air has a redundancy of phlogiston. This assertion leads him to another of equal probability: viz. that mercury, whether it have a deficiency or a redundancy of phlogiston, will, in all chemical processes, exhibit the same phænomena!

It is surprising into what absurdities and contradictions this erroneous theory misleads its ablest advocates. They set out by declaring, that a metal is a compound substance, consisting of a calx and phlogiston (p. 39); that a metal becomes a calx by losing its phlogiston (p. 43);* and that a calx is reduced by acquiring phlogiston. They afterwards tell us, that a metal may become a calx, and at the same time retain its phlogiston (p. 40); and that a calx, in becoming a metal, may part with phlogiston;† and when a metal, in becoming a calx, is allowed to have lost phlogiston, they contend, that that phlogiston was not necessary to the constitution of the metal, provided the calx can be revived by mere heat (p. 41 and 42); and, finally, that a metal has the same properties whether it have a deficiency or an excess of phlogiston! (p. 42).

It must, however, be confessed, that these gratuitous assertions, so fatal to the cause they are intended to defend, are peculiar to Dr. Priestley; at least I have not seen them in the writings of other chemists. It is on the contrary generally allowed, that the red oxyds of mercury, whether obtained by

* "Precipitate *per se* (says Dr. Priestley) is much more easily procured in dephlogisticated than in common air, and probably not at all in phlogisticated air; this air not being capable of taking any phlogiston from mercury, *without which the calx cannot be formed.*" Experiments on Air, vol. ii, p. 185.

† The azotic gas, which is mixed with the oxygenous gas, obtained from precipitate *per se*, is ascribed by Dr. Priestley (p. 41) to phlogiston retained by the calx, with which it parts when it becomes a metal.

calcination, or by means of the nitric and sulphuric acids (provided they are free from the acids) are essentially the same. It must also be allowed by those who maintain that mercury contains phlogiston, that it has lost the whole of it in becoming the red oxyd, as it now shews no longer any properties attributed to phlogiston, and is not capable of farther dephlogistication.

But not only all the oxyds of mercury, but the oxyds of silver and gold also are reduced to their metallic state by mere heat. When silver is precipitated from its solution in nitric acid, it is in the state of an oxyd of a dark brown colour, which weighs 12 per cent. more than the silver which was employed.* Gold, when precipitated from its solution in nitro-muriatic acid, is an oxyd of a dark red colour, and weighs 10 per cent. more than the metal employed.† Both these oxyds are reduced to their metallic state by mere heat, without addition of any substance supposed to contain phlogiston; yet they must have lost their phlogiston, since nitrous air is produced during their solution in the acids.

The hydrogenous gas, which is produced in dissolving some metals in some of the acids, is attri-

* Bergman, Dissertation on Metallic Precipitates.

† Ibid.

buted, by the old chemists, to the phlogiston of the metals; but as the antiphlogistians maintain, that it is owing to a decomposition of water, they are desired by Dr. Priestley to shew, what is become of the other constituent part of water, the oxygen.—He had undoubtedly forgotten, that iron, as well as all other metals, are in the state of oxyds when combined with acids; and that they are precipitated from them in that state with a considerable addition of weight. In dissolving iron, therefore, in diluted sulphuric acid, the oxygen of the water combines with the metal, and it is by this means that the hydrogen is set at liberty; which, combining with caloric, is exhibited as hydrogenous gas. And this process is analogous to the decomposition of water by red-hot iron; the product of which is the same gas and the black oxyd of iron, which is called finery cinder. But Dr. Priestley thinks that this is impossible; because, says he, finery cinder is not soluble in vitriolic acid. This, however, is a mistake. I decomposed water by red-hot iron wire. To avoid all mistakes, I made use of the scales only which fell from this wire by bending it, so that I was sure that no iron, in its metallic state, was present. These scales I put into a phial with some diluted sulphuric acid, and kept it in a heat which sometimes made it boil. After several hours, the whole was dissolved without effervescence. It is rather extraordinary that the Doctor should have fallen into this error, since he informs us, in the 3d vol. of his Experiments, p. 505, that putting 60 grains of finery cinder, finely pulverized, into each of the three acids, and leaving them two or three days to digest, he found, that of that which had been in the vitriolic acid, and also of that which had been in the marine acid, there remained 15 grains undissolved: 45 grains out of 60, therefore, were dissolved. That 15 grains remained undissolved was, perhaps, owing to a deficiency of acid, or want of time; or, what is more probable, to carbonaceous or vitreous matter and other impurities, which the common finery cinder for sale contains. And of this he seems to have made use; for, of a homogeneous mass, either the whole or none can be dissolved by an acid.* This is confirmed by the circumstance, that the same residuum was found in the muriatic acid, which Dr. Priestley admits to be capable of dissolving that oxyd.

That the solution of this finery cinder, in the diluted sulphuric acid, is not so rapid as the solution of iron in the same acid, cannot be surprising, if it be considered, that iron is more oxydated by the decomposition of water, in a red heat, than by a solution of it in diluted sulphuric acid; since much less hydrogenous gas is produced from the same quantity of iron by the last method, than by the for-

* Bergman.

mer:* consequently less water is decomposed, and therefore less oxygen combines with the iron. And it is well known, that the attraction of the sulphuric acid to the oxyd of iron is so much weakened, by an addition of oxygen to the latter, that it is precipitated from its solution: as is the case, when the sulphate of iron is dissolved in water and exposed to the atmosphere. The rust of iron also, which is of a higher degree of oxydation than the black oxyd of iron, cannot be dissolved in sulphuric acid but with much difficulty.

It is next said, that finery cinder contains no oxygen, "because, when it is dissolved in the marine acid, it does not dephlogisticate it as minium and other substances containing oxygen do."—But Dr. Priestley allows, p. 12 and 47, that the red oxyd of iron, or rust of iron, contains oxygen; and yet it does *not* oxygenate the muriatic acid. The truth is, that every substance containing oxygen has not that effect; which can only be produced when the muriatic acid has a greater attraction to oxygen than the substance which contains it; or when a substance, *e. g.* a metallic oxyd, is combined with more oxygen than it can contain, when dissolved in

* Two ounces of iron, when dissolved in acids, will yield about 800 ounce-measures of air; but by passing steam over it, when red-hot, they yield 1054 ounce-measures. See Priestley's Experiments, vol. 1, p. 294.

that acid; in which case, the superfluous oxygen combines with and oxygenates the muriatic acid.

"Iron," continues our author, "which has really imbibed air, or the common rust of iron, has a very different appearance from finery cinder, being red and not black."—Aware that the answer to this is, that these two substances are oxydated in different degrees, he adds, "that if finery cinder were iron partially oxygenated, it would go on to attract more oxygen, and in time become a proper rust of iron."—And so it really does. Iron filings exposed to a long continued heat with access of air, under the muffle of an assaying furnace, become first an oxyd of a blackish colour, and afterwards the red oxyd, which was formerly called *crocus martis adstringens*, which has the same properties as rust of iron. In this process, the iron is first converted into finery cinder (as is uniformly the case, when it is heated in the presence of atmospherical air) which is afterwards converted into rust, by a continuance of oxydation. Moreover the black oxyd of iron (which is obtained by the method discovered by Lemery, viz. by keeping iron-filings under water for some time; during which process the iron acquires weight, and hydrogen gas is produced, according to the observations of Lavoisier and Rinmann) will become the red oxyd of iron, not only by heating it with access of air, but also in the habitual temperature of the atmosphere. (Gren, § 2764 and seq.)

He says, that finery cinder and massicot are similar substances.—I admit the analogy. They are both oxyds of an inferior degree of oxydation; perhaps of the same degree. As massicot, therefore, has the property of receiving a higher degree of oxydation,* he cannot deny this property to finery cinder. The rust of iron is also converted into finery cinder by the abstraction of a portion of its oxygen: which may be accomplished by heating it in close vessels, either alone,† or (more easily) with substances which have a greater affinity with oxygen than the iron has.

* In the preparation of minium, the lead is first converted into massicot, by exposing it to a red heat with access of air. It is afterwards washed, in order to separate any metallic particles it may contain; and the pure massicot when dry is spread on iron plates, and roasted in a moderate heat, till it has acquired the proper colour for minium.

† It may appear extraordinary, since part of the oxygen of some metallic oxyds (such as the red oxyds of iron and lead and the black oxyd of manganese) can be expelled by a certain degree of heat, that the whole of it cannot be expelled by the same means. It is evident that oxygen has a greater attraction to iron, lead, and manganese, than to caloric, since it leaves this and combines with those metals. When, therefore, the reverse takes place, the decomposition of these metallic oxyds can only be partial: the same as the decomposition of some sulphates by the

The rusting of iron in the habitual temperature of the atmosphere, seems likewise to be owing to a decomposition of water, since no rust can take place without moisture. Iron confined in dry oxygenous gas will never rust. From this fact, we learn, why finery cinder, which has been made in a high temperature, will not rust in the ordinary temperature of the atmosphere. Being already combined with a large portion of oxygen, its remaining attraction for it is too feeble to decompose water.

But in order to prove that the addition of weight to the iron (in the decomposition of water by red-hot iron) is really caused by oxygen, we are called upon to exhibit it in the form of dephlogisticated air, or of some other substance into which oxygen is allowed to enter.—I confess that, as far as I know, this has not yet been done; but, though we have not hitherto

nitric acid. If, for instance, the sulphate of potash, or of soda, be dissolved in equal parts of strong nitric acid in a sufficient heat, nitrate of soda, or of potash, is found in the mixture when it is become cold. But Scheele has observed, that *only one third* of the sulphate is decomposed in this way. That the weaker substance should thus displace the more powerful one, is a paradox in the doctrine of affinities, and cannot be satisfactorily explained. Probably the excess in quantity makes up for the defect of the power of attraction. Thus Margraaf decomposed completely the nitrate of soda by muriatic acid, when he distilled one part the former with eight parts of the latter. This process is analogous to the entire decomposition of the red oxyd of mercury by caloric.

found the means of analysing this substance, we are able to shew its composition by synthesis; and this method is at least as decisive as the analytical. Supposing we were unacquainted with the means of decomposing any neutral salt, for example, Glauber's salt, there would be hardly any one so incorrigibly a sceptic, as to doubt that it is composed of sulphuric acid and soda, if by combining these substances we produced that salt. This is exactly the case with finery cinder. We have seen (by the fine experiments made by Dr. Priestley, with iron heated in oxygenous gas, of which I gave an abstract in the former part of this paper) that the oxygen combined with the iron, and produced a calciform substance; which he found, by various experiments, to be the same thing with finery cinder. The conclusion is obvious: finery cinder is iron combined with oxygen. But as Dr. Priestley believes, that it is only water that has combined with the iron; because water, as he maintains, is by far the greatest part of oxygenous gas, it will be proper to examine into the truth of this opinion, particularly as his whole theory must stand or fall with it.—The experiments on which this opinion rests, are to be found in his 1st vol. of Experiments on Air, p. 130—132.* Not being able to expel the carbonic acid from *terra*

* I know of no other experiments on which this philosopher grounds this opinion.

ponderosa aërata by mere heat;* but procuring large quantities from it by means of steam, at the expence of some water (about one half of the weight of the gas), he thinks it impossible to conclude otherwise, than that the water has combined with the carbonic acid gas and makes one half of its weight. M. Berthollet has already noticed the insufficiency of this experiment.† Dr. Priestley should certainly have shewn the presence of the water in the fixed air; or, at least, that it was not combined with, or imbibed by, the barytes, or otherwise lost. In confirmation of the above, he relates the following experiment. Forty-eight grains of *terra ponderosa aërata* were dissolved in spirit of salt, and yielded 7.2 grains of fixed air. The solution was then evaporated to dryness, and the salt exposed to a red heat, in which it lost 4 grains: consequently the weight of the air and of the residuum exceeded the original weight of the aërated barytes by 3.2 grains; and, as he believes *that all the muriatic acid was expelled from the earth*, he concludes that this ex-

* Since this paper was written, I have been informed, that Dr. Hope of Edinburgh has succeeded in freeing the aërated barytes completely from its carbonic acid, by mere heat, without the aid of water. This perfectly pure barytes has the remarkable property of being soluble in a very small quantity of water.

† Dr. Priestley did not even weigh the residuum of the barytes.

cess of weight consisted in water, which the fixed air carried off from the menstruum.—But a red heat cannot expel the acid from the muriat of barytes.* I exposed some of this salt to a very strong red heat for two hours; and, examining it afterwards, I found that it had a saline taste, not caustic as pure barytes; it precipitated a muriat of silver, from the solution of this metal in nitric acid; and when concentrated sulphuric acid was poured on it, it emitted clouds of muriatic acid vapour. I cannot, therefore, admit, that he has demonstrated, as he is pleased to say, that one half of the weight of fixed air is water; and the following experiments will prove, that water enters not into its composition.

EXPERIMENT I.

I dissolved 100 grains of aërated barytes in diluted muriatic acid, consisting of one part acid and three parts distilled water. The whole being weighed before and after the solution, it appeared, that 18.44 grains of carbonic acid had escaped. The barytes was then precipitated from its solution by the sulphuric acid. The precipitate, after being carefully edulcorated and dried, weighed 121.95 grains. One hundred parts of this artificial *spathum ponderosum* contain, according to Mr. Kirwan, 67 parts of earth and 33 of acid and water; hence,

* Gren, Handbuch der Chemie, § 975.

the above 121.95 grains contain of pure barytes
81.70 grains,
which when the above .... 18.44 grains
of carbonic acid are added, reproduce 100.14 grains
of aërated barytes. I should observe, that I added one grain to the weight of the ponderous spar, as an allowance for some loss, which I had reason to think took place; but I really believe I over-rated it.

EXPERIMENT II.

One hundred grains of chalk (which had been kept for upwards of half an hour in a strong red heat, in order to expel any moisture it might contain) were dissolved in diluted muriatic acid: 40 grains of carbonic acid gas were disengaged, so that the pure lime amounted to 60 grains. Having precipitated the lime by means of the oxalic acid, and carefully filtered, washed, and dried the product, it weighed 133¾ grains. The oxalate of lime consists, according to Bergman,* of 48 parts acid, 6 parts of water, and 46 parts of pure lime. The above 133¾ grains, therefore, contained 61.52 grains of pure lime, which corresponds very nearly with the chalk employed when the carbonic acid is deducted from it.

Not being able to account for the excess of 1.52 grains, it occurred to me that it might be owing to insufficient drying of the oxalate: which I found was actually the case. But I choose to give the result as

* Dissertation on the Acid of Sugar.

I found it at first. These little inequalities of exsiccation can hardly be avoided.

EXPERIMENT III.

I dissolved 100 grains of calcined chalk in diluted muriatic acid, with the loss of 41 grain sof carbonic acid. The lime was then precipitated by sulphuric acid; and the residuum in the filtre, being properly washed and dried, weighed........ 97 grains.
But, as a portion of this selenite is soluble in water, it was necessary to evaporate the lixivium, which farther produced 43

140 grains.

As this artificial gypsum, when well dried, contains, according to Kirwan, 42 grains of earth, 39 of acid, and 19 of water in the hundred, the above 140 grains contain 58.8 grains of pure lime, and the carbonic acid being 41 grains: hence, 58.8 + 41 = 99.8 grains; which very nearly corresponds with the weight of the chalk. To assure myself that I had hit on the right degree of exsiccation, I calcined some of the sulphate of lime, which thereby lost its water, amounting to 18¼ per cent.

EXPERIMENT IV.

I dissolved 100 grains of well dried chalk in acetous acid; 47 grains of carbonic acid gas were disengaged. The acetate of lime, which was formed, was carefully collected and calcined in a crucible, with a sufficient heat to destroy the acetous acid; and to expel the carbonic acid, which is formed by the combustion of the former. The earth weighed nearly 53 grains, which agrees exactly with the weight of the chalk when the carbonic acid is deducted from it.

EXPERIMENT V.

One hundred grains of the chalk, of which I made use in Experiment IV. and which were found to contain 47 per cent. carbonic acid, were calcined in the fire of a smith's forge. It lost all its carbonic acid, and the residuum weighed 53 grains.

Now, if it were true, that carbonic acid, when in the state of gas, contains water to the amount of half its weight, the barytes in Experiment I. would have weighed 9 grains more than it did; and the lime in Experiments II. III. would have weighed $40\frac{1}{2}$ grains more than it really did weigh; and the earth of Exp. IV. V. would have weighed 47 grains more; or, if the gas contained water in any other proportion, the earths must have weighed as much more as this water amounted to. But as the weight of the pure earths, and the carbonic acid gas together, corresponded with the original weight of the carbonats from which they came, it follows that nothing extraneous entered into the composition of that gas, except the caloric, which is necessary to its gaseous state. The inference also, which Dr. Priestley has drawn from his experiments on this subject, viz. that the greatest part of other gases is water, must fall to the ground; and my own experiments confirm, as far as analogy goes (what indeed hardly admitted of a doubt), that water does not enter into the constitution of any gas.

The calx, therefore, which was produced by heating iron in oxygenous gas, is a combination of oxygen and iron; and being one and the same thing with finery cinder, this also must be composed of oxygen and iron.

"However," continues the Doctor, "neither this finery cinder, nor any other calx of iron, can be revived unless it be heated in inflammable air, which it eagerly imbibes, or in contact with some other substance, which has been supposed to contain phlogiston."—This proves nothing, but that oxygen has a greater affinity to iron than to caloric, and that, therefore, it cannot be expelled by mere heat. But when a substance is present, to which the oxygen has a greater attraction than to iron, it will, of course, leave this and combine with the other. Does the iron, in this process, combine with phlogiston? Let us see what happens when finery cinder is heated in inflammable air. Dr. Priestley, in his 3d vol. of Experiments, p. 487, relates the following beautiful experiment. He put finery cinder into a glass vessel, containing inflammable air confined by mercury; and both the vessel and mercury had previously been made as dry as possible. "I had no sooner" says he, "begun to heat the slag in these circumstances, than I perceived the air to diminish, and, at the same time, the inside of the vessel to grow very cloudy with particles of dew, that covered almost the whole of it; and, by degrees, gathered into drops and ran down the sides of the vessel, &c. Thus, at one time, I made a piece of this slag imbibe 5.5 ounce-measures of inflammable air, while it lost as much as the weight of about three ounce-measures of dephlogisticated air; and the water collected (by means of bibulous paper) weighed two grains.* Another time, a piece of slag lost 1.5 grains, and the water produced, was 1.7 grains." Nothing can be more plain and decisive. The iron, which had become a calx by combining with oxygen, was reduced to its metallic state, not by imbibing any thing, but by being deprived of its oxygen, which uniting with the hydrogen of the inflammable air, formed water, *equal in weight to both the hydrogen and oxygen* as nearly as possible: particularly when it is considered, that some loss must have taken place by the manner in which the water was collected. Chemistry is greatly indebted to Dr. Priestley for his experiments on iron heated in oxygenous gas, and the reduction of the oxyd by inflammable air. Nothing can be more beautiful and simple; and if there were no other experiments on the subject, these alone would be sufficient to esta-

* This is above a quarter of a grain more than the iron lost.

blish the new theory of calcination, and the composition of water.

It does not seem, that Dr. Priestley has given the attention it deserves to the experiment in which water is decomposed by charcoal, producing inflammable and carbonic acid gas.* The constitution of the carbonic acid being ascertained beyond dispute, by the ingenious experiment of Mr. Tennant, we cannot be deceived in our conclusion, that water is composed of oxygen and hydrogen.

This certainty of the constituent principles of carbonic acid gas, renders it superfluous to make any remark on the appearance of it, when red lead, or precipitate *per se*, are reduced by heating them in inflammable air; and its production in respiration. The cause of it cannot be mistaken.

"No inflammable air," continues our author, "can be procured in the process with steam, but by means of some substance which has been supposed to contain phlogiston."—But may not this objection be retorted with propriety upon the phlogistians, by reminding them, that no inflammable air can be procured, but when water, or substances containing it, is present; and shall we not then have the advantage over them, that our argument rests on a fact, while theirs is supported only by a supposition?

Nothing new is offered against the recomposition

* Traité élémentaire de Chimie, par Lavoisier, chap. VIII.

of water. The public are in possession of all the experiments, and will judge for themselves: they are conclusive and little can be added to them. I shall concude this paper with some remarks on the appearance of nitrous acid in the explosion of hydrogenous with oxygenous gas.—The facts are briefly these. When the quantity of hydrogenous gas is in due proportion to saturate the oxygen, or when there is an excess of it, water is produced. When, on the contrary, there is an excess of oxygenous gas, nitrous gas is produced.—Since it is proved that the basis of this acid is azote, its presence cannot be denied wherever the acid is formed. All that can, therefore, be inferred from the above stated fact is,

I. That oxygen and azote, in the temperature which is produced by a rapid combustion, or an explosion of hydrogenous gas with oxygenous gas, have a greater affinity to each other than they have to caloric; they, therefore, abandon the caloric which kept them in a gaseous state, and, combining together, form nitrous acid.

II. That oxygen has a greater affinity to hydrogen than to azote. Hence, in a rapid combustion, or explosion, the oxygen cannot unite with azote, if there be present a sufficient quantity of hydrogen to saturate it; but, if there be a deficiency of hydro gen, the oxygen and azote will combine. In the former case, pure water is produced; and in the latter, water and nitrous acid.

By applying these laws, it will appear, that when explosions are made with the same bulk of oxygenous gas, of whatever degree of purity it may be, mixed with a given quantity of hydrogenous gas, the production of nitrous acid will be more certain, the more the oxygenous gas approaches to purity; because the proportion of oxygen will thereby be increased, so that at last the hydrogen will be insufficient to saturate it, and the remaining oxygen will then combine with the azote which the mixture contains. This is the reason why Dr. Priestley obtained nitrous acid when he employed pretty pure oxygenous gas; and why the azotic gas, which he purposely mixed with it, was not affected, except when the quantity of hydrogenous gas was lessened.

Thus have I attempted to defend the new chemical theory against the objections of the ablest advocate for the doctrine of phlogiston. I ought to apologize to the Society and to Dr. Priestley, for my presumption. I hope I have said nothing that is, or may seem to be, disrespectful to this great man, whom I sincerely esteem and admire both as a man and as a philosopher.

An Account of three different Kinds of TIMBER TREES, *which are likely to prove a great Acquisition to this Kingdom, both in point of Profit and as Trees for Ornament and Shade. By* CHARLES WHITE, ESQ. F. R. S.

READ APRIL 21, 1797.

In making a collection of such hardy trees and shrubs as would grow, and even flourish, in the open air at Sale in the County of Chester, I soon observed that there were three forest trees, of different genera, which grew much faster than the others in the same soil and situation, viz. the Black American Birch with broad leaves, the Athenian Poplar, and the Iron Oak with prickly cups.

THE BROAD LEAVED AMERICAN BLACK BIRCH, BETULA NIGRA, *Linn. Spec. plant.* 1394, is described by Mr. Aiton in his Hortus Kewensis: *B. foliis rhombeo-ovatis, duplicato-serratis, acutis, subtus pubescentibus, basi integris; strobilorum squamis villosis; laciniis linearibus, æqualibus.* It is a native of Virginia and Canada, and was first introduced into England (where it grows in the greatest luxuriancy and perfects its seeds) by Peter Collinson, Esq. in the year 1736. There is no doubt, therefore, that it will soon become very plentiful and cheap. It is very desirable in pleasure grounds, as it is the first forest tree in the spring which presents us with its leaves: these are of a light and lively green. Its bark, which is white, makes, at all times, a beautiful variety when intermixed with other trees. It is said to be the most useful timber tree in North America, for building both of houses and boats, and will grow fast in any soil or situation, whether wet or dry.

Miller, speaking of trees of this description, says, "they may be propagated by seeds, in the same manner as the common birch tree, and are equally hardy. Some of the trees now begin to produce their catkins in England, so that we may hope to have plenty of their seeds of our own growth, for at present we are supplied with them from America. As these grow more vigorously than the common sort, and thrive on the most barren ground, they may be cultivated to great advantage in England, for their wood is much esteemed in Canada, where the trees grow to a large size; and they are, by no means, an unsightly tree in parks, for their stems are straight, the bark smooth, and their leaves are much larger than those of the common birch, so may be planted in such places where few other trees will thrive."

Mr. Hanbury says, "the black Virginian Birch, being of foreign growth, is propagated for wilderness and ornamental plantations; but, as it begins now to become pretty common, it is to be hoped it will soon make a figure among our forest trees, it being equally hardy with our common birch, and will arrive at a much greater magnitude. This species will grow to be upwards of sixty feet in height. The branches are spotted, and more sparingly set on the trees than the common sorts. The leaves are broader, grow on long foot stalks, and add a dignity to the appearance of the tree; and as it is naturally of upright and swift growth, and arrives at so great a magnitude in a few years, prudence will direct us to let it have a share among our forest trees, to plant them for standards in open places, as well as to let them join with other trees of their own growth, in plantations more immediately designed for relaxation and pleasure." I planted one of these trees nineteen years ago, and it is now forty-five feet six inches in height, and three feet seven inches in the girth.

THE ATHENIAN POPLAR TREE, *Populus (Græca) foliis cordatis, glabris, basi glandulosis, remote crenatis; petiolis compressis; ramis teretibus.*

The Athenian Poplar is a native of the Islands of the Archipelago, and was first cultivated in England by Hugh Duke of Northumberland, in the year 1779. Perhaps, there is no deciduous tree so beautiful, or so proper for pleasure grounds, intended for ornament and shade, as this poplar; having a fine upright stem; the branches well disposed; the bark smooth, and of a silvery hue, resembling satin wood. The leaves, which are of a light green, are produced very early in the spring, and are retained on the tree longer than on any deciduous tree in this country, not falling off till late in the autumn; they are never blighted nor infested with insects, nor does it lose a leaf during the whole summer. Though the poplar is generally termed an aquatic, this will grow in any soil or situation, and is of quicker growth in dry upland, than any tree we are acquainted with in this climate, though not quite of such quick growth as the Huntingdonshire Willow in rich moist meadow-land. In such a situation, I have fallen a Huntingdonshire Willow,* from which I made a staircase when it was only of nineteen years growth from a cutting. The Athenian Poplar is propagated with the greatest advantage by suckers and layers; but is with great difficulty raised from cuttings or truncheons. The common way of raising them, amongst the nursery-men, is by engrafting them on some other poplar; but the trees thus raised are of little value, being very slow in their growth; and it is owing to this circumstance, perhaps, that their real worth has not before been discovered. About twelve years ago I purchased two plants of this poplar, from two different nursery-

* I cannot find that this species of willow has been described by any botanical writer; but it is well known among the nursery-men by this name.

men in London, at one guinea each; one of them was grafted upon a different kind of poplar, the other was upon its own roots. I placed them near together, in a dry situation, in a light soil, underneath which was a stratum of gravel. The grafted one made very little progress; I therefore converted it into a stool, and raised several plants from it. The other, which is upon its own roots, has made a rapid progress, being, at least, fifty-one feet high, and two feet nine inches in the girth. It produces annually a great number of suckers, with which I have supplied many of my friends.*

The third is THE IRON, WAINSCOAT, or TURKEY OAK; so called by Mr. Luccomb. I have long been in doubt what species of oak this really was; but one of mine having borne some acorns this year, has ascertained it to be a variety of the *Quercus Cerris*; and it appears to me to be either a *nondescript*; or what Mr. Aiton, in his Hortus Kewvensis, calls, *frondosa: foliis ovato-oblongis, leviter sinuatis, planiusculis*: common Turkey Oak Tree. It is what Mr. Luccomb generally grafts his Luccomb Oaks upon; and the plants certainly grow faster, when grafted upon this oak, than upon any

* There is another poplar, of very swift growth, which makes a very handsome tree, and will flourish in any situation or soil. It is the *Populus cordifolia canadensis*, or Berry-bearing Poplar, as it is commonly called. This tree will grow freely from cuttings.

other. About twenty years ago, in making a collection of oaks, I received several from Mr. Luccomb both of the Iron and of the Luccomb Oak; but I soon found that the Iron Oak overgrew all the others, and was equally ornamental as the English Oak. From a branch which I have sawed off, the wood appears to be as hard and as ponderous as the English Oak.

The following is an account of the size and age of some Iron, Luccomb, and English Oaks, growing in my collection at Sale.

	Height.			Girth.	
	Feet.	Inch.		Feet.	In.
An Iron Oak 20 years old	36	0	—	3	3
Another of the same age	37	0	—	3	0
A Luccomb Oak, the same age, grafted on an English Oak	32	2	—	2	5
An English Oak of the same age	28	0	—	2	6
Another 40 years old	39	0	—	2	10
Another 56 years old	54	0	—	3	4

The following is the copy of a letter from Mr. Luccomb to Mr. Babington, dated Newbridge, Exeter, September 17, 1795.

"All I can say of them (the Iron Oaks) is, that my father had a few of them, as a present from William Ball, Esq. of Manhead-house (now Lord Lesburne's, near Chudleigh, Devonshire), about fifty years since, by the name of the Iron or Wainscoat Oak, which Mr. Ball received from Turkey by one of his own ships trading there. They are the same sort which you have noticed at Hillersdon, as my father sold some of them to Mr. Creroy about forty years since. They have, as you observe, a very jagged leaf, and the cup of the acorn is rough, like a bur. They are not evergreen."

The following is a letter I received from my worthy friend the Rev. Thomas Gisborne, author of several useful publications.

"YOXALL LODGE, Oct. 20, 1795.

"Dear Sir,

I have this evening received a letter from my brother-in-law, Mr. Babington, respecting the measurements of the Iron Oaks, at Hillersdon, near Cullompton, where he now is; I subjoin what he says on the subject, and have pleasure in finding the result so honourable to the tree which you recommend.

I am, Sir, &c.

THOMAS GISBORNE.

——"To day I have measured some of the oaks about three feet and a half from the ground; and give you the result, which I thought would be fair and satisfactory, in the following way.—

" No. of English Oaks.		Circumference. Feet.	Inches.
8		31	1
9		30	1
9		32	8
26	26)	93	10
	Average circumference	3	$7\frac{1}{3}$

" No. of Iron Oaks.		Circumference. Feet.	Inches.
6		31	6
6		32	10
6		31	9
18	18)	96	1
	Average circumference	5	4

" As circles are as the squares of their circumferences, pieces of the buts, at this height a foot long, would be to each other about as 1877 : 4096. Now, supposing the Iron Oaks to carry their buts as much higher than the others, as their substance below would lead us to expect (and they seem, in fact, to do this or more), there must be four or five times as much wood in them as there is in the English Oaks. An old labourer here informs me, that all were planted at the same time, between forty and fifty years since. They stand in rows, ten feet asunder, and the trees are twenty feet from each other in each row. I measured such trees as first presented themselves, with the exception of one or two which seemed unhealthy. They are on a steep bank, and a gravelly soil. The trunks of the Iron Oaks are covered with a lighter moss on the whole than fixes itself on the English Oak; but make a fine appearance. I measured many outside trees, and observed, that the Iron Oak seemed to have as great a superiority over the other, in this situation, as it has when surrounded by neighbours. As to height, the Iron Oaks very generally out-top the others, and are the master trees; but, you know, that, in a plantation, a slender tree will often be nearly as tall as its sturdy neighbour.

" P. S. On looking over the oaks again, I think the Iron Oaks carry up the thickness of their buts a good deal better, *cæteris paribus*, than the others; and, therefore, they have five or six times the quantity of wood in them.

" There are but two beeches, and they are both outside trees, and therefore, larger than they otherwise would be. The circumference of the two was 12 feet 4 inches; average 6 feet 2 inches.

Spanish

"Spanish Chestnuts, No.		Circumference.	
		Feet.	Inches.
6		31	3
8		34	3
4		24	3
18	18)	89	9
Average circumference		4	11¾ nearly."

N. B. Mr. Babington says, "that there are gates and pales on the premises at Hillersdon, which have been made of the Iron Oak; and that, as far as he can judge, the wood appears as hard and as tough as that of the common oak."

It has always been considered that when men have planted oak, they have not planted for themselves or for their children, but for distant posterity, and even *they* could never be repaid where land bore any annual value: and to the planter himself, little pleasure could arise from trees of such very slow growth. But the same person who plants the Iron Oak may possibly live to reap some little profit as well as pleasure; and it is not at all uureasonable to suppose, his immediate successor may see it arrive to some degree of perfection. From what I have seen of the wood of this oak, and from the account given by Mr. Babington of the gates and pales made with it, there is great reason to suppose it will be equally useful as the English Oak for any purpose whatever.

The general decrease of timber in this island,—the many waste lands unemployed,—and the bill now proposed to be brought into parliament, by that great friend to Agriculture Sir John Sinclair, will be my apology for troubling the Society with this paper; for the planter ought certainly to be furnished with every advantage, and every possible inducement should be held out to him, for promoting so useful and so national a work.

EXPLANATION OF THE PLATE.

A. A. a. a. Leaf, acorn, and prickly cup of the Iron, Wainscoat, or Turkey Oak.

B. B. b. b. Leaf, acorn, and prickly cup of the *Quercus Cerris* of *Linnæus—fol. oblongis, lyrato-pinnatifidis; laciniis transversis, acutis, subtus subtomentosis; calyce hispido; glande minore.* Small-acorned Spanish Oak with prickly cups.

An ANALYSIS *of the* WATERS *of two* MINERAL SPRINGS *at* Lemington Priors, *near Warwick; including Experiments tending to elucidate the* Origin *of the* Muriatic Acid. *By* WILLIAM LAMBE, M. A. *late Fellow of St. John's College, Cambridge.*

Communicated by Dr. HOLME.

READ OCT. 6, & OCT. 23, 1797.

So many analyses of waters have been recently presented to the public, many of them distinguished by the skill and accuracy with which they have been conducted, that some apology may be deemed necessary for offering these papers to the notice of the Society of Manchester. It will appear that many facts occurred in the course of the ensuing investigation, which cannot be explained upon any of the chemical principles with which I am acquainted; and that some attention was requisite to avoid deception from imperfect resemblances. This made it necessary to elucidate the subject by some experiments particularly adapted to the purpose; and these have led to consequences which I apprehend to be important, and some of them of very extensive application. I have thought, therefore, that an attempt, however imperfect, to improve this useful and difficult part of chemistry would not be unacceptable to a society, which cannot but be interested in a subject of so much consequence to the chemist and the physician, and, therefore, so closely related to the general interests of humanity. It is a branch which, in our days, has made a progress proportioned to the general improvement of the science; and owes its rapid advancement, in a great measure, to the order, perspicuity, and consummate skill, with which it has been treated by the illustrious Bergman. But since the elements (or what are to us the elements) of nature are very numerous, and their combinations indefinitely varied, it can hardly be supposed, that perfection is attained in an art, the objects of which are at once so extensive and so complicated. We can only hope to approach to this end by the study of nature in her own school; by patient and attentive experiment. Science must guide, industry must execute, and genius must combine. However, the labours of many eminent philosophers have greatly facilitated the progress of their successors in the same field; in consequence of which, there are many parts of science, in which a common hand may fill up the plan traced out by the pencil of a master: imitation may finish what invention has begun. The progression of knowledge is, from its nature, proportioned to the condition of its present improvement; at least in those branches, in which the ultimate degree of perfection is indefinite: for the ascertainment of a single fact often throws light upon a vast variety of appearances, which were previously detached, solitary, and inexplicable; so true is the remark of a profound writer,* that "our knowledge increases, not as new objects increase, but in a much higher proportion."

Lemington Priors is a village two miles east of the town of Warwick, which receives its name from the Lem, a small stream which passes near it. The salt spring, which rises near the church-yard, has been long known, and used by the country people of the neighbourhood. It has been noticed by most of the older writers on the mineral waters of this country. Another spring rises in the bed of the river, near the bridge; and is frequently overflown by the water of the river. But the two springs, which are the subject of the following analysis, have been recently discovered; and they are now considerably resorted to, being used internally and for the purpose of bathing. In consequence of this, they first engaged my attention in a medical view: but I propose, in this paper, to confine myself to the consideration of their chemical properties. As they both rise a great many feet below the surface of the earth; and as their source is unknown, it seems needless to enter into any particu-

* Maclaurin.

lars concerning the nature of the surface, or the qualities of the soil of the circumjacent district. I hasten, therefore, to the objects of my enquiry.

ON THE WATER OF THE NEW BATHS.

The spring of this water was discovered in the year 1790, and the year following, a very elegant building was erected; which contains two cold baths, two warm baths, and a small warm bath for children. The spring was discovered at the depth of forty-two feet from the surface of the ground. A well is sunk about twenty-four feet deep. In the course of this depth, there is a rock the thickness of eight or ten feet; afterwards there is a bed of marl; after penetrating which, another rock, much harder than the former, is found. Through this second rock a bore is made eighteen feet deep, where there is a small cleft in the rock. There are many little springs found in this course; but from this cleft the water rises with violence to the level of four feet above the surface of the ground; and it affords a constant and copious supply of fresh water.

I. ON THE GASEOUS FLUIDS.

The water, when fresh drawn, smells of *sulphurated hydrogen* or *hepatic** gas; but it quickly becomes

* I must take leave to retain this name in the following pages, preferring it (if for no better reason, at least for its shortness) to the compound term.

inodorous by exposure to the atmosphere. I have obtained no more than four cubic inches of gaseous fluids from a wine-gallon of the water. Of these hardly half a cubic inch is absorbed by water; and, 1. Nitric acid dropped into this solution causes a minute precipitate of sulphur; hence, some hepatic gas has been expelled by the boiling. 2. I put some of the water, which had not been boiled, into a bottle, leaving a part of the bottle empty: I then filled the bottle entirely with lime-water, and stopped it. A white precipitate fell, which, with the addition of distilled vinegar, effervesced sensibly, but not strongly. The half cubic inch is, therefore, mostly carbonic acid. 3. Into the portion of the air which was not absorbed by water, I plunged a lighted candle; it was instantly extinguished. This portion is, therefore, azotic gas.

II. THE SPONTANEOUS PRECIPITATE.

A sediment falls to the bottom, and adheres to the sides of the bath. It is of a yellow colour, and acquires additional brightness from exposure to the atmosphere. A similar sediment may be separated by boiling the fresh water. From a gallon of water, .75 of a grain is procured. This dissolves readily in acids; and shews all the common and well known appearances of oxyd of iron. It must have been united with the carbonic acid, which has been already detected. (1 2.).

III. SOME ANOMALOUS APPEARANCES WITH PRECIPITANTS.

1. After the spontaneous precipitate has been separated, a little oxygenated muriat of mercury was dissolved in a glassful of the water. A white matter separated during the solution, and, in some hours collected in considerable quantity.

2. A piece of sulphate of argill was dropped into a jar of the same water; presently a stratum of white matter was formed at the bottom of the jar: but this appearance is transitory; for, in the course of an hour or two, the precipitate is redissolved and the water resumes its transparency. This is an appearance which I have not found noticed by writers on the subject, though I have reason to think it not uncommon. The waters of Astrop, near Banbury, contain carbonat of lime, dissolved by carbonic acid: and, when the carbonat has been separated, by boiling the water, the same appearance is produced by sulphate of argill. Its origin, and that of the decomposition of the mercurial salt, will be shewn in the sequel.

IV. THE METALLIC SALTS.

Several phænomena demonstrate the existence of some metallic substance in this water, besides the precipitate already described (11.): but it is so peculiarly combined, or otherwise modified, as to elude, in a great measure, the action of the ordinary re-agents. When the water is boiled in contact with some of the metals, it becomes turbid; and the metal is partly oxydated, and partly dissolved. The iron, which has been used in the construction of the baths, is almost destroyed: the tin, which lined a vessel used as a warm bath for children, has suffered in like manner. If the water be only boiled and poured into a wine glass with a bright key in it, the liquor becomes turbid before it is cold. Copper seems to resist its action better; but this is only in a low temperature: for, if the water be boiled in a copper vessel, similar effects are produced; a precipitate is formed and some copper is dissolved in the water. 1. This is readily proved by putting a bright piece of iron into the liquor, which, in a few hours, acquires a coppery coating.* Lead also I have found to be acted upon.

I examined, with some minuteness, the precipitates formed by iron and by copper. 2. The first is of a yellow colour; and, though it is not magnetic, it may be made so by the flame of a blow-

* This remark evinces the strong necessity of a chemical examination of all the substances used as medicines. It is very common to warm the water with a view of quickening its laxative power. It is evident with what caution vessels, in which copper is an ingredient, should be used for this purpose; or, rather, with what care they should be utterly avoided.

pipe upon charcoal. It consists, therefore, of oxyd of iron, either totally or in part; but whether it is derived from the liquor, or from the iron which was used to procure it, cannot be determined by this experiment. But the following observation demonstrates, that iron is contained in the water itself. 3. The water was boiled in a copper vessel, and the precipitate formed was collected. This is also of a yellow colour: and, exposed to the flame of a blowpipe on charcoal, (like the former precipitate) it became magnetic. It seems also to contain copper; for, precipitating its solution in muriatic acid by ammoniac, the liquor became blue; the colour, however, was by no means strong.

When the salts of the water have been concentrated by evaporation, copper is acted upon more powerfully; insomuch, that if a silver spoon be used for the evaporation, it is much tarnished, and the salts acquire a cupreous taste and a yellow tinge, though they are colourless if the evaporation be made in glass. These vestiges of copper must be attributed to the alloy of the spoon.

The appearances I have described surprised me the more, as, from the use of some of the common re-agents, I had formed opposite conclusions. 4. Prussiat of potash, before the water has been boiled, forms a green cloud; but in a quantity hardly sufficient to precipitate. After boiling, there is no decomposition; nor is there any if the liquor be evaporated to half its original bulk. 5. Tincture of galls strikes a purple colour before the water is boiled; after boiling, there is a precipitate likewise, but of a dull lemon-colour; after a partial evaporation, the colour approaches to whiteness. In each of the two last cases, the liquor gradually acquires a deep yellow tinge.—In these experiments, we have none of the ordinary signs of iron, except of the carbonat (11.), though its existence in the water has been proved beyond question (3.).

V. MANGANESE IS DISSOLVED IN THIS WATER.

As zinc is known to have a stronger affinity to acids than the other metals have, I hoped by its mean to obtain some information on the cause of the facts I have described. 1. I boiled, therefore, some of the water in a glazed vessel, in contact with some pieces of zinc: a small precipitate was formed, but enough to be collected for examination with the blowpipe. This was fused with borax; and a globule was formed of a rich red colour, precisely that communicated by *manganese*.* Continuing the fusion the colour vanished; nor could I make it re-appear by the yellow flame of the blast: probably, because the manganese was mixed with another metal. The globule was removed to a sil-

* For the properties of this mineral, see Scheele's Essay on Manganese; or Bergman on the white Ores of Iron.

ver spoon; and, then, by fusion, it regained the red colour. To avoid the possibility of error, a little manganese, fused with borax in the same manner, was placed beside the red globule; and no difference could be perceived in the colour of the two globules, except a slight variety in the intensity.* 2. The *lemon-coloured* precipitate, formed by the tincture of galls (IV. 5.), yields the same result. By using a large quantity of water, I collected sufficient for examination. This I put over a fire on an iron plate: the vegetable part took fire and burnt away: the powder became of an ochry yellow, and was magnetic:—fused with borax, by the blowpipe, it acquired the redness which manganese imparts.

3. The same fact may be proved by a single experiment. It is known, that tartrite of potash decomposes salts of manganese by a double affinity; in consequence of which, tartrite of manganese, which is a substance insoluble in water, precipitates.† I poured, therefore, a solution of tartrite of potash into the water; and there fell a copious crystalline precipitate. Much iron fell down

* I am aware of the observation of Bergman, that zinc does not precipitate solutions of manganese. See his Essays, vol. iii. p. 414. But, besides that I describe only what I have seen, it will appear that the salt in question is of so peculiar a nature, that it cannot be expected to obey the usual analogies of the other solutions.

† See Scheele, as above.

with the manganese, as might be expected from the affinity between the oxyds of these metals: for, by fusing the precipitate with nitre, green spots were formed on the sides of the crucible.

Tartrite of potash decomposes likewise salts of lime, as is well known; but the tartrite of lime is precipitated in the form of a white powder which is not sensibly crystalline. But, as there is a great abundance of lime in this water, (XIV.) it seems probable that it had entered into the composition of these crystals.

Manganese has been but rarely noticed as entering into the composition of mineral waters. The reason, perhaps, is, that it has been seldom looked for, rather than that it seldom exists; since it is now known to be a substance very abundantly diffused through the earth. The waters of Astrop, which I have mentioned above (III. 2.), decompose tartrite of potash and form a crystalline precipitate, when its carbonat of lime has been thrown down by boiling. This water, in these circumstances, hardly affects either tincture of galls or prussiat of potash. Every chalybeate spring may be suspected to contain some manganese likewise; and should, therefore, be examined accordingly.

VI. TESTS OF MANGANESE AND IRON.

As I shall have frequent occasion to mention this mixture of the oxyds of manganese and iron, I will enumerate the tests I have used, to avoid useless repetitions. These are, 1. the magnet: to this the smallest particle of iron may be rendered obedient, by exposing it to a due heat, on charcoal, by the blowpipe. 2. The tinge communicated by the same process to a globule of borax: this from iron is green or yellow; from manganese it is hyacynthine; or, when the globule is more loaded with the metal, a fine rich red. This colour disappears by the blue conical flame; and may be reproduced by the gentler yellow flame which surrounds the cone. 3. Fusion with nitre or with carbonat of soda: manganese imparts to them a fine blue colour; but if it be mixed with the oxyd of iron, the colour is green.

VII. AN HYPOTHESIS TO EXPLAIN THE CAUSE OF THE PHÆNOMENA.

The first hypothesis which I framed, to account for the facts in question, was deduced from the well known property which manganese possesses of oxygenating the muriatic acid. My reasoning was as follows.—Since, during the solution of the black oxyd of manganese in the muriatic acid, a portion of the acid becomes oxygenated, it must follow: that, if this portion should meet and combine with a metallic oxyd, the salt, formed by such an union, must be super-oxygenated. But in this state of oxydation, doubtless, is a great part of the iron which is so abundantly diffused through the earth. Are then the appearances in question the result of these circumstances? In short, are the salts muriat of manganese and oxygenated muriat of iron? In pursuance of this idea, I formed some oxygenated muriat of iron, by mixing some yellow oxyd of iron (the *rubigo ferri* of the shops) with water, and exposing it, by a proper apparatus, to the oxygenated muriatic acid gas. The gas readily dissolves a part of the oxyd, a few bubbles (perhaps of carbonic acid) escaping during the solution. 1. The salt which is formed is deliquescent; colourless; of a pure bitter taste, without any of the sweet astringency of the common salts of iron.* Alkalis precipitate a white oxyd. The mineral acids, also, decompose the salt; and, at the same time, a white matter, of a crystalline form, precipitates, but an excess of acid re-dissolves the precipitate.† 2. If some metallic iron be digested in a solution of this

* By far the best method of making this salt, is to put the rust in a saucer, and to put the mixture of manganese and muriatic acid, diluted to avoid a strong effervescence, in a cup on the same saucer; then to cover the cup with an inverted glass: thus the oxygenated vapour will be confined as it is slowly extricated. If distillation is used, the salt can be hardly made free from an astringent taste.

† The sulphuric acid does not re-dissolve the precipitate; the others do. Some further remarks on this subject will be found (XIV. 6.)

salt, an ochre precipitates copiously, which is very soluble in acids. Copper also decomposes the salt, but the matter precipitated is in small quantity and hardly soluble in acids. 3. Prussiat of potash is totally unaffected by this salt: so, likewise, is tincture of galls, when the salt is quite perfect;* but after iron has been digested with it, galls communicate a yellow tinge, or even precipitate a brownish matter: still the prussiat of potash has no effect. These properties of the salt bear so strong a resemblance to the appearances which I have remarked in the water, particularly in the effects of the metals (IV. 2 & 3.) and the failure of the re-agents (IV. 4 & 5.), that it strongly confirmed me in the hypothesis I had adopted. On pursuing the experiment, however, the analogy failed. I added, to the salt of iron, very minute quantities of muriat of manganese; but how small so ever was the quantity used, and however much it was diluted, the manganese was instantly detected by prussiat of potash. I was therefore forced to conclude, that, if oxygenated muriat of iron is really an ingredient of this water, it must be formed by a process different from that which I had imagined.

* I once saw the galls form a *white* precipitate; but I suspect the oxygenated, was contaminated with some common muriatic acid, formed by its decomposition during the digestion.

VIII. THE APPEARANCES IN QUESTION ARE PRODUCED BY THE ACTION OF HEPATIC GAS ON IRON AND MANGANESE.

An observation of Bergman, though in part erroneous, has conducted me, as I think, to the true cause of these appearances; and I am greatly mistaken, if its consequences, when fully pursued, are not of considerable importance to chemical science. Bergman has asserted, that hepatised water, in which iron filings have been kept for some days, in a well closed vessel, grows *purple* with tincture of galls: if the iron be dissolved by an acid, the colour approaches more to *violet*. He moreover adds, that the solution of iron in hepatised water *is not at all rendered turbid by prussiat of potash.** This latter fact promised to throw some light on the subject of my enquiry, particularly when it was joined to the fact of the hepatic smell, which the water has when recently drawn (1.). I was the more strongly induced to pay attention to this combination, from the contradictory assertion of another very eminent chemist. Mr. Kirwan has denied that hepatic gas can dissolve iron or any other metal.† To ascertain this point, I have made numerous experiments with the greatest caution and accuracy that I have been able to apply. The hepatic gas which I have used was obtained

* Bergman's Essays, Dissertation VII. 4. L.

† Philosophical Transactions for 1786.

from sulphuret of iron, formed by fusing equal parts of iron and flowers of sulphur; and (except in some instances, which will be particularly noticed as they occur) was extricated by diluted sulphuric acid. The gas was collected under water: which method was preferred; to purify it, if possible, from extraneous acid. 1. I digested iron-filings, previously purified by repeated washings with distilled water, in a solution of hepatic gas in distilled water: the bottle was filled with the solution, and corked. The iron was presently acted upon; numerous bubbles arose, which drove the cork out of the bottle; they were strongly inflammable, and probably, therefore, pure hydrogen gas: the liquor gradually lost its hepatic odour; and, at the end of some days, it had a smell a good deal resembling that of stagnant rain water; as the bubbles ceased to be produced, it recovered its transparency. The liquor was then examined by reagents. Infusion of galls struck a yellow tinge; prussiat of potash gave a little whitish cloud; nitrat of silver and muriat of barytes, each very minute precipitates; pure potash a yellow precipitate, but not till the liquor had stood an hour or two. The liquor does not deposit any thing, either by exposure to the atmosphere or by a boiling heat: but, by this last process, something (perhaps a little gas which has escaped the action of the iron) flies off; since the precipitate with nitrat of silver was white after the boiling, which had previous to it been black. Very little can be deduced with certainty from these trials, except the presence of a little sulphuric acid. It seemed of consequence to determine, whether this is generated in the process; or is accidental, from the sulphuric acid which was used to extricate the gas.

2. To determine this point, I repeated the experiment, using the muriatic acid to generate the gas, instead of the sulphuric. In this case, the liquor (as Bergman has said) is not at all rendered turbid by prussiat of potash; neither does the muriat of barytes precipitate any thing; the precipitate by pure potash is now white, but as minute as before; nitrat of silver makes a yellow cloud both before and after boiling; infusion of galls strikes a yellow tinge. Hence it is clear, that hepatic gas, when produced by sulphuric acid, carries with it a little of the acid which cannot be separated by passing it through water. It seems probable also, that a little muriatic acid is carried up in like manner, when this is used to extricate the hepatic gas. We may farther conclude that though the remark of Bergman (on the effect of the prussiat of potash) is true; the remark which accompanies it (on the colour produced by infusion of galls) is erroneous. A purple colour is always, I believe, occasioned by extraneous acid; in which case, the prussiat of potash is also precipitated. From the same facts, we are enabled to detect another error also, into which the same great man has been be-

trayed. In his analysis of the acidulous waters of Medvi in Ostro-Gothland,* he has noticed a residuum of 4½ grains of iron, dissolved partly by hepatic gas, partly by carbonic acid. Now, we have seen, that there is no decomposition of these liquors by boiling; nor does any oxyd precipitate how long soever the evaporation be continued. The hepatic gas seems to be totally decomposed: nitric acid dropped into these liquors precipitates nothing.

Are we then to conclude with Mr. Kirwan, that hepatic gas does not dissolve iron or any other metal? As the gas itself is decomposed, this, in strict propriety of language, must be allowed to be true; but, that some solution is effected during the decomposition, the following remarks evince. 3. A piece of clean and bright iron was put into some of the hepatised solution (if I may be allowed so to call it, while its true composition is unknown); it soon became turbid; a copious ochry precipitate fell down; and, in twenty-four hours, the whole surface of the iron was covered with rust. 4. Let the solution be boiled in a copper vessel, a precipitate also separates of an ochry colour; but it is smaller in quantity than in the former experiment. 5. Digest a piece of clean iron in the solution after it has been boiled in a copper vessel; much ochry matter still separates; but there is no vestige of me-

* Bergman's Essays, Dissertation VIII. 6.

tallic copper on the iron plate. 6. Digest copper filings in the liquor in which iron filings have been previously digested; separate the copper filings, and now let a piece of bright iron be put into the liquor; in this case, copper is deposited on the surface of the iron in its metallic state. 7. Put a small piece of sulphat of argill into a glass of the solution, after fresh iron filings have been digested with it; a white stratum forms at the bottom of the glass, but, after some time, it is re-dissolved and the liquor resumes its transparency. 8. Put a little oxygenated muriat of mercury into a glass of the hepatised solution; as it dissolves, a white matter collects on the sides, and falls to the bottom, of the glass. 9. Infusion of galls, after the fresh iron has been digested with the solution, precipitates the iron of a dark colour; still the prussiat of potash does not become turbid.— From all these facts it is clear, that, as the iron combines with the sulphur of the hepatic gas, a peculiar substance is formed and dissolved in the water, which has hitherto been unnoticed by chemical writers, as far as has fallen within my information. That this substance is contained in the waters of the spring under our present examination seems fully established, by the concurrent evidence of so many phænomena in which they completely coincide. Compare III. 1 & 2. IV. 1, 2, 3, 4, & 5.

IX. MANGANESE EXPOSED TO HEPATIC GAS.

To complete the demonstration, it is necessary to examine the action of hepatic gas upon manganese.

1. I digested some black oxyd of manganese in hepatised water: it had been previously purified, by being boiled repeatedly in distilled water.* The hepatic smell of the gas is quickly impaired; and, in twenty-four hours, if enough of the oxyd has been used, it is perfectly destroyed; still the liquor has a peculiar smell, which can hardly be called offensive: no gas is extricated in this process. The liquor, after filtration, was examined by the same reagents as the hepatised solution of iron (VIII. 1.) with nearly a similar result; a minute quantity of sulphuric acid was detected; prussiat of potash gave a small white cloud, tincture of galls a slight yellow tinge. Repeating the experiment with gas extricated by muriatic acid, there was, in this case, no trace of sulphuric acid, and the liquor was not at all rendered turbid by prussiat of potash. From both these solutions pure potash separates a very minute white precipitate. 2. But, in one respect, these solutions differed from the solutions of iron; for, by

* The readiest method of purifying this substance is, to boil it first in a very large quantity of rain water; after which, a single boiling in distilled water will be sufficient to extract every *soluble* impurity.

these nitrat of silver is instantaneously decomposed, and a copious precipitate separates; it is of a dark brown or yellow colour, as if from a combination of sulphur. Oxygenated muriat of mercury let fall a white matter much more plentifully than from the solutions of iron. Tartrite of potash is decomposed, and a fine crystalline substance is separated, which is the tartrite of manganese.

3. This solution is affected by metals in a manner similar to thè solution of iron. A piece of clean iron becomes quickly covered with rust, and an ochry matter separates. If the liquor be boiled in a copper vessel, some matter also separates of the same colour, and the surface of the vessel is evidently acted upon. Thus the analogy between these hepatised solutions and the water of this spring appears to me completely established; and it may be concluded that this water contains a triple compound, the basis of which is iron and manganese, and the solution of which is effected by hepatic gas.

X. ORIGIN OF THE MURIATIC ACID.

The coincidence between the artificial products and the natural waters of the spring is sufficiently proved: another coincidence remains to be considered, much more interesting and more unexpected, —the coincidence between the hepatised solution of iron and the oxygenated muriat of iron. I had almost concluded, from the resemblance between the properties of this salt and the phænomena of the water, that the water contains this very salt: now, I conclude, that they contain a matter, be it what it may, produced by the action of hepatic gas on iron. But they are the very same facts which form the basis upon which each separate inference is built: —does it not then follow as a necessary consequence that the hepatised solution itself contains a muriat of iron highly oxygenated, and that therefore *in this process muriatic acid is generated?* This conclusion seemed authorized by reason, and experiment has confirmed it. 1. I evaporated a small quantity of the solution (VIII. 1.) in a watch-glass to dryness: a bitter deliquescent salt is left behind: on this salt a little strong sulphuric acid was dropped, and paper moistened with ammoniac was held over the glass; white vapours were immediately formed over the glass: some volatile acid is, therefore, separated by the sulphuric acid. 2. I evaporated about eight ounce-measures of the same liquor, and, as before, dropped a little sulphuric acid on the residuum; in this case a strong effervescence was excited, very pungent acid fumes arose, which, from their smell, were readily known to be muriatic. The same truth was established beyond a doubt by holding a bit of paper moistened with simple water, which made the vapours visible in the form of a grey smoke—a distinguishing characteristic of the muriatic acid.* The evaporation had been performed in a copper vessel, except at its close; and though it was carried on very rapidly, the deliquescent matter had acquired a strong cupreous taste. 3. The hepatised solution of manganese (IX. 1.) evaporated to dryness leaves a deliquescent salt of a peculiar mawkish taste; and it shews the same signs of muriatic acid as the solution of iron, when treated with sulphuric acid in the same way. I have exposed black oxyd of manganese to oxygenated muriatic acid, and find that a deliquescent salt is formed which is affected neither by prussiat of potash nor tincture of galls;—alkalies separate from it (what I did not expect) *white* precipitates; tartrite of potash, a crystalline insoluble salt: all properties resembling the hepatised solution of manganese. 4. Common iron rust, purified by boiling in distilled water, was digested in hepatised water. In a day or two the hepatic odour is destroyed, and the liquor has properties similar to that which was formed with the iron filings. The same kind of deliquescent salt is left by evaporation, shewing the same appearances of muriatic acid. However, this liquor resembles the solution of manganese in precipitating nitrat of silver readily, and of a brown colour (IX. 2.) 5. I treated mercury in the same way: no gas escapes in this experiment, as it does with the iron;

* Bergman's Essays, Dissertation II. II. B. 3.

a black substance is formed; but the hepatic odour was not destroyed, though the hepatic gas was kept in contact with the mercury many weeks. After filtrating the liquor, I boiled it to expel the superfluous hepatic gas. A small portion of the liquor was suffered to evaporate spontaneously: a crystalline matter was left behind of an acrid taste. Another portion was evaporated, with intention to collect more of these crystals; but, by accident it was left exposed to heat too long, by which it became perfectly dry, and the residuum became quite black. A little sulphuric acid was dropped on this black matter, by which it effervesced strongly, and very pungent fumes arose which had all the properties of muriatic acid.

XI. FURTHER PROPERTIES OF THE OXYGENATED MURIAT OF IRON.

The facts I have related are unquestionable: it was in the latter end of the year 1795 that I first made the observation on the effect of hepatised water upon iron; since this time I have verified it repeatedly, and particularly in the month of December, 1796, with some very pure iron, and in the presence of two gentlemen, very competent judges, one of whom assisted at every part of the process. Still it has been asked, how is it possible that this solution can contain muriatic acid, seeing that nitrat of silver, that most delicate test of this acid, is hardly affected by it? To this it might, perhaps, be a sufficient reply, that it is unreasonable to oppose a mere analogy to the direct evidence of the senses; particularly in a new case, where we have found some of the analogies best established in chemistry actually to fail.—But let us recur once more to experiment.

I formed again some oxygenated muriat of iron. I suffered the acid to remain on the oxyd about twenty-four hours; then poured off the liquor, and evaporated the salt to dryness to expel the superfluous acid: the salt, which deliquesces instantly on cooling, was re-dissolved in a little distilled water. 1. I tried the solution with the acetite of lead: not the smallest cloud was produced. 2. The solution was then tried with nitrat of silver: a little white curdy matter was formed. 3. A tea-spoonful of the solution was diffused through two or three ounces of distilled water, and then tried with nitrat of silver: a very slight cloud was formed and a minute purple precipitate fell, but not till after some hours. The appearance was not so strong, nor the precipitate so copious, as when nitrat of silver is dropped into ordinary rain water.—These experiments evince that this salt either does not decompose the salts of lead and silver, or that the new compounds are soluble in water. The first is absolutely conclusive: as to the small appearance of decomposition in experiment 2 and 3, be it considered how difficult it is to prevent a minute quantity of common acid from passing over in the distillation of the oxygenated acid; and how readily this acid is itself decomposed: add to this the imperfect oxydation, perhaps, of the iron. If these circumstances are duly weighed, it seems probable, that this salt, when quite pure, would not at all sensibly decompose nitrat of silver. A slight impurity cannot be detected by acetite of lead, as a small quantity of muriat of lead is soluble in water.

XII. FURTHER CONSIDERATIONS ON THE HEPATISED SOLUTIONS.

Besides the oxygenated salts, I think it probable, that these solutions retain some sulphur; but, under what form, or in what combination, it is not easy to say. The residuum, after evaporation, has a peculiar smell, whereas the pure salts are inodorous. The stain left upon silver by the residuum ought, perhaps, to be attributed to this cause. Also the white matter, formed by the decomposition of oxygenated muriat of mercury, seems to be a combination of sulphur and mercury. In proof of this, it may be remarked, that the precipitate of this salt dissolved in simple hepatised water is white. Further, it is doubless true, that if hepatised water have a small quantity of acid mixed with it, the solution of iron strikes a purple colour with galls. To point out the origin of this colour, mix iron filings and sulphur not washed, and form them into a paste with a little water, and let them remain together some hours: put the paste into water and filtrate: this water now strikes a purple colour with galls. Now, common sulphur is always contaminated with a little sulphuric acid; and (as neither hepatic gas nor oxygenated salt are here concerned) the effect must be attributed to the acid and the sulphur. If this water be evaporated, it leaves a matter which does not deliquesce; but which emits the same smell as the residuum of the hepatised solutions. To shew that the acid is necessary to the production of the purple colour, let the sulphur be well washed with distilled water before it is mixed with the iron; and it will be found, that no such colour can be now produced. The following fact seems to prove, that sulphur may be retained in water in the form neither of sulphur nor of hepatic gas: it is an additional proof, how essentially the oxygenated differs from all the common salts of iron. I saturated a diluted solution of oxygenated muriat of iron, which scarcely affected nitrat of silver, with hepatic gas. A white precipitate fell, but so minute that it was impossible to collect it, nor did it destroy the transparency of the water: hence, I think it probable, that, if the salt were quite perfect, it would not be sensibly affected by hepatic gas. I now boiled the liquor, to expel the gas, till it wholly lost its hepatic smell. The liquor was again tried with nitrat of silver; and there was a copious deposition, but of a dark brownish colour. It seems certain then, that some sulphur is retained by the solution, which cannot be expelled by boiling.

XIII. THE NEUTRAL SALTS OF THE WATER: MURIAT OF MAGNESIA, MURIAT OF SODA, SULPHAT OF SODA.

A gallon of the water was evaporated to dryness; the deliquescent salts were separated from the non-deliquescent; and each of the salts which were thus obtained were carefully examined. Thus, by processes which are sufficiently known, it was found, that the gallon of water contains of muriat of magnesia 11.5 grains nearly; muriat of soda 430 grains; sulphat of soda 152 grains.

The triple compound, of which I have treated, is mixed with the deliquescent salt of magnesia, but not wholly; for it may be discovered with the non-deliquescent salts, though these have been separated carefully by spirit of wine. The tartrite of potash indicates it in both. Oxalic acid does the same thing, separating a white powder with some crystalline grains which are the oxalat of manganese.*

XIV. THE RESIDUUM OF DIFFICULT SOLUTION.

After these salts had been separated, there re-

* Bergman, Dissertation, VIII, 24.

mained a large residuum which was not soluble except in a great quantity of water. This has a crystalline form like sulphat of lime; and the usual reagents shewed it, in fact, to contain both lime and sulphuric acid. But the weight of this residuum, from a gallon of water, was no less than 112 grains: a larger quantity than could be dissolved in a gallon of water, if it were pure sulphat of lime. If it be considered, that the water requires some evaporation before these crystals begin to separate, the proportion is still more increased. There must be, therefore, something peculiar in the composition of the salt, or in the soluble powers of the water. Other experiments shew the same thing. 1. Sulphuric acid dropped into the water precipitates copiously sulphat of lime. This cannot be effected by the decomposition of muriat of lime; since we have already seen that no such salt is to be found (XIII.). Indeed, it cannot exist in the same solution with sulphat of soda, as these salts decompose each other.* 2. Sometimes a more unexpected

* It is astonishing that this fact should have been neglected, and that in recent publications. Mr. Schmeisser, in his Analysis of the Waters of Kilburn Wells (published in the Philosophical Transactions for 1792), has joined together sulphat of soda, sulphat of magnesia, and muriat of lime, as being contained in those waters. Dr. Garnett has also put into the composition of the sulphur-well at Harrogate sulphat of magnesia and muriat of lime; an er-

appearance than this takes place. It is, that a precipitate, seemingly like the former, has been made by the addition of muriatic acid: but by the addition of more acid the precipitate is re-dissolved. Sometimes, indeed most commonly, I have not been able to effect this appearance.

It was natural to expect the solution of any further uncommon observation in the same matter which had already explained so much. 3. I accordingly digested sulphat of lime in the hepatised solutions of iron and manganese; and I found that the latter had a very strong solvent power. After the liquor had been filtered, sulphat of lime was plentifully precipitated by sulphuric acid. The solution of iron seems to have something of a similar property; but as it is very small, and as iron has almost always a little manganese united with it, it is, at least, uncertain whether the whole effect ought not to be attributed to manganese.

It remains to compare this remark with the effects of the artificial oxygenated salts; and thus to confirm, if any confirmation were needed, the analogy which I have laboured so much to establish. 4. Sulphat of lime was digested with the oxygenated muriat of manganese and distilled water by a gentle heat: after twenty-four hours the clear liquor was separated:

ror the more unaccountable, as Bergman has expressly remarked this decomposition in his Dissertation on the Analysis of Waters. See Dissertation 11. 7. M.

into this I dropped a little sulphuric acid; by degrees a large quantity of sulphat of lime was separated. Muriatic acid was dropped into the same liquor, but it did not separate any thing. The oxygenated muriat of iron possessed the same property, but in so small a degree, that here again I am inclined to attribute this power to a little manganese attached to the iron. It follows from these facts, that the large quantity of sulphat of lime is kept in solution by the salt of manganese. And a further examination of the residuum itself shews that it contains the triple salt of manganese and iron. 5. Some of the residuum was perfectly edulcorated and sulphuric acid was dropped upon it: the vapours of muriatic acid arose, and were rendered evident by paper moistened with ammoniac or with simple water. To the sulphuric acid, which was used in this experiment, was added some distilled water and the liquor was filtrated: it was then saturated with an alkali; a small precipitate fell, which was proved (by the usual methods) to contain both manganese and iron.

6. I have noticed a variation (2.) in the effect of muriatic acid when added to the water. I have observed a similar variety in the residuum itself; which is, that sometimes it has been found soluble in the muriatic acid: when this happens, the addition of an alkali precipitates the residuum in its original crystalline form, and this it does before the acid is sa-

turated. But most commonly the muriatic acid does not dissolve it at all. Further, it has been said above (VII. 1.) that the acids precipitate a crystalline substance from oxygenated muriat of iron. This substance I have found to contain manganese. But what belongs to this place to observe is, that it is not always, indeed it is but rarely that this effect can be produced in any great degree; in a very small degree it may be always observed, but when I first remarked it, the precipitate was very copious, so that enough was readily collected for examination by the blowpipe. The salt of manganese does not shew this appearance in the smallest degree. It depends, therefore, on some peculiarity of the iron rust, but precisely on what I cannot undertake to determine.

As the oxygenated salts unite with all the other salts of the water, and consequently cannot be separated by spirit of wine, I have found it impossible to determine the quantity of them.

I had myself concluded from the experiment (1.) that this water contained muriat of lime; and the following remark confirmed me in my error: I mention it, as I think it probable that others have been led into mistakes from the same cause. I reduced some of the water, by evaporation, to about two ounce-measures; taking for granted that, by this process, nearly the whole of the sulphat of lime was separated. By adding sulphuric acid to this liquor, 20 grains of sulphat of lime were precipitated. I concluded, therefore, that this must have proceeded from the decomposition of muriat of lime: in truth, this sulphat was dissolved in the two ounce-measures of water, and was separated by the decomposition of the oxygenated salt.

ON THE WATER OF THE OLD BATHS.

The spring which supplies these baths was discovered in the year 1786, in which year two baths, a cold and a warm bath, were made. Upon sinking the well, a rock was found at the depth of eighteen feet; and the water rises from about the depth of three feet within the rock.

XV. THE GASEOUS FLUIDS.

I could obtain very little gas from this water, not more than three cubic inches from a gallon. This gas was azotic. An hepatic smell is perceptible, when the water is fresh. To the hepatic gas, doubtless, it is owing, that no oxygen is found in this water or in that of the other spring. Dr. Garnett has so well explained the cause of this circumstance, that it is needless for me to enter into it.*

* Garnett on the Mineral Waters of Harrogate, p. 74, &c.

XVI. THE SPONTANEOUS PRECIPITATE.

This water is pellucid when it first rises from the spring; in small quantities it does not lose its transparency; a very small sediment is deposited by boiling, so small indeed that sufficient cannot be collected in this way for examination. However, when the bath has been newly filled, in some hours the transparency of this large body of water is destroyed by exposure to the atmosphere, and it contracts a whitish colour. I collected a large quantity of this precipitate from the bottom of the bath; where it by degrees accumulates, having been deposited from a large body of water, which is very frequently renewed. 1. I first attempted the analysis of this powder by acids, but was disappointed. It is hardly soluble in any of the mineral acids: they all take up a little of it, make a brisk effervescence, and excite heat when first applied; but neither by a long digestion, nor by boiling, could I saturate the acids; nor, by putting a very small quantity of the powder into a large quantity of acid, could I completely dissolve the powder.

2. But, by the use of the blowpipe, it readily appeared, that this sediment is no other than the oxyds of the two metals so frequently mentioned, iron and manganese. It may be made magnetic; it gives the hyacinthine colour to borax, the colour is destroyed by continuing the fusion, and may be re-

newed by removing the globule to a silver spoon: fused with soda and nitre it makes a blue or a green globule. As the sediment may be procured in sufficient quantity, I repeated the last experiment on a larger scale: some nitre being mixed with it, the mass was pulverized and fused in a crucible; when taken out of the fire it was green, and dissolved in water, to which also it imparted a fine green colour: in a day or two a yellow ochre was deposited, when the solution became blue; from this liquor a powder subsided by exposure to the air, which was manganese.*

3. Pursuing the observations of the effects of the mineral acids confirms this conclusion. I mixed some of the sediment with powdered charcoal, and exposed the mixture to a strong red heat: it became of a light brown colour, and now proved to be readily soluble in all the mineral acids.† With the muriatic and nitric it formed a gelatinous compound. The sulphuric acid diluted was soon saturated; the liquor was evaporated, and deposited white crystals, the form of which is rhomboidal. This is a triple salt, the base of which is iron and manganese. If there be a small excess of acid the taste of this salt is very like that of sulphat of argill. The same salt may be obtained by boiling the sulphuric

* See Scheele's Essay on Manganese, XXXVI. B.
† Ibid, XXXVIII. A.

acid with the sediment itself, and continuing the boiling till the mass is become dry: the salt may then be procured by lixiviating the dry mass, and crystallizing the solution.

XVII.

Neither sulphat of argill nor oxygenated muriat of mercury are at all decomposed by this water.

XVIII. THE METALLIC SALTS.

All the appearances which demonstrate the existence of peculiar metallic salts in the waters of the new baths are also to be found in this, as the oxydation and solution of metals and a copious precipitate by galls, while the prussiat of potash is not affected: (IV.) and the same experiments were repeated to shew the presence of the oxygenated salts of iron and manganese (V. VII. &c.) with the same result, and authorize the same conclusions. Still, I believe, there is some difference in regard to these salts between the two waters. 1. I have already observed (XVII.) two points of distinction; and, as the second of those experiments is probably an indication of sulphur, this water seems to be without it. This is confirmed by evaporating the water in a silver vessel to which it communicates no stain. However, 2. its action on copper is very strong, so that, if it be boiled in a copper vessel for a long time, a blue oxyd of copper is separated from the vessel. But here, again, there is a difference between this and the former water; for though copper is dissolved in it, none can be precipitated on iron in its metallic form, as we have seen (IV. 1.). The iron in this water does not seem to be in that high degree of oxygenation that it is in the other. This I infer because, 3. the precipitate formed by tincture of galls is of a much darker colour, even when the water has been much evaporated; sometimes, when the water has been reduced to half its original bulk, I have remarked even a very slight green tinge communicated by prussiat of potash, but not till it has been added to it many hours.

As the oxygenated salts are formed by the action of hepatic gas on the metals, it cannot be doubted that they are very common. Bergman observes, "that cold martial waters, when fresh, almost always have an hepatic smell:"* it seems very probable then that this salt of iron may be found in almost all such waters. I doubt not that it has been frequently mistaken for muriat of lime, to which in its properties it approaches very nearly.

Though I have purposely avoided all medical discussions in this essay, I cannot abstain from bestowing a moment's consideration on one very obvious question. What, it will be asked, are the medical properties of manganese? Is it useful?

* Bergman, Dissertation VII. 6.

Is it innocent? Is it noxious? That it is innoxious, I certainly know. Dr. John Johnstone (Essay on Mineral Poisons, p. 134.) has shewn that it may be taken in large doses without injury; and he has informed me, that he has since confirmed the same fact frequently. I wish I could as well answer the first question; but what the medical virtues of this substance may be, is a subject which still remains in a great measure unexplored. It is certainly well worth the attention of men of science. To those who are inclined to labour in this field I take leave to suggest, that they should use either the carbonat or some other salt of manganese: the black oxyd, I apprehend, must be hardly soluble in the human fluids.

XIX. THE NEUTRAL SALTS.

These are the same as of the other water, but in different quantities. The gallon contains of muriat of magnesia 58;—muriat of soda 330;—sulphat of soda 62 grains.

XX. THE RESIDUUM.

A still larger residuum is obtained, after the separation of the neutral salts, from this than from the other water. The gallon leaves 146 grains. Its properties are the same as of that already described (XIV.).

It is to be observed, that both these springs are affected by rainy weather; and that, consequently, their contents vary considerably according to the seasons.

SYNOPTICAL TABLE OF SUBSTANCES CONTAINED IN THE TWO SPRINGS.

Gaseous fluids contained in a wine-gallon in cubic inches.

	WATER OF THE NEW BATH.	WATER OF THE OLD BATH.
Hepatic gas	*too small to be measured.*	*too small to be measured.*
Azotic gas	3·5	3
Carbonic acid gas	·5	

Solid contents of a wine-gallon in grains.

	WATER OF THE NEW BATH.	WATER OF THE OLD BATH.
Carbonat of iron ..	·75	
Oxyds of iron and manganese		*too small to be weighed.*
Oxygenated muriat of iron & manganese	*unknown, but very small*	*unknown, but very small.*
Sulphur	*unknown, but very small.*	
Muriat of magnesia	11.5	58
Muriat of soda....	430	330
Sulphat of soda....	152	62
Sulphat of lime....	112	146

Extract of a letter to DR. HOLME, *dated September* 25th, 1797.

In the analysis of the waters of the new baths, I have conjectured, that the decomposition of oxygenated muriat of mercury is occasioned by a minute portion of sulphur, attached to some of the substances dissolved in the water (111.). Since writing that paper, I have attempted to verify this conjecture; and not without success. Wishing to collect some quantity of the precipitate, I evaporated a gallon of the water to half its bulk; but found, that now the salt of mercury was dissolved without decomposition. I added, therefore, the salt to the water without boiling; and suffered the precipitate to subside. By this process, I could collect no more than a grain from a gallon of water. I threw this upon alkali heated to redness; but the whole instantly evaporated with a dense smoke. I mixed, therefore, another portion (procured in the same manner) with alkali; and heated them in a crucible: still I failed to collect any sulphur from the alkali (as I had hoped); but I now perceived that, as the crucible became hot, the matter burnt away with a blue flame, as sulphur does.

Sulphat of argill is not decomposed by this water when it has been reduced by evaporation: however, it gradually separates some of the abundant sulphat of lime; which is probably caused by its attracting the water which held it in solution.

I think it right here to observe, that I have recently met with this water in such a condition, that it caused a permanent decomposition of sulphat of argill. This precipitate is extremely minute: I have not as yet determined the cause of it, but I suspect it to be carbonat of magnesia.

WILLIAM LAMBE.

Some Account of the PERSIAN COTTON TREE. *By* MATTHEW GUTHRIE, M. D. F. R. S. *&c. &c.*

Communicated by Dr. PERCIVAL.

READ OCT. 4, 1793.

Cotton is a plant of both the old and the new world. At least it is found wild in both; but I have my doubts whether it was a native of America before the Europeans carried it over; and shall assign reasons for my incredulity when I come to treat of the *Persian Cotton*; which is the very species that is said to be American.

Five species of the cotton tree are enumerated by Linnæus; and there is reason to suspect the existence of a sixth, if what we are told of the extreme fineness and silky nature of a particular kind, reared in some of the Antilles, be literally true. This curious variety is called Siam Cotton; because the reed was originally obtained from Siam.

The first species of cotton is the GOSSYPIUM ARBOREUM, or INDIA-COTTON-TREE; which has been cultivated and manufactured in the East Indies, from the remotest period of the authentic history of that country; or between three and four thousand years. It delights in a sandy soil.

The second species is the GOSSYPIUM RELIGIOSUM; which is, likewise, a native of India; and a tree, or, at least, a high shrub; but why Linnæus dignified it with so singular a specific name, I shall leave the learned Asiatic Society in Bengal to determine; as they must know if it be used for any religious purpose by the Bramins. This species of cotton is said to be that which is cultivated by the French in Martinico.

The third is the G. BARBADENSE, a species of biennial cotton shrub, cultivated in our British island of Barbadoes; from which it obtains its specific name. I believe it is likewise the same species which is cultivated in Jamaica.

The fourth is the G. HIRSUTUM; an American *perennial* cotton shrub in the warmer provinces, but *annual* in the colder; as is sometimes the case with plants in climates where their roots lose their vegetating power by winter frost.

The fifth and last species is the G. HERBACEUM, or G. ANNUUM; an annual cotton plant; which rises to the height of three or four feet, and is sown and reaped, like corn, twice a-year in hot countries, and once a-year in colder climates. It bears a large yellow flower with a purple centre, and fruit about the size of a walnut containing the cotton.

This is the famous PERSIAN COTTON, properly the subject of the paper; although a slight mention of other species was necessary to give a more complete view of the subject. Linnæus calls it a native of America, and there is no doubt but that it is become so; although there is much more reason to suppose America naturalized a Persian plant, than that Persia got it from the new world: especially if we are to credit a paper lately presented, by a British merchant, to the Œconomical Society at Petersburg, in which it is positively asserted, that several of the European nations furnished their American colonies with *Persian Cotton*-seed, procured at Smyrna. Now this fact (if sufficiently authentic, which I do not doubt from my knowledge of the veracity of the author) will easily account for the G. herbaceum being found wild in America: when we recollect the wonderful provision of nature for the wide dispersion of seeds; and Linnæus's assertion, that the *Erigeron canadense* was dispersed from the botanic garden of Paris, by the winds, over a great part of Europe, and several other plants,* from the botanic garden of Upsal, over a whole province.

My reason for suggesting these doubts, relative to the native country of this species of cotton, are, that all vegetables of this genus are supposed to have been indigenous in Persia exclusively; and that even the East Indies derived

* The Antirrhinum minus, the Datura stramonium, the Gnaphalium americanum, &c.

the cotton plants from thence: a conjecture which seems to have acquired some degree of credit from the late discovery of Sir William Jones: viz. that the Hindoos, or inhabitants of India, were originally a colony of the ancient Iran or Persia, which seems to have been the cradle of the human species, since its ancient language appears to have been the mother of all those now existing (with the exception of the Arabic and Tartarian), of which, nevertheless, it contained many words.

Now, it is very possible, that the first colony carried the cotton plant with them to India; and that it was afterwards dispersed from Hindostan to the adjacent countries and islands. The cotton plant is widely dispersed likewise throughout Europe and some parts of Africa; particularly the annual or herbaceous species (the very plant treated of here) reared in the north of Persia, and which is also cultivated in Malta,* Sicily, Chio, Lemnos, and other islands of the Archipelago, although possibly the cotton of these islands may be varieties of the species, from difference of soil, climate, &c.

The best of the European cotton is brought from Cyprus; but Smyrna, Aleppo, Damascus, Jerusa-

* There is a kind of cotton cultivated in Malta, of a nankeen colour, which exceeds in fineness all other cotton and is much superior even to that from the Antilles.

lem, &c. furnish likewise a quantity of cotton, at least, equal to the European.

CULTIVATION OF COTTON IN PERSIA.

The annual cotton, or this last species of which we have treated more amply, is much cultivated in the northern or colder provinces of Persia, bordering on the Caspian Sea (as the perennial is in the southern) and it is from thence that the seeds now sent to Portugal have been obtained through the Bucharian Tartar merchants, and are the production of the *Gossypium herbaceum* of Linnæus, the *Gossypium annuum* of Pallas. It is sown in Persia from the end of March to the end of April, and reaped in September. This species requires a rich soil mixed with sand; and, therefore, where the land is not rich enough, they manure it with cow or sheep dung: although we are told, that when the plants are once raised above the ground any species of soil will answer. The ground is worked in the spring, and the seeds are planted at the distance of eight or ten inches from one another, whilst care is taken to weed it to give air to the young plants. Dry summers give the best crop, as rain is more particularly hurtful when it falls in great quantities during the flowering and ripening of the cotton. It is gathered, as said above, in September, care being always taken to collect a sufficient quantity of seed for the next year. Lastly, watering the young plants with a mixture of wood-ashes and water, in certain situations, is sometimes necessary to guard them from destructive worms.

The Russians have cultivated the same species of Persian Cotton in the government of Caucasus, and rear enough of it to serve their own national manufactures, which are not as yet either numerous or considerable; but on the Terck, at the foot of the Caucasus, where it is reared, they do not sow till the middle of May, lest a late spring-frost, which is sometimes felt in those parts, should destroy the hopes of the planter.—With that one exception, the Russians strictly observe the Persian mode of cultivation.

There is a species of silky cotton much cultivated at present in Germany, which possibly may merit the attention of Portugal for their plantations in America. It is the *Alclepias syriaca* of Linnæus, and affords so fine a species of cotton (if I may so name it) that fabrics have been erected in Saxony where stuffs are made of it, which rival in lustre, &c. the true animal silk. But this new vegetable silk has circumstances attending it that seem to recommend its cultivation in some of the American colonies and islands. First, because it is originally the native of a hot climate, as Linnæus's specific name indicates; and, of course, it is likely to be in its greatest beauty and excellence in climates which approach nearest to that of its native country. Secondly, because its stalks afford a coarse sort of cloth well calculated to clothe negroes, whilst from the pith of them paper is made.

Experiments and Observations on the Preparation and some remarkable Properties of the OXYGENATED MURIAT *of* POTASH. *By* MR. THOMAS HOYLE, JUN.

READ NOV. 10, 1797.

Having an opportunity in preparing the oxygenated muriatic acid for the purpose of bleaching, by a small extension of the apparatus, to prepare likewise the oxygenated muriat of potash, and to make experiments on that substance, I have been induced to digest the most material facts and observations which occurred, and to lay them before the society: especially as I do not find much on the subject in the writings of others, and as many have probably been deterred from the investigation by the exorbitant price of the article, and by some apprehensions of danger attending it.

A few experiments, which are not new, have been introduced, in order to bring under one point of view the principal chemical facts which relate to this salt. I have given, in most cases, an exact account of the quantities of the different ingredients composing the mixtures; and as persons not much accustomed to such experiments may be inclined to repeat some of them, I would caution them *not to use greater quantities than are here specified*, particularly where the terms violent detonation, explosion, &c. are employed.

I would not by any means wish it to be understood, that I have exhausted the subject: many more experiments, and much labour and assiduity, are required before the nature and uses of so active a substance can be fully ascertained.

I find it has been introduced into medicine with success; and I hope its good effects in that respect will not be frustrated by the high price of the article, as it may be procured at a much cheaper rate than it is commonly charged.

I. ON THE PREPARATION OF THE SALT AND ITS SOLUTION IN WATER AND THE ACIDS.

Finding that a quantity of gas escaped occasionally from our apparatus for making the new bleaching liquor, more especially when the fire was not properly managed, or when, by any other means, a greater quantity of gas was produced than the liquor could absorb; I thought it would be useful to adapt to the large apparatus a smaller one, in which this superfluous gas might be condensed; as the escape of it was sometimes disagreeable to the workmen. This I did by filling an earthen-ware bottle with a strong solution of potash in water, (consisting of about three pounds of alkali to the gallon) which I found entirely relieved us from the disagreeable smell we frequently experienced before; and, at the same time, yielded a considerable quantity of the oxygenated muriat. Though the production of the oxygenated muriat in this way be somewhat precarious, depending upon the management of the person who conducts the process; (it being the bleacher's interest to condense the whole of the gas in the liquor he wants for his business) yet I think, if the portion which commonly escapes were thus disposed of a considerable quantity of this salt might be made by bleachers with little additional expence, except what is incurred by the purchase of the alkali, and some more labour and attention. At some of my first trials, about two years ago, I found the gas which escaped from the materials of one distillation sufficient to saturate two gallons of the alkaline solution, from which I procured about six ounces, and sometimes more, of the salt, after being purified by several chrystallizations. But, having made some alteration in the apparatus, I now find, that the same quantity of alkali may remain for three or four distillations, before sufficient gas be furnished to form the salt; except the person employed be remarkably inattentive to his duty. I consider this as a valuable improvement, the making of the salt being only a secondary object. The salt was chiefly formed during the distillation. The alkali became warm towards the latter end of the process, especially if the absorption of gas was very rapid, a quantity of caloric being disengaged. In this case, a considerable part of the salt soon crystallized on the lixivium being set in a cool place, and a great deal of gas appeared to escape, which on one occasion I collected, and found that it precipitated lime from its solution in water, and extinguished flame: and, therefore, though it had a slight smell of the oxygenated muriatic acid gas, I believe that it consisted chiefly of carbonic acid, as the former occasions no precipitation of lime water, which the latter uniformly does. A glass jar, containing 32 ounce-measures of this gas, being left over water one night, was reduced to about one fourth its bulk. The gas that remained seemed to contain more oxygene than the air of the room; two measures of it, with one of nitrous gas, gave 1.53, whilst an equal quantity of common air, gave 1.9.

Before any of the salt appeared to be formed in the alkaline solution, I have constantly observed a quantity of earthy matter to be precipitated. This was carefully separated from the salt, and, after being washed repeatedly in boiling water, was suffered to dry; but not having examined it with sufficient minuteness to say what it is, I shall content myself at with stating some of its properties. It did not detonate with sulphur, and was totally or nearly insoluble in water. The sulphuric acid dissolved it, and gave evident signs of muriatic acid, which appeared to be slightly oxygenated. After being exposed to a red heat for half an hour the above properties still appeared the same, except with the sulphuric acid. I thought the gas that was disengaged had more of the smell of simple muriatic acid gas, though along with it a little of the oxygenated gas might be perceived. The muriatic acid did not appear to dissolve any of this substance either before or after its calcination. With the nitrous acid, a strong smell of the oxygenated muriatic gas was produced. From a dram of this substance, in an earthen retort exposed to a strong heat, about six ounce-measures of gas were produced, consisting of a mixture of carbonic and azotic gas, the latter of which was in the greatest quantity, forming, by estimate, about three fourths of the whole.

The form of the crystals that first appeared in the solution of alkali were quadrangular plates: what were afterwards formed, when the lixivium became cool, were needle-like, as were those that were produced by spontaneously evaporating the remainder of the ley: they appear to have the same property of detonating as the first. These different forms of crystals appeared on dissolving the salt in hot water, and, when cold, seperating the salt and suffering the water to evaporate spontaneously.

I frequently observed, that unless the alkali began to part with a considerable portion of gas, without the admission of any from the apparatus, that little or none of the oxygenated muriat was procured; and that as this gas (which I have before observed to be chiefly the carbonic acid) escaped, the crystallization took place, and increased or diminished according to the evolution of that gas. This I found uniformly the case whether mild or caustic alkali was employed. A given quantity of the strong solution of potash appeared to produce more of this salt than the same quantity of a solution of pearl-ash of the same specific gravity.

The remaining lixivium, on evaporation, did not yield this salt, though a muriat of potash was formed that appeared to be considerably oxygenated: since, with the addition of the sulphuric or muriatic acid, it became a very powerful destroyer of vegetable colours; it would not detonate with sulphur or inflame combustible substances with acids; it was very soluble in water, much more so than the muriat first formed from the same alkali.

I may here remark, that I think the French chemists were right in calling the first salt the hyperoxygenated muriat, as the salt last mentioned is certainly oxygenated in some degree; however, in the following experiments I shall use the term oxygenated muriat, when speaking of the salt formed during the distillation, and on cooling the lixivium after being saturated with the gas.

EXPERIMENT I.

One part of the oxygenated muriat of potash required about seventeen parts of water, at the temperature of 60°, to dissolve it; whilst five parts of boiling water dissolved two of the salt. Repeated solutions did not appear to injure but rather to increase its detonating property. The crystals became much whiter; and a quantity of the earthy matter, before mentioned, was separated at every fresh crystallization.

EXPERIMENT II.

A quantity of this salt was put into a bottle and placed in a situation much exposed to the light: after being kept there more than twelve months, it did not appear to have lost any part of its detonating property. This fact is contrary to Chaptal's assertion, that the mere impression of light is sufficient to decompose it.*

EXPERIMENT III.

Water saturated with this salt was exposed to the light, for several months, without appearing to be

* Elements of Chemistry, I. 250.

at all changed. It was put into a bottle with a ground stopper and tube, to which an apparatus was adapted to receive any gas that might come over; but no gas whatever was disengaged.

EXPERIMENT IV.

Sixty grains of the salt were fused, by the heat of a lamp, in a bottle with a ground stopper and tube. After having been kept in a fluid state for about half an hour, I found that it had lost two grains in weight, and that a small quantity of air was given out, which proved to be oxygenous, by the test of nitrous gas. The salt which had been melted would still detonate with sulphur, &c. The loss of weight was, I am inclined to think, chiefly owing to the escape of the water of crystallization; for the salt, when cool, had lost its transparency.

EXPERIMENT V.

From forty grains of the salt in an earthen retort, I procured, by the application of heat, about thirty-six cubic inches of oxygenous gas; the evolution of which was very rapid and commenced as soon as the retort became slightly red. Forty grains, exposed in a crucible to a strong red heat, appeared, from the mean of two experiments, to have lost about seventeen grains in weight: the remaining muriat, being afterwards thrown into the sulphuric acid, produced a very strong smell of oxygenated muriatic acid; from which I inferred that the whole of the oxygen had not been expelled by the heat; whence the oxygenated muriat of potash may, I think, be stated to contain about half its weight of oxygen, in a concrete state.

EXPERIMENT VI.

Strong nitrous acid disengaged the oxygenated muriatic acid from this salt. During the solution of two or three grains of the oxygenated muriat in this acid, a grain or two of phosphorus was dropped into the glass containing the mixture, when a number of vivid flashes appeared in the liquor, darting forth at intervals, for a considerable time. This is one of the most striking experiments I ever saw; but a little caution is necessary in performing it, the phosphorus being sometimes thrown out of the mixture.*

EXPERIMENT VII.

The muriatic acid dissolved this salt, a great deal of the oxygenated acid being given out. A few grains of the salt added to an ounce of the acid rendered it a very powerful destroyer of vegetable colours. This mixture may probably be used with advantage in taking stains of ink, &c. out of linen or cotton.

Phosphorus added to this acid along with the

* This curious experiment was first noticed by J. Collier, and was communicated by him to the Society, some time ago.

salt did not produce the same effect as with the nitrous acid; no light appearing as in the last experiment.

EXPERIMENT VIII.

On putting a little of the salt into the sulphuric acid, a violent crackling, or a great number of small explosions took place, and a very strong smell of nitrous gas was produced, the mixture at the same time assuming an orange colour, which disappeared after it had stood a short time. A very small piece of phosphorus having been dropped on about two grains of the salt (previously thrown into the acid) an explosion immediately took place, which blew out a great part of the mixture upon my hand; an accident that might have proved serious if I had not had water near me.

EXPERIMENT IX.

Finding a great quantity of gas to be disengaged from this salt by the sulphuric acid, which had a very strong smell of nitrous gas, I put forty grains of the salt into a glass retort and poured upon it nearly an equal weight of sulphuric acid diluted with water. With the heat of a lamp the gas began to come over very rapidly, and was received in a glass jar placed in a bason of water. A considerable portion of it appeared to be absorbed by the water, which acquired a yellowish colour. This colour disappeared on standing a few days, and a brown matter was deposited, which being carefully collected and dried, weighed one grain, and appeared to be manganese; for, a little of it being put into the muriatic acid, so far oxygenated it, that it would destroy the blue colour of a diluted solution of indigo in the sulphuric acid. The precipitate before mentioned, that was first produced in the alkali employed, did not appear to have this effect. The quantity of this sediment that I had an opportunity of collecting was so small that I could not try many other experiments with it; indeed, I did not always succeed in procuring it, for I found that unless the disengagement of the gas was very rapid, but little of it could be obtained.

EXPERIMENT X.

On two drams of the salt in a glass retort, I poured an equal weight of sulphuric acid diluted with a little water, and adapted the retort to Woulfe's apparatus. The heat of a lamp was applied, and presently the gas began to escape, and was absorbed by the water in a considerable quantity; to which it communicated a yellowish colour, and a liquid began to trickle down the neck of the retort into the receiver. This had continued but a short time before a violent explosion took place, which broke the retort and two of the receivers to pieces, together with several other glasses which were on the table. This was several times repeated, but with more caution than before; and I always found that when the mixture acquired a certain degree of heat, an explosion certainly took place, except the retort had a pretty wide neck, and the neck was simply introduced into a receiver with a considerable opening in it without any lute; or, put into water, as in the last experiment: and, even in this case, I would not advise so much of the salt to be used at one time as is here mentioned. The small quantity of acid I was able to collect in this way, by adapting a loose receiver, appeared to be a weak muriatic acid slightly oxygenated; it was of a dilute purple colour, which disappeared on its being exposed a short time to the light; a small piece of iron dropped into it, caused it to become transparent immediately.

It was a matter of much surprise to me to find so strong a smell of nitrous gas produced, on decomposing this salt with sulphuric acid. Now as nitrous gas consists of azot and oxygen, supposing this to be nitrous gas (for I do not assert it to be so, though I should think the smell in this instance an almost sufficient criterion) whence comes the azot? At first, I thought it might come from a decomposition of the alkaline base of the salt; as some chemists have imagined the vegetable alkali to be composed of lime and azot; in that case, I expected the residuum would have been the sulphat of lime, but I found it to be chiefly sulphat of potash, with a little of the oxygenated muriat that remained undecomposed along with it. At present I shall not hazard any opinion respecting the origin of this nitrous smell; but hope some experiments I am at present engaged with, will, if I can find time to prosecute them, throw some light upon this subject.

II. ON THE DETONATION AND INFLAMMATION OF COMBUSTIBLE SUBSTANCES WITH THE SALT, PRODUCED BY FRICTION AND THE ACIDS.

The detonating properties of this salt were tried with various substances in the following experiments; the different mixtures were intimately combined by gently rubbing them in a stone-ware mortar; after this was done, one smart stroke across the mixtures would cause the whole of some of them to explode at once, and others successively by repeating the friction. The sulphuric acid inflamed most of these mixtures of the salt with combustible substances, the nitrous acid also had the same effect with some of them.

EXPERIMENT I. WITH PHOSPHORUS.

Half a grain of this substance rubbed with the same weight of the salt produced violent explosion with flame. I apprehend it would be dangerous to use much greater quantities, as the phosphorus is frequently thrown out with violence before it is consumed. The sulphuric acid inflamed this mixture as I have before stated.

EXPERIMENT II. WITH CHARCOAL.

Two grains of salt with one of charcoal intimately mixed, and perfectly dry, produced by a smart stroke a strong flame without much report. The sulphuric and nitrous acids inflamed this mixture, the latter with most rapidity.

EXPERIMENT III. WITH PIT-COAL.

A grain of dry pit-coal, rubbed with the same quantity of the salt, produced sparks and some small reports. With half the quantity of coal, the reports were much louder.

The sulphuric acid added to about twenty grains of the salt with ten of the coal, produced a bright red flame rising up to a considerable height.

EXPERIMENT IV. WITH SULPHUR.

A grain of the salt rubbed with half a grain of sulphur produced a very loud report, attended with flame and a strong smell of sulphureous acid. When the sulphur was reduced to a quarter of a grain, the explosion was not made at once as before, but successively. When the proportion of sulphur was increased to three fourths of a grain it produced a very loud report, much the same as the first; and the whole appeared to explode at once. Equal parts of sulphur and the salt did not cause so strong reports as when a less quantity of sulphur was employed: this mixture exploded successively. The sulphuric and nitrous acids inflamed it.

EXPERIMENT V. WITH SULPHURET OF POTASH.

One grain of the salt rubbed with the same weight of this substance produced a very loud explosion with flame. With half a grain of the sulphuret, I thought the report fully as violent. A little of these mixtures, melted over the fire, had not the effect of the fulminating powder made with nitre. It only emitted a flash without any report, nor was I able to produce a fulminating mixture by varying the proportions of the salt, alkali, and sulphur. The sulphuric or nitrous acids, dropped on this mixture, gave a very strong bright flame.

EXPERIMENT VI. WITH SULPHURET OF MERCURY. (*Cinabar.*)

Equal parts of this substance and of the salt detonated successively, by friction, a grain of each being used. A change of proportion appeared to weaken the detonating property of the mixture. The sulphuric acid inflamed this mixture, but not so rapidly as in the last experiment. The nitrous acid did not inflame it.

EXPERIMENT VII. WITH SULPHURET OF ARSENIC. (*Orpiment.*)

A grain or two of the salt rubbed with an equal weight of this substance produced little more than a flash: but a grain of the salt with half a grain of the sulphuret gave a strong report, though very little friction was used. Reducing the quantity of sulphuret to a quarter of a grain, the explosions were weak and successive. A larger quantity of this mixture, than is mentioned above, makes a report which is very unpleasant, with considerable flame. I was greatly surprised the first time I made the experiment with two or three grains of the salt and a portion of the sulphuret, by their exploding in a most violent manner, though a very slight friction had been used. The sulphuric or nitrous acids gave a very strong flame the moment they were dropped upon this mixture.

EXPERIMENT VIII. WITH COTTON-WOOL.

A small quantity of very dry cotton-wool was rubbed with a little of the salt; no detonation took place. The wool was afterwards dropped into the sulphuric acid and took fire immediately; but the nitrous acid would not inflame it.

EXPERIMENT IX. WITH LOAF-SUGAR.

One grain of this substance rubbed with two of the salt gave a number of successive reports. The sulphuric or nitrous acids, dropped on this mixture, instantly produced a strong flame ascending to a considerable height.

EXPERIMENT X. WITH FIXED AND ESSENTIAL OILS.

A few drops of spermaceti oil rubbed with a grain or two of the salt produced a number of loud reports. The sulphuric acid inflamed this mixture; the nitrous acid did not.

Olive oil, the essential oils of rosemary, juniper, cloves, carroway, aniseed, cinnamon, nutmeg, amber, mint, and essence of lemon were rubbed with the salt: all of them detonated successively, and such of the mixtures as were tried took fire with the sulphuric acid.

EXPERIMENT XI. WITH SPIRIT OF TURPENTINE.

A few drops of spirit of turpentine rubbed with a little of the salt detonated in much the same manner as the substances used in the last experiment. The sulphuric acid, dropped on this mixture produced a strong flame with a cloud of very black smoke.

EXPERIMENT XII. WITH CAMPHOR.

A little of this substance, on being rubbed with a grain of the salt, produced a number of successive denotations. The sulphuric acid produced flame with some explosions.

EXPERIMENT XIII. WITH ROSIN.

One part of this substance with two parts of the salt detonated successively when well rubbed together. The sulphuric acid inflamed this mixture, but the nitrous acid did not.

EXPERIMENT XIV. WITH GUM ARABIC.

The detonations were very slight. It was mixed with twice its weight of the salt. The sulphuric acid set fire to the mixture, but the nitrous acid would not.

EXPERIMENT XV. WITH PRUSSIAN BLUE.

No detonations whatever were produced by friction, nor did the acids inflame a mixture of this substance with the salt.*

EXPERIMENT XVI. WITH INDIGO.

Half a grain of fine Spanish Indigo, rubbed with a grain of the salt, detonated successively, like the

* Chaptal, (Elements of Chemistry, vol. ii. page 377) says, "Prussian blue takes fire more easily than sulphur, and detonates strongly with the oxygenated muriat of potash. (Quære, did he not make use of indigo here?)

mixture with rosin or gum. The sulphuric acid inflamed this mixture, but the nitrous acid did not.

EXPERIMENT XVII. WITH ETHER.

A few drops of ether on about two grains of the salt rubbed to a very fine powder, produced no detonation by friction. The sulphuric or nitrous acids poured suddenly upon it produced flame.

EXPERIMENT XVIII. WITH IRON-FILINGS.

These alone rubbed with the salt produced no detonation by simple friction; but two grains of the salt, one grain of iron-filings, and half a grain of sulphur, being well rubbed together, about a quarter of a grain of this mixture exploded violently with friction. The sulphuric acid added, caused a few sparks to appear; but the nitrous acid did not produce any. Varying the above proportions did not appear to improve the detonating property of the mixture.

EXPERIMENT XIX. WITH AURUM MUSIVUM.

Equal parts of this substance and of the salt detonated strongly with flame, on being rubbed together in an iron mortar; a very slight friction was necessary. The sulphuric acid gave a small flame, but with the nitrous I could not procure any.

From the foregoing experiments, I think, we may venture to conclude, that the oxygenated muriat of potash is equally harmless as common nitre; except it be brought into an intimate union with something that has a greater affinity with one of its constituent parts, than exists between those parts when combined in the salt, and that some combustible substance be present: but its oxygen being so easily disengaged, renders a little caution necessary; and, as the sulphuric or nitrous acids seem so readily to inflame many of the mixtures, I would not advise any person to make more of them than is necessary for immediate experiment. This precaution may prevent any unpleasant circumstance from accidental mixture with the acids, which appear to disengage a great part of the oxygen almost instantaneously.

I shall not say much about the theory of these detonations, none of the foregoing experiments having been so carefully conducted as to determine accurately what changes took place; yet, I think, we may attempt to explain some of them in the following manner. With phosphorus, the oxygen seems to combine, and form phosphoreous acid gas, or phosphoric acid: with sulphur, the sulphureous acid gas, or sulphuric acid, according to the rapidity of the combustion: with charcoal and other vegetable substances, the carbonic acid: with sulphuret of arsenic there may be sulphureous acid gas, and arsenic acid produced.

The sudden production of gas striking the surrounding air is, most probably, the cause of the loud reports produced by friction, &c. agreeably to the conclusions of Berthollet; and the muriatic acid may remain combined with the potash, and a portion of the combustible substance employed: but when the sulphuric or nitrous acids are used, the muriatic acid is certainly disengaged.

Since the above experiments were made, I have found that a paper has been read before the National Institute at Paris, on detonation by concussion, by citizens Fourcroy and Vauquelin.* They there mention some of the mixtures I have described, and their inflammation with the sulphuric acid. They likewise notice, that very loud reports and sparks were produced on a very small quantity of different mixtures being struck with a hammer on an anvil. This, on trial, I found to be the case; and a little cotton-wool well impregnated with the salt, being struck in that way, immediately took fire. But to get this to succeed, the salt and cotton should be perfectly dry: this is a necessary precaution in all experiments on the detonating property of this salt by friction, &c. In the paper above alluded to, it is stated, that sugar, the gums, fixed and volatile oils, alcohol and ether, do not detonate or take fire by simple trituration; but the experiments I made

* Annales de Chimie, tom. xxi. p. 235. Nicholson's Chemical Journal, I. p. 169.

seeem not to agree with this assertion: for all the above substances that I tried, except ether, detonated either more or less on rubbing them briskly in a stone-ware mortar; some of them required to be intimately mixed, as sugar and gum; but others produced very loud reports, as when fixed and essential oils were used.

Experiments and Observations on FERMENTATION *and the* DISTILLATION *of* ARDENT SPIRIT. *By* JOSEPH COLLIER.

READ FEB. 10 & NOV. 17, 1797.

We may lay it down as an incontestible axiom, says Lavoisier, that in all the operations of art or nature nothing is created; an equal quantity of matter exists both before and after the experiment; the quality and quantity of the elements remain precisely the same; and nothing takes place beyond changes and modifications in the combinations of those elements. Upon this principle the whole art of performing chemical experiments depends. We must always suppose an exact equality between the elements of the body examined and those of the products of its analysis.*

The subject of fermentation, but more particularly the fermenting infusion of malt, has been very little attended to as an object of chemical enquiry. Diodorus Siculus, Herodotus, and Tacitus mention a method of making wine from malt; yet the modern world had made very little progress

* Elements of Chemistry.

either in improving the process or ascertaining its products, until Dr. Priestley and the Duke of Chaulnes made their interesting experiments on the carbonic acid gas disengaged during the operation of fermentation. Soon after these important discoveries were communicated to the public, the accurate philosopher, whom I first quoted, instituted a set of most valuable experiments on this subject; and from their results established a firm base for farther enquiry. Before this period, all was undefined, and the chemist had nothing to direct his attention but scattered facts, many of which rather tended to confound than properly to conduct his investigation.

The distillation of ardent spirits has met a similar fate to that of fermentation. The Egyptians, at a very early period, discovered the art of drawing spirit from wine. Modern chemists have left it in the rude hands of the ill-informed, and accident has been the chief contributor to its improvement. It is true that some of the proof spirits of commerce are pure and good, but this arises more from the nature of the wines from which they are drawn than from the superior skill of the distiller. The sugar-cane and the grape are easily attenuated by fermentation, and contain less fœtid oil than malt; neither are they so liable to contract an empyreuma in the still. I was induced to begin this enquiry from a firm persuasion, that it was possible, by means of known chemical agents, to produce malt spirit equally pure with that which is obtained from any other modification of saccharine matter; and most of the following facts are confined to malt liquor. Many other particulars have occurred in the course of my experiments which I thought worthy of notice, and which I shall transcribe from my notes.—But I shall first make a few observations on fermentation in general.

Fermentation is an intestine motion in the solution of saccharine matter, whereby a new arrangement of its constituent elements takes place, and alcohol is produced, without the intervention of any other agent of any denomination whatever.

From the above definition it will appear that I mean to exclude all other operations from the denomination of fermentation, excepting that in which wine is produced, and, consequently, alcohol. Bellini says, that all things are full of ferments; and Helmont affirms, that fermentation is the sole cause of almost every transmutation; but the excellent Boerhaave was aware of the confusion which was likely to arise from general terms, and, therefore, limited the word fermentation to three distinct operations: the formation of alcohol, the making of vinegar, and the production of ammoniac; since which, a vinous, an acetous, and a putrid fermentation has been generally acknowledged,—the second as a consequence of the first, and the third as a consequence of the two former.

My present object is neither to enter fully into the investigation of acetous acid nor of the cause of putrefaction; but I shall make a few remarks upon each.

To produce vinegar (which is chiefly obtained from fermented liquor) the presence of an acid is necessary, such as the *tartareous acidula*; but it may be obtained by other vegetable acids, as well as from gums and amylaceous fœcula dissolved in water. A heat of from seventy to ninety degrees of Fahrenheit's thermometer, is also necessary to the production of vinegar; and fermented liquors will become acid under this temperature by the solution of the tartar only; yet a perfect vinegar cannot be formed in a close vessel. A free access of vital air is absolutely necessary to the production of acetous acid, but is highly injurious to the spirituous fermentation: add to this, that insipid mucilages or gums dissolved in water become acid without having been discernibly spirituous. Why then should the making of vinegar be called fermentation?

The production of ammoniac, which is called the putrid fermentation, arises from the texture of the solids being destroyed, and the nature of the fluids being altered; and it is a well-known fact, that many vegetable and other substances never undergo any sensible vinous fermentation, or are formed into vinegar previously to putrefaction. In some of my experiments on the fermenting infusion of malt, I have observed a beginning putrefaction without any previous acidity, which I attribute to an entire completion of the spirituous fermentation, whilst the air was excluded and the heat never sufficient to dissolve the tartar. It has been before observed that a heat of from seventy to ninety is required to form the acetous acid, but putrefaction will take place in a temperature of forty-five. The putrefaction of vegetables volatilizes and reduces them to an earthy state; but, to mark the phænomena of vegetable putrefaction, and to distinguish it from the putrefaction of animal matter, will form the subject of another essay, in which I shall endeavour to prove, that the production of nitre and the formation of vegetable mould, might with equal propriety be called fermentation as the production of ammoniac, both being products of putrefaction.

Fermentation must, therefore, be either considered as a general term, and supposed, according to the opinion of Helmont, to be the sole cause of almost every transmutation, or it must be limited to express some definite process by which one or more specific products are obtained. In the latter case, the most common application will certainly be deemed the most proper, viz. the production of alcohol, by an intestine motion, in the solution of saccharine matter, without the intervention of any other agent whatever.

Some chemists are of opinion, that insipid or gummy mucilages, barley, &c. pass by a sort of fermentation into saccharine matter; but of this I shall speak more at large when treating on the subject of malting.

No other but saccharine matter is susceptible of fermentation, and it is necessary to observe a proper proportion betwixt the density of the liquor and the quantity of yest; for, although artificial ferment is not absolutely necessary to produce fermentation, yet it has a tendency (as is well known) to accelerate its progress.

The heat of the liquor must also be regulated according to the density, as a greater or smaller degree of heat is excited during fermentation, according to the quantity of fermentible matter which it contains.

The phænomena which accompany fermentation, together with many other interesting particulars, may be found in an ingenious essay inserted in the second volume of the Memoirs of this society.* I shall, therefore, proceed to a description of the instrument by which I regulated the gravity of my worts.

The scale of my saccharometer corresponds to that of Blake's, but the form is materially different. It is so constructed, that a plate above the ball is sufficiently large to contain a scale of the intermediate

* Experiments and Observations on Ferments and Fermentation, by T. Henry, F. R. S.

gravities, including the strongest and weakest worts with which the experiments were made. It begins with 0, and proceeds by equal divisions to 80°; a density which cannot be properly attenuated without repeated fermentations with large quantities of yest. In the middle of the plate stands a thermometer, graduated from 0 to 100°; the use of which will be explained by and by. The divisions in the scale were made on a supposition that malt was about two pounds the quarter, whereby every varying degree that should be found in two worts, where equal quantities had been drawn from two different kinds of malt, would indicate a difference in value of so many sixpences.

The saccharometer ought to be accompanied by an assay vessel, which is nothing more than a cylindrical jar of tin.

When it is to be used, fill the assay jar with wort, and reduce it to sixty degrees of heat. Then leave the instrument at full liberty in the jar of wort, and it will rest at the proper degree of density, which will be found upon the scale.*

* I believe this saccharometer will be found pretty accurate; yet, "there are other principles in fluids besides their gravity, by which they resist the admission of a descending body, and the first of these is that of attraction; which operating powerfully on bodies composed of heterogeneous particles, must have considerable influence in such a fluid as wort. A tendency to coagulate in the higher temperatures, and an approach to congelation in the

If the liquor be not cooled to sixty degrees, one degree of density should be allowed for every additional five degrees of heat.

When the above regulations are attended to, the following will be found to be nearly the densities or gravities of some of the most celebrated malt liquors, both before and after fermentation.

Porter has sixty-six degrees of density before fermentation, but is reduced, by a proper fermentation, to twenty-six degrees of density.

Ringwood ale, seventy-four before fermentation, but is reduced to thirty after.

Dorchester ale, eighty-four before, and forty-five after.

Table-beer has forty degrees of density before fermentation, but only twenty-two after.

From the foregoing observations, the use of the saccharometer is sufficiently manifest, as it not only serves as a guide to the quantity of liquor to be drawn from malts of different qualities, but also to determine whether those liquors are properly attenuated by fermentation.

The first part of the experiments are arranged in the following order.

I. On the production of artificial ferments,

lower, are certainly co-operating principles in all gelatinous liquors.

See the Appendix to Richardson's Statical Estimates.

with a view to ascertain their relative values.*

* Boerhaave arranges, what he calls principal ferments, in the following order. 1. All those things which of their own nature are greatly disposed to ferment, so as immediately to begin this operation without any other ferment; such as the juices of ripe summer-fruits, which are so strongly disposed to ferment as scarcely to be kept quiet without the help of things that prevent fermentation. So likewise a paste made of flour and water, and laid in a warm place cannot then be hindered from fermenting. Hence, we need not be solicitous about a first ferment, because nature spontaneously affords it every where. 2. The recent flowers, thrown to the top of beer, in the act of fermentation; for, if this rarified frothy matter be mixed with other fermentable liquors it greatly promotes their fermentation. 3. The same matter now become heavier and sunk to the bottom, provided it be not too stale, still retains the same virtue, though in a less degree than the former. In this state it is called lees; and being by motion mixed with its own wine, it often occasions a new fermentation, and will excite it in other subjects. 4. Cassia, honey, manna, sugar, and the like inspissated juices. 5. And paste of flour fermented, or baker's leaven: for, though meal may be preserved, for years, fresh and sweet, in a dry place, and kept from insects; yet, if wrought with water into a soft, sweet, and close paste, and lightly covered in a warm place, it will, in an hour's time, begin to heave, swell, rarify, become all-over full of cavities, change its smell, taste, and tenacity, prove acid both to the taste and smell, and thus becomes that proper ferment, which gave the original name of this whole operation; because, when thus prepared, if a part of it be mixed with other fresh paste, not yet fermented, it now causes it to ferment much sooner and stronger. 6. The remains of former ferment-

II. Whether the fermentation ought to be carried on in open or close vessels.

III. The effects of different factitious airs on fermenting liquors.

I. From a considerable number of experiments on artificial ferments I shall select the six following.

1. I took four ounces of wheat flour and a pint of water. The water, when mixed with the flour, was about eighty degrees of heat, and the mixture was placed in a temperature varying from sixty-five to seventy-five, for five days.

2. To a mixture similar to the above, and of the same degree of heat, I added an ounce of muriat of soda, (common salt) which was also left in a temperature varying from sixty-five to seventy-five, for five days.

3. The same proportions of water and flour were mixed as in the first and second experiments, and put into Nooth's apparatus, and subjected (as in T. Henry's Experiments) to the action of the carbonic acid gas for five days.

4. A quart of strong infusion of malt * was plac-

ing matters, sticking to the sides of casks, every way penetrated by the subtilty of the wines they before contained, become extremely apt for raising a quick and violent fermentation in fresh liquors put into them. 7. The white of eggs beat up to a froth, &c.

See the Practice of Chemistry. p. 108.

* The method proposed by W. Mason, (which is inserted in the Transactions of the Society for the Encou-

ed in the same temperature for an equal length of time.

5. To another quart of malt liquor was added three ounces of a strong infusion of hop, and treated as before.

6. An equal quantity of the infusion of malt was put into the middle part of Nooth's apparatus, and subjected (like the flour and water) to the action of the carbonic acid gas, for five days.

All the foregoing compounds began to ferment about the same time (four days after the mixture was made); but in order more fully to assertain their real value, I took six vessels, into each of which

ragement of Arts, Manufactures, and Commerce) is as follows.

"Procure three vessels, of different sizes or apertures; one capable of holding two quarts, the other three or four, and the third five or six. Boil a quarter of a peck of malt for about eight or ten minutes, in three pints of water; and when a quart is poured off from the grains let it stand in a cool place, till not quite cold, but retaining the degree of heat which the brewers usually find to be proper when they begin to work the liquor. Then remove the vessel into some situation near the fire, where Fahrenheit's thermometer stands between seventy and eighty, and there let it remain till the fermentation begins; which will be in about thirty hours; add then two quarts more of a like decoction of malt, when cool; and stir it well in the larger sized vessel; then proceed as before; after which, add an equal quantity more and mix all in the largest vessel, and it will produce yest enough for a brewing of forty gallons."

I put three gallons of wort, of forty-five degrees of density. One of the above ferments was put into each vessel of liquor, and they were all placed together in a temperature of fifty-one degrees, for eight days; at the end of which time, each parcel was separately distilled, but the quantity of spirit obtained was so nearly similar in all, that I have kept no account of their respective products. It must, however, be observed, that the quantity of spirit formed was much less than might have been produced by the assistance of a little yest; at the same time it is obvious, that fermentation may be effected by subjecting solutions of saccharine matter to a proper temperature without the addition of an artificial ferment. This is a farther proof of Dr. Pennington's opinion, that worts would ferment without yest, though a much greater length of time is necessary, and after all the operation is not so perfect.

With respect to the Doctor's opinion on the raising of bread, I think it is by no means conclusive. He takes a quantity of dough, and subjects it to the action of yest for three quarters of an hour, and then commits it to distillation;—he obtained some water but no spirit; from which he concludes that bread is not raised by fermentation. Had the Doctor said that a complete fermentation was not necessary to raise bread, I should have had no objection to his hypothesis. An operation is certainly begun, which in nine or sixteen hours, according to his own experiments, forms a spirit. This observation was suggested on my first reading Dr. Pennington's Inaugural Dissertation; but, in order more fully to satisfy myself, I took a gallon of wort, to which I added, at a proper temperature, two ounces of yest. The mixture was made in a glass jar, and I observed all the signs of a beginning fermentation, such as a motion excited in the liquor, the bulk of which was increased and rendered turbid by the appearance of opake filaments, together with an increase of heat. After it had stood three quarters of an hour, I distilled it, but not a drop of spirit came over. It may be urged, that this is but a negative fact; but (when we observe that a similar change is effected in the dough to that in the fermenting malt liquor, in an equal length of time, when we see the products of each turn out exactly alike, and when we consider that dough of itself ferments in a few days, without the addition of yest) we have, at least, presumptive evidence that the raising of bread is a beginning of fermentation.

I have made some additional experiments to produce artificial ferments from molasses, sugar, honey, and the expressed juice of ripe fruits; but as I found them all much inferior to the flowers of wine, or common yest, my subsequent experiments were all assisted by the latter.

When I mention the quantity of yest used, I mean to be understood, one part of dry * yest (dried upon canvas) and seven parts of water.

II. I shall now proceed to the second enquiry. Is a greater quantity of spirit obtained by a free admission or a total exclusion of the atmospherical air?

Boerhaave mentions a free admission and emission of the common air as one of the things necessary to promote fermentation, and his opinion has prevailed to the present day: for Mr. Chaptal observes, that in order to develope this fermentation there is required, first, the access of air. Now if these assertions were true, my account of the operation of fermentation would be essentially wrong; but the following facts will prove, that so far from a free access of common air being necessary to the spirituous fermentation it is highly injurious.

I took a bushel of malt and infused it in a sufficient quantity of water of 180° of heat, for an hour, and then drew off six gallons.

With water of 200° of heat I again infused the same malt another hour, and drew off an equal quantity.

With water of 212° of heat, infused for an hour longer, I drew off six gallons more.

* 2.7608507 lbs. of dry yest, consist of - - - - - - -	Hydrogen	.2900716
	Oxygen	1.6437457
	Carbon -	.7876519
	Azot - -	.0393815

Lavoisier.

When the liquor was reduced to sixty degrees of heat, the gravity of each, by my hydrometer, was as follows.

The first infusion had fifty-four degrees of density, the second forty-five, and the third twenty-five.

The six gallons of each infusion were divided into two equal parts, the one for close the other for open fermentation. These were pitched with four ounces of yest respectively: the first two parcels at 66° of heat, the second at 60°, and the third at 55°.

The reason of operating on liquors of different densities, and pitching them at different degrees of heat, will be explained hereafter. They are now to be considered as six different experiments in support of one fact.

The vessels used for the open fermentation were jars equally wide, or rather wider, at the top than the bottom. Of those for close fermentation I have given two drawings in plate IV. fig. 1 & 2. I prefer having the ends of the tubes immersed in water, as I think it answers the purpose better than common valves. If the vessel be closed round the tube it is equally air-tight, and the resistance of the water will not be too great for the elasticity of the carbonic acid gas and other fluids which are raised during the intestine motion; nor is there any fear of the elasticity of the air in the vessel being overcome by the pressure of the atmospherical air on the surface of the water.

The fermentation was continued till all signs of fermentation had subsided; the contents of each vessel were then carefully distilled, and their products were as follow.

TABLE OF THE QUANTITY AND STRENGTH OF THE SPIRIT OBTAINED BY THE FOREGOING PROCESSES.

Gallons of wort.	Density in each experiment.	Heat at which the yest was added.	Ounces of spirit produced.	Degrees below proof. In the closed vessel.	Degrees below proof. In the open vessel.
I. Six gallons divided into two parts.					
3	54	66	96	56	74
II. Six gallons divided into two equal parts.					
3	45	60	96	65	83
III. Six gallons divided into two equal parts.					
3	25	55	96	93	103

N. B. The gravity of the spirit was ascertained by Dicas's hydrometer, and the density of the worts by my own saccharometer.

I have frequently repeated the above experiments, varying the density and heat of the liquors, as well as the quantity and quality of the ferments; and the spirit produced was always equally in favour of close in preference to open fermentation. This was also the case whatever modification of fermentable matter I used, whether molasses, sugar, or potatoes.

I may here observe on the subject of potatoes, that they never answered the expectation which I had formed from the account given by Dr. Anderson. To procure five quarts of spirit from seventy pounds of potatoes appeared very extraordinary; but, from repeated experiments, I am thoroughly satisfied, that it is not possible either by the plan proposed by Dr. Anderson, or any other with which we are acquainted. The farina obtained from seventy pounds of potatoes does not exceed fourteen pounds, (a fact which I have proved by carefully collecting it) and good molasses, by a well regulated fermentation, will not form more than its bulk of spirit; from which it will appear very improbable that ten pints of spirit can be produced from the above quantity of farina.*

In the course of these experiments, I was astonished to find so great a disproportion in the quan-

* This fact shews us how erroneous the common opinion is, of mixing potatoes with bread as an object of œconomy.

tity of my liquors after fermentation. On reflection it would readily occur, that there would be a diminution of bulk in that which was fermented in the open vessel; but it was so great, that I at first suspected an error had been committed in dividing the liquors: however, from repeated observation, the difference was so evident, that I made the two following experiments more accurately to determine the fact.

I took eleven quarts, three ounces, and a half of wort, to which I added four ounces of yest, and fermented it in the close vessel for twelve days, at the end of which time it had lost eight ounces by measure.

An equal quantity of wort and yest was fermented in an open vessel for the same length of time, and exactly in the same temperature. On measuring the second, I found a diminution of forty ounces.

To determine with certainty whether the liquors remaining in each vessel were equally good, I separately distilled the two, leaving out thirty-two ounces of the latter, (which was the difference in quantity) and the spirit produced from each was exactly alike.

From the two foregoing facts our information is still more complete, as we not only observe the great saving in the liquor by close fermentation, but we also see that a diminution of eight ounces had taken place in the close vessel, and we have good grounds for supposing that it is an actual diminution of the whole of the fermenting mass; from the consideration of which we shall not be surprised that Chaptal made vinegar from the fluids thrown off by fermentation.*

This part of the subject appeared too interesting to be left without a little farther investigation, as some chemical writers of eminence have stated, that it was pure unadulterated carbonic acid gas which was thrown off by fermentation; others, that it was carbonic acid gas combined with pure alcohol; while many have supposed to be an union of gas, alcohol, and water;† but none, whose opinion I have seen, have stated it to be what it is:—all the elementary

* M. I. A. Chaptal communicated to the Academy at Paris (1786) an observation of some curiosity respecting the formation of vinegar. He placed some distilled water above the vinous fluid in fermentation, to impregnate it with carbonic acid. The water, thus impregnated, afforded vinegar; and, at the end of some months, a deposition was made of a substance in flocks, which was analogous to the fibrous matter of vegetables.

† Lavoisier says, "when this gas is carefully gathered, it is found to be carbonic acid perfectly pure, and free from admixture with any other species of air or gas;" but his translator observes, "that the perfect purity must be taken with some allowance; for it almost always (I believe constantly) contains some alcohol, besides a considerable quantity of aqueous gas or water, in solution. The latter does not affect its purity, the former does in some degree."

principles of the fermenting liquor highly surcharged with carbonic acid gas.

To determine this more fully, I made the following experiments.

To a fermenting tun, holding about ninety gallons of liquor, I connected two casks with bent tubes, in the manner of Woulfe's apparatus. The first cask was sufficiently large to hold all the yest and liquor which ran over the top of the tun, and was left empty to receive it. The second held a quantity of pure water, into which one end of the connecting tube was immersed. The apparatus was adjusted about six hours after the liquor had begun to ferment; and the water was subjected to the action of the fluids, which came over in a gaseous state for sixty hours.

The liquor was divided into three parts, the first of which was immediately distilled and yielded a small quantity of spirit.

To the second I added, at a proper temperature, a little yest, and a new fermentation was excited; by means of which the spirit produced was nearly double.

The third was placed in a proper degree of heat to make vinegar, and it is already acidulous.

III. THE EFFECTS OF DIFFERENT FACTITIOUS AIRS ON FERMENTING LIQUORS.

It has been before observed, that nothing but sacharine matter can be fermented, and the indefatigable Lavoisier has proved, "that sugar is a true vegetable oxyd with two bases, composed of hydrogen and carbon, brought to the state of an oxyd by means of a certain portion of oxygen. Fermentation is the mere separation of its elements into two portions, in which one part is oxygenated at the expence of the other so as to form carbonic acid; whilst the other part is disoxygenated in favour of the former, and is converted into the combustible substance called alcohol."

It was from a consideration of these circumstances, that I was induced to try what effect an atmosphere of some factitious airs would have on fermentation.

My experiments on this part of the subject have been confined to hydrogen, oxygen, and a mixture of the two. The hydrogen gas was made by decomposing water on iron-filings by heat, not by the sulphuric acid; and the oxygen gas was obtained from the oxyd of manganese. The processes were conducted in the following manner. I took three bottles similar to that represented in Pl. IV. fig. 1. into each of which was put thirteen quarts of wort of forty-five degrees of density. To each bottle was added four ounces of yest. After the fermentation had begun I fixed a bag, containing one of the gases, in the stopper of each vessel, by means of a stop-cock and screw.

The respective gases were occasionally forced into the bottles, and mixed with the elastic fluids already produced. These operations were continued for eight days, and the bags were replenished with fresh gas every twenty-four hours.

The particular phænomena attending the fermentation of the liquor in each vessel were in no respect important, excepting that the flowers on the surface were not equal to what might have been expected if no gas had been forced in, and that into which the hydrogen gas was thrown was much inferior to the the other two.

From observation I was, at first, scarcely able to determine, whether that supplied with a mixture of one part oxygen and two of hydrogen, or that which was supplied with pure oxygen gas, had the better head; but, from close and repeated inspection, it appeared in favour of the latter.

On distillation, the spirit produced from each was as follows.

By pure oxygen gas	30 oz.	110°	Below proof
By pure hydrogen gas	30	111°	
By a mixture of oxygen and hydrogen gas	30	106°	

Most that we have learned from the three last experiments is, that they are none of them worth repeating as objects of profit; but they serve to confirm our opinion against the admission of air to fermenting liquors. It is true that none of the above gases were similar to atmospherical air, as it is a mixture of oxygen, azot, and a little carbon.* I did not think it necessary to add any experiments with a mixture of oxygen and azot; for, as there is a column of air constantly rising from fermenting substances, the weight of which is much greater than that of atmospherical air, we cannot suppose that the latter is ever freely admitted, or even admitted at all, by any of the common processes of fermentation.

My observations on malting, mashing, fermenting, distilling, and rectifying, will be as brief as the nature of the different processes will admit.

MALTING, which consists in developing the saccharine matter by germination, has been denominated, by some, the saccharine fermentation; but I see no more propriety in the term, than if it were adopted to express that progress in vegetation which changes mucilage into fœcula.

* One hundred parts of atmospherical air usually consist of twenty-seven parts of oxygen and seventy-three of azotic gas; but the pure air is diminished, and carbonic acid formed, according to the number of persons breathing, or the quantity or quality of fuel burning, in an atmosphere formed as above.

Margraaf extracted sugar from most vegetables, but it is more completely formed in some plants than in others, such as the *arundo saccharifera*, and the *acer saccharinum*. Manna is obtained from the leaves of fir, oak, juniper, and the maple-tree. The ash, which is very plenteous in Calabria and Sicily, affords that which is commonly sold both in flakes and in tears. It affords, by distillation, the same products as sugar. The analogy between saccharine matter, mucilages, and gums, is deducible from their containing the radical principle, which, in combination with oxygen, constitutes the oxalic acid; and *amylaceous fæcula* is only a slight alteration of mucilage, which may be converted into saccharine matter by germination. Saccharine matter must, therefore, be considered as one of the immediate principles of vegetables, formed by the natural progress of vegetation.

In order to prepare barley for germination, it must be fully saturated with water, which generally requires it to be completely covered for sixty hours. After it is fully saturated, it must be removed; not only for fear of injuring the texture of the grain, but to prevent the water from robbing the barley of its most valuable quality: an occurrence which in some degree takes place by an ordinary and proper steeping. This is a fact not generally understood, however, it may be easily proved by subjecting the cold infusion of barley to fermentation, whereby it will produce beer from which alcohol may be distilled.

When the barley is taken out of the water, it must be laid in a heap twenty-four hours; for, if it were immediately spread thin, it would become dry, no heat would be generated, nor would any vegetation ensue. The grain must be afterwards spread on a cool floor (if rather moist it would be better), sprinkled with water, and turned two or three times a day, to give the whole an equal temperature and keep it sufficiently moist. The appearance of the *aqua-spire* at the end of the corns is the usual criterion for taking the malt to the kiln; but I should advise its being urged a little farther, that is to say, I would have the *aqua-spire* longer before the grain is removed from the floor.

In drying, or what is generally called curing malt, the heat should only be continued until all the moisture is dissipated; for the spirit evaporates during the whole process. This fact may be ascertained by drying malt in a retort, to which a receiver is luted containing water; for the water will be found impregnated with spirit in proportion to the malt dried and the degree of heat which has been employed.

The method lately adopted of crushing malt between metal rollers, is certainly preferable to the common process of grinding.

Soft water ought to be invariably used for MASHING; and the heat should, in some degree, be varied according to the different qualities of the malt. But I shall suppose the malt to be good, and then lay down rules for extracting the saccharine matter.

If the water be of too great a degree of heat at the first mashing, it will have a tendency to coagulate the malt, or unite the whole into a pasty mass. On the contrary, if the heat be too low, the liquor will not become transparent until some degree of acidity takes place. I would, therefore, recommend the following method as the best calculated to preserve transparency, and to obtain all the fermentable matter.

Infuse the malt in a sufficient quantity of water, of 160° of heat, for an hour; and then draw off the wort, which will be found very smooth, soft, and sweet.

With water of 180° of heat, infuse the same malt another hour, and draw off the liquor as before. Some sweetness will still remain in the malt, for the obtaining of which it must be again infused in boiling water for another hour. When these three infusions are mixed and reduced to a proper heat, the artificial ferment (yest) may be added.

I have devoted so much of the first part of this essay to the subject of FERMENTATION, and the application of the saccharometer, that it is not necessary to enter at large upon them here. The great object is to regulate the heat according to the density of the liquor, before the yest is added; to keep the fermenting mass in a proper temperature; and pay strict attention to the construction of the vessels, all of which may be deduced from the former experiments. The close vessels, to which tubes are adjusted and immersed in water, have some additional advantages besides those already enumerated. They prevent, in some degree, the temperature of the surrounding atmosphere from affecting the fermentation; and most effectually prevent any acidity from taking place however long the operation is continued.

It is of considerable importance to have the liquor clear and freed from all heterogeneous matter; and, if this has not been done by previous management, the usual method of refining ale or porter ought to be adopted before the liquor is committed to distillation. A quantity of isinglass dissolved in water, to which a little sour beer must be added, has the best and quickest effect.

There is another necessary precaution to be observed. Care should be taken that the sediment be not disturbed in removing the liquor, from the vessel in which it has been fermented, to the still. This may be done either by a tap at a small distance from the bottom of the vessel, or by the introduction of a syphon.

The DISTILLATION of the spirit is equally, if not more, important than any of the previous processes: for, however good the malt may be, or whatever care may have been taken in the mashing and fermenting, if the distillation be not well conducted, the quantity of spirit will be small and its quality bad.

To obtain spirit from fermented liquor is the business of the distiller, but to refine and purify it belongs to the rectifier. The second operation is so dependent on the first, that unless the distillation be carefully conducted, the rectification will be rendered both tedious and difficult.

The art of distilling malt spirit may be reduced to the following principles. 1. To obtain the spirit free from the oil of malt. 2. To raise the vapours in the most œconomical manner. 3. To condense them as speedily as possible. And 4. to prevent empyreuma.

The first may be done by mixing a small quantity of sulphuric acid with the wash; and the remaining three by a proper construction of the still and the necessary care in distillation.

The still should be so constructed as to be capable of containing a column of fermentable matter considerably broader than high, to prevent the liquor at the bottom from being burnt before the upper part is heated. The top should be as wide as the bottom to give the vapours free and complete liberty to escape. By the common construction of stills they are incessantly returned into the boiler, especially at the commencement of the process.

The still recommended by Chaptal is well calculated for the distillation of ardent spirit. The bottom is concave, in order that the fire may be nearly at an equal distance from all the points of its surface; the sides are elevated perpendicularly, in such a manner, that the body exhibits the form of a portion of a cylinder; and this body is covered with a vast capital, surrounded by its refrigeratory. This capital has a groove or channel projecting two inches at its lower part within: the sides have an inclination of sixty-five degrees. The beak of the capital is as high and as wide as the capital itself, and insensibly diminishes till it comes to the worm-pipe. The refrigeratory accompanies the beak or neck, and has a cock at its further end which suffers the water to run out, while it is replaced by other cold water, which incessantly flows in from above.

When the water of the refrigeratory begins to be warm a cock is opened, that it may escape in proportion as it is more plentifully supplied from above.

The distillation of the wash may be kept up until the quantity limited by act of parliament is obtained; or until the product is no longer inflammable.

Various contrivances have been adopted by the distillers to prevent the wash from burning in the still. A bundle of clean sticks is sometimes thrown loose into the liquor to agitate the same during the ebullition. This is more effectually done by a cylinder, fixed in such a manner that it will turn by the action of the steam, and continue a more regular agitation by means of chains of wood or metal connected with the cylinder; but these precautions are scarcely necessary if the wash has been rendered sufficiently limpid.

RECTIFICATION is simple and easy, provided the previous operations have been well managed; but if an empyreuma has been contracted in the still, or the fœtid oil has been combined with the spirit, then it becomes more difficult. On the contrary, if these have been avoided, nothing more is necessary than to mix the spirit with an equal quantity of pure water and recommit it to distillation, when it will come over pure.

When the liquor has been burnt in the still, it ought to be kept, for some weeks, in charred vessels; and a quantity of charcoal should be mixed with the spirit and water, previously to the distillation. This will, generally, be found a sufficient remedy for empyreuma, but will not correct the disagreeable flavour communicated by the admixture of the fœtid oil. Many substances have been used for this purpose, none of which, I think, are fully adequate to the end proposed. Filtration has been recommended, but the oil is so intimately mixed with the spirit that a considerable quantity will pass through the filter. The operation is also tedious, and some of the spirit evaporates during the process. Alkaline salts are frequently mixed with the spirit, previously to rectification, such as the carbonat of potash, but more frequently the carbonat of soda. They, however, are both liable to considerable objections, when unassisted by any other substance; for, although they combine with the oil, and, in some degree, prevent its rising in vapours, yet they communicate an urinous flavour to the spirit which is highly injurious. Neutral salts, quick-lime, calcined bones, and chalk are equally liable to objection, as they do not effectually deprive the spirit of the oil which it holds in solution, and an improper flavour is also contracted from them.

The method which I have adopted, and which is generally attended with the most favourable result, is the following:

The body of the still is constructed like the former, but the capital is so formed as to admit of a bent tube into the boiler, or rather into a vessel connected with the beak, and fixed in the boiler in such a manner as to form a balneum, to be heated by the liquor in the still. From the top of this vessel an additional beak is continued, similar to that described in the still for the distillation of the wash, and like it, connected with the worm in the tub.

The liquor intended for purification must be put into the still, with a proportionable quantity of carbonat of soda, and into the vessel in the still a quantity of diluted sulphuric acid. After the two capitals are luted, the fire may be lighted and the vapours will be driven over into the balneum, from which they will regularly rise with the heat communicated by the boiling liquor in the still. By this means the oil will remain combined with the alkali, with which it will form a soap, whilst the bad flavour contracted by the alkali will be totally destroyed, nor will the sulphuric acid be raised in sufficient quantity to injure the taste or quality of the spirit. This will be found equally valuable by inverting the order of the process, that is, by mixing the sulphuric acid with the liquor in the still (about two drams to a gallon) and putting a strong solution of carbonat of soda into the vessel in the still.

Perfectly pure alcohol may be obtained from the spirit made by the foregoing rules; but as the distillation of alcohol has engaged the attention of many eminent chemists, I shall not trouble the Society either with the method of distilling it or its affinities.

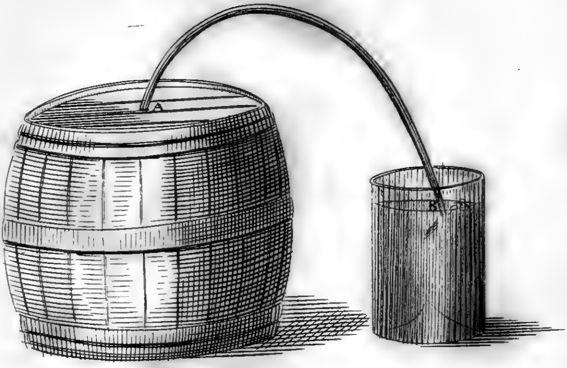


Fig. 1

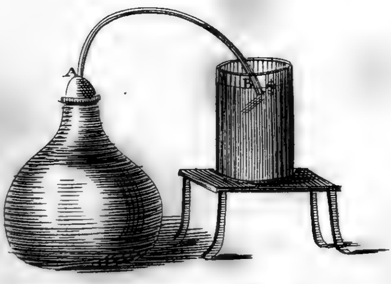


Fig. 2

Hints on the Establishment of an UNIVERSAL WRITTEN CHARACTER. *In a Letter to the* REV. DR. JOHN KEMP. *By* WILLIAM BROWN, M. D.

Communicated by Dr. HOLME.

READ JAN. 26, 1798.

My dear Sir,

About three years ago, my much esteemed friend Dr. James Anderson, threw out in conversation some things concerning a character, by means of which men of all nations might have intellectual intercourse, though their languages were unknown to each other. Since that time, I have occasionally turned my thoughts to this subject, and now take the liberty of communicating to you the hints which my reflections have suggested.

The intention of those gentlemen, who have employed themselves in devising means for universal communication among men, seems rather to have been to introduce a new system of language, than to invent a plan for representing the already existing tongues, in such a manner, as to be intelligible to men of all nations. But unless this be accomplished, we gain little; because if a new language is to be learned, it would be as easy to induce the world to agree in the use of any of the constituted tongues, as to persuade them to employ any other, however philosophical. The idea suggested by Dr. Anderson, is to invent a mode of notation which can be rendered by every one into his own language, with as much ease and perspicuity as we are now accustomed to do in the case of numbers by means of the Indian cyphers. As the figures 1797 are rendered by all the nations, where the meaning of these signs is understood, into their respective tongues: so in an universal character, a man should find the same facility in rendering a book, written by a person whose language he does not understand, and who does not understand his speech, into his mother tongue, or into any other which he may happen to know.

The possibility of effecting this seems to be evident, from the consideration of the sameness of the human intellect, and the sameness of the objects about which this intellect is employed. It is further confirmed by the use of the Indian numerals, and of the musical, and algebraic methods of notation.

It is almost unnecessary to point out the advantages that would arise from the invention of which I speak. They would obviously be very great to the commercial, literary, and religious interests of mankind.

In considering the way in which such a character as I have mentioned, may be contrived, it appeared to me best to attend to the method which men have employed for expressing their thoughts by oral signs. It seems pretty certain, that if the things denoted by articulate sounds, can be denoted by visible marks, these marks will be equally well understood. This is what, in some measure, we do every day by the use of letters. In this instance, it is true, we only represent the sounds denoting things; but can we not also transcribe the import of these sounds into visible marks? All language consists of a certain number of arbitrary vocal marks or sounds, to which mankind have appropriated determinate meanings, or perceptions of the mind. These sounds are different in different nations, but are used to signify the same perceptions: if visible signs were invented to denote these perceptions, would they not be rendered into audible language according to the custom of the individual who so renders them?

What has been said will convey some general notion of the principle on which an universal character might be made. But, in order to explain it more fully, let us endeavour to point out some particulars in the structure of language.

Every language seems to be composed of two sorts of signs merely arbitrary. The number of these signs is small, in comparison of the number of words of which language consists. One kind of these arbitrary signs, is employed solely in denoting simple ideas.* Thus ὕπνος, SOMNUS, *son*, *schlaf*, SLEEP, *sommeil*: φαγω, EDO, *yem*, *Ich esse*, I EAT, *Je mange*: CALOR, *jar*,† *hitze* HEAT, *chaleur*: ερως, AMOR, liubov, liebe, LOVE, *amour*.

I do not say that the examples above stated are absolutely arbitrary signs in all the languages from which these words are taken. Perhaps some of them may be compounds, but they must be ultimately traced to signs merely arbitrary; and that is sufficient to establish my assertion concerning the structure of language, as far as relates to this class of signs.

As the names of things alone cannot afford subject of discourse, which is entirely occupied in telling to others, the relations which one thing bears to another, it became necessary to invent signs which denote these relations. Farther, as the extent of human knowledge increased, new objects of discourse were discovered, which necessarily came to be denoted by distinctive marks. Some of these last have, I doubt not, been represented in some languages by new arbitrary signs: but, for the most part, mankind, by a very simple device, have obviated the necessity of multiplying signs, which it

* If I do not speak with metaphysical precision, I hope I shall be excused; because I dare say my meaning will be understood.

† Pronounced like the French J in *Jour*.

must have been equally inconvenient to invent and to remember.

This device is twofold. Words are used figuratively;—thus the word *express* is used to denote the manner in which one person conveys his thoughts by language to another, though properly the word has no such meaning. But this is not the part of the device I mean to speak of. I wish you to attend to the second class of arbitrary signs, which I have said form a constituent part of language. This class is intirely different from the one I have already mentioned: its signs convey no meaning when they stand alone, and serve only to modify the meaning of those of the other class. As all things denoted by the first class of signs may be placed in various relations, and be in some measure modified; the application of this class is as extensive as are the situations and degrees in which things denoted by the other class can be placed or modified. This class consists of prepositions and terminations.

Before exemplifying the use and application of the two classes of constituent signs of oral language, suffer me to make an observation concerning our English compounds. It has been the practice of those who have endeavoured to enrich our tongue, to borrow compounds from other languages, without having previously introduced the simple words of which such compounds are formed. In so doing, they have not only disfigured the English tongue, but have rendered it in many cases obscure, and in some absolutely unintelligible to by far the greatest number of those who use it. To give compound words their beauty and significancy, it is necessary that their constituent parts be known. If otherwise, the new word is not a sign, which, from its structure, conveys its own meaning: it is nothing more than a new arbitrary mark, which must be explained in a dictionary or elsewhere, before the thing it is meant to represent can be known. Thus to the mere English reader, the words *omniscient*, *omnipotent*, *predestination*, *idolatry*, *arbitrary*, and many others less generally understood, do not convey the ideas which in their original languages they are well fitted to represent: nay we have even the word *superstition*, which, as far as I see, the Romans themselves did not understand. The Russians use a word to denote what we call superstition, which is easily intelligible. Their word is *sueverie*,* from *sue*, vain; and *verie*, belief. And in English we have words universally understood to denote what is meant by the foreign compounds above noticed:—thus we know what is meant by all-wise, all-mighty, fore-appointment, idol-service, free-willful—But to return.

If visible signs are made to represent the constituent vocal signs of language, and if those are ap-

* Pronounce every letter, as in Latin.

applied in the same way, in which mankind have been accustomed to use their oral signs, will not the same effects follow?

To enable us to answer this question in the affirmative, it will be necessary to shew, that all languages are perfectly similar in their structure, and differ only in the form of the signs employed.

I have paid some attention to the Latin, Russ, and English languages; and I have also looked into the Greek, German, and French. By examples from these six tongues, I shall endeavour to shew that the structure of all is alike; and I hope I shall be likewise able to render it very probable, that the number of constituent signs is nearly equal in all.

What has been said concerning the first class of constituent signs is, I imagine, enough to point them out. It is, however, to be observed, that I am not to affirm, that the number of these, in every language, is precisely the same. Nor is it necessary for our purpose that this should be the case. One people may have used compound signs for denoting certain ideas, which another may have done by arbitrary marks. Thus we say *mute*, when the Russians say *bessglassnoi*, (*bess*, without; *glassnoi*, of, or belonging to voice). We say *blind*, where the Latins say *oculis captus*, and the Greeks αοψ, αλαος. This, however, does not contradict my general position; because, if enquired into, we shall find, that the compound word is formed from an arbitrary sign denoting a simple idea, bearing some relation to the idea now to be represented, modified by one of the second class of arbitrary marks.

I have only farther to observe concerning this class of signs, that I consider every word, the meaning of which is not indicated by its structure, as an arbitrary mark, whether it has been so originally or not. If we have lost the signification of its constituent parts, we can only get a knowledge of its meaning by being told that it means so and so; and this knowledge is preserved by memory alone. Thus *neobhodimost* is merely an arbitrary mark for *necessity* to a person who does not understand the Russian tongue, as *necessity* is for *unavoidableness* to a person who does not know the derivation of the word *necessity*. But a Russian would be at no loss to know the meaning of *neobhodimost*, though he had seen it for the first time. He knows the signification of the various signs of which it is composed, viz. *ne*, negative; *ob*, preposition, expressive of circuity; *hodit*, verb to walk; and *ost*, the terminational sign, denoting quality abstractly substantiated. Farther, he would observe that the verb *hodit* is modified by the terminational *my*, expressing that the substantive with which verbs modified by this sign is joined, possess the capacity of undergoing the action of the verb so modified: thus, *hodimy* signifies *that can be walked, or gone, walkable, or go-*

able. Hence the Russian sees that *neobhodimost* conveys the clear idea of necessity, or of something that cannot be shunned or avoided by going round it, or out of its way. To translate it into English, some such word as *unwalkaboutableness* would be produced. It is evident, that to the mere English reader the words catalogue, philosophy, philology, biography, astronomy, preposition, expedition, and many others, are as much arbitrary signs as *neobhodimost* would be, were any person to take it into his head to introduce it into our tongue. There is this exception, however, that the terminational sign added to the foreign words I have mentioned, being in some of them purely English, the class to which the idea denoted by them belongs is understood, though the meaning of the word is not. We can only perceive that these words express some general idea, defined by the meaning of the constituent parts of which the compounds are formed.

The other class of arbitrary signs of oral language are, we have said, of a very different nature from those which denote simple ideas only. This class is not so numerous as the other, and may of course be more easily represented by visible marks.

The great advantages that arise from this simple contrivance of rude men, if we believe language a human invention (which, indeed, the apparent necessity of a thorough knowledge of the nature and relation of things, to the first inventors of this beautiful device, renders very doubtful) is worthy of admiration. By it the need of a new sign for every new perception of the human mind is superseded, and language made copious, without fatiguing the memory. There are two divisions of this class of signs. The one contains those which are permanently fixed to, and constitute part of words; the others are occasionally employed in denoting the relation words bear to each other, as in the declension of nouns, &c. I shall take no notice of this division of modifying signs here: it is evident they could be easily provided for in visible language.

The permanent modifying signs may be divided into prepositional and terminational. If we compare the prepositions of one language with those of another we shall find, that if the simple arbitrary signs are not equal in number, yet there are, in every tongue, either simple or compounded ones to express all the possible circumstances which can be denoted by this kind of sign.

It must be allowed that every preposition has not always the same determinate meaning in every compound word into which it enters. This cannot, however, be admitted as an argument against this scheme of an universal character, because the meaning of a preposition is oral, and in characteristic language must be determined by the same means. The absolute meaning of a word is determined by the precise signification of the parts of which it is composed; but its application, in a particular case, does not depend on its absolute meaning alone: we take into account its place in a sentence, its relation to other words, and the nature of the subject about which the speaker is employed. Thus we say, *to undo a knot*, when we mean to *untye it*: but we also say, *to undo a man*, when we mean *to ruin him*. The Russians speak of a man's *vuigovor*, when they speak of his pronounciation; but they also say, *delate komou vuigovor*, to give a man a rebuke. Now as mankind find no inconveniency in this indeterminate meaning of words in their own tongue, it appears to me, that they will find no great difficulty in perceiving the meaning of the application of prepositions by a writer in a strange language. But should any inconveniency arise from this cause, I think I may venture to say it would scarcely extend beyond the present generation, because an uniform mode of writing would soon introduce an uniformity of idiom, or naturalize all foreign idioms.

It seems unnecessary to enumerate the prepositions of the six languages I have mentioned, and shew that each of them can be translated into every other;—this I think is obvious;—I shall only give some examples of their use in composition; and thereby, I hope, I shall establish a point very necessary to be ascertained, viz. though compound words, used to denote certain things, may be composed of different simple words, and of different prepositions, by people of different nations; yet they all serve to denote with perspicuity the meaning to be conveyed, and are readily understood by all. Thus to express the idea we annex to the word *pronounce*, the Greeks use εκφωνεω; the Latins, *pronuncio*; the Russians, *vuigovorivat*; (*vui*, out; *govorit*, to speak) the Germans, *aussprechen*; the English and French have adopted the Latin word. To denote the idea denoted by the English word *foretell*; we have in Greek προλεγω, in Latin, *prædico*; in Russ, *predskazat*; (*pred*, before; *skazat*, to tell) in German, *vorhersagen*; in French, *prédire*.

In these examples there is a considerable degree of sameness in the mode of composition, though the different words used have not absolutely the same meaning. The compounds, however, all express distinctly the same thing. But other words are used in these tongues to express the ideas indicated by the above examples, which convey very different meanings if taken *positively*; and yet, when used *figuratively*, are easily understood. Thus to express what we mean by *pronounce*, the Latins sometimes use *proferre*; the Russians, *proisnocit*; (*pro*, through, is out off; *nocit*, to carry.) I see that απαγγελλω is translated *pronuncio*. To express the meaning of *foretell*, we find also that vatious words are used, thus προκαταγγελλω, προσημαινω, *præmonstro*, *prænuncio*, *predyavlyayou*, (*pred*, before; *yavlyayou*, I make manifest); *predvestchayou* (*vestchayou*, I inform). In English also we use *predict*, *foreshew*.

We may remark that the meaning of these words is much modified by the subject to which they are applied: thus we say indifferently to pronounce a word, to pronounce judgment, to pronounce a man innocent, &c. The Latins, by no means, confine the use of the words I have taken notice of to denote what we call pronunciation. The Russians say, *dai mnæ retch vuigovorit*, (let me finish or speak out my speech). This figurative use of words gives additional facility to the construction of an universal character, as it shews that it is customary among all men to use words in a sense different from their positive meaning. This is a circumstance of very great utility in beautifying language, and fitting it for a wonderful variety of expression.

As prepositions are signs denoting some circumstances immutably connected with the nature of things, they must exist in every language, under some form or other. It appears to me, that a common sign for each of these would recal to the mind the circumstance denoted by it; and, at the same time, the way in which it is expressed in the oral language of the reader of an universal character.

I hope these hints will be sufficient to shew that the constituent part of language, which may be called prepositional, is probably the same in all languages, and that it would be easily represented in characteristic writing.—I shall now beg leave to call your attention to those signs of oral language, which I term permanently terminational. Here, I think, we see the same simplicity of contrivance, and efficacy of operation, which is conspicuous in the prepositions.

A small change in the ending of any of the first class of signs, is made to denote an idea perfectly distinct from that intimated by the original sign, and yet inseparably connected with it. As many terminationals have been contrived, as there have been perceived kinds of these distinct yet inseparable ideas.

The number and efficacy of this sort of signs seem to be the same in all languages. It is needless to exemplify the whole here; their number is not great, but their operation is extensive.

Terminationals modify the meaning both of verbs and nouns: they transform nouns into verbs, and verbs into nouns. A very few examples will explain their use; and at the same time, point out how easily this part of the structure of language can be imitated in characteristic writing.

One set of them change a substantive noun into an adjective, expressive of the quality of the noun from which it is formed, thus;—

In Greek, *ἁιμα, ἁιματωδης*: *σωμα, σωματωδης.*

In Latin, *sanguis, sanguineus; corpus, corporeus.*
Russ, *krov, krovavoi; telo, telesnoi.*
German, *blut, blutig; leib, leiblich.*
English, *blood, bloody; body, bodily.*
French, *sang, sanglant.*

You will remark that, in all these examples, so much of the original sign is retained as to enable us to perceive it. The change produced on it is only to fit it to join with the terminational mark, in a manner that does not occasion a disagreeable sound.

The meaning of the noun thus modified is uniform and determinate. The mode, however, of modification is not uniform, I believe, in any language. Several terminations are used to express the same thing, and are used according as they are best fitted to produce an agreeable sound. Several adjectives appear to me to be synonymous; thus, *beauteous* and *beautiful, bounteous* and *bountiful.* It is with much hesitation that I differ from my much valued friend Dr. Anderson in this respect.* But however this be, it would be easy in characteristic writing to invent signs even for the terminations which may be deemed perfectly synonymous. Irregularities in the formation of this class of adjectives produce no confusion in oral language; the idea, though expressed in a different way, is still the same. If in an universal character a sign is

* See Bee, vol. vii. page 275.

made to represent a general idea, that idea will be recalled to the mind whenever its sign is seen, and will be expressed in the manner usual in the language into which it is translated.

It is farther to be remarked, that every language has not adjectives derived from the same words. This defect is supplied by constructing with the genitive case. But as both these ways of construction, viz. with an adjective and genitive case, are proper to all languages, as far as I know, though not in the same individual words, I think the difference of idiom, in this case, can produce no confusion. Thus, in English, we say equally, *the soul's powers*, or *the powers of the soul*. In Russ, they use *katchestva dushi*, & *katchestva dushevnij*; viz. *qualifications of the soul*, or *the soul's qualifications*. In Latin we can only say *vires animi*, that language not possessing an adjective derived from *animus*. It is evident, however, were a Roman to see *soul's powers*, or *dushevnij sili*, written in characters, he would be at no loss to know the meaning, and express it in Latin words.

It is not my intention, at present, to exemplify the mode of formation, or to define the import of the various classes of adjectives which are found in language; I shall only hint that an appropriate termination will be found predominant in each class. To define the meaning of adjectives, and reduce them to determinate classes, is a work of great importance; but, from some little enquiry into this subject, I am apt to think, it would not be so difficult as it may appear to be at first sight, and would therefore present no insurmountable obstacle to the formation of an universal character.

Another terminational sign converts an adjective, however formed, into a word expressive of the quality denoted by such adjective, *abstractly substantiated*; thus—

In Greek, *αγαθος, αγαθοτης: πραος, πραοτης: ἁγιος, ἁγιοτης.*

Latin, *bonus, bonitas; lenis, lenitas; sanctus, sanctitas.*

Russ, *blagoi, blagost; smeernoi, smeernost; sevyatoi, sevyatost.*

German, *gütig, gütigkeit; gelind, gelindigkeit; heilig, heiligkeit.*

English, *good, goodness; meek, meekness; holy, holiness.*

French, *bon, bonté*; — — *saint, sainteté.*

As in the formation of adjectives, so in this case also, there are in all languages more ways of forming this class of words than that which I have exemplified. There is in every tongue, I believe, a predominant mode; and I believe the examples I have given are instances of this mode in the tongues from which they are taken. In Latin, we have many terminations in *tudo* and *tia*: thus, *for-*

titudo, solitudo, prudentia, constantia, &c. In Russ, there are many examples in *stvo*, as *postoyanstvo*, (constancy) *sweerepstvo*, (cruelty) *ubivstvo*, (murder; from *ubit*, to kill). In English also most of our foreign compounds retain nearly their former terminations in preference to our own in *ness*; thus, *constancy, variety, fortitude, solitude, tranquillity, &c.* It appears to me that this variety of termination in the constituted languages would produce no confusion in characteristic writing. The marks denoting this class of ideas would be translated into oral language, according to the custom of readers; thus, if joined to *prudent*, it would not be rendered into English, *prudentness*, but *prudence*: if to *fortis*, the Latins would read *fortitudo*, and not *fortitas*; and so of others.

Another terminational sign, which may be called adverbial, is to be found, I believe, in every language. Its operation is well known, and examples easily found: thus, *δικαιως, juste, pravedno, justly, justement.*

Terminations are also found to modify verbs; thus, by changing the termination of the verb, the person who performs the action indicated by it is denoted.

In Greek, *εργαω, εργατης: ακεω, ακουςης.*

Latin, *facio, factor; lego, lector; audio, auditor.*

In Russ, *delayou, delatel; tchetayou, tchetatel; slushayou, slushatel.*

German, *Ich mahce, macher; Ich lese, leser; Ich höre, zuhörer.*

English, *I make, maker; I read, reader; I hear, hearer.*

French, *Faire, faiseur; lire, liseur.*

Verb seems to indicate that a person is employed about something—it asserts that something is done or suffered. Language has a sign to denote the thing about which the verb asserts its nominative is employed. It seems probable, that this sign was invented before the verb itself, which appears to me only a noun so modified, as to denote that a person is employed about it. We find of consequence that many verbs are formed by adding a terminational sign to the mark, denoting the name of the action or passion its nominative is engaged in; thus, we have from *amor, amare; labor, laborare; Liubov, liubit; trude, trudit. Love, to love; work, to work. Liebe, lieben; arbeit, arbeiten.*

There is a verbal termination which seems to indicate the name of the action of the verb, in a way that supposes this action is still going on. This termination is easily discovered in all languages, and is very much in use: thus—

In Greek, *παραινεω, παραινεσις: συντηρεω, συντηρησις: διοικεω, διοικησις.*

In Latin, *admoneo, admonitio; conservo, conservatio; admiror, admiratio; guberno, gubernatio.*

Russ, *uveschevayou, uveschanie; sochranyayou, sochranenie; udivlyayous, udivlenie; upravlyayou, upravlenie.*

German, *Ich erinnere, erinnerung; Ich erhalte, erhaltung; Ich verwundere mich, verwunderung; Ich regiere, regierung.*

English & French. } The words are nearly the same as the Latin.

It is easily seen, that in common language these words are often perverted from their strict meaning; thus, we use *admonition*, for *advice*; *caution*, for *care*, and even *carefulness*, &c. we do not, however, find any inconveniency from this incorrect way of speaking. We are, in fact, so accustomed to speak figuratively, that we do not perceive any impropriety in it. The existence of this class of words shew us, how mankind, even in their rude state, discovered the need of inventing signs to denote their ideas with accuracy. In marking such ideas, the simple artifice of a small change in the original sign is still employed. In my opinion, in characteristic writing, the course pointed out by the original inventors of language must be followed. We shall find their mode of operation the most simple and the most effectual.

Besides substantive nouns derived from verbs, we have adjectives from the same source. I do not mean participles, which have been considered as a part of this class of words.

There seems to be two kinds of verbal adjectives; one may be called active, and the other passive.

The active verbal adjective denotes, that the substantive noun with which it is joined, is capable of exerting the action indicated by the verb from which it is derived. It differs from the present participle in this, that the participle asserts, the substantive with which it is joined to be actually performing the action denoted by the verb: the verbal adjective only points out its capacity of doing so. In English, this class of verbals terminate in *ive*: some of them may have been perverted from their original meaning; but if any person will consider the import of this ending, I make no doubt he will readily allow that their true signification is that which I have given.

Terminations of this kind, I believe, are not frequent among the Latins. They seem to have expressed this idea by a relative and verb, or by the gerund in the genitive case: thus, the phrase *perceptive faculty*, they translate *facultas quæ percipit*, or *facultas percipiendi*. There are, however, some examples of this class of words in Latin, *adhæsivus*, *passivus*, *indicativus*, *&c.* The Russian language is very rich in this class: thus, *pobeditelnoi*, capable of overcoming, from *pobedit*, to overcome;

vinitelnoi, from *vinit* to accuse; *mstitelnoi*, vindictive; *vziritelnoi*, intuitive; *krepitelnoi*, from *krepit*, to strengthen, &c.

The other verbal adjective I mentioned is formed, in English, by the terminational sign *able* and *ible*. It denotes, that the substantive, with which it is joined, is capable of undergoing the action of the verb from which such adjective is formed. This termination occurs often in Latin, Russ, English, and French, and, I doubt not, exists in other tongues. Examples.

In Latin, *visibilis, amabilis, mutabilis, sanabilis.*
Russ, *vidimij, liubimij, menaimij, letchaimij.*
English, *visible, amiable, changeable, curable.*
French, *visible, aimable, changeable.*

I have thus given a few examples of the use of the two kinds of signs which I think constitute language. If the mechanism of speech be such as I have represented, I hope the partial enquiry I now present to you, will afford some hints that may contribute to render the formation of an universal character more easy, by establishing a principle, on which persons who may be induced to prosecute this important object, may proceed with a certainty of success. A project of such magnitude cannot be executed without much labour; and if executed, its practical application could not be easily established. Yet there is reason to hope that this will one day be accomplished. The invention and use of letters, I make no doubt, at one period of society, would have appeared as chimerical and impracticable, as the invention and introduction of an universal character may appear to many at this day. Yet letters have been adopted almost universally among mankind; and their use has been found so important, that to introduce them, where they are not known, may be reckoned one of the greatest blessings that can be bestowed by an enlightened nation. May we not hope then that an invention, still more universally useful than letters, will at last meet with the countenance, and engage the attention of every true friend to mankind?

In addressing these hints to you, I hope, I shall in some measure contribute to your amusement, and entitle them to some attention; but I contribute still more to my own satisfaction, in having an opportunity of declaring how much I have profited by social intercourse with you, and how much I am honoured by your friendship. I am,

with the warmest sentiments of
love and esteem,
your much obliged and
most humble servant,

WILLIAM BROWN.

Edinburgh, Ramsay Garden,
August 9, 1797.

On the Process of BLEACHING *with the oxygenated muriatic Acid; and a Description of a new Apparatus for Bleaching Cloths with that Acid dissolved in Water, without the Addition of Alkali. By* THEOPHILUS LEWIS RUPP.

READ FEB. 9, 1798.

The arts, which supply the luxuries, conveniencies, and necessaries of life, have derived but little advantage from philosophers. A view of the history of arts will evince the justice of this observation. In mechanics, for instance, we find that the most important inventions and improvements have been made, not through the reasonings of philosophers, but through the ingenuity of artists, and not unfrequently by common workmen. The chemist, in particular, if we except the pharmaceutical laboratory, has but little claim on the arts: on the contrary, he is indebted to them for the greatest discoveries and a prodigious number of facts, which form the basis of his science. In the discovery of the art of making bread, of the vinous and acetous fermentations, of tanning, of working ores and metals, of making glass and soap, of the action and application of manures, and in numberless other discoveries of the highest importance, though they are all chemical processes, the chemist has no share. But no branch of the useful arts is less indebted to him than that of changing the colours of substances. The art of dyeing has attained a high degree of perfection without the aid of the chemist, who is totally ignorant of the rationale of many of its processes, and the little he knows of this subject is of a late date. The process of dyeing the turkey-red has been known and practised from time immemorial, by the most uncultivated nations, but its theory is not yet understood by philosophers. The manufacture of indigo and its application have been long known to the planter and the dyer; but it is not more than ten years since a true theory of them has been formed. The art of printing, or topical dyeing, is of the greatest antiquity; but the theory of this process, and of adjective colours in plain dyeing, was unknown, till Mr. Henry developed it in the Memoirs of this Society.* He was the first who thought and wrote philosophically on this subject. The bleaching or whitening of vegetable substances has been long practised; but the knowledge of its theory could not be antecedent to the æra of pneumatic chemistry. We might, even at this moment, have been unacquainted with the cause of the destruction of the colouring matter of vegetable substances, if the discovery of the oxygenated muriatic acid, and its effects on colouring matter, had not pointed it out to us. For this discovery and its inestimable advantages, the arts are

* Manchester Memoirs, vol. iii.

indebted to the justly celebrated Scheele; and I am happy to pay this tribute to chemistry after the mortifying truths, which I have stated above.

M. Berthollet lost no time in applying the properties of this curious and highly interesting substance to the most important practical uses. His experiments on bleaching with the oxygenated muriatic acid proved completely successful, and he did not delay to communicate his valuable labours to the public. The new method of bleaching was quickly and successfully introduced into the manufactures of Manchester, Glasgow, Rouen, Valenciennes, and Courtray; and it has since been generally adopted in Great Britain, Ireland, France, and Germany. The advantages which result from this method, which accelerates the process of whitening cottons, linens, paper, &c. to a really surprising degree, in every season of the year, can be justly appreciated by commercial people only, who experience its beneficial effects in many ways, but particularly in the quick circulation of their capitals.

Great difficulties, for a time, impeded its progress, arising chiefly from prejudice and the ignorance of bleachers in chemical processes. These obstacles were, however, soon removed, by Mr. Watt at Glasgow, and by Mr. Henry and Mr. Cooper at Manchester. Another difficulty presented itself, which had nearly proved fatal to the success of the operation. This was the want of a proper apparatus, not for making the acid and combining it with water, for this had been supplied in a very ingenious manner by Mr. Watt and M. Berthollet;* but for the purpose of immersing and bleaching goods in the liquor. The volatility of this acid and its suffocating vapours prevented its application in the way commonly used in dye-houses. Large cisterns were therefore constructed, in which pieces of stuff

* M. Berthollet's apparatus, however, is too complex for the use of a manufactory; Mr. Watt's is better; but a range of four, five, or six hogsheads, or rum-puncheons, connected with one another, in the manner of Woulfe's distilling apparatus, is preferable to either of them. Agitators, on M. Berthollet's principle, may be applied. The retort or matrass should be of lead, standing in a water-bath; its neck should be of sufficient length to condense the common muriatic acid, which always comes over, and it should form an inclination towards the body of the retort, so that the condensed acid may return into it. I beg leave to observe here, that I always found the liquor to be strongest when the distillation was carried on very slowly. I have also found, that the strength of the liquor is much increased by diluting the vitriolic acid more than is usually done. The following proportions afforded the strongest liquor.

Three parts manganese.
Eight parts common salt.
Six parts oil of vitriol.
Twelve parts water.

The proportion of manganese is subject to variation according to its quality.

were stratified; and the liquor being poured on them, the cisterns were closed with lids. But this method was soon found to be defective, as the liquor could not be equally diffused; the pieces were, therefore, only partially bleached, being white in some parts and more or less coloured in others. Various other contrivances were tried without success, till it was discovered that an addition of alkali to the liquor deprived it of its suffocating effects, without destroying its bleaching powers. The process began then to be carried on in open vessels, and has been continued in this manner to the present period. The bleacher is now able to work his pieces in the liquor, and to expose every part of them to its action, without inconvenience. This advantage is unquestionably great; but it is diminished by the heavy expence of the alkali, which is entirely lost. It is moreover to be feared, that the alkali which is added to the liquor, though it does not destroy its power of bleaching, may diminish it; because a solution of the oxygenated muriat of potash, which differs from the alkaline bleaching liquor, in nothing but in the proportion of alkali, will not bleach at all. This is a well known fact, from which we might infer, that the oxygenated muriatic acid will lose its power of destroying the colouring matter of vegetable substances, in proportion as it becomes neutralized by an alkali. But as we should not content ourselves with inferences however plausible, when the truth may be established by experiment; and as I thought the matter of sufficient importance, I made the following experiments on the subject.

I beg leave to premise, that in all these experiments, I made use of one and the same acid, which was kept in a bottle with a ground-glass stopper, and secured from the influence of light. The manner in which I made the experiments was simply this. I weighed, first of all, a bottle filled with the colouring substance which I meant to employ: I then weighed, in a large and perfectly colourless bottle, half an ounce of the acid, to which I immediately, but very gradually, added of the colouring substance contained in the former bottle, till the acid ceased to destroy any more of its colour. The bottle with the colouring substance was then weighed again, and the difference between its present and original weight was noted. The same method was observed in all the experiments.

EXPERIMENT I.

To half an ounce of oxygenated muriatic acid, I added a solution of indigo in acetous acid,* drop by

* It has been usual to estimate the strength of the oxygenated muriatic acid by a solution of indigo in sulphuric acid. This method was inadmissible in these experiments on the comparative strength of the bleaching liquor, with and without alkali; because the sulphuric acid would have decomposed the muriat of potash, and thereby produced

drop, till the oxygenated acid ceased to destroy any more colour. It destroyed the colour of 160 grains of the acetite of indigo.

EXPERIMENT II.

A repetition of Experiment I. The colour of 165 grains of acetite of indigo was destroyed in this experiment.

EXPERIMENT III.

A repetition of Experiments I. & II. The colour of 160 grains of the acetite was destroyed.

EXPERIMENT IV.

To half an ounce of the oxygenated muriatic acid, were added 8 drops of pure potash in a liquid state. This quantity of alkali was about sufficient to deprive the acid of its noxious odour. This mixture destroyed the colour of 150 grains of the acetite of indigo.

EXPERIMENT V.

A repetition of Experiment IV. The colour of 145 grains of the acetite was destroyed.

errors. I therefore added to a solution of indigo in sulphuric acid, after it had been diluted with water, acetite of lead, till the sulphuric acid was precipitated with the lead. The indigo remained dissolved in the acetous acid.

EXPERIMENT VI.

To half an ounce of the oxygenated muriatic acid, 10 drops of the same alkali were added. It destroyed the colour of 125 grains of the acetite of indigo.

EXPERIMENT VII.

A mixture of half an ounce of the oxygenated acid, and 15 drops of the alkali, destroyed the colour of 120 grains of the acetite of indigo.

Though I had taken the precaution of avoiding the sulphuric acid, for the reason stated in the foregoing note, I was not quite satisfied with these experiments, on account of errors which might have taken place through a double affinity. I therefore made the following experiments, in which I employed a decoction of cochineal in water, instead of the acetite of indigo.

EXPERIMENT VIII.

To half an ounce of the oxygenated muriatic acid, a decoction of cochineal was added till the acid ceased to act on its colour. It destroyed the colour of 390 grains of the decoction.

EXPERIMENT IX.

A repetition of Experiment VIII. The colour of 385 grains of the decoction was destroyed in this experiment.

EXPERIMENT X.

To half an ounce of the acid, 6 drops of the liquid alkali were added. This mixture destroyed the colour of 315 grains of the decoction.

EXPERIMENT XI.

Eight drops of the alkali were mixed with half an ounce of the acid. This mixture destroyed the colour of 305 grains of the decoction.

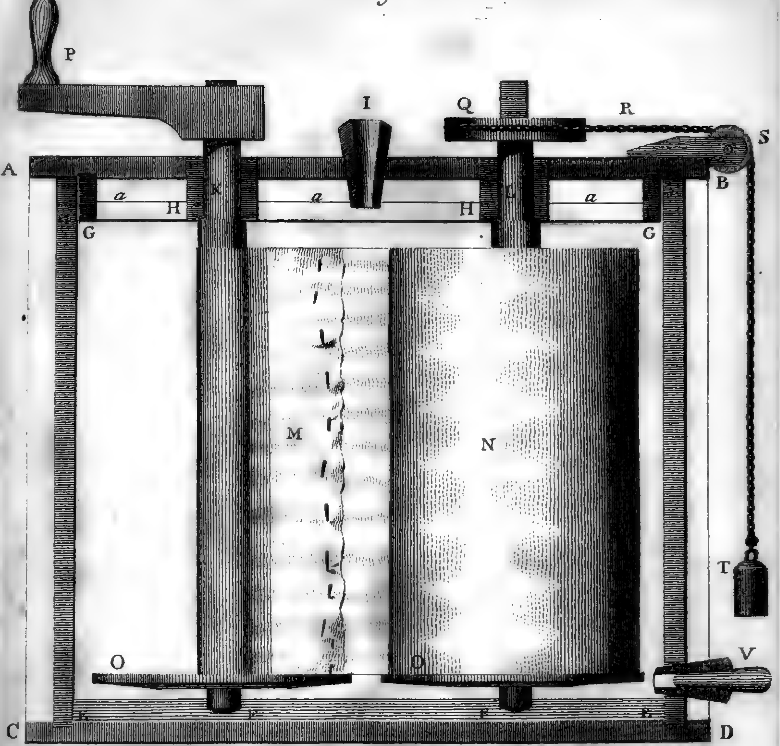
On a comparative view of the results of these experiments, it will appear, that an addition of potash to the bleaching liquor impairs its strength considerably. This diminution of power, and the expence of potash, are a serious loss in an extensive manufacture. It would, therefore, be desirable to have an apparatus for the use of the pure oxygenated muriatic acid simply dissolved in water, which is at once the cheapest and best vehicle for it. This apparatus must be simple in its construction, and obtainable at a moderate expence; it must confine the liquor in such a manner as to prevent the escape of the oxygenated muriatic acid gas, which is not only a loss of power, but also an inconvenience to the workmen and dangerous to their health; and it must, at the same time, be so contrived, that every part of the stuff which is confined in it, shall certainly and necessarily be exposed to the action of the liquor in regular succession. Having invented an apparatus capable of fulfilling all these conditions, I have the pleasure of submitting a description of it to the Society, by means of the annexed drawing.

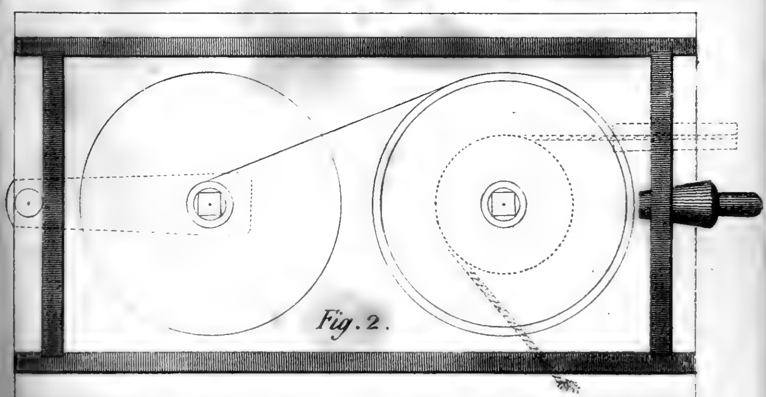
EXPLANATION OF THE PLATE.

Fig. 1, is a section of the apparatus. It consists of an oblong deal cistern, A B C D, made water-tight. A rib, E E, of ash or beach wood, is firmly fixed to the middle of the bottom C D, being mortised into the ends of the cistern. This rib is provided with holes, at F F, in which two perpendicular axes are to turn. The lid, A B, has a rim, G G, which sinks and fits into the cistern. Two tubes, H H, are fixed into the lid, their centres being perpendicularly over the centres of the sockets, F F, when the lid is upon the cistern. At I, is a tube by which the liquor is introduced into the apparatus. As it is necessary that the space within the rim, G G, be air-tight, its joints to the lid, and the joints of the tubes, must be very close; and, if necessary, secured with pitch. Two perpendicular axes, K L, made of ash or beech wood, pass through the tubes, H H, and rest in the sockets, F F. A piece of strong canvas, M, is sewed very tight round the axis K, one end of it projecting from the axis. The other axis is provided with a similar piece of canvas. N, are pieces of cloth rolled upon the axis L. Two plain pullies, O O, are fixed to the axes, in order to prevent the cloth from slipping down. The shafts are turned by a moveable handle, P. Q, a moveable pulley, round which passes the cord, R. This cord, which is fastened on the opposite side of the lid (see fig. 2), and passes over the small pulley S, produces friction by means of the weight T. By the spigot and fausset V, the liquor is let off, when exhausted.

Fig. 2. A plan of the apparatus, with the lid taken off.

Fig. 1





THE MANNER OF USING THE APPARATUS.

The dimensions of this apparatus are calculated for the purpose of bleaching twelve or fifteen pieces of $\frac{4}{4}$ calicoes, or any other stuffs of equal breadth and substance. When the goods are ready for bleaching, the axis, L, is placed on a frame in a horizontal position, and one of the pieces, N, being fastened to the canvas, M, by means of wooden skewers, in the manner represented in fig. 1, it is rolled upon the axis by turning it with the handle, P. This operation must be performed by two persons; the one turning the axis and the other directing the piece, which must be rolled on very tight and very even. When the first piece is on the axis, the next piece is fastened to the end of it by skewers, and wound on in the same manner as the first. The same method is pursued till all the pieces are wound upon the axis. The end of the last piece is then fastened to the canvas of the axis K. Both axes are afterwards placed into the cistern, with their ends in the sockets F F, and the lid is put on the cistern by passing the axes through the tubes H H. The handle P is put upon the empty axis, and the pulley Q upon the axis on which the cloth is rolled, and the cord R, with the weight T, is put round it and over the pulley S. The use of the friction, produced by this weight, is to make the cloth wind tight upon the other axis. But as the effect of the weight will increase as one cylinder increases and the other lessens, I recommend that three or four weights be suspended on the cord, which may be taken off gradually, as the person who works the machine may find it convenient. As the weights hang in open hooks, which are fastened to the cord, it will be little or no trouble to put them on and to remove them.

Things being thus disposed, the bleaching liquor is to be transferred from the vessels in which it has been prepared into the apparatus, by a moveable tube passing through the tube I, and descending to the bottom of the cistern. This tube being connected with the vessels, by means of leaden or wooden pipes provided with cocks, hardly any vapours will escape in the transfer. When the apparatus is filled up to the line *a*, the moveable tube is to be withdrawn, and the tube I closed. As the liquor rises above the edge of the rim G, and above the tubes H H, it is evident that no evaporation can take place, except where the rim does not apply closely to the sides of the box: which will, however, form a very trifling surface if the carpenter's work be decently done. The cloth is now to be wound from the axis L upon the axis K, by turning this; and when this is accomplished, the handle P and pulley Q are to be changed, and the cloth is to be wound back upon the axis L. This operation is, of course, to be repeated as often as necessary. It is plain, that by this process of winding the cloth from one axis upon the other, every part of it is exposed, in the most complete manner, to the action of the liquor in which it is immersed. It will be necessary to turn, at first, very briskly, not only because the liquor is then the strongest, but also because it requires a number of revolutions, when the axis is bare, to move a certain length of cloth in a given time, though this may be performed by a single revolution when the axis is filled. Experience must teach how long the goods are to be worked; nor can any rule be given respecting the quantity and strength of the liquor, in order to bleach a certain number of pieces. An intelligent workman will soon attain a sufficient knowledge of these points. It is hardly necessary to observe, that, if the liquor should retain any strength after a set of pieces are bleached with it, it may again be employed for another set.

With a few alterations, this apparatus might be made applicable to the bleaching of yarn. If, for instance, the pulley O were removed from the end of the axis K, and fixed immediately under the tube H;—if it were perforated in all directions, and tapes or strings passed through the holes, skains of yarn might be tied to these tapes underneath the pulley, so as to hang down towards the bottom of the box. The apparatus being afterwards filled with bleaching liquor and the axis turned, the motion would cause every thread to be acted upon by the liquor. Several axes might thus be turned in the same box, and being connected with each other by pullies, they might all be worked by one person at the same time; and as all would turn the same way and with the same speed, the skains could not possibly entangle each other.

In order to shew the usefulness of this apparatus still more clearly, I request the Society to attend to the following statement of the expence of a given quantity of bleaching liquor, with and without alkali, but of equal strength.

WITH ALKALI.*

	L.	S.	D.
80 lb. of salt, at $1\frac{1}{2}$ d. per lb.		10	0
60 lb. of oil of vitriol, at $6\frac{1}{2}$ per lb.	1	12	6
30 lb. of manganese		2	6
20 lb. of pearlashes, at 6 d. per lb.....		10	0
	£2	15	0
But it appears by the foregoing experiments, that the liquor loses strength by an addition of alkali. The value of this loss, which on an average amounts to 15 per cent. must be added to the expence		8	3
	£3	3	3

* I make no mention of the expence attending the *preparation* of the liquor, it being the same in both cases.

WITHOUT ALKALI.

	L.	S.	D.
80 lb. of salt		10	0
60 lb. of oil of vitriol	1	12	6
36 lb. of manganese		2	6
	£2	5	0

It appears from this calculation, that a certain quantity of the liquor, for the use of my apparatus, costs only 2 l. 5 s. but that the same quantity of the alkaline liquor costs 3 l. 3s. 3d. which is 40 per cent. more than the other. The aggregate of so considerable a saving must form a large sum in the extensive manufactures of this country.

Account of a remarkable Change of Colour in a NEGRO. *By* MIERS FISHER.—*Extract of a Letter from* Mr. James Pemberton *to* Mr. Thomas Wilkinson.

Communicated by Dr. HOLME.

READ DECEMBER 15TH, 1797.

PHILADELPHIA, September, 13th, 1796.

This day Henry Moss, of African descent, visited me; and produced a certificate, of which the following is a copy:

"I do hereby certify, that I have been well acquainted with Henry Moss, who is the bearer hereof, upwards of thirty years, during the whole of which time he supported an honest character. In the late war he enlisted with me in the continental army as a soldier, and behaved well as such. From the first of my acquaintance with him, till within two or three years past, he was of as dark a complexion as any African, which has changed without any known cause to what it is at present. He was free-born, and served his time with Major Brent, late of Charlotte county.

"Given under my hand the 2d day of September, 1794,

JOSEPH HOLT, *Bedford County.*

Henry Moss has all the features common to the African race, though not strongly impressed. He is forty-two years of age, and five feet six inches high. The borders of his face, at the roots of the hair on the sinciput and descending by the right ear, are, for nearly an inch in breadth, of a perfect European complexion. This stripe, somewhat enlarged in its dimensions, is continued under the chin, and rises on the left cheek to within two inches of the ear, where it is intercepted by an irregular blotch of negro skin, about an inch broad, which detaches it from a corresponding stripe on the left side of his face. It passes down the neck of the left side about three inches, and is there two inches wide. Its margin is irregularly indented, resembling islands and peninsulas as represented on the chart of a sea-coast. The back of the neck, the breast, arms, and legs (as far as these could be exposed with decency in a mixed company) are of a clear complexion, interspersed with small specks of African colour, not unlike the freckles which appear on the skin of a fair woman in summer. The African complexion is completely discharged from the upper eyelids. There is a small white streak under the right eye; and a larger one, nearly half an inch broad, with a margin irregularly defined under the left. A white list passes round the mouth, shaded by one of his native hue reaching nearly to the chin, below which he has a very fair complexion. The back and palm of the hands are perfectly fair; yet stripes of his former colour pass from the wrist along the sides of his hands to the ends of his fingers: and appear on the outsides of all his fingers. But in general on his limbs, where they are covered by clothing, or where skin meets skin, the transmutation is complete. The whole area of negro skin would not, I am persuaded, if properly measured, exceed a square foot. His hair is undergoing a correspondent change; and whenever a white spot can be discovered, it appears soft like that of an European, and may be drawn out with ease to a length of several inches: where the skin retains its pristine hue, it is crisp like wool. On pressing his skin with my finger, the part which I pressed appeared white; and, on removing my finger, it was suffused with red, as happens in Europeans. I examined the borders of the black and white skin, with a glass which magnified considerably, and which is known in Ireland by the name of a linen-teller. It was evident that the change was not external, or occasioned by the casting off of the epidermis; but that it was owing to an affection of the *corpus mucosum*. No fissures were discernible, but I perceived that there was a small and gradual elevation where the white and black portions met, without any discontinuity on the external surface.

He gave me the following account of his genealogy. His paternal grandfather was born in Africa, and married a native Indian of this country. His father married a Mulatto, born of an African father and an Irish mother. His maternal grandfather was born in Africa.

He was first sensible of a change of colour in his skin in February 1792. It commenced at the roots of his finger nails, extended to the first joints, and went no further at the time. Two months afterwards the back of his neck underwent the same change, which proceeded along the body, and gradually descended to most parts usually covered by his clothes. The progress was slow in the first, but more rapid in the ensuing years. The alteration was made chiefly in summer or warm weather; and could not be seen to make any progress in the cold months. He says, he came to this city on the 26th of July: and that the remains of African complexion on his face and hands has sensibly diminished since his arrival. This account is confirmed by Stephen Paschal and others, who saw him twice, at an interval of thirteen days, in which time the difference was abundantly manifest. He was this morning shaved by a barber; and says, that he felt no obstruction to the razor, when it passed over the white to the black part of his face, or on its return, which must have been experienced if these had been separated by any discontinuity of the skin.

Since the change of complexion took place, he has been more sensible of variations in the temperature of the air; and has had blisters and freckles in every part of his body, which was exposed to the sun by holes in his clothes.

I put many questions to him concerning his diet and mode of life, the state of his health, the diseases to which he had been subject, and the remedies employed for their removal; but nothing could be extracted from his replies, which had the least tendency to solve this curious phænomenon.

November 22d, Henry Moss visited me again. I examined his face, hands, breast, legs, and thighs. The black parts are considerably diminished since I saw him last. Hence I entertain no doubt that the change is gradually proceeding; and, should he live another summer or two, that it will be complete.

MIERS FISHER.

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and

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ERRATA.

Page 31, line 24, for *yellow* read *blue.*
—— 38, add to the note, *Cahier de Fevrier,* 1779.
—— 355, line 2, for 600 read 6000.
—— 357, line 3, for 150 read 154—for 182 read 189.
—— 600, line 5 of Note, for *on,* read *as.*
—— 607, for *amse,* read *same.*

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ON TRAGEDY & THE INTEREST IN TRAGICAL REPRESENTATIONS.

AN ESSAY.

BY THE REVEREND GEO. WALKER, F. R. S.

And Professor of Theology in the New College, Manchester.

The propensity which human natute has, in every age and nation, discovered for spectacles and representations of a tragic kind, though it be universally confessed, that the sensations and passions excited thereby are in their nature painful, and often exquisitely so, is at first view so singular and contradictory a phenomenon, as could not fail to draw the attention of moralists and philosophers; and challenge all their ingenuity to reconcile so irregular a trait of the human character, with the most approved likeness of the human mind. There are, indeed, some examples of this propensity so rude, uncivilized, and inhuman, as mock all efforts of ingenuity to reduce them to a consistent and agreeable system. Such were the exhibitions of gladiators, among the Romans; the tournaments and justs of gothic chivalry; such are the bull-fights of the Spaniards; the combats with the broad sword; the bull-baitings, cock-fights, and Shrove-tide amusements of our own nation; together with the horrid jollity of the North American tribes, exulting over the tortures of their ill-fated prisoners.

Most or all of these national reproaches are, in a greater or less degree, the offspring of a rude military genius, and savage heroism; which, by an early familiarity with the excesses and cruelties of war, let loose in all its wildness, have triumphed over nature, over the kinder dictates of a general humanity. In these, the pleasure of the spectators, unnatural as it is, is pure and unmixed; by whatever means they have subdued their minds to the capacity of this pleasure, when once the relish is acquired, their continued propensity to such scenes is perfectly natural, as it is not combated by any feeling of sympathetic pain during the exhibition. This is clearly attested of the Roman people, by the uniform accounts of their own historians: foreign nations ascribed their hardiness in war to this familiarity with blood and death in the amphitheatre; and a Syrian monarch, ambitious, at any rate, to rival the Roman grandeur, hoped to render his effeminate Asiatics equally intrepid and unappalled amidst the horrors of battle, by introducing the same sanguinary entertainments in peace. It is a proof that humanity had little interest in the fatal consequences of the Gothic tournaments, and bull-fights of the Spaniards, when that sex, whom compassion and an abhorrence from spectacles of blood may be supposed the last to forsake, was admitted to the most conspicuous seats; as if to gratify them were the chief object of the entertainment. All is mad mirth, and drunken joy, with an American village, while their captive is wasting under their protracted tortures; compassion, or even indifference, would vitiate the festival. Humanity may have repelled from, but never invited a single guest to the cruel entertainments of our own nation. It is to that polished humanity, which a cultivated philosophy and a purer religion have introduced amongst us, that we owe the disrepute into which these vulgar jollities have at length happily fallen.

Such representations are, therefore, utterly dissimilar, in their effects upon the heart, to the representations of tragedy and romance. Humanity renounces the one, but welcomes the other. In those a brutal joy reigns triumphant;

in these—if there be a joy, it is of a singular kind; it wears all the dress of sorrow; and the heart feels that there is a pain more than proportioned to the joy. It is surely, therefore, unphilosophical, to reduce, under one class, propensities which are of so different a cast and influence; nor can that ingenious French critic, the Abbè du Bos, be justified, in deriving them, without distinction, from one common source in the human mind.

But whence then is derived, and how are we to account for, this strange intermixture of pain and a something like joy, excited, in the same instant, by the same object, each apparently dependant on each other, and yet not blended together in one undistinguished mass.—Before I attempt the solution of this singular, but universal character of man, it may not be amiss to take a brief view of some of the most celebrated theories on this subject, and the rather, as the examination of these may lead to the true solution.

The French critic just mentioned, the Abbè du Bos, whose reflections on poetry, painting, and music, form a very entertaining work, refers the solution of this difficulty to that aversion which we have to indolence; and in consequence to the delight we feel in having our most active and lively passions roused.

This account is striking and bold, as it derives a great deal from one simple and uniform principle. If, indeed, there be such a principle in human nature, exactly as he represents it, original and independant; and if it be adequate to the effect which he ascribes to it; and if the mind be sensible of such a reference in the instant of its most interesting emotions; and if the inclination to be thus moved, be proportioned to the force of this supposed principle; we could not wish for a more satisfactory solution, for one which more happily applied itself to the whole subject in question.

Another ingenious Frenchman, Monsieur Fontenelle, so well known to the literary world by his Dialogues, History of Oracles, and Plurality of Worlds, in some reflections on the subject of poetry, has hazarded the following fine-spun theory:—Pleasure and pain, says he, like many other extremes, approach, and, at a certain point, pass into each other. Pleasure, pushed too far, becomes pain; and the movement of pain, a little moderated, becomes pleasure.—He is obliged to adopt into his system the fundamental principle of Du Bos; for the heart, he says, loves to be moved, and therefore objects, which are melancholy, and even disastrous and sorrowful, are adapted to it; provided that they are softened by some circumstance. This softning circumstance, according to him, is the comfortable reflection, that the whole is but a fiction;

without which, the spectacle would be painful beyond the degree, in which it is capable of passing into pleasure.

This is the spirit of Fontenelle's theory; a theory so exceedingly refined, that we hardly know how to lay hold of it. It does not present us with any thing analogous to the real feelings of the heart; and is, indeed, contradictory to the very nature of things. Pleasure and pain, as simple sensations, have no intercourse with each other; though the transitions from the one to the other may be exceedingly quick, and may have their origin from the same external objects. For as objects are of a mixed character, the sensations may be mixed also; and in some, the painful circumstance, after a certain interval, may disappear, and vice versâ. But where the characters of the painful and the pleasant continue undiminished, the sensations which correspond to them will continue also; and each, as the causes of them are alternately contemplated, be separately excited; or that, which is the balance of the separate sensations will remain.

If, as Fontenelle asserts, pain can of itself pass into pleasure, and without any additional cause, and it be in the moment of the transition that the pleasureable sensation presents itself, it will be exceedingly difficult to determine, according to this theory, what the predominant sensation will be. If the painful sensation be then evanescent, the pleasureable one ought to be nearly unmixed; if the painful one be then at its height, the sensation of pleasure must be hardly perceivable, and cannot, methinks, account for the interest which we have in the representation. For the feelings, during a tragical representation, are not of this dubious and indeterminate character; the pain and the pleasure, if we must give the denomination of pleasure to the interest which we have in the spectacle, are distinct, and at the same moment are each highly exquisite. There is, in truth, no passing of the one into the other.

It is a farther objection to the theory of Fontenelle, that he has assigned no proper source of pleasure, which can give its complexion to the pain; the circumstance which he has noticed is merely an alleviation, and can only account for a diminution of the pain. This circumstance of the whole being but a fiction, has exceedingly little, if any, influence in softening and alleviating our painful sensation; though it be true, that if it were a spectacle of real misery, we should be repelled from approaching it at all. But it is also true, that the more the fiction is kept out of view, the more perfect is the art of the poet, and the more perfect the effect of the imitation upon the mind of the spectator; whose interest rises to its greatest height, when, by a kind of divine power, he is carried entirely out of the consideration of self, and contemplates nothing but the misery, as if it were real, and enters into it with all the glow of natural feeling. The solution, therefore, must be sought for in some other principle than the whimsical conceit of a middle something, between pleasure and pain, founded on the cold reflection, that the whole is a delusion. It may account, in some degree, for the phlegmatic dialogues of a French tragedy-maker; and for the dubious sensation, the middle something between pleasure and pain, which they excite; but will never unfold the feelings which the magic genius of Shakespeare stirs up in the soul.

The selfish system in morals, which extracts a joy out of a painful scene, from the groveling consideration, that, whatever sufferings are exhibited to our view, we are ourselves in a state of perfect security, is so grossly false, that a moment's consideration shakes it off with indignation, and leaves it to the sordid soul, which first conceived the idea. This would suppose that suffering and distress were in themselves a grateful spectacle, if they affect not ourselves; that there was a real malignity in the human heart, and that it recurred to them as to a feast. This is a horrid untruth; such spectacles are, in their nature, painful; and all that the consideration of our own security can effect, will be only to render them less painful; but of itself can give us no interest in them, nor render them at all attracting to us.

The celebrated David Hume has offered a more plausible theory; or rather, has added to the systems of Du Bos and Fontenelle; by referring the greatest part of that pleasure, which springs out of the bosom of uneasiness, and yet retains all the features and symptoms of distress and sorrow, to the bewitching power of that eloquence, with which the melancholy scene is represented. The effect, he says, is like to the composition of two forces, which, combining together, produce a new direction, a direction not contrary to that of either, but partaking of both.

Of these four illustrations of the question, the first and the last, viz. of Du Bos and Hume, require a particular discussion, in the progress of which, the truth will probably unfold itself.

It is a misfortune, in moral as in natural philosophy, that the theory, which is to account for important phenomena, is often the creature of a bold and lively imagination, and not the modest result of careful observation and experiment. As a theory, which is built on this solid ground of observation and experiment, will always follow us downwards to the explanation of facts; so every fanciful system is found to decline this test; and, if compelled thereto, confesses its insufficiency to account for the phenomena of nature.

It is, therefore, a general objection to both these systems, in the first view of them, that the principles, to whose operation they ascribe such singular effects, are either not at all present to the mind, while their influence is supposed to be exerted and felt; or make a very dubious appearance, and utterly vanish at that critical moment, when the effect of the tragic imitation is the greatest; viz. when their presence ought to be most conspicuous and manifest to the very sense. Who in the moment, when the heart is rent and agonized with a tragic scene, can say to himself: that he converts the exquisitely painful feelings of that moment, into the character of pleasure; or so as to be attracted by, and be passionately interested in the scene; because his soul abhors the languor of indolence, and delights to be violently moved? Or who, with Mr. Hume, is ruminating on the ingenuity and eloquence of the artist, which can give to a fictitious scene all the glowing colours of nature? Who, under the possession of sympathetic sorrow, has his eye fixed upon an object of intellectual taste, and feasts in proportion to the opinion which he has of the poet's skill?

If the system of Du Bos be true, our attraction to spectacles of a tragic character, and the interest which we feel in the representation of them, ought to be proportioned to the perturbation of the mind, and to the violence of the emotions which are excited. But the most violent emotions shall be attempted to be raised, while we are only disgusted with the scene; because the whole is destitute of that single requisite, which alone has power to attach us to misery. The play of the Libertine abounds with scenes, which address themselves to our terror and indignation; but we abhor the scenes, because they exhibit no field for a benevolent compassion; they are not the tragedy of a man, but of a fiend; it is not human nature, but hell, which is exhibited. There is enough of violent action, enough of terror and distress, to rouse and agitate; but being out of the field of man, we cannot sympathise; or our horror and indignation are stronger than our sympathy, and we detest a picture, which awakens not those divine feelings, to which the soul of man delights to commit itself.

Otway, the eldest son of Shakespeare, has greatly offended in this view, and greatly lessened the impression of his genius, by the immorality and profligacy of his principal characters. We cannot feel for them as we wish, and our interest in their sufferings is diminished, as they appear to deserve them. Some touching pictures of innocent and virtuous distress, to which a pure and benevolent sympathy attaches itself, have rescued him perhaps from our utter disgust. The innocent, the gentle Monimia, and the more dignified virtue of Belvidera, relieve the horror of the villains with whom they are unhappily associated, and support in us an interest through the whole drama.

The tragedy of the Robbers, and other productions of the German drama, have the vice of Otway, but with more extravagance and scorn of nature, and therefore are more repulsive of the heart. With them to create what God never designed, and what human wickedness never meditated, is Genius; and to terrify is Sublime. If the system of Du Bos, therefore, be true, these are the perfection of the drama; but, in defiance of his principle, they do not attract, they repel; and their admirers mistake astonishment for an impression of the grand; and a horror and revulsion from scenes of dreadful suffering, for an impression of the pathetic.

But on this system, the interest in the representation ought to be proportioned, not only to the bustling of the scene, but to the bustling disposition of the spectators. This must be allowed, if the abhorrence of indolent repose, and the delight in being stirred, be the secret cause which attracts us to misery; and derives to us a pleasure in spectacles, which in their nature are painful. But this is utterly contrary to fact; and the attention to fact, in this instance, as in what I have already noticed, will demonstrate the incompetence of Du Bos' theory; and discover the true source of the phenomenon. Men of the most active turn, who with hardly any other motive than to follow the violent impulse of their own turbulent spirits, can throw society into convulsions, and feast as it were on those continually renewed scenes of distress and terror which mark their ferocious path, are not the persons on whom you would expect the representations of tragedy to produce their natural and most powerful effect; but the gentle, the flexible, the compassionate, and benevolent. The former resemble the characters which, in the introduction to this essay, I have noticed among the Romans, Goths, Spaniards, Indians, and bull-baiting Englishmen. But the man of composed and tempered manners, in whose breast compassion, mercy, and benevolence sovereignly reign, is shocked at such characters; nor could possibly encounter their rude and brutal entertainments; yet his heart is the theatre whereon tragedy acts all her glorious wonders.

The same objection bears with almost equal force against the system of Hume. It is not the man of letters, who may be supposed to be the best judge of composition and eloquence; nor yet the man of a lively imagination, to whom the effect of tragical representations is peculiarly appropriate. Though if a heart mellowed to pity be joined to these advantages, the interest in such spectacles will perhaps receive an increase from this superadded source; but tragedy exercises her utmost power on even the unlearned and untutored, if there be found a feeling and benevolent heart.

The same judgment is farther illustrated from the powerful effect on an audience of a story happily adapted to the purpose, though the composition be materially faulty. It shall awake all the passions in which tragedy rejoices, more than all the faultless productions of the Greek and French drama. Banks and Southern are poets of but a middle fame; yet the Earl of Essex and Oronooko will dissolve an audience in tears, so long as the human heart, and the inclinations which it has received from its maker shall endure. If tragedy owe her attractions to the eloquence of the poet; it is to the eloquence of nature, not of art. An untutored genius, having strong conceptions, a heart that can enter into the feelings of a fellow heart, quick in catching the most striking features of distress, judgment to select a happy tale of virtuous suffering, and simplicity to follow nature in her plain walk, will in the fabrication of tragedy reach its highest excellence. Such was Shakespeare, and such, in a less degree, were a few of his neglected cotemporaries; it was to their exquisite sensibility, to their ignorance of art and fastidious refinement, which might have diverted them from the resistless eloquence of nature, that they owe their superiority over the lettered sons of every age and nation. But, whatever be the skill of the poet, whether that of nature, or art, or of both; this skill is not critically examined into during the representation; it is felt; it no more requires the critic's acumen to capacitate us for this effect, than the philosopher's penetration into nature to feel the lightning. It is not wisdom, but the affectation of it, which in so interesting on hour, is attentive to all the finesses, delicacies, and ingenuity of the poet. The ingenuous simplicity of a plain feeling heart is better employed; it is worth a thousand such wise ones; it is the spectator and judge, whom tragedy more delights in, from whom she will receive a more abiding sentence.

Mr. Hume very justly observes, that the force of imagination, the energy of expression, and the power of numbers, are all of themselves naturally pleasing to the mind. But the connection between this position and the following conclusion, is wide as heaven and earth, when he adds: that if the object lays hold of some affection, the pleasure still rises upon us, by the conversion of this subordinate moment into that which is predominant. The predominant emotion he assumes to be the pleasure excited by the eloquence of the artist; the affection laid hold upon is the painful sensation of the spectacle. Which of these two emotions is most likely to be predominant has been just now discussed; but that a pleasurable emotion of one kind should lay hold of a painful emotion arising from a very different source, and derive augmentation to itself from this combination, is an extraordinary position; and as contrary to the laws of nature in the moral as in the material world. Whenever contraries act upon each other, diminution and not augmentation is the result. The pain continues to be pain so long as the character of the representation is preserved, and undergoes no conversion at all.—Eloquence, on whatever subject, is pleasing, but what then? If there were not some other circumstance, some powerful law of our nature which attached us to, and gave us an interest in a subject highly painful, the pleasure merely arising from the display of talents would be less pleasant, because counteracted every moment by what is painful; it must inasmuch as the pain amounts to be diminished, and if the pain greatly over-balanced the pleasure, it might be entirely obliterated.

He reasons thus himself, when he converts the proposition; observing, that if the pleasure arising from the conduct of the representation were not predominant, the effect would be destroyed, and sorrow would absorb the mind. Before the pain had no effect upon the pleasure to destroy any part of it; but as if it were of the same family, very obligingly ministered to its increase. Mr. Hume has endeavoured to avail himself of an allusion to the well-known laws of motion; but to answer his purpose he has assumed, that they resemble two forces moving in different, but not opposite directions. But their resemblance is to two forces moving in absolutely contrary directions; the effect of which is, that the greater force continues indeed to move onward in its proper direction, but with a diminution of force, equal to that of the lesser.

It is farther to be observed, on the theory of Hume, and indeed of both the French philosophers, that the one principle which satisfactorily accounts for the whole phenomenon, must be supposed to be admitted. If not, what is meant by the concession of each, that the objects represented are in their nature painful to the minds of the spectators? And why painful, but from the power of that sympathy, which enters into fellow suffering? There is no other principle in man adequate to the effect. If this be not allowed as an overpowering law of human nature, no account can be given, why a being, interested in himself, and averse to pain, should transplant into his own breast the pain of another, and court a partnership with affliction. Mr. Hume may be supposed, though perhaps involuntarily, and while nature, not theory, was speaking, to have conceded something more than this; when he observes that the pleasure, notwithstanding the supposed conversion, wears the features and outward symptoms of sorrow and distress.

Again, if to the eloquence of the poet, as to its proper cause, be ascribed the pleasure, or to speak more properly, the interest which we take in tragedy; why will not eloquence, employed on other subjects, equally interest and captivate us? Why will not pictures of other objects, equally just and animated, equally engage our affections? If it be said, that the objects must be in themselves interesting, then the effect is not derived wholly nor principally from the eloquence and manner of the artist, but from some other consideration, which previously interests us in the objects that are represented.

Mr. Hume has very artfully managed his iliustration from facts, nor is it to be denied that the principles, to which he ascribes the whole, have their influence. But with less attachment to a pre-conceived theory, it is, methinks, impossible to avoid the discovery of the great master principle, even in the very facts which he himself adduces.

The notion of conversion, which he borrows from Fontenelle, is an arbitrary assumption; and may be classed with the Cartesian notion of nature's abhorrence of a vacuum. But if, by the delight of being moved be understood, that every passion of the soul delights in its proper exercise; it is true that the mind of man will, under the impulse of any affection, be moved towards the object that is united to the affection. And it is also true, that rich imagery, strong expression, the harmony of numbers, and the charms of imitation, are all grateful to the mind, independant of any end to which they are directed. It is to these that certain productions owe all their interest; but they are light auxiliaries in the grander productions of tragedy, unless so far as they are necessary to the perfection of the imitation; without which there is no representation adapted to nature, and therefore nothing fitted to lay hold of the heart.

What Mr. Hume intends by his reference to the deserted parent, is difficult to say; when he asks the question, Who would ever think of it as a good expedient for comforting an afflicted parent, to exaggerate, with all the force of oratory, the irreparable loss, which he has met with by the death of a favourite child? Certainly it were a ridiculous part to think of comforting the parent by any such means; but the idea of comforting by such a procedure, or the idea of comforting in the thing to be illustrated by this allusion, if any thing be meant to illustrate, are quite out of the question. No one ever conceived the intention of the tragic poet to be, to comfort his audience; he means to distress them; he exerts the utmost force of his genius to distress them; to give them as touching a feeling of the sorrow which he paints, as mere sympathy is capable of receiving. So is it with the deserted parent: He who would ingratiate himself with him cannot take a more effectual means than to catch him in his tenderest moments, and with all the eloquence of words, expatiate on the virtues, the shining qualities, the promising hopes of his child. Such a conduct would be unkind; it would be cruel; but it would be effectual. He would win the heart of the parent, in the very moment, and by the very act, which was rending it in pieces. It is through the gate of sympathy that he gains this access to his heart; the parent embraces the man, in whom he acknowledges a fellow heart, one who appears to feel up to the very height of that sense, which he has himself of his loss.

I have bestowed, perhaps, more attention on these systems than they may be thought to merit; as whatever ingenuity they may lay claim to, they have little ground of experiment in human nature to stand upon. But the examination of them has answered the principal purpose. The analysis of their defects discovers in every step the real source of the whole phenomenon; and what is of more importance, it discovers the wise provision of the great author of all for conducting the œconomy of the moral as the material world. But the simplicity of nature offends some; to discover only what every one may discover, and what nature forces upon the notice of all, argues no superiority of genius; they suppose, they invent, they create, and in defiance of nature they erect vain monuments of their own wit and ingenuity.

In every view of the human mind, during the exhibition of tragic imitations, compassion, or sympathy in a more extended sense, presents itself as the operating principle, the immediate sense to which such scenes address themselves. This is the only principle within us, which is sufficient to attach us to misery; to connect a being who is interested for himself, and is in the constant pursuit of his own proper happiness, to connect such a being with the unhappy, and as by an irresistible impulse introduce him to a partnership in their afflictions.

The contradiction, therefore, which this propensity, at the first view, carries with it to a leading principle of our own natures, vanishes when we consider it in this important light; we appear to act in perfect consistence with an acknowledged, and powerful, and highly valuable principle of our natures. While our other senses are continually opening themselves to their proper objects, it would be strange, indeed, if this internal sense, whose aim is directed to the noblest character of man, were reluctant to its proper exercise, and averse to those objects and to those scenes, which immediately address themselves to it. This would argue indeed a defect in his constitution, such as could not easily be reconciled to our ideas of that designing wisdom, which intended him to be one beautiful and harmonious whole.

If, indeed, the end of compassion, as a principle of human nature, were directed only to particular exigencies in human life, as an instant stimulus to acts of kind protection, and humane alleviation of fellow misery; it might be thought sufficient if it were reserved for such interesting occasions; and the mind were not led by a farther impulse to the participation of distress, when no immediate object of our benevolent interposition is before us. But compassion was implanted in us with more extensive views, not merely that it might come in aid of our good will on pressing occasions, which may justify the pain it gives us; but that, by a more regular and uniform exercise, it might minister to the sublimest virtue of man, and dispose us, on every occasion, to wish and do well to the creature like ourselves.

There is a striking difference in the exercise of this sense, as referred to the real distresses of human life, and to the fictitious ones of tragedy; and this difference is wisely adapted to their respective uses. When we are summoned to immediate action, the sympathetic feeling is pain unmixed, in order to give power and velocity to the benevolent stimulus. We have no propensity, therefore, to such scenes; we do not wish them to exist, in order that our compassion and benevolence may have a field to action; though he who orders, or rather permits them, has wisely provided that the calamities of human beings shall operate to the moral improvement and perfection of their minds. But where the distress is merely fictitious, or the representation of what is past; and no kind humane interposition is expected from us, but only the cherishing an uniformly benevolent temper may be supposed to be in view; then the pain is mixed and tempered with something that we know not to give a name to, something that must attend on every mind in the exercise of its best affections, a complacence such as a superior spirit may be supposed to feel, if he were viewing the infirmities and distresses of some inferior system. To such scenes, which imply no augmentation of the real calamities of our fellow creatures, but may minister to the augmentation of our good will towards them, we are moved by an internal impulse; by an impulse which we approve of in reflection, and which those who are little accustomed to reflection do however obey.

In speaking of the affection of the mind to tragic representations, I have adopted the language of the writers I have opposed, while I was discussing their theories; and I may myself, in contemplating the impulse to tragical representations, and the complacence in those benevolent affections which are excited, and distinguishing these from the impressions on the heart which the spectacle of pain excites, have used the term pleasure, yet with a visible dubiousness and reluctance, because language immediately suggested no other term; though it by no means corresponded to my idea, nor to the real truth of the sensation. When the mind is but moderately interested in any tragic scene, and has leisure to attend to no other circumstances than what are appropriate to sympathy, it may be sensible to feelings which are in their nature pleasant, but chiefly, if not entirely, springing out of these collateral circumstances. But when the increasing distress of the scene entire possesses the mind, all semblance of pleasure vanishes, and the feelings are those of pure compassion; but not unless in some particular instances, painful up to the degree of aversion. It is not strictly just, therefore, to say, that the feelings at such an instant are in any degree pleasant; as it would be grossly false to say, that we are instigated to this participation of distress by the view of pleasure; unless all the sympathetic feelings be referred to the class of the agreeable ones. We are carried, indeed, by a virtuous impulse to converse with distress; the certainty that we shall not be spectators of any real suffering, withdraws all aversion to this impulse; but under this assurance we surrender ourselves up entirely to the poet; we enter into his views; we are carried out of ourselves into his fictitious scenes, as if they were real. We often feel from them an exquisite pain, which oppresses our minds for a considerable time after the representation is over, and sinks too deeply into those of a delicate and susceptible make. Yet we return to such scenes; not that pain is desirable, not to seek for pleasure in the field of pain; but the better inclination of our natures determines our conduct; and the distressing sensations, to which we are exposing ourselves, appear with that softened aspect, that grace, which a virtuous and benevolent melancholy always wears.

This investigation of the effect of tragedy on the mind, will account, in a great measure, for the superiority of the best productions of the moderns above those of the ancients, and of the English tragedy above that of the French. The pictures are more exquisitely finished; the characters of the sufferers are more interesting; and more powerfully lay hold on our affections, and plead for our compassion. Domestic life and domestic manners were more gross and undressed among the antients; the social passions were but half awakened among them; and, therefore, the pictures of domestic happiness are not near so interesting, nor can, to our improved taste, present such rich subjects of compassion. The French tragedies are in this respect also far inferior to the English; wit, gallantry, and philosophic declamation, are more displayed than touching scenes of pure and ingenuous distress. Tragedy, in order to be perfect, ought to be throughout an animated picture; enlivened, enriched by grandeur of sentiment, by every exhibition of mind which is fitted to interest a fellow mind; but still it must be a picture. When this is conducted by a masterly artist; it is then that all yield to the genius of tragedy; we feel that there is an eloquence in the exhibition of virtuous distress, suffering from the incidents of our natures, from the pardonable errors of human judgment, from the follies or vices of others, or under the iron hand of oppression and cruelty, which mocks all the power of wisdom to equal; which the lettered and the polished can no more resist than the most uncultivated child of nature. And this eloquence is the instrument of a wise providence, whereby he forms and fashions our hearts according to what he designs and approves, and calls forth those benevolent affections which move not at the voice of reason and calm philosophy.

Experiments and Observations to determine whether the Quantity of RAIN *and* DEW *is equal to the Quantity of* WATER *carried off by the Rivers and raised by Evaporation; with an Enquiry into the* ORIGIN *of* SPRINGS.

BY JOHN DALTON.

READ MARCH 1, 1799.

It is scarcely possible to contemplate without admiration the beautiful system of nature by which the surface of the earth is continually supplied with water, and that unceasing circulation of a fluid so essentially necessary to the very being of the animal and vegetable kingdoms takes place. Naturalists, however, are not unanimous in their opinions whether the rain that falls is sufficient to supply the demands of springs and rivers, and to afford the earth besides such a large portion for evaporation as it is well known is raised daily. To ascertain this point is an object of importance to the science of agriculture, and to every concern in which the procuration and management of water makes a part, whether for domestic purposes or for the arts and manufactures.

For the sake of perspicuity I have distributed the subject under four heads:

1. Of the Quantity of Rain and Dew.
2. Of the Quantity of Water that flows into the Sea.
3. Of the Quantity of Water raised by Evaporation.
4. Of the Origin of Springs.

SECTION I.

An Estimate of the Quantity of Rain and Dew that falls in England and Wales in a year.

Rain-gages have been fixed of late years in almost every part of the kingdom; by means of them we are enabled to determine, with considerable exactness, the depth of water that the rain yields in any given place. Inland counties have less rain than maritime ones, especially those which border on the western seas. But a still greater difference seems to take place between a mountainous country and a champaigne, or flat country: In the former there often falls double or triple the quantity of rain in a year, that there does in the latter, and never less than an equal quantity. It may be observed, that several years account of the rain at any place is required before a medium yearly quantity can be obtained with sufficient accuracy. The following is perhaps the largest collection of accounts of rain fallen in different places in England that has hitherto appeared: They are mostly taken from the Transactions of the Royal and other Societies.

Counties (maritime).	*Places.*	Mean annual depth in inches.
CUMBERLAND	Keswick, 7 years	67. 5
	Carlisle, 1 year	20. 2
WESTMORLAND....	Kendal, 11 years...........	59. 8
	Fell-foot, 3 years	55. 7
	Waith Sutton, 5 years	46
LANCASHIRE	Lancaster, 10 years.........	45
	Liverpool, 18 years.........	34. 4
	Manchester, 9 years	33
	Townley	41
	Crawshawbooth, near Haslingden, 2 years	60
GLOUCESTERSHIRE	Bristol, 3 years	29. 2
SOMERSETSHIRE ..	Bridgewater, 3 years	29. 3
CORNWALL	Ludguan, near Mount's Bay, 5 years	41
	Another place, 1 year	29. 9
DEVONSHIRE	Plymouth, 2 years	46. 5
HAMPSHIRE	Selbourne, 9 years	37. 2
	Fyfield, 7 years	25. 9
KENT	Dover, 5 years	37. 5
ESSEX	Upminster, ...	19. 5
NORFOLK	Norwich, 13 years	25. 5
YORKSHIRE	Barrowby, near Leeds, 6 years	27. 5
	Garsdale, near Sedbergh, 3 y.	52. 3
NORTHUMBERLAND	Widdrington, 1 year	21. 2

Counties (inland).	*Places.*	*Means.*
MIDDLESEX	London, 7 years	23
SURREY	South Lambeth, 9 y.	22. 7

Hertfordshire ..	Near Ware, 5 years	25
Huntingdonshire	Kimbolton, 7 years	25
Derbyshire	Chatsworth, 15 years	27. 8
Rutlandshire ..	Lyndon, 21 years	24. 3
Northamptonshire	Near Oundle, 14 years	23
	General Mean	35 2

This general mean of 35. 2 inches is, I apprehend, a little above the medium for England and Wales, as the greater number of places are those where much rain falls. If we take a mean for each of the above-mentioned counties (where more than one place in a county is given) and then a general mean from the counties, the result is a reduced mean of 31. 3. Even then it may be objected that the greater part of the counties are maritime; but it must be observed, that there is no account of rain in Wales; and we may safely conclude, that the rain in Wales would exceed the last-mentioned mean as much as the inland counties of England, not in the above list, would fall short; because Wales is both a mountainous country, and exposed to the sea.

We will, therefore, conclude, that the mean annual depth of rain in England and Wales, deduced from these 20 counties, is 31 inches: A quantity which subsequent observations, I am confident, will not diminish, and probably not increase much.*

It remains to estimate the quantity of dew that falls in a year.—Some have doubted whether dew is derived from the air or the earth; but a proper attention to the phenomena will satisfy us, that it is a deposition of water, evaporated during the heat of the day. With respect to the quantity that falls in a year, we are much at a loss, as no daily observations have been made for a series of time that I know of: indeed, it would be difficult to prescribe a mode of observation. Dr. Hales† relates some experiments made to determine the quantity of dew that falls upon moist earth, from which he estimates the annual dew at 3. 28 inches. But it is probable that the dew which is deposited on grass is much more copious than what falls on moist earth, because grass exposes much more surface in a given acre of ground. If we take the dew at 5 inches annually, it will probably not be much over-rated: supposing it should be over-rated, the

* The editors of the Encyclopedia, under the article Weather, from 16 places of observation, make the annual mean for Great Britain 32. 53 inches; and M. Cotte, in the Journal de Physique for 1791, gives a mean derived from 147 places in different parts of the world equal to 34. 7 inches.

† Veg. Statics. Vol. 1. Page 52.

excess may stand against the rain that is lost by evaporation from the surface of the rain-gage each time it rains.* Wherefore, upon the whole, we

* Since writing the above paragraph on dew, I have had occasion to make several experiments on the subject of aqueous vapour, as it exists in the atmosphere, the result of which will, I am persuaded, materially illustrate this important question in physics.—At present I shall only observe, that the following conclusions seem deducible from the experiments above referred to.

1. That aqueous vapour is an elastic fluid *sui generis*, diffusible in the atmosphere, but forming no chemical combination with it.

2. That temperature alone limits the *maximum* of vapour in the atmosphere.

3. That there exists at all times, and in all places, a quantity of aqueous vapour in the atmosphere, variable according to circumstances.

4. That whatever quantity of aqueous vapour may exist in the atmosphere at any time, a certain temperature may be found, below which a portion of that vapour would unavoidably fall or be deposited in the form of rain or dew, but above which no such diminution could take place, chemical agency apart. This point may be called the *extreme temperature* of vapour of that density.

5. And that whenever any body colder than the extreme temperature of the existing vapour is situated in the atmosphere, dew is deposited upon it, the quantity of which varies as the surface of the body and the degree of cold below the extreme temperature.

N.B. The *extreme temperature* of vapour in the atmosphere varies all the way from the *actual* temperature of the atmosphere to 10, 15, 20 or more degrees below it.—The point may generally be found in the hottest months shall have 36 inches of water at a medium annually on the surface of the earth in England and Wales, reckoning 31 for rain and 5 for dew.

According to Guthrie, the area of England and Wales is 46.450 square miles. This reduced to square feet, gives 1.378.586.880.000: which, multiplied by 3 feet the annual depth of rain and dew, gives 4.135.760.690.000 cubic feet of water=153.176.320.000 cubic yards, or 28 cubic miles=115 thousand millions of tons in weight, nearly.—We must now consider how this enormous quantity of water is disposed of.

There are two principal ways by which the water derived from rain is carried off again: One part of it runs off immediately into rivulets, or sinks into the earth a small way, breaks out again in lower ground in the form of springs, thence makes its way to some river, by which it is conveyed into the sea—another part is raised into the atmosphere by evaporation. We take no notice here of the decomposition of water by vegetables; because it is presumed that in the course of nature the principles are combined and water formed again.

by pouring cold spring water into a dry and clean glass, and marking what degree of cold is sufficient to produce a dew on the outside of the glass; at other times frigorific saline solutions may be used.

§ 2.

An Estimate of the Quantity of Water that flows into the Sea from England and Wales in a Year.

To calculate the quantity of water that flows down any one river into the sea in a given time, seems at first view a question of great difficulty. The necessary data, however, may be obtained with considerable exactness, by proper observations, and then it becomes an easy case of mensuration. Dr. Hutton, in his Philos. and Mathemat. Dictionary, article River, proposes a very good method to determine by experiment the velocity of a river:—A cylindrical piece of light wood, its length somewhat less than the depth of the waters, is to be taken, and a few small weights attached to one end in order to make it swim upright. To the other end a small rod is fixed in the centre in direction of the axis.—This being suffered to float down the stream will move with the velocity of the water; and if the rod be observed to incline towards the river upward or downward, it shews the current to be more rapid at the bottom or surface respectively.

This experiment being made in the middle and near the sides of a river, a medium velocity may be obtained. Then the medium, breadth, depth, and space run over in a certain time being multiplied together, will give the quantity of water that flows down in that time.

Dr. Halley, in order to estimate the quantity of water that flows into the Mediterranean Sea by means of rivers, makes a comparison of the great rivers of Italy, &c. with that of the Thames. (Philos. Transact. Abridg. Vol. 2. Page 110). He assumes the breadth of the Thames at Kingston Bridge to be 100 yards, its depth 3 yards, and velocity 2 miles per hour. He professedly overrates the dimensions, in order to allow more than a sufficiency for the streams received below Kingston. This assumption gives the area of a transverse section of the river=300 square yards, and the quantity of water flowing down=20.300 000 tons in a day. This must be overrated by at least, I think, one third:—If the breadth be assumed 100 yards, the depth 3, and velocity 2 miles per hour, it will then give $\frac{2}{3}$ of the result above mentioned; or it will amount to the same thing if we take $\frac{1}{8}$ part from all the three data assumed by Dr. Halley, the result being $\frac{2}{3}$ of that above; amounting in the year to 166.624.128.000 cubic feet, which is a little more than $\frac{1}{25}$ part of all the rain and dew in England and Wales in a year, as above deduced.

By an inspection of the annexed map of the rivers of this country, as well as by a fair calculation, it appears, that the water of the Thames is drawn from an extent of country of about 600 square miles, or $\frac{1}{8}$ of the area of the whole, nearly. The Severn, including the Wye, spreads over an equal or greater extent of country: And that collection of rivers which constitutes the Humber is superior to either of the other two in this respect. As far as my own observation goes, the Severn and Wye must disembogue as much or more water than the Thames; the Humber I have not seen collectedly, but have noticed most of the branches constituting it, and should apprehend it can not be inferior to the Thames: All other circumstances being the same, the quantity of water carried down by any river should be as the area of the ground from which the water is derived, and on this account the Humber ought to exceed the Thames.*

The Severn, which is partly derived from the mountainous country of Wales, is certainly the most rapid of the three rivers, and probably carries down the most water: As the Thames, however, is generally considered to take the lead, we will suppose, upon the whole, that these three rivers are equal in this respect.

The counties of Kent, Sussex, Hampshire,

* A more perfect theorem will be given afterwards, for finding the quantity of water carried down by any river.

Dorsetshire, Devonshire, Cornwall, and Somersetshire, from the Medway to the lower Avon inclusively, in an extent of 11.000 square miles, do not present us with many large rivers. From their number and magnitude, we cannot form a high estimate of their produce. The quantity of rain for those counties is indeed near the average for the kingdom, as far as the preceding observations determine; but the milder temperature of their winters and greater heat of their springs and summers, will cause a greater evaporation than in some other parts: It is probable the rivers in these counties may amount, when taken together, to $1\frac{1}{2}$ times the magnitude of the Thames. The rivers that disembogue their waters on the coast of Lincolnshire, Norfolk, Suffolk and Essex, from the Humber to the Thames, though drawn from a country of 7000 square miles, manifestly fall far short of the Thames. The two places in this district, for which we have accounts of the rain, Norwich and Upminster, give a mean of only $22\frac{1}{2}$ inches annually. This, with the flatness of the country, which prevents the water from running off in some degree, makes the rivers much less than what might otherwise be expected from the extent of ground. There are but 3 or 4 of any consequence. Probably all the rivers may amount to half the size of the Thames. There

SKETCH of the RIVERS
in England and Wales
divided into districts

remains above 6000 square miles in Wales, from the Wye to the Dee, inclusive of the last, and the northern counties of Lancaster, Westmorland, Cumberland, Northumberland, and Durham, with part of Cheshire and a small part of Yorkshire, from the Mersey round by the Tweed to the Tees, amounting to 7 or 8000 square miles, to be estimated.

These two divisions, though not larger than some others, abound in rivers, many of which are considerable in magnitude and of great rapidity. The rains at an average, it is probable, are double what they are in the S. E. counties of the kingdom. The rivers in these two districts cannot fairly be estimated, I think, at less than *four* times the Thames.—It appears, then, that by this estimation, the water carried off by all the rivers in England and Wales, may amount to *nine* times that carried off by the Thames= 13 inches of rain. There remains still *sixteen* times the water of the Thames, or 23 inches of rain to account for, before we have disposed of all the rain and dew.

§ 3.

An Estimate of the Quantity of Water raised by Evaporation.

Upon looking over the surface of any country, three principal varieties of surface present themselves to view, as far as respects evaporation, namely, *water*, ground covered with grass and other vegetables, and bare soil. The difficulties that occur in attempts to find the quantity of water evaporated in those three cases, are perhaps the principal reason why our knowledge on this head is so imperfect.

As far as experiments hitherto made authorise us to draw conclusions, it should seem that the evaporation from water is greatest; that from green ground is probably next, and that from bare soil the least: though we may presume, that the copious dews upon the grass more than supply the excess of evaporation above what takes place from a moist uncovered soil.

The most satisfactory experiments I have seen an account of, relating to the evaporation from a surface of water, are those of Dr. Dobson, made at Liverpool, in the years 1772, 73, 74 and 75. (Vid. Philos. Transac. Vol. 67) —He took a cylindrical vessel of 12 inches diameter, and having nearly filled it with water, exposed it besides his rain-gage of the same aperture, and by adding water to it, or taking it away occasionally, he kept the surface nearly of the same height, and carefully registered the quantities added or taken away, by a comparison of which with the rain, the amount of the evaporation was ascertained. The mean monthly evaporation for 4 years was—January 1.50 inches.—February 1.77.—March 2.64.—April 3.30.—May 4.34.—June 4.41.—July 5.11.—August 5.01.—September 3.18.—October 2.51.—November 1.51.—December 1.49.—In all 36.78 inches. The mean rain for the same time was 37.48 inches.—In the year 1793 I found the evaporation from water in a similar way at Kendal for 82 days in March, April, May and June to be 5.414 inches. The greatest quantity evaporated on one of the hottest and driest days in Summer was a little above ,2 of an inch in depth.

The experiments to determine how much is evaporated from green ground and from moist earth, are very few that have come to my knowledge. Dr. Hales, from a few experiments, calculates that moist earth only throws off $6\frac{2}{3}$ inches annually.—This calculation must be far below the truth. Dr. Watson, Bishop of Llandaff, found that in a dry season there evaporated from a grass plat that had been mowed close, about 1600 gallons in an acre per day, which amounts nearly to ,07 of an inch in depth; and that after rain the evaporation was considerably more. Now supposing ,07 to be the medium daily evaporation for May, June, July and August, and that as much is raised in these 4 months as in all the rest of the year, the annual evaporation in such circumstances will be 17 or 18 inches, which is but half that observed from water at Liverpool, and 6 inches less than the reserve of rain stated above.

In order to ascertain this point more fully, and to investigate the origin of springs, my friend Thomas Hoyle, jun. and self practised an expedient as follows, beginning in the autumn of 1795. Having got a cylindrical vessel of tinned iron, 10 inches in diameter and 3 feet deep, there were inserted into it two pipes turned downwards for the water to run off into bottles: The one pipe was near the bottom of the vessel; the other was an inch from the top. The vessel was filled up for a few inches with gravel and sand, and all the rest with good fresh soil. It was then put into a hole in the ground and the space around filled up with earth, except on one side, for the convenience of putting bottles to the two pipes; then some water was poured on to sadden the earth, and as much of it as would was suffered to run through without notice, by which the earth might be considered as saturated with water. For some weeks the soil was kept above the level of the upper pipe, but latterly it was constantly a little below it, which precluded any water running off through it. Moreover, for the first year the soil at top was bare; but for the two last years it was covered with grass the same as any green field. Things being thus circumstanced, a regular register has been kept of the quantity of rain water that ran off from the surface of the earth through the upper pipe (whilst that took place) and also of the quantity of that which sunk down through the 3 feet of earth, and ran out through the lower pipe. A rain-gage of the same diameter was kept close by to find the quantity of rain for any corresponding time.

The following Tables shew the Result.

Water through the two Pipes.				Mean.	Mean Rain.	Mean Evap.
	Inch.	Inch.	Inch.	inch.	inch.	inch.
	1796.	1797.	1798.			
Jan.	1.897—	,680—	1.774+	1.450+	2.458	1.008
Feb.	1.778—	,918—	1.122	1.273	1.801	,528
March	,431—	,070—	335	,279	,902	,623
April	,220—	,295—	,180	,232	1.717	1.485
May	2.027—	2,443+	,010	1.493+	4.177	2.684
June	,171—	,726	——	,299	2.483	2.184
July	,153—	,025	——	,059	4.154	4.095
Aug.	——	——	,504	,168	3.554	3.386
Sept.	——	,976	——	,325	3.279	2.954
Oct.	——	,680	——	,227	2.899	2.672
Nov.	——	1.044	1.594	,879	2.934	2.055
Dec.	,200	3.077	1.878+	1.718+	3.202	1.484
	6.877—	10.934—	7,379	8.402	33.560	25.158
Rain	30.629—	38.791—	31,259			
Evap.	23.725—	27.857—	23.862			

The following observations were made when the water passed through both pipes: that is, when the vessel was filled up with earth above the level of the upper pipe.

			Top pipe Inch.	Bottom pipe. Inch.
1796.	Jan.	25	,190	,280
		30	,080	,114
	Feb.	2	,100	,254
		8	,196	,140
	May	1	,163	,000
		10	,060	,400
		12	,312	,175
		15	,190	,200
	June	3	,120	,040
		Total	1.411	1.603

The column of mean evaporation is derived by taking the difference of the two columns preceding it; but it should be observed that though this method is sufficiently exact in taking the year together, it is not so in taking the months severally, because it presumes that the earth in the vessel contains the same quantity of water at the end of each month, or is saturated with it; whereas in the Summer months it is frequently short of saturation. The consequence is, that the evaporation appears from this table to be something less than it really is in the Summer months, and something more in the autumnal.*

From these experiments it seems we may conclude—1st, That the quantity of water evaporated, *in the circumstances above related,* amounts to 25 inches of rain annually; to which if we add 5 inches for the dew, it will give 30 inches of water raised annually.

2d. That the quantity of evaporation increases with the rain, but not proportionally. Thus, 1797 gave the most rain and the greatest evaporation, &c.

3d. That it does not appear there is much difference betwixt the evaporation from bare earth, when there is sufficient depth of soil, and that from ground covered with vegetating grass. The account in 1796 is much what might have been expected, if the earth had been covered with grass.

As this account of evaporation, amounting to 30 inches, exceeds the medium reserve of rain of 23 inches, it demands an enquiry whether the rain is adequate, or whether the earth derives a supply of water from some subterranean reservoir, according to the opinion of some philosophers.

* N. B. The earth in the vessel always appeared as well supplied with moisture as the ground around it, in the driest weather.

With respect to the deficiency of 7 inches, there are *three* causes to be assigned for it, which appear to me fully sufficient, without having recourse to any source but that of rain for the supply of the earth in general.

1st. In the account of the rain that passed through the earth in our evaporating vessel, there are a few monthly products marked,+ those were occasioned by the bottle that received the water through the pipe being found with the water running over; this loss was placed to the account of evaporation; it could not be much, as the water was taken several times in a month, but possibly might amount to one inch in the year.

2d. The rain at Manchester, being $33\frac{1}{2}$ inches annually, exceeds the medium of 31 inches; and consequently, according to the preceding observations, the evaporation ought to exceed the medium.

3d. But the principal cause of the excess in our account of evaporation, I conceive to be the prevention of the water running off from the surface of the earth at the top, by having the earth below the level of the upper pipe: It has been seen, that when the earth was above that level, a great part of the water came off that way, by which the surface was sooner dried: whereas by forcing all the water to sink through the earth or stand on its surface, a greater degree of moisture perpetually existed at the surface, and consequently afforded a greater scope for evaporation, than the surface of the earth in general would do.

Upon the whole then I think we may fairly conclude—that the *rain* and *dew* of this country are equivalent to the quantity of water carried off by evaporation and by the rivers. And as nature acts upon general laws, we ought to infer, that it must be the case in every other country, till the contrary is proved.

This conclusion being admitted, we are enabled to deduce a general theorem for the quantity of water carried down into the sea by any river in any country (on the supposition that all rivers are ramified alike) provided we have certain *data*: these *data* are the length of the river, and the excess of the rain above the evaporation in the country from which the water of the river is drawn: Also, it should be known by observation, how much water some one given river carries down.

For, from the principles of geometry, the area of country from which any river is supplied, will be as the square of the length of the river; and the quantity of water carried off, will be in the compound ratio of the area of the country, and the excess of the rain and dew above the evaporation.

Thus, let L=the length of any river, E= the excess of rain and dew above the evaporation, and Q=the quantity of water disembogued in any given time by that river; l=the length of any other river, e=the excess, &c. and q=the quantity of water; then we shall have $q=\frac{Q\,l\,e}{L\,E}$

Ex. gr. Suppose the length of the Thames =200 miles, and the excess=5 inches, estimating the rain and dew at 30 inches and evaporation at 25; and suppose the river Kent, in Westmorland, to be 20 miles in length, and the excess 35 inches, the rain and dew being supposed 65, and evaporation 30 inches.

$$\text{Then, } \frac{20^2 \times 35 \times Q}{200^2 \times 5} = \frac{7\,Q}{100} = q$$

or $Q = 14\frac{2}{7}q$; which result, I believe, will be found to accord nearly with the measurement of the two rivers on the principle before mentioned.

§ 4.

On the Origin of Springs.

The Origin of Springs has always been justly considered as a question of natural history worthy of investigation.—In the infancy of science hypotheses are formed to account for phenomena; but when facts are discovered totally inconsistent with an hypothesis, it ought to be discarded. This does not seem to have been the case in the subject before us; for various opinions are still held by some, which it is impossible to support by facts. The object of the following remarks and experiments is to ascertain the disputed point if possible.

There are *three* opinions respecting the origin of springs which it may be proper to notice.

1st. That they are supplied entirely by rain and dew.

2d. That they are principally supplied by large subterranean reservoirs of water.

3d. That they derive their water originally from the sea, on the principle of filtration.

It is obvious, that before we pay any attention to the two latter opinions, the causes assigned in the first ought to be proved insufficient by direct experiment. M. de la Hire is the only one who has attempted to do this, as far as my information extends, in the Parisian Memoirs for 1703. He procured a leaden vessel 8 feet deep, having a pipe at the bottom; this he buried in the earth, and filled with soil of sand and loam, exposing the surface to receive all the rain that fell. After 15 years trial, he found that no water had run through the pipe at the bottom.

Again, he took another vessel, 8 inches deep, which he filled with earth and exposed in like manner. No rain penetrated so as to run out at the bottom from June to February; but after that time it yielded a quantity after most rains. Another vessel of twice the depth, or 16 inches, gave a result much like that of 8 inches. Farther, M. de la Hire found, that when herbs were planted in the soil of the last mentioned vessel, and grown up, no rain penetrated through the soil, but instead thereof it was not sufficient to sustain the vegetation; for the plants would require to be sprinkled occasionally, or else they began to droop and wither.

With respect to the first mentioned fact, we need not wonder that no water penetrated through 8 feet of earth at Paris, where the annual rain is but 20 inches, when only 8 or 9 inches penetrated through 3 feet of earth here, where the rain is 33 or 34 inches annually. But it does not follow that rain may not descend down declivities of the ground into vallies or lower parts, at Paris as well as here, and being accumulated may penetrate into the earth to a considerable depth, especially if it meet with channels or chasms of any kind, or declining strata of earth that are impenetrable by water. Paris, I believe, however, is not very liberally supplied with springs, as might be expected. As to the experiment upon vegetation, it only proves that the rain in spring and summer is sometimes not sufficient to support vegetable life, a fact which may readily be granted; but then in his experiment the plants were precluded from a supply of moisture from the earth beneath the vessel, which is a reserve of the utmost consequence in dry seasons.

This circumstance of water ascending again in the earth, on whatever principle it is effected, cannot be denied.—There were $4\frac{3}{4}$ inches of rain here in July last, none of which passed through the earth in the evaporating vessel; this earth, however, at the end of the month, was far from that degree of dryness which is unfit for the support of vegetation.—During the first four days of August there fell about 3 inches of rain, and only $\frac{1}{2}$ an inch penetrated through the earth in the evaporating vessel. Consequently 3 feet in depth of earth that was moderately moist imbibed nearly 3 inches of rain before it was saturated; whence we may conclude that 3 inches nearly had ascended and been evaporated. This evidently shews, that earth is capable of holding a very great proportion of water, that in summer the water ascends to supply the exigences at the surface, and that earth far under the point of saturation with moisture is still fit to support vegetation.

This observation suggested the following question—How much water is there in a given depth of earth when the soil is at the point of saturation, or in that state when it begins to yield water from the lower pipe of the evaporating gage?

To determine this I took a quantity of garden soil that had been soaked with rain a day before, and pressed it into a crucible; in this state I found its specific gravity to that of water as 5 to 3. It was then exposed to a moderate heat till it appeared, as near as I could judge, of the same moisture as garden soil two inches deep in dry summer weather; afterwards it was exposed almost to a red heat till it became a perfectly dry powder; in the former case it lost $\frac{1}{12}$ of its weight, and in the latter $\frac{1}{3}$.—When it had lost $\frac{1}{6}$, it did not appear too dry to support vegetation. When it had lost $\frac{2}{9}$, it appeared like the top soil in summer.—Hence it follows, that every foot of earth in depth, so saturated, contains 7 inches of water, and that it may part with one quarter of its water, or even one half, and not be too dry for supporting vegetation.

Clay, just dug out for the purpose of making bricks, was tried in the same manner: It gave the same specific gravity as the earth, and yielded not much less water.

These experiments and observations prove, that M. de la Hire's conclusions, drawn from the vegetation of plants in a given quantity of soil, precluded from any communication with the earth at large, are erroneous, or at least unwarranted: As it does not thence appear that the evaporation for the *whole* year exceeds the rain in the year, whatever it may do for a month or two in summer.

The origin of springs may still therefore be attributed to rain, till some more decisive experiments appear to the contrary; and it becomes unnecessary to controvert the other two opinions respecting this subject.

Upon the whole it should seem, that at the commencement of spring, the ground is nearly saturated with water for 5 or 6 feet in depth, as the rains and dews in autumn and winter far exceed the evaporation: There are then 5 or 6 inches of water at least to be raised up again to the surface in case of exigence in the spring and summer: If this happen to be so, then it is at the expence of springs; for we find the generality of springs become languid, or entirely cease to flow at the end of a long drought. As to the few springs that seem to be little affected by dry or wet seasons, they form exceptions which it would not be difficult to account for.

EXPERIMENTS *and* OBSERVATIONS *on the Power of Fluids to conduct* HEAT; *with Reference to Count Rumford's Seventh Essay on the same Subject.*

BY JOHN DALTON.

READ APRIL 12TH, 1799.

THE nature and properties of fire or heat are subjects which present themselves to our consideration in almost every department of physics: It is no wonder therefore that new experiments, which point out and define the modes of operation of fire, before unobserved, or at least too much overlooked, should attract the attention of philosophers.—These observations were suggested upon reading Count Rumford's very ingenious experiments, in his essay abovementioned, which exhibit a fact in a more striking point of view than it has appeared before—namely, *that the quickness of the circulation and diffusion of heat in fluids, is occasioned principally by the internal motion arising from a change of specific gravity effected by the heat.*—But the conclusion he has drawn from them—*that fluids are perfect non-*

conductors of heat, in the way in which solids conduct it, appears to me totally unwarranted from the experiments, and erroneous in itself. And as it may be an error of practical consequence, if adopted, the exposition of it seemed desirable—which is the object of the following remarks and experiments.

My first attempt was to ascertain the precise degree of cold at which water ceases to be further condensed—and likewise how much it expands in cooling below that degree to the temperature of freezing, or 32°. For this purpose I took a thermometer tube, such as would have given a scale of 10 inches with mercury from 32° to 212°, and filled it with pure water. I then graduated it by an accurate mercurial thermometer, putting them together into a bason filled with water of various degrees of heat, and stirring it occasionally: As it is well known, that water does not expand in proportion to its heat, it does not therefore afford a thermometric scale of equal parts, like quicksilver.

From repeated trials agreeing in the result, I find, that the water thermometer is at the lowest point of the scale it is capable of, that is, water is of the greatest density at $42°\frac{1}{2}$ of the mercurial thermometer. From 41° to 44° inclusively the variation is so small as to be just perceptible on the scale; but above or below those degrees, the expansion has an increasing ratio, and at 32° it amounts to $\frac{1}{8}$th of an inch, or about $\frac{1}{160}$th part of the whole expansion from 42°$\frac{1}{2}$ to 212° or boiling heat.—During the investigation of this subject, my attention was arrested by the circumstance, that the expansion of water was the same for any number of degrees from the point of greatest condensation, no matter whether above or below it: thus, I found that 32, which are 10°$\frac{1}{2}$ below the point of greatest density, agreed exactly with 53°, which are 10°$\frac{1}{2}$ above the said point; and so did all the intermediate degrees on both sides. Consequently when the water thermometer stood at 53°, it was impossible to say, without a knowledge of other circumstances, whether its temperature was really 53°, or 32°. Recollecting some experiments of Dr. Blagden in the Philosophical Transactions, from which it appears that water was cooled down to 21° or 22° without freezing, I was curious to see how far this law of expansion would continue below the freezing point, previously to the congelation of the water, and therefore ventured to put the water thermometer into a mixture of snow and salt, about 25° below the freezing point, expecting the bulb to be burst when the sudden congelation took place. After taking it out of a mixture of snow and water, where it stood at 32° (that is 53° per scale) I immersed it into the cold mixture, when it rose, at first slowly, but increasing in velocity, it passed 60°, 70°, and was going up towards 80°, when I took it out to see if there was any ice in the bulb, but it remained perfectly transparent: I immersed it again and raised it to 75° per scale, when in an instant it darted up to 128°, and that moment taking it out, the bulb appeared white and opake, the water within being frozen: Fortunately it was not burst; and the liquid which was raised thus to the top of the scale was not thrown out, though the tube was unsealed. Upon applying the hand, the ice was melted and the liquid resumed its station. This experiment was repeated and varied, at the expence of several thermometer bulbs, and it appeared that water may be cooled down in such circumstances, not only to 21°, but to 5° or 6°, without freezing, and that the law of expansion abovementioned obtains in every part of the scale from 42°½ to 10° or below; so that the density of water at 10° is equal to the density at 75°. But as the discovery of this curious, and I believe hitherto unnoticed property, has little to do with the object before us, I shall say no more of it at present.

Count Rumford's principal experiments are those in which a cake of ice was confined on the bottom of a cylindrical glass jar, of 4.7 inches in diameter, and 14 high, and water poured upon it of different temperatures suffering it to stand, without agitation. He found that about 6lb. of boiling hot water melted little more ice than as much water of 41°; and that by making such allowances as the experiments seemed to warrant for deductions when hot water was used, water of 41°, or 9° above the freezing point, melted quite as much, and often more, than the hot water: From which he infers, that water, and by analogy all other fluids, do not transmit heat in the manner that solids do, but circulate it solely by the internal motion of their particles.

The existence of this internal motion he has proved decidedly; that water of a certain temperature being of the greatest density, will always take the *lowest* place, and water either warmer or colder than that degree will ascend. This degree of greatest condensation he takes on the authority of others at 40°; it appears however from the experiment related above, to be still more favourable to his position, namely 42°½: And that water of 32° must ascend till it comes to water of 53°, if it be not cooled in its progress, which circumstance he admits.

Upon considering the facts related in his experiments therefore, there are *three* causes which suggest themselves as conspiring to circulate and diffuse the heat, by which the ice is melted.

1st. The internal motion of the liquid, by which water of 32°, incumbent upon the ice, is perpetually ascending into a warmer region of 53°, and warmer water of 42°½ descending to take its place.

2d. The proper conducting power of the liquid independent of internal motion.

3d. The conducting power of the glass jar. But as glass is known to be a very bad conductor of heat, it can produce no material effect in these experiments: For which reason Count Rumford does not appreciate the third cause.

With respect to the operation of the first cause, it will *generally* be supposed that *cold* water rising into *warmer* and remaining with it, the heat is impaired, and the two reduced to a common temperature. But Count Rumford does not admit of this communication; he maintains, that the two still retain there proper share of heat, notwithstanding they are mixed together. This hypothesis of his is of no peculiar consequence as far as respects the effect of the internal motion: For the temperature indicated by a thermometer immersed in an equal mixture of water at 32° and 53°, would be the same as if the water was uniformly of the temperature 42°½. But it has material consequences in other respects; for, if it be admitted, it annihilates the *second* cause abovementioned, and it would follow that warm water being put upon cold water above the temperature of 42°½, the heat could not in any degree be propagated downwards, unless by agitation, and even then, upon subsiding, the warm part ought to rise to the top, and the cold fall to the bottom.

These positions are so manifestly contradictory to common opinion, that they can not be received without proof. But Count Rumford has not given us a single experiment to prove them. It seemed necessary therefore, to clear up this point by direct experiments.

Experiment 1.

Took a large tumbler glass, 3½ inches diameter, and 5 inches deep, and filled it half way with water of 51°, then gently filled up the rest by means of a small syphon, with water of 88°; a thermometer, with its bulb and stem detached from the frame, being previously immersed to the bottom. The temperatures at the top and middle were had by gently immersing the bulb of another thermometer into the water.

Time elapsed.	TEMPERATURE *at top.*	*in the middle.*	*at bottom.*
—	88°	—	51°
5 min.	85	—	54
12	83	75°	56
18	80	72+	58
30	76	—	60
40	73	—	61
50	70	—	61
60	69	—	61

(Air in the room 50°.)

Experiment 2.

The same as before; only a circular piece of wood floated upon the surface of the water, on the centre of which the stream of the syphon was directed to prevent the current downwards.

(Air in the room 55°.)

Time.	TEMPERATURE *at top.*	*at bottom.*
Before the water was poured on		56°
—	116°	56+
10 min.	105	$56\frac{1}{2}$
20	92	57 —
30	85	$57\frac{1}{3}$
40	80	$57\frac{3}{4}$
50	77	58+
1 h. —	75	$58\frac{1}{2}$

	TEMPERATURE		
Time.	*at top.*	*at bottom.*	
1 h. 10 min.	72°½	58°½	
— 20	70	59 —	
— 30	67	58½	
— 40	65	58+	
— 50	63	58	
2 —	62	58 —	
— 15	61	57½	
— 30	59½	57	
3 —	57½	56	(Air 52°)
5 —	53½	53	Do.

A similar result was obtained in a different way by the following.

Experiment 3.

Took an ale glass of a conical figure, 2½ inches in diameter and 3 inches deep; filled it with water that had been standing in the room, and consequently of the temperature of the air nearly—Put the bulb of a thermometer to the bottom of the glass, the scale being out of the water: Then, having marked the temperature, I put the red hot tip of a poker, half an inch deep into the water, holding it there steadily about half a minute; and as soon as it was withdrawn, I dipt the bulb of a sensible thermometer into the water about $\frac{1}{4}$ inch, when it rose in a few seconds to 180°.

	TEMPERATURE		
Time.	*at top.*	*middle.*	*bottom.*
Before the poker was immersed			47°
—	180°		47°
5 min.	100	60°	47½
20	70	60	49
1 h. —	55	—	52

These experiments all evidently agree in proving water to have a proper conducting power, independent of any internal motion.—It surely will not be said that any slight motion unavoidably made at the beginning of an experiment, could continue with a powerful effect for upwards of an hour.—However, to determine this matter, I made the two following experiments.

Experiment 4.

Took the glass tumbler of the first experiment, and filled it half way with rain water, deeply tinged with archil; then filled it up with clear warm water, as related in the 2d experiment.—The upper half was but just perceptibly tinged by the process and uniformly so; it remained for an hour not visibly altered in this respect, though by frequently putting the bulb of a thermometer down to the middle, the colour at last rose in a small degree.

(Air 45°)

	TEMPERATURE		
	at top.	*middle.*	*bottom.*
Before the warm water was poured on . .			44°
Time	105°	—	—
7 min.	97	—	47+
17	86	—	48
27	79	—	49+
37	75	68	50
47	70	66	50+
57	66	62	51½
1 h. 7	60	62	51½
— 17	60+	59	51½
— 27	59	—	51½

Experiment 5.

A glass tube near an inch in diameter, and 16 inches long, was half filled with a coloured solution of common salt in water, warm; a small thermometer was wholly immersed in it, and cold clear water carefully poured upon the whole so as to fill the tube; the colour ascended very little, and continued invariable after the process of filling.—The warm solution was of course made of greater specific gravity than the cold water.

(Air 45°)

	TEMPERATURE	
	at top.	*bottom.*
Time.	45°	85°
5 min.	53	79
10	53	74
21	52	69
31	51	66
45	50½	64
58	50	61
1 h. 31	49	56½
3 30	47	51
— 55	47	50
4 15	—	49
7 5	46	48

To determine whether hot and cold water being suddenly mixed, and agitated, the hot would afterwards rise to the top, was the object of

Experiment 6.

Air in the room 50°.—About ½ pint of water of 130° was poured into a cold tumbler glass, and immediately after as much water of 50°; the mixture was agitated for half a minute by a deal rod; after which an immersed thermometer stood at 85°, both at top and bottom; it was then set by in a still place for examination.

Time.	TEMPERATURE *at top.*	*bottom.*
15 min.	77°$\frac{1}{4}$	77°
30	73	72$\frac{2}{3}$
45	68	67$\frac{2}{3}$
1 h. —	64.8	64.6

From all these experiments it is evident, that water has a proper conducting power: In the last experiment, if the particles of water during the agitation had not actually communicated their heat, the hot ones ought to have risen to the top, and the cold ones subsided so as to have made a material difference in the temperature.—It is, however, equally evident, that water is a *bad conductor* of heat, probably as it is of electricity; the descent of the heat in the second experiment is wonderfully slow; a slight agitation for one *second* would do as much to induce the equilibrium as standing still one *hour*. In repeating the third experiment, in a wine glass, I have several times known water $\frac{1}{2}$ an inch deeper to differ 50° in temperature from the incumbent water.

We must conclude, therefore, that the quick circulation of heat in water over a fire, &c. is owing *principally* to the internal motion excited by an alteration of specific gravity; but not *solely* to that cause as Count Rumford has inferred.

If it be proved that water conducts heat, it will scarcely be necessary to prove, that other fluids conduct it, and that they communicate it one to another:—The two following experiments shew that mercury conducts it, and that water and mercury reciprocally communicate it.

Experiment 7.

Took a cylindrical glass tube, of 1 inch internal diameter, and put 1¾ inches in depth of mercury into it, and immersed the bulb and stem of a thermometer to the bottom, the scale as usual being above the liquid; then put 2¼ inches of warm water upon it by a syphon, and let it stand without agitation.

	TEMPERATURE.	
	Merc.	*Water.*
Time.	56°	122
3 m.	70	118
6	73	110
11	75	100

	TEMPERATURE.	
Time.	*Merc.*	*Water.*
14 m.	74°½	92°
19	73	87
27	71	78

Experiment 8.

Into a tumbler glass, 2½ inches in diameter, poured an inch in depth of mercury, and heated it to 110°; upon which was poured an inch of water at 50°, and then kept still.

	TEMPERATURE.	
	Merc.	*Water.*
Time.	110°	50°
4 min.	74	70
8	71	$70\frac{1}{2}$
10	$70\frac{1}{2}$	70

Finding that water was so bad a conductor of heat, I was desirous to learn how ice would conduct it, and tried it as follows.

Experiment 9.

Feb. 9th. Out of a mass of ice, by means of a hot iron, I shaped a cylindrical piece, 3 inches in diameter, and $5\frac{1}{2}$ inches long, clean and pure; its weight 17 ounces. Made a small round hole at one end, one inch deep, and the size of a thermometer bulb, which was inclosed in it.—The other end of the piece was put into a bason of snow and salt, to the depth of from $\frac{1}{2}$ to $1\frac{1}{2}$ inches, the temperature of which was kept below 10° for $1\frac{3}{4}$ hours. Air 37°.

Time elapsed.	Therm. in the liquid.	Therm in the ice.
	5°	32°
$1\frac{3}{4}$ h. at a medium	7°	$31\frac{1}{2}$

N. B. This descent of half a degree was graudal, but did not commence till long after the beginning of the experiment.—After this the piece of ice was inclined to one side, by which nearly one half of it was immersed in the cooling liquid, and the inclosed bulb of the thermometer was now not more than an inch from the cold mixture.

H.	M.	Therm. in the liquid.	Term. in the ice.
1	50†	14°	28°
2	20	19	28
2	50	22°	Ice along with the therm. slipped down into the cold liquid.

The ice now weighed 12½ ounces: the rest had been liquified by the operation of the saline liquor.

This experiment, I think, decidedly proves that ice is a worse conductor of heat than water:—Indeed this is not wonderful; for it is said, that ice at a low temperature becomes an electric.

It is certainly a remarkable circumstance, but not at all inconsistent with the known laws of heat, that in a mixture of hot and cold

† From the beginning of the experiment.

liquids, the uniform temperature should be so soon induced by agitation and so slowly by rest: But when we consider, that in the former case, hot and cold particles are brought together, and that in the latter there is a series of particles one upon another, gradually rising in temperature, but differing by insensible degrees, we shall not wonder at the facts. When any one particle of water, or any other body, has one above it, warmer by an insensible degree, and another below it, colder by an insensible degree, its power to transmit heat must be very small.— These considerations gave rise to the two following experiments.

Experiment 10.

A mercurial thermometer was taken, its bulb $\frac{4}{10}$ inch in diameter, and hanging clear of the scale: It was heated by the flame of a candle to 600°, and then laid upon a table with the bulb projecting over the edge, and was thus left to cool by the mere operation of the air in the room, which was 52°.—The following is the medium result of two experiments, which, however, agreed with each other almost in every observation.

Time.	*Temp.*	*Time.*	*Temp.*
0	600°	18 half m.	66°
1 half m.	450	19	64
2	350	20	62
3	280	21	60
4	229	22	59
5	195	23	58
6	168	24	57
7	145	25	56
8	128	26	55
9	115	27	54
10	104	28	53+
11	95	29	53
12	88	30	53
13	81	31	53
14	77	32	52+
15	73	33	52
16	69+	Air in the room	52
17	68		

Experiment 11.

Another thermometer, having a similar bulb, but a scale with much larger degrees, was heated and cooled in the same manner.

Time.	*Temp.*	*Time.*	*Temp.*
—	85°	8 half m.	61°½
1 half m.	79½	9	60
2	75	10	59
3	71+	11	58¾
4	68½	12	58
5	66⅓	13	57¼
6	64½	14	57
7	63	15	56.6

Time.	*Temp.*	*Time.*	*Temp.*
16	56°·3	24	55°·3
17	56¼	25	55·25
18	56	26	55·2—
19	55·9	27	55·1+
20	55·7+	28	55·1—
21	55·6+	29	55·+
22	55·5	30	55
23	55·4		

In these experiments we may consider mercury and air mixed together of unequal temperatures, with a thin partition of glass—and from the last we may conclude, that the thermometer imparted to the air 40 times more heat in half a minute, when its temperature was 30° above the air, than when it was only 1° above it.

We shall now advert a little to Count Rumford's experiments.—It will easily appear, that arguing fairly upon his own hypothesis he can never account for the phenomena observed: For, hot water being poured upon ice, an internal motion would take place near the surface of the ice, by which a stratum of water of a certain thickness would be reduced to 32°, and then all further reduction of the ice must cease; because all the superincumbent water being above 53° would be lighter and could not descend to the ice. But this is quite contrary to what took place. The facts, however, will admit of a satisfactory explanation upon established principles.

By experiments 10 and 11, it appears, that the quantity of heat given out by a body, during any small given portion of time, is nearly as the excess of the temperature of the body above the cooling medium. Hence, then, we may conclude, that the effect of hot water upon ice arising from the proper conducting power of water, will be nearly as the heat of the water. What effect the other cause may produce, it will be difficult to determine from theory: Experience will be the best guide. One thing, however, appears pretty certain, that its effect must be a *maximum*, when the temperature of the water at large is $42°\frac{1}{2}$; because then there can never want a determination of the particles *downward* to supply the place of the lighter water of 32° ascending. If the temperature of the water exceed $42°\frac{1}{2}$, then the effect of the internal motion will be less, diminishing by some unknown ratio. As far as I can judge from Count R.'s experiments, the joint effects of those two causes should be nearly the same with water of 42° and water of 190°. Taking this, therefore, for granted, we shall be enabled to sketch a table of the values of these two causes for every 10° of temperature. The numbers expressing the effect of the proper conducting power, are derived from the 10th experiment, and consequently are not purely hypothetical: those expressing the other effect, except 42° and 192°, are put down hypothetically, because the law of decrease has not been ascertained.

It is to be supposed, that a given quantity of water, of the several temperatures mentioned, is carefully poured upon a cake of ice at the bottom of a cylindrical glass jar, and stands without agitation for a given time, as half an hour; then the proportionate quantity of ice supposed to be melted by the two causes separately are stated in numbers, and then the sums are taken to express the joint effects.

Temperature of the water on the ice.	32°.	42°.	52°.	62°.	72°	82°.	92°.	102.°	112.°	122.°	132.°	142.°	152.°	162.°	172.°	182.°	192.°	202.°	212.°
Ice melted by the conducting power of the water	0.	2.	3.	6.	9.	11.	12.	14.	17.	19.	21.	23.	25.	27.	30.	33.	35.	38.	40.
Ice melted by the internal motion of the particles of water.	0.	36.	35.	31.	25.	19.	16.	14.	12.	10.	8.	7.	6.	5.	4.	3.	3.	3.	3.
Total quantity melted by both causes.	0.	38	38.	37.	34.	30.	28	28.	29	29.	29.	30.	31.	32.	34.	36.	38.	41.	43.

After what has been said, I need not caution my readers not to consider this table as accurate. The principle of it, however, cannot I conceive be disproved: that the operation of the conducting power must be proportionate to a series of numbers beginning from 0 at 32°, and gradually increasing in some ratio with the temperature above 32°, cannot, I think, be controverted; and that the operation of the internal motion must begin from 0 at 32°, and increase till it arrives at its *maximum* at 42°½, and then decrease again ever after, is also, I apprehend, unquestionable; thus, when the jar had water of 42°, in Count R.'s experiments, this internal motion must have had a range of 8 inches in depth; whereas, when hot water alone was used, it had not more than ¾ of an inch to range from the temperature of 32 to that of 53°.

The following table exhibits a concise view of all the material varieties of Count Rumford's experiments, with their result.

	Temperature of the water when poured on the ice.		Medium Temperature.	Water in the jar surrounded by		Ice melted in 30 minutes.
	At the beginning	end				
Experiment 32	41°	40°	40°$\frac{1}{2}$	Air	41°	617 Grains
35	189	180	184$\frac{1}{2}$	*a warm covering of cotton wool.*		747
38	41	43	42	Air	61	676
39	180	157	171$\frac{1}{2}$	Air	61	559
45	188	64	128	Ice & water	32	406
51	61	49	55	Ice & water	32	660
53	61	60	60$\frac{1}{2}$	Air	61	642

Count Rumford attempts to explain why there was less ice melted in such experiments as the 45th than in those like the 39th, and attributes the diminution of the effect to the descending currents, occasioned by the cold mixture surrounding the warm one, which he thinks would obstruct the opposite ones ascending from the ice. But the effect in the 51st, compared with the 53d, being just opposite, he passes over without explanation.—I have no doubt myself, but that the true cause of the differences in both cases, is to be found in the column expressing the *mean temperature* of the water, and not in that expressing its situation, which I consider as having nothing to do in the business, but as it affects the general temperature. The *maximum* effect with cold water will be when it is of the temperature of about 48° or 50°, and the *minimum* above it probably about 100° or 120°; and in proportion as the mean temperatures, in any experiment, deviate from those points, the effects vary accordingly, let other circumstances be what they may.

Thus I have attempted to explain the rationale of these very curious and interesting experiments, in a manner different to what their ingenious author has done. And must now leave it to the reader to form his opinion.

EXPERIMENTS *on the* VELOCITY *of* AIR *issuing out of a Vessel in different circumstances; with the Description of an instrument to measure the force of the Blast in Bellows, &c. By* Mr. BANKS, *Lecturer in Natural Philosophy. Communicated by* Mr. DALTON.

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THE object of this enquiry may be announced in the following proposition. If an elastic fluid is generated in a given vessel, or any way contained in it, and at liberty to issue out of the said vessel through a given aperture, to determine the resistance which the vessel meets with from its action, or the power which it has of communicating motion to the vessel, as in a sky rocket, Saddler's steam-engine, &c.

Before we proceed to relate the experiments it may be proper to premise certain principles deduced from Theory. If a tube be filled with any kind of fluid, as *air*, *water*, *mercury*, &c., and placed in a vacuum, every fluid will flow out with the same velocity. For though the pressure of a column of mercury of a given altitude, be much greater than an equal column of water, yet the weight of the particles to be projected is greater in the same ratio. On the other hand, if air is lighter than water, the particles projected are also lighter in proportion. If a tube 16 feet high be filled with air of any density, that air, like water, would flow into a vacuum with a velocity of 32 feet per second, no corrections being made for resistance.*

And if we take the gravity of air to water as 1 to 840, then a column of one foot of water, compressing air, will produce as great a velocity in that air as a column of air 840 feet high, supposing it was of uniform density.

If we take the whole pressure of the atmosphere equal to 33 feet of water, or its height (supposing it to be equally dense, which in this case will make no difference) equal to 33 multiplied by 840, or 27720 feet. Then as the square root of 16, is to 32, the velocity at that depth; so is the square root of 27720, to 1332 feet per second, the initial velocity of the atmosphere into a vacuum.

* In the supposition of a perpendicular tube open at the top, filled with air or any elastic fluid, the author takes the density of the column at the bottom or where the aperture is made, to arise solely from the weight of the elastic column; and the altitude to be that which would be if the whole column were reduced to the density of that at the bottom.

To prove whether air compressed by 33 feet of water would be impelled into the atmosphere with the above velocity, I have made, amongst many more, the following experiments.

A is a vessel of a known capacity, into the top of which is screwed an aperture of a known area. The tube *T d* recurve at *d* is soldered or screwed into the top of the said vessel. The hole *a* is stopped, and water poured into the tube at *T* till it is full, at which time a quantity of water will have passed out of the tube at *d*, and condensed the air in the vessel, more or less as the tube *T d* is longer or shorter.

At this time a person who has closed the aperture at *a* with a finger of one hand, and held a half second pendulum in the other, removes both at the same time, while at the same moment an assistant opens a cock over the tube *T*, which supplies it with water as fast as it can descend into *A*. The moment that the water appears at *a*, the time piece is stopped, and the time of expelling the air is noted, from which, by knowing the capacity of the vessel, the velocity may be obtained.

If the tube *T d* should be continued near the bottom of the vessel *A*, while it was filling with water, the length of the compressing column would be gradually diminishing, and of consequence the pressure would be constantly changing, hence the open end of the tube is as near the top of the vessel as is consistent with a free passage for the water.

Experiments.

The vessel *A* contained 15lb. 6 oz. of water, from which we find its capacity is 425.088 cubic inches.

The area of the aperture *a*, through which the air is expelled is ,0046 inches.

Exper. 1*st*. The altitude of *T* above the vessel is 30 inches. Time of expelling the air by several trials is 33 seconds.

Exper. 2*d*. The altitude of *T* is 6 feet. The time of filling by several trials is 21.3 seconds.

In the first experiment, 425.088, the solidity of the vessel, divided by ,0046, the area of the hole *a*, gives 92410.4 inches for the length of the stream of air driven out in 33 seconds; divide that length by 33, and we shall have 233.3 feet, the velocity per second, communicated by 30 inches of water.

The second experiment by the same process gives 361.6 feet per second. If we would compare these together, we may say, as the square root of 30, the head, is to 233.3 the velocity; so is the square root of 72, the second head, to 361.8 feet the velocity per second.

Again:—As the square root of 6 feet, is to 361,6; so is the square root of 33 feet, to 845.2 feet per second, the velocity produced by that head; or the initial velocity with which the atmosphere would enter a vacuum. This velocity found by experiment is 487 feet per second less than has been assigned by theory.

It appears however that the results as determined by theory and experiment do not differ more than in the case of effluent water. For, if we would reduce the velocity of effluent water, found by theory to that which experience gives, we must multiply it by ,634. Accordingly, if we multiply 1332 feet, the velocity of the atmosphere entering a vacuum, as calculated above by ,634, the product is 844.5 per second, differing but $\frac{7}{10}$ of a foot from that just found by experiment.

I have also made experiments by sinking vessels in water, till their tops were even with its surface, and opening the aperture that the rising water might expel the air, by which I obtained the same velocities as above; but the method of computing is much more intricate, for which reason I shall not insert them.

From the above, it appears that a pressure equal to 33 feet of water, will expel air out of bellows into the atmosphere with a velocity of 845 feet per second, that *one foot* of water in depth will produce a velocity of $147\frac{1}{4}$ feet, and *one inch* a velocity of 42 feet per second, or 20 miles an hour.

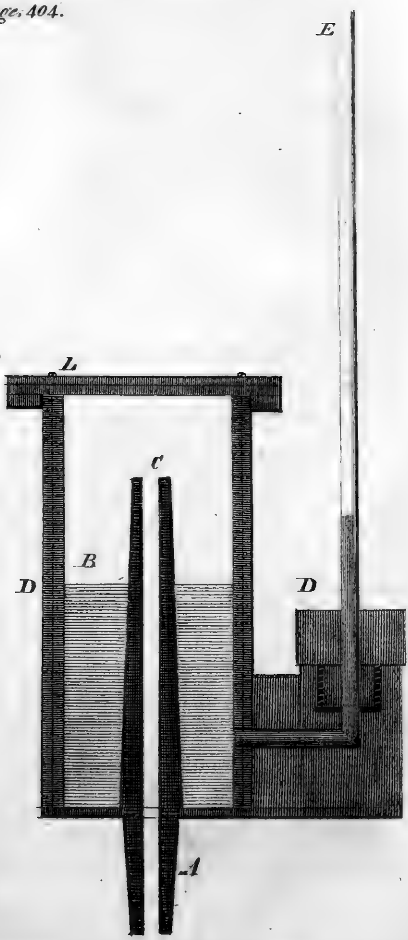
Hence we may construct a table shewing the velocity communicated to air by any head of water. For, as the square root of 6 feet, is to the velocity produced by that head; so is the square root of any other depth, to the velocity produced by that depth.

We may also, from the above, construct an instrument which will shew the velocity with which air flows out of any kind of bellows, with as much accuracy as the experiments have been made, on which its construction depends.

Description of the Instrument, &c.

The metal box or tube *B*, may be about the size of the figure; the top must be made air tight by the cover *L*; into the bottom is fixed the small tube *AC*, and into the piece *D* is cemented the glass tube *ED*; the instrument is then inverted, and some water poured through the tube *AC*, till when in its proper position it is visible at *D*. It is now ready for use, and the end *A* may be fixed in a hole made in the upper board of the bellows, and the water will rise in the glass tube, in smiths' bellows generally from 9 to 12 inches, furnace bellows generally 4 feet or more. But where the compression is great, quicksilver may be used instead of water, only in this case the instrument should be made of iron, as quicksilver causes the screws of brass to break. Or, instead of quicksilver, the tube *ED* may be sealed at the top *E*, and then a length of 12 or 18 inches will be enough for any blast. The glass tube needs not be more than one eighth or one tenth of an inch in diameter.

Whatever compression there may be in the bellows there will be the same in the upper part


E
L
C
B
D
D
A

of the tube *B*, which will force the water into the glass tube *DE*, and make the air in its upper part of the same density, deducting from the compressing force, the altitude of the water raised above *D*, which however will be of little or no importance; if the gauge is placed in a horizontal position, with the glass tube downward, there will be no difference of density.

The computation for the force in the case where a tube hermetically sealed at the top is adopted in the instrument, will be effected by considering that the space occupied by any elastic fluid is inversely as its force.—Thus, let the tube be 12 inches long, and suppose the water to be raised 1 inch; then it will be 11 : 12 :: the force of the atmosphere : the force of the air in the tube :: 1 : $1\frac{1}{11}$.—Hence a scale may be adapted to the instrument, to express the force of condensation over and above the common atmospheric pressure; which force is signified in the instance above by the fraction $\frac{1}{11}$, unity being the atmospheric pressure. If we denote the atmospheric pressure by 30 inches of mercury, or 32 feet of water, then the force $\frac{1}{11}$, in the above example will be expressed by 2,727 inches of mercury, or 2.91 feet of water; and the like for any other instance.

If a mercurial instrument of the above construction be preferred, it becomes necessary to add the height of the mercurial column to the force found as above: thus, if the condensation of air be from 12 into 9 inches, then the addition to the force of the internal air in the tube is equal $\frac{1}{3}$, or 10 inches of mercury, to which must be added the 3 inches raised in the tube, and the whole force will be 13 inches of mercury, exclusive of that of the atmosphere.

This sort of instrument or guage serves equally well for finding the expansive force of any kind of elastic fluid, as for measuring the velocities with which they issue out of the place of their confinement. It may be applied to all kinds of bellows, to condensed steam, and to the air pump.

ESSAY *on the* BEAUTIFUL *in the Human Form; and Enquiry whether the Grecian Statues present the most perfect* BEAUTY *of* FORM, *that we at present have any Acquaintance with.*

Communicated to the Society from a CORRESPONDENT, through the REV. GEORGE WALKER.

READ, Oct. 3, 1800.

IN order to judge whether the Grecian imitations, or any other imitations, of the Human Form be the most beautiful, it appears necessary that some standard, some general law or rule should be admitted, in conformity to, or in deviation from which, the sentence of beauty or deformity may be safely passed. That this standard has an existence in nature can hardly be doubted; for, if man be the work of a designing artist, he must have been formed according to some model; and this model in the contemplation of the artist must be the standard of what is the most perfect of the species, and as far as the form is concerned, of what is the most beautiful in the form. The mind of the artist may then be investigated in his work, and it might seem to be no very difficult thing to collect a tolerably accurate idea of what answers to the more perfect idea of the artist, by omitting what is incidental and peculiar to every individual of the species, and retaining what is universal. And perhaps by a standard thus collected, though insensibly, and without any deliberate purpose, every one does judge of what he deems to be beautiful in the form of his own kind, and in every form whatever. We find therefore that, in the estimation of the beautiful in the human form, there is a general agreement as to the contour or outline of the whole and of the parts, the comparative magnitude of each part, the proportion that each bears to each other and to the whole, and the order and degree in which each swells and falls. Whatever is remarkably excessive or defective, whatever strikingly offends against the general character and proportion of the parts, whatever beyond the general rule is abrupt and extravagant in the swell or fall, is almost universally rejected as not beautiful;

because not answering to that medium standard which every one has erected in his own mind, and which he has collected from the exhibition of his species. There is therefore in the imagination of every one a standard, collected from observation, but insensibly and without design, to which he refers every form, that attracts his attention, and agreeably to which he pronounces that it is beautiful or otherwise, and in what degree it pleases or offends. It is an ideal figure, which the eye of the mind can contemplate, and does contemplate and does refer to, though the rational mind cannot describe this figure; because the figure has been imperceptibly formed, corrected, improved through life, in which the senses, and not reason, have been altogether employed. The ultimate figure, as a picture of the imagination, is the abstract of all the impressions which have been received from a multitude of original forms; preserving what is characteristic of all, and rejecting whatever is incidental, excessive or defective, superinduced by violence or art, or in any respect offending against the general character of the form.

This perhaps is the secret foundation of what we call taste, or the perception of the beautiful, whether as referred to the human or to any other form whatever. I do not say, that this is the only foundation, and that there is no other rule or principle, by which our estimation of the beautiful is influenced; but I think it to be the principal foundation: and the remarkable consentaneity of taste and decision of the beautiful, especially of the human figure, proves that our rule is derived from nature, from our constant observation of the originals, as they have issued from the forming hand of the great artist; and that thus acquiring an abstract idea of the whole, we enter as it were, whether intending it or no, into the mind of the artist himself, and erect a standard of what he designed as the most perfect, and therefore the most beautiful, in the form which he has given to man.

In this investigation of the standard of taste and the decision of the beautiful, although a general consent be acknowledged, yet it is manifest that the rule or standard will be more or less perfect, as the field of observation has been more or less extensive, and as successive comparisons with the standard already attained, and the introduction to more perfect forms, with fewer deviations from the medium character, have chastened in the imagination the picture of the beautiful. Every step in the progress towards this perfect image is the selection of a few from the general mass; from continued observation rejecting some from this few; admitting others; till the field of observation is exhausted, and we rest in the image which is the result of the whole.

But as the originals which are the subject of observation will in some respects vary from the influence which climate, occupation, manners, and even the cultivation of mind, have on the human form, it will follow that a variation in the idea of the beautiful is to be expected, and that different circumstances may be so favourable to some, as to render their conception of the beautiful more approximate to the faultless truth and standard of nature.

This secret and imperceptible progress towards an ideal standard of beauty may be illustrated by the supposition of an experiment, easily to be conceived, though not easily to be carried into execution. If impressions from the faces of all the women in this kingdom at the age of twenty, were taken on any plastic substance, as suppose plaster of Paris; excluding however those who come into the world with obvious excess or defect, who have been maimed by injury, or blemished by any superinduced cause, as excess of labour or rest, intemperance, deficiency of sustenance, or any excess or defect of the passions of the mind; and an artist were to form a face that was the mean of all these; it would surely be admitted that this face would be the perfect model of our national beauty. If the same experiment were made in other nations; excluding those in whom the extremes of climate necessarily induce a depravation of the form, the model of beauty in the female face would be equally obtained in these nations as their appropriate standard. And if from these several national standards the mean of them should also be taken, this last image must be admitted to be as perfect a representative of the beauty of face, of the whole female race, as is possible to be obtained.

This judgment is founded on the supposition that the design of the Creator is evidenced in his productions, and that the mean character of his productions of any species must approach the nearest to the perfect model contemplated in the Creator's mind. Our sense of beauty, our delight therein, can find its object only in the production of the Creator, our taste conforms itself to what is done, and the mean character must be the highest standard, the utmost conception of beauty that we can form. Either our ideal standard or image of beauty is born with us, or it is thus acquired; the former would be a mere arbitrary supposition, the latter accords with experience, is confirmed by reflection on our daily experience, it is the necessary consequence of every moment's impression from the objects presented to our view; and if our minds were not insensibly led to this result by being committed to the field around us, yet, as the means to the end, it might in this very way be accomplished by our deliberate act, by experiments similar to what I have supposed, in collecting the standard of national female beauty at the age of twenty one.

It might be expected therefore, that the standard of beauty would be one and the same to all; and so it is, as far as one and the same rule and judgment on any subject can be expected in the vast range and diversity of human beings. It has already been observed that the greater is the variety and number of the objects that have been viewed, and attentively viewed by any individual, the nearer will the standard of beauty in his mind conform to the mean character of the species, which has been submitted to his view, and probably therefore to the truth of nature.

To a perfect uniformity in the judgment of beauty it is requisite that the field of observation be equal to every one, that the mind of each observer be equally directed and equally attentive to the subject, that the circumstances which are favourable or unfavourable to beauty should be equal in the objects observed. But this equality has no existence, and therefore smaller variations in the estimation of beauty are certainly found; but withal, there is so much of consentaneity in the opinion of all mankind, as proves that the standard in every mind is derived from a common source, and has much of a common character. I can have no doubt that a Grecian Beauty among the Greeks, a Circassian Beauty honoured by the admiration of the Asiatics, would be acknowledged as a beauty of high distinction by the western European, and that the European female whom the European taste has selected would be in no small degree applauded by the Greek and Asiatic. Perhaps, even a first rate African in the estimation of her fellow Africans would be allowed by the European, the Greek and Asiatic, excepting colour, to possess the essential characters of beauty.

It is probable therefore that the idea of beauty, though acquired by observation only of the examples presented to our view, goes beyond the limit of the materials from which it is derived, and is more perfect than the mean character of the objects observed. In acquiring the idea of this mean character we reject all obvious excess or defect; we obtain thereby an abstract idea of excess or defect; we admit this as still in some degree adhering even to the most perfect originals that we have seen; we conceive that still more faultless examples may be found in the vast field of human life; and our imagination creates to itself a more perfect idea than our experience has presented.

The result of the whole is this, every one has within him a standard of beauty, and this standard is acquired by every one from the same source, and is in all of a common character; but will be more perfect, that is, more conformable to the truth of nature, as the field of observation has been more varied and extensive, as the attention has been more excited and directed to the investigation of beauty, and as the objects of observation have been in those circumstances, which are most favourable to the preservation and perfection of the human form, and wherein the original stamp and design of nature may be, either not at all, or in the least degree, counteracted and injured by adventitious causes. I pretend not to judge, whether in this investigation of the standard of beauty there be any thing of novelty or ingenuity; it will be sufficient to my purpose, if it be agreeable to truth, and be pertinent to the second object of this essay; and support the claim, which I assert for the Greeks, that in their statues are found the most perfect imitations of the beautiful in the human form, that have been introduced to our knowledge. If no proper standard of beauty can be acquired in any other way; if the Greeks perfected their conception of the beautiful by a nice and delicate attention to this rule; if they had more favoured originals to collect from, and had a freer access to these rich originals; if the Greek statuarists were men of the first consideration, and besides their special art, had their minds raised and chastened by all the advantages, which learning, science and cultivated taste can confer; then it is a well-founded presumption, that their statues as imitations of the beautiful, are of the first form, and have probably not been equalled by the similar productions of any other nation, if to the statuaries of no other nation the same means and advantages have been extended.

This standard of beauty thus acquired, and the only standard common to all men, may be called the Sentimental standard; as its primary derivation is from the immediate sense or impression of the human form upon the mind of the observer, and its ultimate result is the mean of all the agreeable sensations which the beautiful of human form have made upon the mind.

But there is also a Rational standard, which more contemplative and reflecting minds derive from a consideration of the uses to which the human form subserves. Beauty, though in itself an object of regard distinct from every other consideration, must, in the productions of every wise artist, be subordinate to utility; and therefore in those works, which we refer to the Great Artist, we expect that beauty shall be reconciled with utility. Nor are we disappointed; all that is beautiful in the human form, which we contemplate with so much delight, is, as a mean, in perfect harmony with the numerous and diversified uses for which the human form was designed by its Creator.

Beauty could not be a primary object in the mind of the original artist, nor are we authorized perhaps to say that in our sense of beauty it was any object at all. When this artist designed man, *that* must to him have appeared to be the best form, which was the best fitted to the field of action in which man was intended to move, and in which it was intended that he should reap the conveniences and utilities of his being. If man had been designed to be an animal of speed, a form similar to that of the hare, of the greyhound or the antelope might have been assigned to him; or if not precisely a similar form, yet in the limbs adapted to motion, a lightness of bone, and firmness of muscle similar to what characterises these animals. And by parity of reason, the qualities of the ox or elephant, if slowness of motion united with great capacity of burthen had been designed to be his predominant character; or if a temperament of speed with burthen, then a tempered union of the forms which distinguish the hare and the ox, as in the horse, might have been the character of the human form. But as each of these uses in a very limited degree, and subservient to many other higher uses; such as quickness of movement towards every part; the *os sublimé* to contemplate heaven as well as earth, and each with the greatest range; exquisite sensibility, particularly in the hands, which are the instruments of mechanic operations; and in fine, the unnumbered and varied uses, which the inventive and creating mind of man can meditate, were intended to constitute the character of man, we find therefore that a form is assigned to him, which is adapted to all these uses, and to each in that degree in which it contributes to the concentered and harmonious utility of his whole being. But I mention not this, with any of the views of the naturalist or divine, but merely to shew that man can in some degree enter into the mind of his Creator, and, pleased with his form, pronounce also that it is good; and believe that it is beautiful, because it is eminently useful; and as, without any consideration of utility, he pronounces that to be the most beautiful of all the varieties of the human form, which is the mean of them all, so infer the same conclusion, when he estimates the mean of all utilities, regard being had to the respective worth and dignity of these utilities.

It is therefore more than probable, that whether we estimate beauty by the mean of utility, or simply by the mean of form, we shall arrive at the same, or nearly the same conclusion; we shall attain the same standard. But the investigation of these utilities in all their comprehension and reference to their best result is more difficult, and except in obvious and familiar instances, does not excite attention in common observers. This common sense of man however in the most familiar instances is an evidence of vast import, the mind expects and is reconciled to the more delicate judgments of more attentive observers; and a persuasion is induced, that if man were competent to the complete analysis of himself, he would find that the mean of his whole form is admirably adapted to the mean of all his utilities.

It is surely agreeable that the standard of beauty derived from two very different sources should be found to be one and the same. In receiving that as beautiful which is, we defer to the divine wisdom; in the appeal to the reason of man, the mind and act of Deity is justified to the mind of man.

But there is also a use in referring the judgment of beauty to these two tests, as they mutually serve to obviate some difficulties, to reply to some objections, and to correct some erroneous judgments to which we might be liable, if we had only one test to appeal to. The expectation of the beautiful, according to the sentimental standard, in all of the human kind, is repelled when we take into our view the varieties of human life, and admit that these varieties require a conformation nicely adapted to the useful in all. Whether it be that difference of climate, of situation, of employment bodily or mental, and of age, induce, as the mere effect of a cause, such smaller variations of the human form; or that man generally issues from the hand of his Maker with such variations of form as may furnish all the diversified agents fitted to bear their several parts in the vast community of men, makes no difference in the conclusion. Wherever the diversities of the useful require a deviation from the absolute standard of sentimental beauty, we submit our judgment thereto, and acknowledge a specific beauty; and with delight acknowledge, where adaptation to strength, to toil, to hardihood, to boldness, to enterprise, to speed, to versatility or any other peculiar end, is the predominating character of the form. In the distribution of men therefore into classes according to their situations and subservience in life, the rational standard admits a beauty, peculiar and appropriate to each; which if not found would be deemed to be a defect, though the degree in which this specific character is required would not be admitted either in the rational or sentimental standard of the mean perfect beauty of the whole human kind. Thus a length of arm, and lightness of leg are well adapted to the maneuvres of the sailor, but would be unsuited to the pedestrian, in whom is required a strong and muscular leg with as little super-incumbent weight of body as is consistent with general vigour. And thus in the different occupations of men, the estimated beauty of form in each class will be adapted to the part which each has to sustain in life. A general, when he looks with pride on the manly figure of his soldiers, has a different standard of beauty in his eye from that by which he would judge of the graceful and elegant in an assembly of the higher ranks of life. We observe a similar rule in the judgment we form of the different classes of animals. The beautiful in the lion, the tyger, the elephant, the bull, the horse, and the dog, is referred to a different standard in each, and even in those species, which come more under the observation and use of man, as they are subdivided into different kinds, they have each their appropriate destination and appropriate character, and the mean form in each subordinate kind is allowed to be the standard of beauty in that kind, though varying from the standard of beauty in each other kind, and varying from the standard of the beautiful in the whole genus, where an aptitude to a particular destination is not contemplated. The dray, the road, the race, the field of chace and the field of war have each their proper beautiful of form, while each partakes of those qualities, which enter into the mean beautiful of them all. And the same is observed of the subdivisions of the canine race; the shepherd's dog, the terrier, the spaniel, the fox-hound, the greyhound, the bull-dog and the mastiff, to which we may add the favourites of the ladies, are all estimated by very different standards of beautiful form, and yet are all referable to a form which is the mean of them all, and is conceived to be the most perfect idea of beauty in the genus.

This attention to the useful, and a fitness for the part which each is intended to act in the plan of their Creator, is strikingly witnessed in the idea which every one entertains of what is beautiful in the form of each of the two sexes of the human race. No one conceives the beautiful in the man to be exactly transferable to the beautiful in the woman, and if the most perfect beauty of the female were found in a male, the eye would be disgusted; that male would be the object of scorn or ridicule, not of approbation.

Perhaps the mere abstract idea of beautiful form, independant of every other consideration, would pronounce the gentler undulations of the masculine outline to be more conformed to its standard, than the richer swell and deeper falls of the female figure. Yet in despite of this abstract judgment, the most perfect masculine figure, if contemplated in a female, would not be considered as beautiful.

The same submission to the useful, or to what we presume to be useful, because it is according to the order of nature, is observed in the estimation of beauty, as it is referred to the different periods of human life. There is a different beauty in infancy, in boy-hood, in youth, in manhood, and in age. The child in the arms of the Madonna, and John attracting the notice of his infant cousin by his playful admiration, though the difference of their age be but very inconsiderable, yet would each, independant of all regard to the part which each is designed to sustain in riper years, be expected to exhibit a difference of form and aspect. In the full-blown rose of Lucretia, and the opening bud of Virginia, which equally enflamed the passions of Tarquin and the Decemvir, and led them to the perpetration of the most daring and dangerous crimes, though in each we imagine something that answers to our perfect idea of female beauty, yet in each we look for a delicate discrimination of exterior, something appropriate of form and feature, expressive of their respective age and situation.

In the domestic groupe of mother, wife, children and attendants, which issued from Rome to avert the vengeance of Coriolanus from his country; if imagination give a beautiful figure to each, it will be a beauty proper to the age of each, and proper also to the relation which each bears to the avenging hero, and to the shades of affection and interest with which each must be impressed from all the circumstances of the interview; while in Coriolanus himself, that which we should acknowledge as beautiful and proper in his figure would very materially differ from that of all the preceding.

There is therefore indubitably, as is generally allowed, a peculiar beauty of form and feature adapted to each of the passions; but here also, in order to the perfect, the mean must be observed; and this perfect form, as expressive of the passions, rejects the predominance of any one feature, which usurps over the others, which subdues the whole form and countenance to one characteristic and generally disagreeable expression. This must enter in no small degree into all our judgment of the human figure; the sense both of the beautiful and the proper must associate with our conceptions of the mind, which the configuration of the form in all its parts, and in all the acting of mind thereon, is fitted to express. In every human figure, which on any account attracts our attention, the immediate impulse is to read the mind in the face. Animals read it; it is an universal character, and designed to be read in some degree by all who have an interest in its information; for it is the hand-writing of the Almighty, and inasmuch as mind is superior to body, that form is pronounced to be more beautiful, which in every expression presents a beautiful mind, and at the same time is conformed to the interesting affections which are proper to the varied situation and circumstance of mind. Perfect beauty therefore supposes perfect beauty of mind, and deformity in both is the violation of the mean. Perfect mind admits of no predominant passion but benevolence, but even benevolence itself would be tame and insipid, if it excluded every other passion. It would not be suited to the field of human action and human sympathy, and could not invite our sympathy in return. The only influence of passion that can be delineated with an attention to perfect beauty is that which admits the expression of every passion; but of each according to its worth and dignity, and the power which each in its place and rank ought to have over the form and countenance; and that face which bears the expression of this temperament and proportion will be, so far as respects the influence of mind, the most beautiful.

But even in the expression of the passions, the male and female standard will differ, nor can the temperament required in the one, be expected in the other. That mildness and placidness of countenance, which is the lovely picture of a gentle female mind, would not give the promise of a masculine mind, nor answer to our standard of masculine beauty; and though the morality of mind will not submit her rule to any examples of human life, and will without hesitation pronounce that countenance and figure to be deformed, on which the violent and intemperate passions have stamped their character; yet in the imitations of man as he is, the very imitation will soften the deformity, and render that not only acceptable, but approved, and as a beauty in the art, which we should fly from with disgust and horror in the real object.

So far then we have proceeded in the examination of the beautiful, not merely as a prelude to our deciding on the excellence of the Grecian statues, though even as a prelude this appeared necessary; for without some rule founded in nature, all decision on the beautiful in the imitative arts of the Greeks, might be considered as mere opinion. If we have not erred then in this previous analysis, it has appeared that, in the subject of the human form, the sentimental and the rational judgment of beauty are not discordant, but that the beautiful as a matter of feeling harmonizes with the useful as the end; and as an influence from this, that the perfection of beauty as an absolute standard must be looked for in the middle form of the whole collected human race, excluding all whom accident, violence, monstrous births, or the extremes of climate, food and labour may have disfigured; and also in the middle form between the extremes of age, in which the subject has not attained to or declined from the point of perfection, because all the varieties of the human form seem to respect this medium both of number and age. It has also appeared, that in the distribution of occupation and character, and in the expression of the passions, that interesting language of the human form and feature, there are separate standards, which have each a specific beauty as their appropriate character, though subordinate to the more universal standard, and acknowledging it as their genus. Not greatly deviating from this higher standard, each of these distributions must be distinguished by their characteristic form, or they would not be beautiful in their place.

Having therefore to the best of my ability laid this foundation, I shall proceed

II. To enquire whether those statues which have been preserved to us as monuments of Grecian art and taste do correspond, and in what degree, to those high conceptions of beauty in the human form, which both from sentiment and reason we are led to entertain.

Appeal then to the sentimental standard, that is, to the most abstract idea of simple beauty, without any consideration of utility, or destination to particular purposes and offices in the distribution of human action. Submit to the eye the best of the Grecian Statues of this character, submit them to the severest examination, place them before the ideal standard in the mind, and say whether imagination can figure to itself more exquisitely finished models of perfect human form. Can the most fastidious critic say that this part is too large, that too small, that the wave of the contour might be more happily exhibited. That timidity wherewith nature fears to exceed, when she delights to present to us the perfect of beauty, is delicately charactered; every thing is in that happy medium, that mild temperament and proportion which perfect beauty requires; no muscle harshly obtrudes itself to break the charming roundness of the form; the minutest parts, the joints, the fingers, the toes, the very dimple, as impressed by the playful finger of love, is touched in the truth of nature; in fine, the eye of the delighted spectator may look for hours and days, and not dare to say, not be inclined to say, in what respect they could be altered, so as to be rendered more grateful to him. The eye is not satiated with viewing them, they strike not at first with their full impression, but every return to them makes a deeper impression, unfolds new beauties, discovers the grace of nature in her most finished works, and this perhaps is the most decisive proof of their superior excellence. If any thing be wanting, it is colour and the expression of the eye, but of these stone and marble are incapable; the artist has done his part, he has effected all that the material was susceptible of. Perhaps it is owing to this, the impossibility of communicating to stone the soul which speaks in the countenance, that the face of the

Venus de Medici answers not to our expectation, that it appears to be not so beautiful as the whole and every other part of the form. But the artist has given to the whole attitude the expression of apprehensive modesty, and of the will to move, you expect the statue to step from the pedestal. In the Apollo Belvidere is presented every thing that elegance and grace with a chastened severity and dignity can require in a perfect masculine form. The beauty of Antinous, the Catamite of Hadrian, is feminine, but the form of Apollo is such as mind giving its character to form and countenance would prepare you to expect. He is not Jupiter, nor Mars, nor Bacchus; there is blended in his figure the excellence of each, but with no extreme; he is such as you would look for in the ruler of the day, the president of the muses, in the handsomest and the wisest of the gods.

This delight, which the Grecian antiques excite, and as a matter of feeling, without any philosophical enquiry, has been witnessed by all, in various ages and nations. The Greeks copied from nature, her most perfect forms were their standard; succeeding artists have received their productions as a standard, and looked for reputation as they approximated to the perfection of their works. The antient Romans in the highest pride of their empire deemed nothing excellent in the art of statuary but what was of Greek fabrication. Their admiration knew no bounds; and though of the statues preserved to us, many are of an age posterior to the earlier Cæsars, it is probable that they were all the workmanship of Grecian genius. The school, denominated the Roman, though the Roman empire had been long extinct, was formed on the basis of Greek art; it aspired to no higher honor than to be the imitator of the antient Greek; yet inimical as imitation is supposed to be to great exertion, this school is allowed to have surpassed all other moderns in the beauty and character of its figures. While other subsequent schools of statuary, which have wrought from their own ideas, and been laborious enough in the execution, have each exhibited less beauty, less correctness, less conformity to what every one feels to be *το καλον* in the original. This concurrent testimony may almost be allowed to be decisive. Among the artists of Italy, and succeeding to them, in other countries, there are who appear to have devoted their whole lives to the study of whatever is most beautiful in nature or in art; and if their unqualified deference to the Greek be not admitted, I wonder from whom, with a more refined and better furnished mind, we may expect a juster sentence!

The artists of later days ascend no higher in the pursuit of the beautiful than the models which Greece has transmitted to us. Have any of them retraced the steps of the Greek artist in the formation of one celebrated statue? Have they selected from individuals of approved form, whatever is most beautiful in each, and thus composed a more perfect whole. Until this be done, it is mere presumption to suppose that they will surpass the Greek, that with all the aid of the antients they will produce a more perfect model of beauty, that this second extract of the beautiful would more answer to our most perfect idea from nature.

There is reason to believe that the Grecian artists did select the most beautiful originals, and from what appeared to be most perfect in each, formed their mean figure of the beautiful. It was, inasmuch as art could execute, the summary of what nature had dispersed in a variety of the best chosen subjects.

It is farther to be observed, and, if true, very important to the purpose, that no climate, no state of society, no modes of life and manners, were ever more happily adapted to the preservation of beauty, to the production of the most beautiful and most perfect designs of nature. A happy temperament of elements, inviting to enjoyment, and also to exercise, activity and sportiveness, simplicity of diet and simplicity of manners, were eminently favourable to the primary production, and to the preservation of beautiful form. The concurrence of these advantages is so powerful as to resist the barbarism and oppression of the Turkish government, and even to this day preserve to the native Greek race their pre-eminence of beauty.

The Grecian games invited the most perfect of the species, in varied character and exhibition, with all the advantage of exposure, and with all the action proper either to strength or grace; which must have furnished to the Grecian artist models of perfection in nature, and of the truth of nature in her best specimens, such as modern artists can have no access to. Our modern manners admit not of such exhibitions, and if in a moral view we have gained thereby, the loss to the imitative arts of sculpture and painting is incalculable. Nor, if it were otherwise, is it probable that in our climates and with our modes of life, any such specimens of the beautiful working of nature can be furnished. It will be allowed at least, that the hirelings, which are exhibited in our modern academies, are no substitute for the ampler and richer display of form and character, from which the Grecian sculptor and painter copied their admired productions.

The freedom of the Grecian mind must also have had a considerable influence on the feature and form. The influence of the mind on the feature, where the soul speaks in all its energies and character and dominant affections, is acknowledged by all; but it is not equally considered to what degree the mind, cherished from infancy in all that generous freedom, which is the gift of its author, may determine the form, and rear it up in the grace and elegance and beauty which answers to the best intention of nature. The easy, flowing dress of the Greeks corresponded with this freedom of mind; whatever nature designed, she freely operated; no restraint forced her into awkward, ill-proportioned and ungraceful deviations; and what freedom of mind, ease of dress, salubrity of climate, and simplicity of diet and manners, left unfinished, their gymnastic exercises compleated. In modern Europe every thing almost is adverse to the production and preservation of beautiful form; mind is not so pure and unadulterated; modes of life are not so equal, nor so conformed to simple elegance; the form of the great mass of the community is as much injured by excess of labour, depression of mind, and exposure to unequal climate, with scantiness of food, or irregular supplies of it, and not of a simple and salubrious kind, as the form of the higher ranks is impaired by excess of food, equally insalubrious; exclusion from air and exercise; manners that awake no mental energies, invite to no pleasant, healthful, sportiveness; intemperance in the hours of rest; and, what alone is sufficient to every depredation of form and beauty, confinement in the poisoned vapour of crowded and heated rooms. The inference is obvious. The modern European cannot rival the artist of antient Greece. He has not the same originals. Nature presented herself unviolated to the Greek; injured and perverted, she can exhibit to the European only her weaker productions.

To these considerations may be added, what I have already alluded to, that the Greek artists were men of the first form, well educated, and of high consideration. Superior instruction, and admission to the highest honours, elevate the mind, excite grander conceptions, and exalt the taste; especially in an age and country, where the passion for fame was the stimulant to all great exertions, and furnished to every one the most generous gratification. The philosophic Socrates, that wondrous man among the Greeks, was himself a statuary, and is said to have sculptured three very beautiful figures of the Graces. Without intending any thing unhandsome to later artists, the same elevation of mind cannot be equally affirmed of them. The motive of gain, always sordid and depressing, and equally accessible to the lowest minds, has too much usurped over the more generous one of fame.

I shall conclude this essay, which perhaps is already too long, with another very powerful argument in favour of Grecian art; which may be inferred from the great length of time that it flourished, and the innumerable productions which it furnished. This argued a degree of fame attached to it, and an encouragement to rivalship, of which we have no example. We may judge of this from the immense number of works which escaped the repeated plunder of the Romans; and from the valuable remnant, which to our day has survived the destruction of ages, of successive revolutions, and the rudest barbarism. Memmius, Æmilius Verres, and Proconsuls, and Prætors and Generals, and Romans of rank and taste, beyond all calculable amount, might have been thought to have exhausted Greece of her rich treasures of art; but succeeding to them Tiberius Nero carried off a valuable plunder from the Acropolis, Delphi and Olympia, and yet in these very places not fewer than three thousand statues were remaining in the time of Pliny. Can any thing in modern times compare to this? Can modern artists recur to so grand a feast of the senses, so glorious a school for instruction in their art? Does the patronage of later times present any thing like such a provocative to genius and rivalship, as we may presume to have been the character of Greek and Roman antiquity?

A Defence of LEARNING *and the* ARTS, *against some Charges of* ROUSSEAU. *In two Essays.* *By the Rev.* G. WALKER, *F. R. S.*

ESSAY I.

(READ NOV. 15, 1799.)

That learning is not the parent of politeness, nor chargeable with the duplicity fraud and vice, which he supposes to be her attendants.

IT is a failing, and not of common minds alone, who surrender themselves to the impression of the moment, but also of men, from whom a more just appreciation of the past and the present might be expected, to indulge to a spirit of discontent whenever they speak of their own times; and with a kind of holy veneration to fix their eye on those days of old, wherein, as they suppose, ingenuous virtue and sincere enjoyment were alone to be found. This failing, for a failing assuredly it is, has its origin in human nature, and even in the best dispositions of human nature. Candour forgets the bad, but piously remembers the good, of what is gone. The failings of the dead are buried with them, while their virtues, whatever was attractive and engaging, are rescued from the grave; and acquire new splendour by being separated from every thing that offends. But, while offence lives, it arrests our principal attention; it irritates our tempers; it crosses our pursuits; it provokes our moral indignation; and therefore the vice of the day is the subject of everlasting complaint. But in all this proceeding neither truth nor justice is observed, and without ill intentions we often defeat every good purpose. The good and the bad of every day ought to be fairly stated, and neither candour nor prejudice ought to be heard at the bar of impartial justice. By exaggerated praise or exaggerated blame we give a sanction to folly, to error and vice, while we throw discouragement on the ingenuous pursuit of wisdom, truth and virtue.

In Rousseau we find a striking example of intemperate censure and intemperate praise. The preference of the past to the present, of barbarity to refinement, of ignorance to knowledge, is his favourite theme: it directly or obliquely insinuates itself into all his writings. Barbarity with him is simplicity; it is nature in her pure ingenuous walk; while refinement, taste and elegance are only the gilding of duplicity, fraud and vice. To know more than simple nature obtrudes upon us, is only to know the instruments of mischief; it is to awake a temptation, and capacitate man to be the enemy of himself and his fellow man. With a splenetic turn of mind he finds more to offend than please in the whole view around him; with a passion for fame he courts reputation by the singularity of his doctrine, by the boldest contradiction to the common sense of mankind; and with abilities wonderfully fitted to give a grace and a charm even to the grossest absurdities, he ventures on an open hostility to every thing which man conceives to be his highest ornament and praise.

I have observed that this humour of depreciating the present state of man, and overrating that of the past, tinctures most of the writings of Rousseau; but it is the professed object of the celebrated essay to which the academy of Dijon adjudged the prize. He maintains that the progress of society and its boasted improvements have been only to make man progressively acquainted with misery. He charges this crime particularly on the sciences and the arts. "Antient days, he says, were more virtuous than our own, and the degeneracy of our own days owes its origin to our knowledge." In fine, knowledge or science appears to him to be Pandora's box, replete with every evil. A poor prisoner in a house of lunatics, being asked the cause of his confinement, replied, that he thought the whole world to be mad, while the world thought him to be mad, but unhappily the world out-voted him. Roussseau must have thought, if he thought as he wrote, that a mania had possessed man from a very early period, and that in his day this mania had risen to its greatest height. It was well that the question did not come to issue between him and the world, for the world would assuredly have out-voted him. Rousseau is however entitled to a philosophic and argumentative reply.

In the first part of that discourse Rousseau objects to knowledge, that it is the parent of that external civility and politeness, by which the foundation of candor and plain dealing are undermined, and fellow-intercourse becomes constrained and disguised. Before art, says he, had fashioned our manners, imposed concealment on our passions, and taught us to speak a borrowed language, our behaviour though rustic, was natural. He admits that human nature, at the bottom, might not perhaps be better; but he asserts, that men derived security from being able to read each other's thoughts, and that this advantage, of which we now know not the value, preserved them from many vices. To this part of his charge the present essay is confined.

In answer to this charge it is asserted, that external civility and politeness are not the offspring of learning or knowledge, but claim other parents; that disguise, borrowed looks and language, and false exhibitions of the heart are not peculiar to any period or state of man; that sincerity and honesty are not irreconcileable with politeness; and that whatever of evil can be charged to the account of politeness, is amply compensated by the real good which it produces.

Politeness may certainly associate with learning, and may be separate from it; but ther first origin is in the good-will and sympathy of man, in the desire of being agreeable in the form as well as in the substance of our fellow-intercourse.

This is so obvious, that it is impossible to discover any special connection of cause and effect between a learned mind, and a polite mind. A learned man, without a kind and sympathetic heart, without a desire to please, may be as blunt a rustic as Rousseau can contemplate in his golden age of simplicity. Learning is very far from being the character of the polite world, and politeness in a still less degree is the character of the learned world. The weakest persons, to whom literature has not opened her very door, may lead in the dance of fashionable politeness. They are perfectly innocent, poor creatures! of the horrid crime of learning; but they are the arraigned before Rousseau's tribunal, they are the convicts of unmeaning profession, of prostituted language, and of all the idle waste of words. Observe the learned man! He may possibly be polite; he may be courteous in his address, in his speech, in all his manners; but he has not learnt this from his books; he has acquired it from an habitual commerce with the dressed and fashionable world. Such a union of attainments is however a rare spectacle; for, learning abstracted from other circumstances has a contrary tendency, and the world is so persuaded of this, that it expresses something like astonishment, if in the acknowledged scholar or philosopher it find the polite man. The love of retirement and even of solitude, as conducive to the pursuits of learned men; the little attraction which they feel for the lighter amusements of life, the straws in their estimation which float upon its surface; the little attention which they have bestowed in order to acquit themselves with propriety and grace; the disgust which is excited in them by the trifling conversation and important nothings of men of the world, render what is called good company as unfit for a philosopher as a philosopher is for good company. What a figure does he often exhibit in a gay and brilliant circle, with his solemn air, his stiffened attitudes, his unmanaged limbs, his absorbed mind, his inattentions, his constrained recollections, his studied expressions, his deep and sensatious discourse!

He is an object of ridicule to the circle around him; but he knows to estimate himself, and he returns the contempt with which he is received. He feels that he is not on his proper ground; no common sympathy attaches him to his company, nor his company to him; each are under restraint, but a modesty yet unsubdued in him subjects him to truly painful feelings, while a happy confidence which the polish of the world often confers, administers to the company the enjoyment of a secret triumph. He retires from the scene without regret, and his absence excites no regret in those whom he has quitted. A few reflections on the strange interview for a while occupy the thoughts of either party. The one laments the littlenesses and follies of which he has been a witness; the others laugh at the awkward mortal for his oddities and unaccommodating wisdom. While the fruit of these reflections differs in each as much as the reflections themselves. The one are strengthened in the persuasion that the accomplishments of politeness are the finishing of the human character, and with more self satisfaction go on in a course, which as a whole is but a waste of time, of talents and of character. The other owes it perhaps to his keen disgust that he is not swallowed up in the gulph of dissipation, that trifling and unimportant attentions are not over-rated by him, do not debauch his mind, nor lead him to the borders, if not into the open field of vice. There is certainly an extreme in the judgment of each, but with which extreme the just estimate of character and dignified enjoyment is most likely to be found, is left to the Genevan philosopher to calculate.

In fine, learning and politeness may be cotemporaneous events in the history of man, and they may each be derived from their proper source; but they are neither of them the parent of the other, nor do they kindly mix in any relationship or affinity. To derive politeness from learning, and charge to her account the vices of the former, whatever these vices be, merely because learning and politeness may be co-existent, is one of those sophims, which logicians class under the name of *non causa pro câusâ*. If the error proceed from design, it is criminal; if it proceed from ignorance, and a confused understanding, it is contemptible in one, who pretends to the character of a philosopher, and a censor of the follies of his day.

Learning and civility appear both to be derived from the same circumstances in the condition of man; but if civility be a vice, learning is not to be accused on this account. The field produces the grain and the weed. Is the salutary plant accused, because the useless or even the noxious plant vegetates by its side? As man betters his external condition, multiplies the conveniences of life, his views enlarge, he connects himself more closely with fellow man, he widens his connections and dependencies, and to accomplish this his intellect and ingenuity are provoked; his manners also become more insinuating; he courts his fellow by courtesy as well as by kindness; and thus science, the arts, urbanity and the agreeable in form, address and intercourse, advance with a step proportionate to the growing interests, utilities and conveniences of life.

This appears to me to be the true origin of learning and civility, considered in a general view, and to account for their co-temporaneous existence in every instance, whenever they have been found to enter into the human character. But learning and civility, though issuing from the same source, and always in some degree accompanying each other in their progress, may vary in their respective character from other circumstances; and therefore it may require the aid of some other cause to account for the peculiar politeness of later Europe, which is the very object of Rousseau's invective. To discriminate the urbanity of Greece and Rome from the politeness of the present century, and to account for whatever is peculiar to and characteristic of the latter, may not therefore be impertinent to the object of the present essay; especially if it shall appear that learning is as innocent of the guilt, which may be imputed to modern, as to antient manners.

Accurately to define the characters of antient urbanity and modern politeness, may be no easy task, nor is this difficulty peculiar to the present subject, as there are many instances, in which we cannot define, what we strongly feel. Cicero has given a definition of urbanity, which may apply to the civility of any day, as it is a mere definition of the abstract principle. Had he illustrated it with a familiar instance in the manner of his day, we should have been better able to estimate the standard of Roman urbanity. We have not a history of the private life of the Romans, but from some incidental facts which are recorded, we may decide that their manners were not so dressed and elegant and delicately finished as those of modern Europe. Yet we must allow to Greece and Rome all that polish which answers to their high condition of prosperity and cultivation. The wants of nature are common to all, but a richer possession of the external conveniences and accommodations of life cannot fail to awaken an artificial taste, the idea of a grace which may be superadded to every enjoyment. All of the polite and elegant which the collision of man with man can be expected to accomplish, may be ascribed to the higher personages of these antient days, but the iufluence of woman on manners could not be experienced by them, because woman held not the same rank in society as in these later times; and therefore this must constitute one characteristic feature in which the politeness of the most accomplished periods of Greece and Rome could bear no resemblance to that of modern days. Commerce, wealth, science, arts, and urbanity have made a rapid progress in Western Europe, and independant of any other cause must have produced their effects; but the state of woman, which is absolutely novel in the history of man, has given an appropriate character to our manners; it preceded every other cause; it operated in our rudest and most ignorant periods; it was the originating source, it has been the cradle, it is the highest aspiring of our politeness, and from this is derived whatever is peculiar in the manners of the masculine sex.

It is certain that woman had little estimation in antient times, unless as subservient to a purpose which it becomes me not to name before this assembly; or, as necessary to posterity; or, as the superior oeconomists of domestic supply and order. They were passed as a property from the parent to the husband; they were not introduced on the public stage of life; they were even secreted from the open face of day; they were not admitted to the councils of man, to an equal participation of the hopes and fears, the joys and sorrows of the lordly male. This, whether it were to the honour or reproach, whether it were the blessing or misfortune, of antient days, was assuredly the character of antient nations, though with some difference of degree; it was that of the Asiatics, of the Greeks and Romans, and continues at this day to be the unvaried character of the eastern world. To the rough inhabitants of northern Europe, barbarians as history affects to call them, but who were our progenitors, the female sex are primarily indebted for their vindication to the equal dignity and privileges of human nature. To this singular character of these northern nations, long unknown to the rest of the world, Cæsar and Tacitus have born testimony, while they were confined to their native land. The romantic gallantry of the days of chivalry, and the romanzas of the succeeding period, in which it is difficult to decide whether superstition, heroic courage, or idolatry of the sex, be the predominant feature, and the gradual passing of the old romance into the modern novel, all demonstrate that this character has never been parted with, but been mellowed into the more rational and tempered gallantry of the present day, such as we their posterity, the inhabitants of modern Europe, unreluctantly bow to. If man have imposed chains upon himself, he feels not the weight of them: he is happier in the participation of power and influence with the female, than when he held her under his absolute dominion, when she was the slave of his will, or the passive instrument of his selfish pleasures.

This generous sympathy of our northern progenitors with the partners whom nature and God designed for them, happily co-incided with the equal and liberal spirit of Christianity; and to these two powerful agents, which were nearly cotemporary, we owe that wonderful revolution of social and moral sentiment, which constitutes the distinction of later Europe. Woman has now been permitted to resume her proper rank in society, and to her we are greatly indebted for the present polish of ruder man; for that ease, propriety, grace, attention and desire to please in the manner of every intercourse, which offends the cynic eye of Rousseau. To every thing that is human some accusation may be brought, whether in consequence of defect, excess, or association, and therefore it is not wonderful if politeness committed to the management of men should be subjected to censure; but be her errors and excesses what they will,

Look on her face, and you'll forgive them all!

To her this generous acknowledgment is due, that she smooths the asperities of life, veils the deformities of selfish vice, and gives to ingenuous virtue and goodness its highest lustre.

I do not mean however in this part of my essay to be the advocate of politeness, but to prove, that learning is perfectly innocent of its birth, in modern as well as antient times, and let its present character be what it will, to fix the filiation upon its proper parents. If it be true, that the politeness of modern Europe derives itself from the restoration of woman to her proper rank in society, and that woman is the legislator of politeness; there is a circumstance in the character of woman, which demonstratively proves the folly of Rousseau in ascribing to learning either the blessings or the curses of politeness. In no day, not even in the present day, has woman been eminently distinguished for learning; and therefore the empire of politeness, sovereign as Rousseau supposes it to be, neither derives itself from, nor is maintained by learning.

I return to the direct subject of the essay. Having rescued learning from the imputation of politeness; I wish also to rescue politeness from the more odious charge of insincerity and dissimulation, as if they were her necessary and appropriate character. Dissimulation and artifice may and often do assume the attractive form of politeness, but they are not her natural offspring; they have their origin in those interested passions, which are common to man in every age and nation, and which pure and spotless innocence, if it have ever existed on this earth, is alone exempt from. The savage, the rustic and illiterate know to decieve under a fair exterior, whenever a selfish end awakes the desire, and deceive as artfully and as successfully as any of the polished and lettered sons of modern Europe. Instructed by mere interest, they can adopt even a seducive address, and in the very form of their natural simplicity and bluntness they can still more successfully deceive.

Uncultivated society has its virtues, it has its vices also, of which none hold a more distinguished rank than art, deceit, deliberate fraud and imposture. The history of the Arabians, Tartars, North-Americans and Chinese, who are at best only managed and disciplined savages, is much more the history of lying and imposture than of plain truth and honesty. They can practise insinuating address, they can mislead with deep designs, and deceive with fatal success. In their national treaties they can shroud their intentions with as artful concealment as the most lessoned adept of our diplomatic schools. With war in their hearts, they can send an embassador of peace, out of whose breast not the shrewdest politician of Europe can dig the secret. In war, they are treacherous more than brave; stratagem and ambuscade constitute their military tactics, and they measure their triumphs, not by the manly resistance which as men they have overcome, but by the insidious violation of faith, by springing upon their destined prey in all the confidence of security, by their merciless use of an inglorious victory, and the undistinguishing massacre of age and sex.

In this representation I may be supposed to exhibit the portrait only of the North-American tribes; but the histoiy of every rude and uncivilized nation presents the same features; and in this very century the Chinese, whom I have not excepted from the class of barbarians, have exhibited a dreadful specimen of this insidious, unmanly, and savage character. The late emperor of China, jealous of a numerous and powerful Tartar horde, and, if I remember aright, one of the very tribe from which himself was descended, allured them by faithless promises, till he had drawn around them a cordon of his numerous hosts. Then in the true spirit of a savage, he issued the word, and appeased his fears by putting them all to the sword. He exterminated nearly three millions of human beings at a blow.

Such are the representatives of the native simplicity and innocence of undressed man; such are the patterns of frankness, sincerity and manliness, to whom Rousseau offers the incence of an almost ideot praise, in comparison to whom the European character is one uniform blot. He rivals the Papal power in its greatest plenitude; he blackens an angel, and whitens a devil.

But if contrary to fact, and what the history of man in the progress of society reports to the plain enquirer after truth, it should be allowed that politeness derives itself immediately from learning, yet learning is not therefore to be charged with that duplicity and dissimulation, which in the practice of politeness has excited the indignation of Rousseau. Politeness in its primary designation presents a fair and amiable character, but like every blessing to man, vice may seize the blessing, and as far as vice can operate, may convert the blessing to a curse. Has learning taught to vice this lesson? If this be a truth, it presents a novel idea of learning, contrary to every idea which has been entertained of her genius and spirit; for before the Genevan philosopher was pleased to undeceive man, she was supposed to be favourable to truth, to the discovery and to the ingenuous communication of truth.

What gave to the openings of science that charm, which roused man from the indolence and torpor of savage life, but that it ministered to this native love of truth in the mind of man, to that ardour of a rational mind, which, when awakened, pursues with avidity and with unsated gratification the discoveries which nature unfolds to the diligent enquirer. This is the spirit and genius of learning, and most unlikely to have inspired the wily arts of polished life.

The question is not therefore, whether politeness, and even learning, may not be seized by vicious men, and perverted to vicious purposes, but whether it be of their genius and character to favour this abuse. The question is, whether politeness be not of a virtuous origin; whether it be not this very circumstance which recommends her to vice; whether science, whose object is truth, and the communication of truth, can intend disguise, deceit and perfidy. Vice exists. Granted. Let the crimination be carried to the baser passions, which originate vice of whatever form, but not to those laudable passions of man, which contemplate truth and kindness.

How many other blessings does vice pervert and abuse? From the Creator of man is derived the very capacity of learning, the impulse to, the delight therein, and all that is the genuine offspring of learning. If learning is to be accused, because vice may prostitute learning itself, and whatever is the progeny of learning, to uses which the generous spirit of learning abhors; then *a fortiori*, the Creator is to be accused, who has inspired in man the desire and the capacity of learning; nor is there one gift, nor one blessing for which we may not with equal justice reproach our maker, as there is not one gift nor one blessing which vice may not, and unfortunately does not, equally misapply. It is not of the genius of learning and wisdom to favour vice in any of its forms; science and virtue are naturally allied, and to the sons of learning the world is indebted for the most animated and persuasive pleas in the cause of virtue; that the wisdom of her ways has been illustrated; and that vice has been proved to be as foolish, as it is detestable. To learning this is an easy, a pleasant and a natural office; while all attempts to press her into the service of vice, have reflected disgrace and infamy on the perpetrators of so rude a violation. However individual men may pervert the plan of their Maker, science and virtue are united in the service of man, as wisdom and holiness in the conduct of the divine being. I make no apology for this momentary use of the stile of the theologist; it becomes me as a philosopher, who demands for theology a place in the great philosophy of nature. He who designed man, designed science and wisdom to be of the highest attributes of his being, and to traduce them, is to traduce both man and God.

But to the innocence of learning as not answerable for the duplicity and fraud and dishonesty, which Rousseau charges upon politeness, it may be farther added to its praise, that if any thing can guard a man against the impositions of dissembling address and language, it is to the cultivation of the intellect that he must be specially indebted for this guard. Learning places a man upon a higher ground; it confers upon him the advantages of superior intelligence; and when directed to the judgment of character, and assisted by an acquaintance with men, their objects, their modes, and varied import of language, it acquires a kind of intention into the very heart. A scientific man is so accustomed to yield conviction only to evidence, that he is rescued from that precipitate decision which is the pit of fools; he is possessed of more nice discernment, more accuracy in weighing every thing in the scale of sober judgment, more facility in resolving, combining, comparing, deciding; so that if imposture must haunt the best walks of life, no man can pass these walks with more security than the well-educated and well-informed man. I do not ascribe this praise to the verbal critic, the mere mathematician, or the simple sciolist of any form; but to him, who has studied man as well as books, which alone deserves the name of true science. And it is to this science that Rousseau's accusation can alone apply; for the learned only, who mix with men, can give to imposture those advantages, which he supposes learning to furnish. It is but in a small degree that learning and politeness can aid the dishonest and the knave; but it is in a very high degree that learning and a polished acquaintance with men can protect against the knave. I might illustrate the truth of this observation by an appeal to very striking facts, facts of the greatest magnitude and importance. Since the study of literature and the study of man have united to enlighten the mind, to give to it more intuition and vigour, false taste, false philosophy, false religion, and, I may add, false politics, have lost their power of fascination. True science, like Ithuriel's spear, when applied to satan in the guise of an angel, has compelled them to unveil, and stand forth to view in their naked deformity.

Having rescued learning from the imputation of politeness, it is an act of justice to rescue politeness from the imputation of insincerity and dishonesty, which Rousseau supposes to be her necessary attendants.

Every virtue and excellence of man invites a counterfeit, which assumes an honoured name, and tricks herself out in a respected garb and form, but has nothing within of the loveliness and worth of the original. It is thus that for a while, and for ever with the ignorant and the credulous, supersition may be mistaken for religion, the traitor for the patriot, the sycophant for the friend, the seducer for the lover, and the hypocrite for the honest man. But the fraud is discovered, and while the imposture sinks into irredeemable contempt and infamy, the solid virtue remains unimpeached, and acquires renewed splendour from the odious contrast. Thus it is with politeness; her origin is honourable; her foundation is firm; her seat is in the heart; her range of action is the whole intercourse of man; but her favoured residence is the breast of woman; woman, who with

Love in her eye, and grace in all her walk,

was designed to allure, to attract, to scatter roses on all the paths of life.

Such as is woman in her best character, such is politeness. She visits undressed uncultivated man; in her whole intention, in her whole manner she is kind. The ruggedness of manners is smoothed under her steps, her smiles calm the turbulence of passion, and man is not allowed to approach man but in the form and language of kindness. The ingenuously honest and good embrace her in sincerity; the designing and bad have no access but in her name, and their hypocrisy is so far from being to her dishonour, that it is the homage which even vice finds itself obliged to render to her excellence and worth.—But to quit the poetic stile, which is not natural to me, the truth assuredly is, that politeness has not in her contemplation to furnish a language for the insincere and dishonest; her intention is in the less important and ordinary intercourses of human beings, which singly seem of small worth, but altogether make up the sum and mass of social enjoyment, to intermix those little sweet courtesies, which give a grace, a charm, an acceptableness to every thing. She is therefore of the family of good-will and benevolence, and he who cannot separate her genuine character and spirit, from the abuses which have attached themselves to her, deserves not the name of a philosopher, and hardly the name of a man. These abuses however, like most incidental evils, have provided their own correction. The ordinary forms and language of politeness have their appreciated value; they pass for no more than they are worth; while the higher value of the heart, of which politeness was designed to be the agreeable expression, is estimated by more accurate and certain tests.

I may add to what I have already observed in opposition to the magnified evil of a polished language, that there is something in ingenuous truth and honesty, which cannot be counterfeited, and which no artful language can convey. In a greater degree than is imagined does the mouth speak from the heart, and therefore along with what is common to truth and to imposture, to simplicity and to art, there is something which discriminates the one from the other; there is a reserve, a modesty, a delicacy in the one; a prurience, a boldness and a grossness in the other, which constitute a marked distinction of character, and tell the truth to a discerning world. Sure there must be an angel of detection, which follows the dissembler in his path, and compels the dishonest and designing to reveal the precious secret which they would hide. It is certainly the interest of the impostor to appear to be the perfect character which he assumes, but not all his practice in the art, not all the cool deliberation of his plan, not all his knowledge of the world, aided by genius and by learning, can enable him perfectly to act the part, which it is his interest to personate. Truth cannot betray itself, for having nothing to conceal, it has nothing to betray; but Art has, and therefore cannot guard every avenue of detection; in the very texture of its art it cannot present any thing like the simple and beautiful work of nature. Innocence and guilt are essentially different in their nature, and so in a great degree is their appearance, in spite of a general conformity of exterior. The artful courtezan cannot personate the inimitable simplicity, ease and grace of the untainted female.

In fine, vice must be allowed to exist in the polished and the learned world, and perhaps in more multiplied forms than in ruder and ignorant periods, though with less deformity and horror. But conceding this, it is still a truth, that whatever be the moral reproach of any day, learning and politeness are not the cause, and they are still a real blessing to the nation which possesses them. I may sum up their praise in a few words; they add to the acceptableness of real worth and goodness, they lessen the deformity of vice, and on the whole contribute largely to the melioration of society, to the general stock of human comfort and happiness.

ESSAY II.

That luxury and corrupt manners are not the progeny of science and the arts, in answer to Rousseau.

In the same celebrated essay to which the Academy of Dijon adjudged the prize, Rousseau aggravates the charge against science and the arts, by imputing to them the introduction of luxury and a general corruption of manners. In this I do not scruple to charge upon him the same confused and illogical understanding as in the former accusation. If he find but a contemporaneous existence of two facts, he hastily concludes that the one is the cause of the other. Now as an argument is deemed to be bad, which concludes too much, so a rule of assigning a special cause to an effect ought to be considered as bad, which infers too much. He might, if he had pleased, and he ought, whether he had pleased or no, to have observed a multitude of other characters to be co-existent with luxury, such as law, political science, philosophy natural and moral, christianity, deism, atheism; and for aught that I can observe of a connection of cause and effect in what he has assigned, he might by the same rule and with equal propriety, have referred to any one or to all of these conjointly, the introduction of luxury and that corruption of manners, which is opposed to simplicity.—Sir Isaac Newton has in physics established this rule of judging of cause and effect, that, where two phenomena are invariably found to be co-existent, and where the disappearance of the one is instantly attended with the disappearance of the other, and the re-introduction of the one withdrawn is immediately followed with the re-appearance of the other, and so uniformly in every instance; we may then safely conclude that there is a connection of cause and effect between these phenomena. But this rule of judging is only meant to be applied, where the *actus operandi* and the *modus operandi* are concealed from our view. Now no one can pretend, that if learning generate luxury, the very act, and the whole operation, can be concealed from our observation as in the history of natural causes. It is incumbent therefore on any one, who criminates learning as the operating cause of luxury, to state in clear terms and agreeably to obvious experience, the will, the act, and the history in the production of luxury by learning. This Rousseau has failed to do, and this every one will fail to do, who shall be hardy enough to make the attempt.

But admitting that the rule of Sir Isaac Newton is equally applicable to the discovery of moral as of natural causes, which perhaps it is, though the application be unnecessary; yet Rousseau would derive no advantage from the concession, for his conclusion would fail if examined by the test to which the guarded mind of this sagacious philosopher has subjected his rule. The phenomena of learning and luxury are not always united, nor, having been previously united, is the disappearance of learning always followed by the disappearance of luxury. Let the court of Caligula, of Nero, of Heliogabulus, the Asiatic Monarchies, and many other striking examples in antient and modern history make the reply? The truth is, there is nothing in the character of learning, which inclines her to luxury, but there is much that is unfavourable and averse to it. The Antonines were learned princes, particularly Aurelius, and they were equally eminent for temperance and moderation, for sober and chaste manners. A learned man may indeed be luxurious, but instances of this mixed character are singular; for learning does not smile upon luxury, nor is luxury propitious to learning.

Luxury is a general term, and answers to very different standards in different minds, and in different circumstances. What a cynic would call luxury, a more correct judge of manners would denominate taste and elegance. But it is to the praise of learning that taste and elegance are of her progeny; if not, it is a vice in the most learned of all beings, that he has embellished his creation with such a profusion of beauty and elegance, and exhibited to us those perpetual models, which according to Rousseau have misled man.—The Genevan philosopher finds luxury and corrupt manners in learned nations alone. But with a more discerning eye, and a less prejudiced mind, he would have found them with equal excess, and with equal or more deformity, in rude and illiterate nations. The feast of a Kamschadale or an Esquimaux on a whale, a bear or an otter, is a true and proper luxury, and marked with as intemperate indulgence, as the rich and varied board of Lucullus or Apicius. The mind is of the same character in both; they differ only in external circumstances, and condition being changed, but the mind unaltered, Apicius would have been the Esquimaux. The corrupt manners of the Otaheitans have not been surpassed, nor perhaps equalled by any of the learned nations, to whom the unfriendly eye of Rousseau is directed. Minds more surrendered to effeminate luxurious ease and every lust have not been exhibited in the page of history; and yet these are an unlettered people, and strangers to the arts, which have vitiated modern Europe. They know not this wicked thing, called learning, which has brought in luxury and all prostitute manners like a flood upon us; nor those arts, which have been the panders of this luxury and corruption.

Rousseau seems to feel the difficulty of fixing immediately upon learning the crime of luxury and its corrupt attendants; but he is more at home when he refers them directly to the arts, which he supposes to be the offspring of learning. Now in this statement, and in the first hasty view of it, there does appear to be a connection between luxury and the arts, and between the arts and learning. But the first view of a subject is not always the true one, and there will be found in this, as in the former instance, the same inaccuracy, the same confused understanding, the same unfounded presumptions, the same illegal conclusion.

That luxury and very corrupt manners may exist without the arts has already appeared from what has been noticed in some of the rudest and most uncivilized nations, to whom may be added the Turks and the Moguls, examples of notoriety and magnitude, who from the commencement of their empires to the present day have been singularly hostile to science and the arts, but not hostile to luxury and the corrupt manners, which are justly supposed to be in her train. A nation ignorant of and unexperienced in the arts will and must, in proportion as it is possessed of the means, be luxurious and corrupt. Indolent ease finds the mind vacant to luxury and lust; this is in the constitution of human nature, and is verified in the constant history of national and individual man. A field of active exertion is therefore provided for man as his refuge; it gives a better direction to his mind; it arouses him from that listless repose, in which he has nothing to brood upon but the indulgence of his appetites and lusts, to which in such a state he is so prone, and which in such a state are almot necessary to support the burthen of existence. Hunting and war alone can rouse the savage to action, and then he is more temperate and abstemious than the philosopher or the hermit. The inexhausted allurements of scientific pursuit or the activity of the arts provide a more constant and salutary refuge to a civilized and wealthy nation from their devouring passions and lusts.

Science and the arts are therefore more properly the moral friend and guardian of man; they may minister to some of the productions which a luxurious and corrupt mind seizes upon, but they are innocent of the misuse, they neither suggest nor favour the misuse. The earth itself produces a very considerable part of the food of luxury. Is the earth to be accused as the criminal minister of luxury? The earth and science and the arts are all liberal in their gifts to man; these gifts are in their nature directed to utility and to blessing; they are in the reach of the virtuous and the vicious; virtue enjoys them in their proper character; vice converts them into a curse. To maintain the theory of Rousseau, the earth and science and the arts should be more sparing in their gifts, and all the virtue which his theory aspires to is the impossibility of being vicious. The earth and the arts resemble each other in the variety and richness of their produce, and in not directing nor answering for the use to which their productions shall be applied. But to science are annexed mind and will, and to the deliberate mind and will of science cannot be charged one approbation of abuse, excess, intemperance, luxury, or licentious, corrupt and profligate manners.

There is therefore no natural, no necessary connection between the arts and luxury or vice. The productions of the arts are illustrious monuments of human ingenuity, and in no small degree imitative of the superior productions of the great Artist; they are in their nature innocent of all criminal construction and tendency; they are much more applicable to virtue and to happiness than to vice and misery; luxury and corruption may exist without them, and therefore it is to other parents and to other causes that we must refer their existence and progress.

If luxury do not derive herself from the arts, however the arts may be violently forced into her service, learning is acquitted of the charge, though the arts should be allowed to be the progeny of learning. For learning, no more than God, is to be interdicted and accused in her most honourable walk, of contributing to the ornament, utility and happiness of man, because vice may seize upon and misuse her gifts. But the argument of Rousseau is even more unfounded in the supposed connection between learning and the arts, for the arts do not perhaps at all derive themselves from learning as their source, and in their progress derive a very inconsiderable part of their utmost improvement therefrom.

The elements of the arts, and many of the most valuable discoveries on which the practice of the arts depends, are derived from unlearned men, or are traced up to unlearned and, what at this day we call, barbarous periods. Who can name the learned day, in which the lever, the moveable pully, the wheel, the inclined plane, and the screw have been introduced to the knowledge and to the use of men? The arch, the pillar, the roof, and much perhaps of the stability, proportion and ornament of architecture, are probably indebted for their first conception to unlearned and unscientific men. The Greeks do not appear to have considered the Persians as a lettered and scientific nation, and though the vanity of the Greeks may render them very justly suspected in their estimate of foreign merit, yet it is probable that in this instance they erred not much from the truth. But the ruins of Persepolis present the idea of a structure, which might have rivaled the proudest monument of Grecian architecture. They are at this day the admiration of European artists, to whose judgment and taste the highest defference is paid. To rude ancestors we owe the first idea of a ship, and no inconsiderable progress to that complex and wonderful state in which it now exists. To the Greeks and Romans in their more rude and unlearned state we are indebted, if a debt it may be deemed, for the discipline, the order, the combinations, the evolutions and the general tactics of war; nor, unless perhaps in the application of gunpowder has all the science and ingenuity of the moderns much surpassed them in this dangerous art. The practice of astronomy, though without a sufficient knowledge of its theory, yet founded on principles derived from an observance of the motion of the planetary bodies, is of very remote antiquity, and has been applied with considerable accuracy by nations of no scientific character, and but in a moderate degree removed from barbarism. If a simple elementary language, wherein from a few characters infinite combinations are formed, be a necessary instrument to the progress of science, the Chinese can have no pretensions to the character of a learned nation, although for no other reasons this attribute should be refused to them. For as language is the vehicle of ideas, how slow must be the progress of literary improvement, where only to know the language itself requires the application of a whole life. Yet on the first visit of the Europeans the Chinese were found to be possessed of the elements of almost all the arts, those very elements, which under the culture of the more ardent European have so exalted him amongst men. The same may be observed of the Mexicans and Peruvians, who had no form of written language whatever. The date of their empires was indeed comparatively of yesterday, but at the period of their highest improvement they could support no claim to the character of learned nations. Yet many of their productions of art, magnificence and taste were objects of admiration to the more improved European. The practical principles of chemistry have been known, and successively acted on, by many of the rudest and most ignorant nations of the earth, and the communication of some of their processes would be a valuable acquisition to the European artist of the present day. Indeed without disparagement to the present state of scientific improvement it must be acknowledged that the valuable arts of mechanics and chemistry have been indebted for discoveries of high estimation to rude and unlettered practitioners, who had eluded all the penetration of the theoretic artist. The fact appears to be, that mere accidental observation, excited by the continually working hand of nature, and agreeably to those eternal laws which govern her operations, has revealed to man, in every state and condition, the fundamental principles of all the arts; that they are thus brought home to his very feeling, and that, the discovery being made, the necessities and interests of man seize the discovery, and apply it to his use. Man, in a state of literary culture, digests these experimental discoveries, compares them, reasons upon them, and reduces them into an orderly and harmonious system, which is without doubt of great assistance, in applying the practice of the arts to progressive improvement and utility. Sensible of this truth, the ingenious theorist will acknowledge that the arts, in the whole extent of their subservience to the use of man, have derived their richest treasures from the discoveries and operations of rude and unlearned men.

Rousseau therefore fails in every view in which it can be attempted to fix the odium of luxury and its concomitant vices upon learning. There is no necessary connection between the arts and luxury, and the arts, in what degree they may be required to minister to luxury, ask little, very little, aid from learning.

Perhaps Rousseau had no view to honest truth in this celebrated essay, but by a bold singularity to raise himself into general notice. Had truth been his object, he could not have avoided to observe, what must strike the common mind, that the appetites and tastes of men are the parents of luxury, and that wealth, or a supply of what wealth purchases, is the nurse of luxury. Where-ever or whenever these two are found to be co-existent, luxury in a greater or less degree will be found to exist also. These may be co-existent, and to any extent, and have os existed, without any thing of what answers to the scientific arts of modern Europe,

Observations *on the* Nervous Systems *of* different Animals; *on* Original Defects *in the* Nervous System *of the* Human Species *and their* Influence *on* Sensation *and* Voluntary Motion.

BY JOHN HULL, M.D.

READ NOVEMBER 28th, 1800.*

" Le mécanisme des sensations, le rapport des nerfs avec le sujet senti à la surface du corps & avec le centre, où se réunit le sentiment, sont aussi obscurs, qu' ils l' ont toujours été, malgré tous les faits, qu' on a recueillis depuis plusieurs siecles." Fourcroy *Syst. des Conn. Chim.* T. IX. p. 348.

In another publication I have enumerated some of the most remarkable deviations from the natural form, that have been observed in human fetuses on the side of excess, both in the number and proportion of their parts. Astonishing as these may appear, the deviations on the side of defect are not less so and the latter are perhaps more particularly interesting to the anatomist and physiologist, as serving to point out the comparative necessity of the different parts, concerned in the performance of the various functions of

* Some additions have been made to this paper, since it was read to the society.

the animal economy. Numerous instances might be adduced, wherein one, or more of the most important organs of the human body have been found wanting in the fetal state. But, passing over the defects, which occasionally take place in the organs of respiration, circulation, digestion, generation, &c, I shall in this paper confine myself to the consideration of the defects, observable in the nervous system of man, and their influence on the important faculties of sensation and voluntary motion; premising some general observations on the diversity of parts, structure, substance, texture, proportion, laws, &c, of the nervous system of man and the inferior animals in their natural, or perfect state.

§ I.

On the Nervous System in the different classes of Animals.

By a celebrated modern writer on Comparative Anatomy, M. Cuvier, animals are referred to two grand divisions: The *first* comprehending all those, which have a dorsal spine, or vertebræ: The *second* comprehending all those, which are destitute of vertebræ.

To the former division are referred the first four Linnean classes of animals, namely, *Mammalia*, *Aves*, *Amphibia*, and *Pisces*. To the latter belong the two remaining classes of *Insecta* and

Vermes. As there is a general resemblance in the nervous systems of the animals, referred to each of these great divisions, particularly the former, I find it convenient to adopt them here.

1. The nervous system of man and other animals, included in the first division, is more extensive and complicated than in insects and worms. Viewed with respect to its situation, it consists of three primary parts, namely, the *Encephalon, Spinal Marrow* and *Nerves.* The two former have been considered as constituting the common trunk, the last as the branches of the system.

The ENCEPHALON, which is so named from being seated in the head, is subdivided by anatomists into three principal parts, the *cerebrum,* or *brain* strictly so called, the *cerebellum* and *medulla oblongata:* But for brevity and to avoid the unnecessary use of technical terms I shall, in general, speak of these three parts collectively under the title of *brain,* taking care to guard against ambiguity, when it becomes necessary to mention any of the three parts distinctly from the others. It may not be improper to state here, that in those animals, which have no skull, and therefore, strictly speaking, no encephalon, that portion of the nervous system, which is the most bulky and occupies the highest, or foremost place, will be named brain, since this term is adopted by writers on comparative anatomy.

The SPINAL MARROW is so named from its occupying the canal, formed by the vertebræ, or bones of the spine, and is generally considered as a continuation, or production of the brain.*—In animals, which have no vertebræ, that part of the nervous system, which is continued from the brain and is connected with the nerves, is also named spinal marrow, though not very properly.

The NERVES are cordlike bodies, rising in pairs from the brain and spinal marrow and ramifying in order to be distributed to the different parts, that are influenced by them.—That

* By some writers the spinal marrow is considered as the largest nerve of the body, but improperly according to Soemmerring: "Notæ enim ejus ab omnium nervorum notis sunt diversæ, cerebri vero notis respondent. Etenim 1. Non ea est medullæ spinæ structura filata, quæ omnibus quidem nervis. 2. Mollior est nervo. 3. Intrinsecus, ut aliæ quædam cerebri partes, portionem cineream continet. 4. Eodem modo ex ea nervi oriuntur, ut v. g. ex cerebro tertius & sextus nervus, nequaquam vero ea ratione, qua nervi ex truncis, qui dividuntur, vel ex gangliis oriuntur. 5. Bestiis maxima pars est massæ cerebri. 6. Homini, ratione cerebri habita, minor est, quam ulli alii animali. 7. Stimuli metallorum per spinæ medullam spasmos non excitant, ut per nervos." *De Corp. Hum. Fab.* T. IV, p. 81.

extremity of a nerve, which is attached to the brain, or spinal marrow, is usually named its origin* ; whilst that, which is distributed to the organs of sense, muscles &c, &c, is named simply its termination and by some writers its sentient extremity, when so disposed as to receive impressions from external bodies. Of the ganglions and plexuses of nerves it is not necessary to take notice in this place.

The nervous system is composed of two principal *substances*,† named the cineritious, or cortical, and medullary; which differ considerably from each other in their colour, consistence, and other properties: The former being of a reddish ash colour, semitransparent, softer, without distinct fibres, and possessing little, if any, sensibility: The latter being white or yellowish, opake, firmer, consisting of very fine fibres variously disposed and evidently possessing sensi-

* Dr. Monro thinks this extremity is improperly so named and assigns a good reason, which will be noticed hereafter.

† Besides these, there are two other substances noticed by anatomists, namely, 1. The *portio intermedia*, which is whitish or yellowish, and is found in the cineritious substance of the posterior lobes of the cerebrum. 2. The *portio nigra*, which is found in the crura of the cerebrum. These substances are only found in the brain and are inconsiderable. See Soemmerring, T. IV. p. 47.

bility. Both these substances are very evident in the brain and spinal marrow: But the cineritious matter is not so easily observed in the nerves, being in a very small proportion to the medullary. Dr. Monro, however, informs us, that he has by an attentive examination found it in most of the nerves.† The relative position of the cineritious and medullary substances differs in different parts of the nervous system. The *consistence* of the brain is softer in cold-blooded animals and in some fishes is almost fluid.

† "Although the nerves have been universally considered as a continuation of the pure medullary substance of the brain and cerebellum; yet I find, on accurately examining them, that with a few exceptions, particularly of the optic nerves and portio mollis of the auditory, they are all of a browner colour than the medullary substance, their pia mater seeming to furnish a quantity of cineritious matter."—"The optic nerves and portio mollis of the auditory seem, indeed from their bright white colour, to receive from their pia mater little or no cineritious matter in their progress to the eye and ear: but, as soon as they enter these organs to form the retina and to be spread out on the membrane of the cochlea and semicircular canals, instead of remaining white and opake, they become cineritious. The cause of which is, that, contrary to what has been alleged by all authors, they carry with them their pia mater, and from that membrane every fibre of the nerve receives cineritious matter." *Observations on the Structure, &c. of the Nervous System, p.* 32.

The *structure* of the nerves and spinal marrow is very simple, compared with that of the brain. The last possesses a complicated and beautiful organization, which we shall perhaps never understand. In viewing the structure of the heart, we easily comprehend, in what manner each of its parts contributes to the peculiar function of this organ, the circulation of the blood: But we do not perceive how any one of the cavities, processes, &c, &c, of the brain contributes to sensation, volition, or the intellectual functions. It will not therefore be necessary for my present purpose to enter particularly into the consideration of the structure of the brain in man and still less so in the other animals, referred to this division. It may be proper, however, just to remark, that in each of the higher classes of animals, the brain has its peculiar characters, arising from the presence, or absence of certain parts, or from the position &c of these, and that M. Cuvier has specified these peculiarities.* The spinal marrow and nerves are of a fibrous structure, and the fibres of the latter, except at their remote extremities, are included in both a proper and a common coat, or sheath.

The *proportion* of the brain, compared with the rest of the nervous system, varies in different

* Anatomie Comparée. T. II. p. 172—175.

animals. In the higher orders, the brain is larger in proportion to the spinal marrow, and the spinal marrow is also larger in proportion to the nerves connected with it. The human brain is by much the largest in proportion to the rest of the nervous system: It commonly weighs from two to three pounds, or upwards; but very rarely reaches four pounds. In other warm-blooded animals the brain diminishes in volume, in proportion as the spinal marrow enlarges, and in some fishes the bulk of the two other parts of the brain scarcely surpasses that of the medulla oblongata, and this scarcely exceeds the spinal marrow. The intellectual powers of animals seem to correspond in extent with the proportion of the brain to the rest of the nervous system, and the perfection of its organization: Whilst the acuteness of sensation and the force and rapidity of voluntary motion appear to depend upon the proportion of nerve, distributed to the organs of sense and voluntary motion rather than on the proportion of brain. All animals, which are as large as man, have larger nerves and greater strength. The smaller animals have much larger nerves in proportion to their limbs. Fishes, which have very small brains, have very acute feeling and move with great rapidity and force. Many animals have more acute senses and greater powers of motion than man; although all are infinitely inferior to him in intellectual powers.

All the animals, which have vertebræ, have the same number of senses as man.

2. The animals, which are destitute of vertebræ, when viewed with respect to their nervous systems, may be properly subdivided into three orders.

The *first* comprehending the insects and some worms; which have a brain, spinal marrow, and nerves, or three parts corresponding to these at least. The *brain* in these animals is placed above the alimentary canal and sends off two branches, or legs, which inclose the œsophagus like a collar.—The continuation of this, or the *spinal marrow*, is situated under the alimentary canal and contained in the same cavity with the other viscera. It is double, the two legs remaining distinct throughout a great part of their length and being only united, at different points, by means of the knots or protuberances, from whence the nerves arise, and which are nearly as large as the brain. Mr. Cuvier observes, that "the great sympathetic nerve, which is constantly found in all animals with red blood, does not exist in any white-blooded animal, unless we consider as such the two nervous threads, which unite all the ganglions and which are named *moelle épinière* in the crustaceous animals, insects and worms: In which case these animals would have no spinal marrow, and the absence of this medullary production would be the common character of all the animals with white blood."*

The *second* includes some *Mollusca,* which have a brain and nerves passing off from it in a radiated manner, but no elongation of the brain, analogous to the spinal marrow of the former order. There are however scattered ganglions, almost as large as the brain itself,† in various parts of the body.

The *third* comprehends those animals, which are of a gelatinous texture and have no evident nervous system as many Zoophytes, or Polypes. In these nothing corresponding to brain, spinal marrow, or nerves, can be discovered; nor can vessels or muscular fibres even be detected.‡

Thus we find, as we descend in the scale of animal creation, the common trunk of the nervous system gradually lessening and the medullary

* Anatomie Comparée T. II. p. 124.

† Ibid. p. 98.

‡ "La faculté de sentir & celle de se contracter, qui dans la plupart des animaux sont exclusivement propres, l'une à la substance nerveuse & l'autre à la fibre charnue, paroissent être également répandues dans toutes les parties de certains animaux gélatineux, dans lesquels on n' apperçoit ni fibres ni nerfs." *Cuvier Anat. Comp. T. I. p.* 27.

substance less and less concentrated, till it becomes at length imperceptible and equally distributed amongst all the parts of the most simple animals.

The number of senses in this division is less than in the animals, that have vertebræ. Sight is wanting in some of them; hearing in a greater number: But the remaining senses, especially feeling, appear to be never wanting.† Have these animals any sense, which is not possessed by those of the former division? Some ingenious physiologists think this probable.

I shall in the next place mention a few of the *phœnomena*, or, as they are usually termed, *laws* of the nervous system, relative to sensation and voluntary motion in different animals.

1. *a.* In man and other animals, belonging to the higher classes, when naturally formed, no sensation is excited by an impression, made upon any part of the body, if the nerves, distributed to that particular part, be divided, tied, or so strongly compressed in any part of their course as to destroy the communication betwixt it and the brain, or spinal marrow. Hence it is proved, that neither the termination of the nerve, nor any part of it below its connection with the brain, or spinal marrow, is the seat of sensation.

† Cuvier Anat. Comp. T. I. p. 37.

b. Neither is sensation excited by an impression made upon a part, the nerves of which are connected with the spinal marrow, if the spinal marrow be divided, or strongly compressed, above the point, where these nerves are joined to it. Hence it is equally proved, that the spinal marrow below its junction with the brain is not the seat of sensation.

c. If the whole of the nerves of a limb, or any part, be divided, or so strongly compressed, as to intercept all communication betwixt the limb or part, and the brain, no motion can be excited in the former by volition: Consequently the power of beginning voluntary motion is not in the nerves.

d. If the spinal marrow be divided, or strongly compressed, in the neck or any lower point, none of the parts, supplied with nerves from the spinal marrow below the injured part, can be excited to action by the will. Hence it is proved, that the power of beginning voluntary motion is not resident in the spinal marrow.

e. When the spinal marrow is injured in the manner just mentioned, the mental faculties are not necessarily impaired, and sensation may be excited and voluntary motion produced in those parts, which are supplied with nerves from the brain, or from the spinal marrow, above the injured part. Hence it is demonstrated, that the brain, or the junction of this with the spinal marrow and nerves, is the exclusive seat of sensation, of the power of beginning voluntary motion and of the intellectual powers in man (and other animals), whose nervous system is naturally formed and not affected by any gradual disease. A further proof of this is derived from hence, that a sudden and violent compression of the brain destroys all sensation, voluntary motion and intellectual powers.

Upon this matter physiologists are generally agreed; but they differ very considerably as to the part * of the brain, which is the immediate

* "Sedes animi auctore *Cartesio* est in hypophysi, *Bontekoe*, *Lancisio* & *La Peyronie* auctoribus in corpore calloso, *Digby* auctore in septo cerebri medio, *Vieusseno* in maximo medullæ orbe, *aliis* in colliculis nervorum opticorum, *aliis* in nodo cerebri, *Arantio* in ventriculo cerebri tertio, *Willisio* in corporibus striatis, *Drelincurtio* in cerebello, *Miegio* in spinæ medulla."

"Verum tamen hypophysis ab *Hallero*, *Virideto*, & a me sine ingenii labe læsa est inventa; corpus callosum *Heuermann*, *Zinn*, *Lorry* & *Laghi* sine animi labe læsum invenerunt, verum tamen hæc experimenta, super bestiis capta, parum certi super labe ingenii produnt, *la Peyronie* vero observationes in hominibus instituit. Septum cerebri medium non raro sine animi labe ex cerebri hydrope laborat—Corpora bigemina *Viridet* læsa invenit—Cerebellum plures viderunt vitiatum—Corpora striata *la Peyronie* læsa vidit—Spinæ medullam *ego* sæpius sine ingenii labe in ominibus læsum inveni." Soemmerring de Corp. Hum. Fabrica T. IV. p. 100.

Soemmerring seems inclined to consider the liquor of seat of sensation, volition and reason, which directly influences and is influenced by the immaterial part of man, and which is by many writers named the *Sensorium Commune*, *Seat of the Soul*, and lately by Fourcroy *Sensibilité Centrale*. There are few, if any parts of the encephalon, which have not upon some occasion been injured or diseased without any remarkable diminution of the vital and mental powers. If any one part can with propriety be fixed upon, as entitled to this distinction, it should be a part, which cannot be injured without affecting sensation, volition, &c, and which is found in all animals, that have a visible nervous system. The most constant part of the encephalon, according to M. Cuvier,* is the cerebellum. Haller thinks it not improbable, that the seat of the soul is in the beginning of every nerve and that the conjoined first origins of all the nerves constitute the true sensorium commune. — " Et denique per conjecturam non absurdam, ubi initium est nervi cujuscunque, ut omnium ner-

the ventricles as the seat of the sensorium commune. He says " Peculiare organum sensorii communis si ponere fas sit, vel si propria sedes sensorio communi in cerebro est, haud sine veri quadam specie hoc in humore quæri debet." Ibid p. 69. See also his Dissertation on the Organ of the Soul. *Berol.* 1796. 4.

* Anat. Comparée Tome II. p. 109. & p. 121.

vorum junctæ primæ origines efficiant verum sensorium commune."*

The following case, whilst it, in common with many others, shews the excessive injury, nay almost complete destruction, which the human brain may sustain, consistently with sensation and voluntary motion, provided this be gradually produced, makes strongly in favour of the conjecture of Haller. "—— —— was born with a very large head, but seemed well in health, increased in strength and grew fat. The head soon became so unnaturally large and the features were so much altered, as to leave no doubt concerning the nature of the disease; the child however increased in size, grew strong in his limbs and took food: he could both hear and see well and so continued, until he was 18 months old; he then died suddenly, without any convulsive attack. On opening the cranium, more than five quarts of very limpid water were found within it, there was not the smallest trace of membrane, or brain, except opposite the orbits and meatus auditorios, where something like medulla still remained." This case was communicated to Dr. Quin by an eminent surgeon in Dublin:† It is to be wished, that it had

* Primæ Lineæ Physiologiæ § 372.

† See Quin's Treatise on the Dropsy of the Brain page 104.

been still more circumstantially detailed, but it appears to me allowable to infer from what is actually stated, that those extremities of the nerves, usually named their origins, were in this case at least the seat of sensation, &c.

2. In some animals, as the turtle and frog for example, the brain does not appear to be exclusively the seat of sensation and volition, or the power of producing voluntary motion. For, if the brain be removed, these animals on the application of a stimulus to their limbs, continue to shew indisputably by their movements, that they are possessed of sensation and volition. Here then the spinal marrow, if not the nerves, evidently participate the above powers with the brain and can exert them independently of it.*

3. Some insects and worms, when cut into two, or more pieces, become two or more distinct and perfect individuals, each possessing sensation and voluntary motion. In polypuses the nervous substance is not formed into distinct visible fibres, but distributed equally in every part of the body; and these animals may be divided into an almost infinite number of pieces, each of which becomes a distinct individual, evidently possessing sensation and voluntary motion.† For they perceive the agitation of the water, in which they are placed: They appear

* Cuvier Anat. Comp. T. II. p. 94. † Ibid. p. 95.

sensible to heat and light and are excited to action by these stimuli.* Here then every part of the system has an equal claim to being the seat of sensation and volition.

It is only in the more complicated, or perfect animals, as M. Cuvier very justly observes, that the assemblage of the different parts of the nervous system and especially the presence of its central parts are absolutely necessary for the exercise of the functions of this system.† When we consider what takes place in polypuses, we may be led to conceive, adds this author, "that at the bottom all the parts of the nervous system are homogeneous and susceptible of a certain number of similar functions, nearly as the fragments of a large magnet, when broken, become each a small magnet, having its poles and current, and that it is accessory circumstances only and the complication of functions, which these parts have to discharge in the higher orders of animals, that render their concurrence necessary and occasion each to have a particular destination."‡

* "L' expansion des actinies correspond parfaitement à la sérénité de l' air, le polype à bras s' apperçoit très-bien de la présence de la lumière ; il l' aime & il se dirige constamment vers elle. *Ibid.* p. 362.

† Anatom. Comparée, T. II, p. 94.

‡ Ibid. p. 95.

The wisdom and power of the Creator are, perhaps, no where more strikingly evinced than in the wonderfully diversified structure of the animal kingdom. We find an almost infinite variety in the organs, subservient to each of the numerous functions of the animal body, and yet each function performed in the manner, best adapted to the particular economy of every distinct species.

§ II.

On original Defects in the Nervous System of the Human Species.

Having pointed out generally in the preceding part of this paper the preeminence of man with respect to the proportion, organization, and functions of his nervous system, I shall now proceed to shew, that, great as the proportion of this system is, beautiful and complex as its organization is, and wonderful as its functions are in a perfect human being, his offspring, in consequence of mal-conformation, is upon some occasions unfortunately reduced to a level, with the lowest animals in the simplicity, proportion, &c, of the system under consideration.

The original defects in the nervous system of human fetuses differ considerably in their degrees; all of which I shall enumerate, beginning with the smaller ones and proceeding to the greatest.

1. Fetuses have been born, in which only a part of the brain, of greater or less extent, was wanting. In these instances the upper, or vaulted part of the skull, or at least a considerable portion of the bones, composing it, has been also found wanting. The brain is also sometimes very different from the natural state, in form, colour and consistence as well as in bulk: Dr. Monro, in one case, found the substance, occupying the place of the brain, not more bulky than a small nut and of a red colour throughout, resembling a clot of blood. The proper integuments of the skull are for the most part wanting, the degenerate portion of brain being only protected by a thin pale or reddish membrane: In one fetus, however, of this kind, of which a description and figure are given by Professor Sandifort, the covering of the brain approached more nearly to the nature of the scalp and was thinly covered with hairs.

This kind of monster, which is generally, though not very properly, styled *acephalous*, since the base of the skull and face are not wanting, is by no means an uncommon occurrence. Most accoucheurs in extensive practice have met with one or more. I have brought one into the world which was a twin of rather small size; and it is worthy of remark, that a considerable proportion of these defective fetuses has been twins.† As far as I could determine from an exterior view it was not entirely destitute of brain, nor did it present any other deformity or defect; but I had not an opportunity of examining it by dissection. Many of these monsters, however, have exhibited other kinds of deformity. A tumor, containing a thin watery fluid, has in several instances been found attached to the base of the skull at its fore, or back part— A hare-lip, sometimes with, at other times without a division of the upper jaw and palate, has been observed—The affection of the spine and spinal marrow, termed spina bifida or hydrorachitis, has been occasionally met with—An umbilical hernia of very large size has also been found— Examples of all these complicated deformities may be seen described and admirably delineated in Sandifort's Musæum Anatomicum Tab. 122, 123, 124, 126.

A fetus was lately born in Manchester, in which the brain was not only defective, but

† The very curious acephalous monster, described by Curtius, was a twin. See *Sandifort's Thesaur. Dissert.* &c. Tom. 2. The monsters, described by Drs. Monro and Clarke, to be mentioned more particularly hereafter, were also twins.

misplaced. The parietal bones and the upper part of the frontal bone were not wanting, as is generally the case in acephalous fetuses; but they were very small and pressed down flat upon the base of the skull, so that there was no room for the brain. The superior portion of the occipital bone was wanting and the canal of the spine was incomplete, being open behind from the top of the neck down to the os sacrum. The spinous processes of the vertebræ were not wanting, but appeared as if divided and turned down on each side, so that a kind of spoon-like cavity was formed, which was deepest at the upper part, owing to the bodies of the cervical vertebræ projecting forwards. In the upper part of this cavity the cerebellum was lodged and the cerebrum extended down to the os sacrum. The optic nerves arose from the base of the brain pretty low down; they were consequently much elongated; they were also much slenderer than usual, though the retina was as large and pulpy as is natural. The spinal marrow was divided into two slender cords, which were disposed one on each side of the encephalon and gave off their nerves as they descended. The nerves of the extremities were of the natural magnitude. The brain and spinal marrow had only a membranous covering posteriorly. This fetus was born dead, but not putrid; though born at the full time, it was considerably smaller than the ordinary size. It had a club foot and three of the ribs were united on the right side. The mother had perceived the motions of this fetus in the womb. Mr. Gibson dissected this singular fetus and is in possession of the skeleton.

2. The brain has been found entirely wanting, the spinal marrow being in a perfectly natural state. A remarkable case of this kind is related by Dr. Heysham, an ingenious physician at Carlisle, in his account of the Bills of Mortality of that city for the year 1788, and accompanied by some valuable remarks.

I shall give the whole of the case in his own words.

"At eight o'clock on Monday morning, May 26, 1788, Mary Clarke, aged 26 years and the mother of six children, some of whom are healthy and others unhealthy, was delivered of a living female child, at the expence of the Carlisle Dispensary. The midwife, shocked at the strange and unusual appearance of the child's head, sent for me immediately. I got there about an hour after the delivery and at first sight it appeared evident, that the bones, which form the upper part of the skull, were wanting and that the brain was only covered by its proper membranes, the pia and dura mater, and resembled a large excrescence, which projected a little over the common integuments, especially towards the forehead, where it extended over the root of the nose. The colour of this substance was a dark reddish brown, and upon examining it more particularly I thought I could perceive the division of the two hemispheres of the brain and likewise the division of the cerebrum from the cerebellum. *I gently raised with my fingers a part of it, which projected over the integuments, which made the child cry and produced a considerable starting, similar to what is occasioned by an electric shock.* The child was full grown and seemed in perfect health, her limbs were plump, firm and well proportioned, and *she moved them with apparent agility.* The external organs of sense were also perfect. *She swallowed well and took a sufficient quantity of nourishment for several days, but sometimes during the action of swallowing started a little.* She lived till five o'clock on Sunday morning, June the 1st, when she expired. Some time before her death, she was affected with slight convulsions. During the three or four days, preceding her death, there was a constant discharge of a thin watery fluid, somewhat tinged with blood, from the excrescence, which greatly diminished its bulk, for at her death it was only about half the size of what it had been, when she was born, and the surface was in some places beginning to put on an appearance of mortification."

"A few hours after her death, Dr. Blamire and Mr. Charles Farish accompanied me to the house, where Dr. Blamire very cautiously dissected away from the bones the whole of the substance; when we found the greatest part of the frontal, the temporal and the occ pital, and the whole of the parietal bones wanting. The substance removed was then carefully examined, and what was our astonishment to find it entirely to consist of membranes, blood-vessels, but principally of several bags, one of which was as large as a nutmeg, the rest of different sizes, but much smaller. They were all filled with a brownish coloured fluid; which, when the *cysts* were punctured, gushed out with some violence. *There was not the least appearance of cerebrum, cerebellum, or any medullary substance whatever.* The spinal marrow had a natural appearance, but did not seem to have been connected with the parts above described."

"Having accurately related the facts, as they appeared to Dr. Blamire and myself, which for their singularity deserve to be recorded, I think the few following obvious inferences may be drawn from them. 1. That the fluid, discharged from the excrescence during the life of the infant, and which produced the greatest diminution of its bulk, was occasioned by the rupture, or erosion of *cysts*, similar to those, which remained sound and full of water after death. 2. That *the living principle, the nerves of the trunk and extremities, sensation and motion may exist independent of the brain and that the natural, vital and animal functions may be performed without the brain.* And, as the external organs of sense, viz. the eyes, the nose, the tongue and the ears, all seemed perfect, may we not therefore suppose, that the optic, the olfactory, the gustatory and the auditory nerves may exist independent of and unconnected with either the brain, or the spinal marrow?" P. 8—11.

Dr. Heysham has favoured me with an account of the following additional circumstances, relative to this accephalous infant—The eyes were as full and as lively as in any other child of the same age. The iris evidently contracted on the application of light and from other observations, which he then made, he had no doubt, that her vision was perfect. The child voided both fæces and urine in a regular and natural manner and, for the first three or four days after her birth, seemed in perfect health. No stimulants were applied to her nostrils and he does not know whether she sneezed naturally or not. As the absolute want of brain was not known, till after the child's death, he was less attentive to minute circumstances, than he would otherwise have been.

3. The brain and upper part of the spinal marrow have been wanting, where the lower portion of the latter was entire and nearly of the natural size and conformation; as in the case of the human male monster, of which Dr. Monro has given an interesting account, illustrated with engravings in the Trans. of the Royal Soc. of Edinburgh, Vol. III. p. 216 &c—This child was a twin and born at the full time. It had no head, or neck; it wanted also about one half of the ribs; the larynx, trachea and lungs; the heart; the pharynx, œsophagus and stomach with all the small intestines, except the end of the ilium; the anus; the liver, spleen, pancreas and omenta; the renal glands; terminations of the ureters; the middle part of the urethra; the right testicle; both arms; both patellæ; with several of the bones of the feet and toes"——" The spinal marrow was of a conical shape with the top, or small part of the cone at its upper end, and at its lower end it formed a cauda equina. From its two ends and sides it sent off 18 pairs of nerves; which at their origin and in their progress were nearly as large as they are in a perfect fetus, or where the brain and cerebellum are connected with the spinal marrow."

The Dr. makes the following remarks on the nervous system of this monster. " 1. As the spinal marrow and pairs of nerves, sent off from it, had nearly the usual size and structure, although the brain, cerebellum and medulla oblongata were entirely wanting, we find reason for calling in question the common doctrine of authors, which teaches that the spinal marrow and nerves derive their origin from the brain and cerebellum and are dependent upon it, as much as the ducts of glands are upon the glands, which send liquors into them. 2. Further, as the several parts of this monster were furnished with nerves, and as we have found, that its arteries and veins, by a well-regulated, varied and complicated action, circulated the blood, we must suppose, that their muscular fibres were actuated by those nerves. We therefore find in this monster, not only the existence and common appearance of the spinal marrow and nerves connected with it, although the brain and cerebellum were wanting; but we have proof, that these, independent of the brain and cerebellum, may actuate the muscular fibres in the vessels of an animal, or that nervous energy, or fluid, as it is commonly called, is not derived from the brain and cerebellum solely; that is, we conclude, *that the nerves, as well as the brain and cerebellum, are capable of furnishing nervous energy; and that there is no more reason for believing, that the nerves are derived from the brain, than that the brain is derived from the nerves; or all the parts and* *branches of the nervous system appear to possess the general power, or office of furnishing nervous energy.*" Dr. Monro had previously attempted to establish these points in his *Observations on the Structure and Functions of the Nervous System:* He says, "for the reasons given in last section, I have long thought and endeavoured to prove, that our nerves, independent of the encephalon, possess an energy, or principle of life, which they derive from their proper pia mater and its vessels." p. 35.

4. A fetus has been found on dissection to be destitute of brain, spinal marrow, and optic nerves. This fetus was brought into the world by Mr. Barlow of Bolton, and an account of the case is given in the Medical and Physical Journal for September 1800, pages 189—191; from which I shall beg leave to present the following abstract.

The mother positively asserted that she went two months over her time, and "was not sensible, during pregnancy, of any difference from what she had been formerly accustomed to, either in her own feelings, or *in the motion of the child*, and she had had many children. The birth was marked by no particular occurrence." But this fetus was still born, which the relater of the case supposes "has always been the case, when the brain was wanting."*—" In this child," says he, "the upper part of the cranium is entirely wanting; and there remains only a thin plate of bone, covered with a doubling of membrane, in place of the cervical and the greater part of the dorsal vertebræ. This fold contained no medulla, though it exhibited, on being slit open, some slender fibrils, which might be construed into nerves. I should compare it to the proper coverings of the medulla spinalis, of a thinner texture. Lower down a displaced portion of vertebræ is shewn, which was hollow, but contained no medulla: the rest of the spine consisted of a solid column of bone, without any spinous processes. The child had besides a slight inversion of the feet and a hare-lip on the right side, in other respects it was full grown and the colour of the skin was natural. There did not appear to be any deviation from the common structure and arrangement in the viscera of the thorax and abdomen."—" Though the eyes were outwardly well formed, I could not find by dissection any optic nerve. The nerves in the upper and lower extremities were, nevertheless, perfect."

* " Infantes in lucem eduntur omni fere cerebro atque spinæ medulla destituti, qui haud plantarum ratione tantummodo aluntur atque pinguescunt; sed etiam vociferantur atque sugunt," *Soemmerring*. T. IV. p. 158.

The conclusions, which this writer draws from the case just related, are diametrically opposite to those of Prof. Monro and Dr. Heysham. They are, moreover, so curious as to deserve being noticed here. He says, "In comparing the defective structure of this child with the ascertained uses in others of those parts, of which it was deprived, I have been led to conclude that *nervous influence is not at all necessary to the growth of the fetus in utero*"—And again, "Assuming then that the nerves serve merely to convey the influence of the brain and medulla spinalis, it is obvious, that when deprived of these sources, they can impart none. Thus it is evident, that, although *this fetus had attained the full size and its motions were not perceptibly different from another*, yet, having no sensorium, it could possess no sensation."*

* This writer also endeavours to prove, that *the perfect fetus in its uterine state does not possess sensation*, by stating that sensibility is not only unnecessary during the fetal state, but would expose the fetus to hazards, and by intimating, that sensation is coeval with respiration—I have in a former work satisfactorily shewn, that the fetus in utero most assuredly does possess both the powers of sensation and voluntary motion.

I shall, therefore, confine myself here to the following observations.

1. The proportional bulk of the nervous system is as great and its organization as complete in the fetus, as in the new-born infant.

5. A fetus has occurred, in which not only the brain and spinal marrow were wanting, but in which there was no evident appearance of nerves. A case of monstrosity of this kind is related by Dr. Clarke in the Philosophical Transactions for 1793. P. II. p. 154 &c, and accompanied by two engravings. This monster was included in a distinct set of membranes and had a placenta belonging to it, the side of which was attached to the placenta of the perfect twin. It was covered with the common integuments; was of an oval

2. The communication betwixt the different parts of the nervous system is as free in the fetus in utero as after birth; the brain, spinal marrow and nerves being neither under the influence of pressure, nor of the action of any narcotic or other power, which can diminish the nervous energy.

3. The fetus in utero gives every indication of its possessing sensation and voluntary motion, which a being, so situated, could possibly be expected to give.

4. In whatever manner the respiration of the infant contributes to its power of sensation after birth, in the same manner does the respiration of the mother contribute to the sensibility of the living fetus in utero.

5. If we grant, that the fetus, born at Bolton, from having no sensorium could have no sensation, no argument can be drawn from thence against the power of sensation in a perfect fetus in utero; because a perfect fetus does possess sensation after birth and, agreeably to the above assumption, this defective fetus must, had it been born alive, have remained for ever destitute of sensation.

figure, about four inches in length and three inches in breadth. One edge of it was rather more concave than the other and near its centre there was a slender funis umbilicalis, about $1\frac{1}{2}$ inch long, which had one artery and one vein. There were two imperfect feet and a small projection having the appearance of a finger. Internally this monster was composed of soft and bony matter; the former "appeared of a homogeneous fleshy texture, but without any regular or distinct arrangement of muscular fibres and was very vascular throughout; the bones, which were surrounded by this fleshy substance, were the os innominatum, the os femoris, the tibia, the fibula."—The os innominatum and os femoris were both perfect and as large as those of a fetus at the full period of utero-gestation. "There was not the smallest appearance of head, or vertebræ, or ribs. There was neither brain, spinal marrow, nor nerves. It had no heart, nor lungs." It contained none of the viscera subservient to digestion, except a little portion of small intestine, which had a peritoneal covering and was very vascular. It had no organs of digestion, nor any glandular substance whatever —Amongst other remarks, Dr. Clarke observes "that the deficiency of nerves renders it extremely probable, that their use is very small, if any to the embryo."

6. Dr. Clarke informs us in the same paper, "that in other cases, where the brain has been perfect, the spinal marrow has been deficient in a great part of its extent and sometimes throughout" p. 159: But I do not recollect an instance of this kind and Soemmerring says:—"Non raro spinæ medulla est sine cerebro, *numquam* vero cerebrum sine spinæ medulla est visum, deficiente enim spinæ medulla, cerebrum simul abfuit."*

§ III.

On the influence of original defects in the nervous system of man on sensation and voluntary motion.

From considering merely the laws of sensation and voluntary motion, mentioned above, as obtaining in a perfectly formed human being, a person might be led to conclude *a priori*, that a fetus, having no brain, or neither brain nor spinal marrow, must necessarily be destitute of sensibility and incapable of throwing into action any of the muscles subservient to voluntary motion.

However, when he considers, that the human

* De Corporis Humani Fabrica T. IV. p. 89: where he refers to Huber de *Medulla Spinali.* Götting. 1789. p. 4.

brain has in various instances been very extensively injured and diseased, that it has been nearly, perhaps wholly, destroyed as in the very extraordinary case, copied above from Dr. Quin's Treatise on the Dropsy of the Brain,† without destroying sensation or voluntary motion, some doubts must, I think, arise in his mind as to the justness of such an inference, because he will perceive, that the nervous system of man in its entire and diseased states is governed by different laws.

Again, when he reflects upon the amazing diversity of structure, proportion, disposition &c, observed in the nervous systems of different orders of animals, from man to a zoophyte, and attends carefully to the various tenures, on which sensation and voluntary motion are held by the animal creation; he will find further reason for questioning the propriety of the ground, on which the possession of these two important faculties is denied to a human fetus, unfortunately labouring under the privation specified above: For he will be led to suppose, that its nervous system may be governed by the same laws as that of the inferior animals, which it more nearly resembles than the nervous system of its own species in the natural state.

† See page 489.

When he finds a mother, describing the motions of a fetus in utero, which happens to be still-born and is, after its birth, proved by dissection to have neither brain, nor spinal marrow, as exactly similar to those of her former perfect children, he surely can entertain no doubt, that this fetus, when alive, possessed the same power of voluntary motion: And, as this faculty, as well as sensation, is derived from the nervous system, he will see a strong reason for believing, that this same fetus also possessed sensibility.

Further, when he finds one of these defective beings ushered into the world alive and exhibiting, as far as can be determined, the same powers, which a perfect child of the same age displays, crying when touched rudely, moving its limbs with agility and swallowing food, living more than five days, and then dying with an incipient mortification of the head, he cannot, I apprehend, reasonably withhold his assent to this child's possessing the faculty of sensation as well as that of voluntary motion—Who will contend, that this child could not feel, because it had no sensorium commune? Who does not perceive, that the encephalon and sensorium commune are not exactly synonymous terms and consequently not always to be used indiscriminately in speaking of the animal faculties?

In one of these two cases I have no doubt, that the powers of sensation and voluntary motion were derived from the nerves independently of the brain, and in the other case, independently both of the brain and spinal marrow.

But what shall we say of the monster, described by Dr. Clarke in the Philosophical Transactions? The Dr. is a good anatomist and appears to have dissected this rude mass with great care and attention, yet he could not discover a single nerve. By a nerve, strictly speaking, is understood a continuation of the substances of the brain, or spinal marrow, wrapped in its proper membrane. By nervous matter I understand the substances of the brain, spinal marrow, or nerves, separate from their proper membranes. Now, shall we suppose that in this being no nerves, or nervous matter existed? Or, shall we suppose, that there were nerves, or nervous matter, but distributed in such a manner as to elude Dr. Clarke's observation, and that the limited powers, possessed by this rude animal production during its life, were ascribable to nervous influence?

I am inclined to adopt the latter opinion, for the following reasons.

1. Nervous and muscular fibrils may be detected by magnifying glasses in parts, where they can not otherwise be perceived, and it does not appear, that Dr. Clarke availed himself of any instrument of this kind in his search for nerves.

2. The cineritious and medullary substances are the essential parts of the nervous system, the coats or membranes of the encephalon, spinal marrow and nerves being given them merely for protection and not contributing directly either to sensation, or motion.

3. Nervous matter is distributed to many points, where we cannot demonstrate its presence to the eye and where we can only infer its existence from their sensibility. We cannot prick the skin with the smallest instrument, without exciting pain, whence it is evident, that there is nervous matter in every point of the skin, though we have no other means of proving it.

4. The blood vessels had evidently possessed the power of circulating the blood in this monstrous fetus and, though it was much inferior in size to its fellow-twin, its growth had proceeded to a certain extent, and bone, skin, cellular membrane, ligament, cartilage, intestine, &c had been formed: Now I do not admit any power as capable of giving energy to the muscular fibres of the arteries and veins, except the *vis nervea*, or nervous power.

I am of opinion, that no such power exists in muscular fibres as a *vis insita*, or inherent power, distinct from, or independent of, a nervous power. The experiments and arguments, adduced by

Dr. Monro,* seem to prove fully that the contraction of a muscle, which has been attributed to this supposed vis insita, may be equally well accounted for from the nervous power alone, and that the former would be a superfluous power in the animal economy, the supposed vis insita being excited, or destroyed by the same means as the nervous power. When the brain or spinal marrow is irritated, the muscles or muscular fibres contract, tremble, or are convulsed; when the point of a needle or other sharp body, is pushed into a nerve, distributed to a particular muscle, a contraction of that muscle ensues; in which cases the muscular fibres act confessedly by virtue of their nervous power. When a needle is pushed into the fibres of the muscle itself, a contraction also takes place, and in this case it has been supposed by the celebrated Haller and others, that the fibres contract by virtue of their vis insita: But the only difference appears to consist in this; that in the former experiments the stimulus acts upon the brain, spinal marrow or the trunk, or an evident branch, of a nerve, whilst in the latter it acts upon one, or more of those nervous filaments, or portions of nervous matter, which, though not always demonstrable to the sight, we have every reason

* Observations on the Nervous System, p. 91—94.

to believe are constantly and invariably distributed to every muscular fibre.—It has been urged, as an argument in favour of a vis insita, that muscles, or muscular fibres, will contract for a considerable time, after their separation from the body, on the application of a stimulus.* But this fact may be perfectly explained by the admission of a nervous power only; for why cannot the nerves, which evidently possess an energy independently of the brain, retain this energy after the removal of muscles from the body, as long as the muscular fibres can retain their supposed vis insita?

From what has been stated in the preceding part of this paper, the following conclusions amongst others may, I conceive, be very fairly drawn.

1. That every perfect animal, from man to the polypus, possesses the powers of sensation and voluntary motion.

2. That infants, though born destitute of brain, or even of brain and spinal marrow, possess these two important faculties.

3. That the fetus in utero is neither destitute of sensation, nor voluntary motion.

* In warm-blooded animals this takes place for an hour or more; in cold-blooded ones more than a day afterwards.

4. That the power of action in the arteries and veins, by which the circulation of the blood and the formation of the different parts are effected in the most defective human monsters, is derived from a nervous energy, independently of brain, spinal marrow, or even evident and distinct nerves.

Experiments and Observations on the HEAT *and* COLD *produced by the* MECHANICAL CONDENSATION *and* RAREFACTION *of* AIR.

BY JOHN DALTON.

READ JUNE 27, 1800.

If a thermometer be inclosed in a receiver and the air suddenly condensed, the thermometer rises a few degrees above the temperature of the atmosphere; and if the air be exhausted from a receiver inclosing a thermometer, the mercury sinks a few degrees immediately; but in both cases after some time it resumes its former station. These facts are well known to philosophers of the present age, but they do not all agree in the explanation of them. Thinking the subject worthy of elucidation, I was induced to institute a series of experiments for the purpose; which I apprehend have led to a clear demonstration of the cause of the phenomena, and moreover make the facts themselves appear in a somewhat different point of view from what they are seen in at the first moment.

One circumstance is very remarkable, that whether the mercury rises or falls in these instances, it is done *very rapidly;* whereas in the open air, if a thermometer be only two or three degrees above or below the temperature, it moves very slowly. This seems to have suggested to every one the idea that the elasticity of the glass bulb of the thermometer has a principal share in producing the effect, by causing the bulb to yield a little to the pressure of the air. It has however been found upon trial that the same effects take place whether the thermometer is sealed or not. My experiments accord with this, having made a thermometer and left it unsealed for the express purpose; in all the experiments with condensed and rarefied air, there was no sensible difference observed to arise from the inequality of pressure on the external and internal surfaces of the bulbs, the sealed and open thermometers varying the same in kind and also in degree, except from circumstances to be noticed hereafter.

It being certain then that a real change of temperature takes place, it remained to determine the quantity and manner of that change. Having chosen a small and consequently sensible thermometer, with a scale of degrees sufficiently large to admit of distinguishing one tenth of a degree, I proceeded to ascertain several facts experimentally.

EXPERIMENT 1.

Took a receiver, the capacity of which was about 120 cubic inches, and suspended the thermometer with its clear bulb in the central part of it; then letting the whole acquire the temperature of the room, which was without a fire, I exhausted the air and afterwards restored it, marking the effects upon the thermometer. The medium of several trials nearly agreeing with each other was as under:

The Thermometer

———in the air of the room stood at....36°.8
———sunk upon exhaustion to ....34 .7
———rose when the air was restored to..38 .9

The *suddenness* of the fall and rise puzzled me most: after reflecting upon it for some time, I conjectured that the real change of temperature of the air or medium was much greater than the thermometer indicated, but that the inequality existed only for a few seconds of time, because the receiver, &c. immediately impart heat to or abstract it from so small a quantity of air as 120 cubic inches, which are only equal to 40 grains in weight.—The phenomena of the thermometer seemed very well to accord with the supposition of *great heat or cold* acting upon it for a few seconds only.

EXPERIMENT 2.

Pursuing this idea I imagined that if two thermometers whose bulbs were very unequal in magnitude were inclosed together, the smaller bulb ought to give the greater variation: accordingly I inclosed two, the diameters of their bulbs being .35 and .65 of an inch respectively; and having exhausted the air and restored it again repeatedly in succession, and found a mean of the variations, that of the small bulb was 2°. 8, and that of the large, 2°. 2.

EXPERIMENT 3.

Repeated the exhaustion with the small thermometer inclosed in three different circumstances successively; 1st with the bulb in the centre of the receiver; 2d with the bulb resting on the wet leather of the plate; and 3d with the bulb resting against the side of the receiver.

1st Case—Reduced by exhaustion	2°.45
2d Case—	2 .15
3d Case—	1 .2
1st Case—Raised by restoring the air	4 .05
2d Case—	2 .25
3d Case—	2 .8

EXPERIMENT 4.

Inclosed a wine glass with about a cubic inch of water in it, containing the bulb of a thermometer, in a receiver; and, exhausting the air, the thermometer sunk half a degree suddenly, and then continued stationary; upon restoring the air it suddenly rose half a degree.

All these experiments confirmed my conjecture of a much greater degree of heat and cold being produced in these cases than the thermometer points out, but that its continuance is so short as not to effect a material change in the temperature of the mercury. The following experiments were made to ascertain what may be the *real* degree of heat and cold generated in those operations.

EXPERIMENT 5.

The same receiver & small thermometer as above being used, I found the exhaustion was effected by working the pump *one minute.* The thermometer sunk nearly 2° in the first half minute, and the remainder, a few tenths of a degree, in the latter half minute. The operation being stopped, and things remaining in the same state, it required some minutes of time before the thermometer recovered *one* degree of the heat lost. Upon opening the cock, the receiver filled with air in five seconds, and the greatest volocity of the rising mercury was about the end of that time. The rising continued for 30 or 40 seconds from its commencement; but $\frac{3}{4}$ of the effect were produced in the first 10 seconds. The greatest velocity of the rising mercury is 1° in $3\frac{1}{2}$ seconds. After the thermometer had attained its utmost height, it began to fall again at the rate of $\frac{1}{10}$ of a degree in a minute.

EXPERIMENT 6.

Took the same thermometer and heated it to 50° above the temperature of the air, then let it be cooled by the medium of air, and it began to fall at the rate of 1° in $3\frac{1}{2}$ seconds.

The two last experiments seem to prove that when air is let in to the receiver in the ordinary way, *an increase of temperature of* 50° *is produced in the medium within the receiver for* $3\frac{1}{2}$ *seconds.* This high temperature is reduced *in a few seconds* by the receiver and surrounding bodies, to their own temperature.

EXPERIMENT 7.

On condensed Air.

Took a large spherical glass receiver, the capacity of which was something more than twice that of the former (above one gallon), and suspended a thermometer in the centre of it, of a larger bulb than that before used; the receiver had a brass cap and stop-cock adapted to it: Then doubled the density of the air within it by a condenser. The thermometer rose 2° or more. Let out the air suddenly and the thermometer immediately sunk each time from 3° to 3°. 5; at the same time an exceedingly dense mist was produced in the receiver, which soon subsided.

Suspecting that *aqueous vapour*, which always exists in the atmosphere, and is liable to assume the liquid or aërial form according to circumstances, might be the principal agent in the production of heat and cold by condensation and rarefaction, I thought that an increase of it might produce a greater effect, and that cold air, which contains less vapour, might have a less effect. The reverse however was the fact, as appears by the following,

EXPERIMENTS 8 & 9.

In a cold morning last winter when the air was clear and the thermometer without stood at 20°, I took the receiver and condenser into the open air, and let them stand for 15 minutes to acquire its temperature; then repeatedly condensed the air to a double density, and suddenly liberated it again. On a medium of 5 trials the mercury fell 3°.3 on opening the cock.—The vapour precipitated was whiter than usual and not nearly so dense.

Again, took the receiver and condenser into a dyer's stove where the temperature was about 100°, and the air abounded with vapour in a transparent state: after some time, condensed the air and liberated it as before, when on a medium of 5 trials the mercury sunk only 3°, and a very copious mist was precipitated, so dense that one could but just distinguish the degree of the thermometer through it.

These experiments shew that the greater the quantity of vapour condensed the less is the change of temperature; and that consequently, if air was entirely free from vapour, the change of temperature would be a *maximum*. Indeed this is clearly consistent with the known law, that when vapour is condensed, heat is given out. Any process to cool the air must be retarded by the condensation of part of the vapour it contains. Suppose for instance that a portion of the atmosphere contained $\frac{1}{60}$ of its weight of aqueous vapour, and that $\frac{3}{5}$ of this vapour were condensed by 50° of cold; that is, $\frac{1}{100}$ of the whole elastic mass was converted into water; then the heat given out would be sufficient to raise the temperature of the remaining mass of air and vapour 6 or 8°, which sufficiently accounts for the small difference observed in the results upon warm vapoury air and cold dry air. Hence vapour, far from producing the change of temperature in question, tends to diminish the effect.

If any doubt remained with me respecting the *real* change of temperature that takes place in the operations related above, it was completely removed by the results of the two following experiments.

EXPERIMENT 10.

Inclosed a small graduated glass tube of $\frac{1}{15}$ of an inch internal diameter, & 10 inches long, with a short column of mercury in it, in the large receiver; the tube was sealed at one end and open at the other, so that a portion of air of given capacity was confined by the mercurial column, which was near the open end of the tube, and subject to rise or fall by any variation of elasticity of the air on either side, being a proper manometer: then doubled the density of the air in the receiver, and opening the stop-cock, the mercurial column soon ran up to its former station, but instantly turning the cock again, the mercurial column returned or fell down gradually for 5 or 10 seconds, to the amount of nearly $\frac{1}{10}$ of the whole aërial column, and then became stationary. Again opening the cock, a quantity of air rushed out, and the mercury resumed its original station. These effects were always the same, on a repetition of the experiment.

EXPERIMENT 11.

Let the mercurial column of the manometer down by a wire to $\frac{1}{4}$ of the length of the tube from the sealed end; then exhausted $\frac{3}{4}$ of the air from the receiver, which was seen by the mercury rising to the top of the tube; and upon opening the cock the mercury fell to its former station, but then suddenly turning the cock, the mercury gradually rose for the space of 5 or 10 seconds to *more* than $\frac{1}{10}$ of its original height above the stationary point, and remained there till the cock was opened; after which it resumed its proper station.

The phenomena in the two last experiments can be explained only on the following principle:—The air in the receiver and in the manometer is subject to a like degree of rarefaction and condensation in those experiments, or very nearly so. When the equilibrium of heat in the air is disturbed by the operations of condensation and rarefaction, it is restored in the manometer *instantly* by reason of the contiguity of the glass to the air; but in the large receiver it requires a sensible time of 10 seconds or more to restore the equilibrium throughout the whole internal capacity. It is this restoration that increases or diminishes the elasticity of the air confined in the receiver, and thereby causes the retrogradation of the mercurial column. Now I have found by former experiments that a change of 50° in temperature effects a change of $\frac{1}{10}$ nearly, in the capacity or bulk of air. It follows therefore that in the case of restoring the equilibrium in condensed air, about 50° of cold is produced; and in letting in air to an exhausted receiver something more than 50° of heat is produced. The small difference seems to arise from this, that the condensation of vapour in the former case *diminishes* the effect, and in the latter, if any there be, *increases* the effect, that would arise from operating upon purely dry air.

The experiments and observations hitherto related go principally to ascertain facts without any reference to the theory of them: This however may be given in a few words, and is the same that is ascribed to Mr. Lambert by Messrs. Saussure and Pictet and by them adopted. He conceives that a vacuum has its proper capacity for heat, the same as air, or any other substance; and that the capacity of a vacuum for heat is *less* than that of an equal volume of atmospherical air; also that the *denser* air is, the *less* is its capacity for heat: upon these principles the phenomena are easily referable to that class of chemical facts where heat and cold are generated by the mixture of two different bodies.—If this theory be right, and I think there is little doubt of it, we may hence be led into a train of experiments, by which the absolute capacity of a vacuum for heat may be determined; and likewise the capacities of the different gases for heat, by a method wholly new:—but this must be left to future investigation.

ACCOUNT

of some

ANTIQUES,

LATELY FOUND IN THE RIVER RIBBLE, &c.

BY MR. THOMAS BARRIT.

READ SEPTEMBER 26, 1800.

The articles here exhibited for the amusement of the society are principally a few different specimens of antiques, generally denominated Celts, lately found in the river Ribble in this county.

As the particular uses of these celts have been very learnedly discussed and still left undetermined, little more can be advanced upon the subject than to quote the remarks our best antiquaries have made. Mr. Pegge (Archæologia, page 85, vol. 9,) says, these brazen instruments seem at present undetermined, it not being yet ascertained, whether they were for military purposes, or for civil and domestic employments; and, after all that has been advanced by writers upon the subject, it remains an undecided point. I shall not therefore attempt to resume the consideration of this question, but may venture to embrace the opportunity of making a few observations concerning them.

These celts are always of brass, let their form be ever so various, always with a thick crust of *aerugo* upon them. They have been found in many different places in England, Wales and Ireland; at Herculaneum and other places upon the Continent; and are supposed to have belonged to the Celtæ, or first inhabitants of this island, from whom the name is given by antiquaries to these instruments: in France they are called Gallic hatchets.

They have been supposed older than the invention of iron, which perhaps may be the case from whence they came, and in Britain where they were brought; but a conclusion from this cannot be drawn that iron was unknown in other places.

Mr. Lort, in his observations upon celts, says they were too awkward to have been invented and fashioned by the Romans, and at the same time that they were too correct and shapely to have been the work of the Britons before the invasion of Julius Cæsar. All authors however have agreed to allow them to be of high antiquity: as to my opinion I am inclined to think them the greatest antiquties this island can boast of, and that they surpass all others in point of age. Their resemblance is not found upon any

Greek or Roman coin; neither are they to be found amongst those models of armour and weapons upon the Trajan or Antonine pillars at Rome; nor do any of the writers upon the Roman military art mention or describe any offensive weapons of this sort; and therefore when any have been found in undoubted Roman stations, and accompanied even with Roman coins, &c. we are obliged to suppose either that they came thither by chance as the spoils of some British or Celtic enemy, or that they were the arms or tools of barbarian auxiliaries. Sir James Ware observes, "It is past controversy the arms of the ancient Irish were made of brass, and likewise those of the ancient Greeks, Germans and Britains." Some again have thought, and with great probability, that they were introduced into this island by the Phœnician merchants of Tyre and Sidon, who had in return British tin. Whoever were the people that used them, I am inclined to believe that some of the tribes of the Indians in North America were their descendants, from the agreement observable between the ancient Celt and the modern Tomahawk, both in size and shape; and this last is used both for a weapon and for domestic purposes.

Nearly all these celts have or have had loops at the sides, beyond all doubt to tie them to a handle, yet I do not find that any writer upon the subject has had any idea of the handles being other than straight ones; which leads one to conclude the common name of battle ax to be very improperly applied, and that of pike, pilum or javelin to be more consistent. One of these, No. 6, I make no doubt was of this kind, although now without a considerable portion of its length. No. 2 is in the form of an ax, and with Nos. 1, 3, 4, 9, whose edges have the appearance of chizels, may all have been used as axes with very great propriety, by being fitted with handles bent at the end, and thereby would become formidable weapons either against man or beast; or being mounted upon a straight shaft, might be used for pushing forward or piercing the earth, as an hoeing tool, and so capable of serving the two-fold purpose of peace or war; and with the loops at the sides might be suspended by a thong over the shoulder or round the waist when not wanted; and, upon occasion, it was an easy matter for inhabitants of the woods, as the ancient Britons were, to break from a tree a bough with an acute angle, and immediately to accommodate himself with a utensil for his present purpose.

It seems a little strange and what I have often wondered at, that these celts are so often called battle axes, and none of our antiquaries have mentioned the idea of a crooked handle which certainly gives them a much greater importance as military weapons. It is not at all unlikely but that a certain sort of these celts might have been used by the Druids of this country, for cutting down the mizeltoe from trees, and chopping it to pieces for their mystical purposes. Medea (in one of Seneca's plays) is represented cutting roots and herbs for her inchanted chaldron with a brazen knife. Pliny says, the mizeltoe was cut down with a golden sickle, which words I apprehend being taken too literally, have been the means of leading more than one learned antiquary some years back into a mistake, causing them to introduce into engravings the representation of a Druid with a sickle in his hand, of the shape used by the farmers of the present day; but as the Druid's sickle or bill has never been described, and no weapon of the form of our present sickle, fabricated of the metal now denominated gold, has ever yet been found, we have reason to suspect the truth of the relation as to the metal, and ought rather to credit the celt to have been the sickle of the Druid, which was made of brass or copper, and perhaps from the high price, scarcity and colour, might have been esteemed precious and valuable as gold.

No. 10 was found in the summer of 1799, near Leigh in this county, and is evidently the head of a spear. The loops on each side would almost lead one to suppose it was of celtic origin; but I am inclined to think it was the head of a Roman standard, to which the silken or linen *labarum* was affixed, which was suspended from the top of a spear by means of a small yard, like the sail of a ship. This conjecture will be strongly corroborated by attending to Roman coins of the later Emperors, whereon is seen a standard of the above description, as on some of the coins of Constantines, Valens and other Emperors; these standards or colours were introduced after the metal eagle, the boar, the hand, the head of the Emperor, &c. were laid aside.

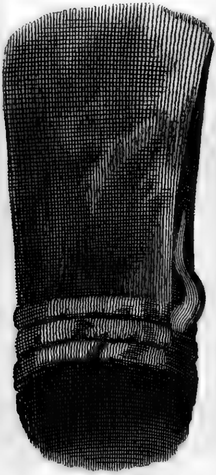
No. 11 is a lump of sal ammoniac found at the entrance of Gaythorn Row, the top of Deansgate, near Castlefield, Manchester, in the year 1788, with a Roman coin of Tetricus, who ruled in Britain under the Emperor Aurelian.* What this salt might have been used for by the ancients, if it had any use, or indeed whether it may not be a natural production, is uncertain; many fragments of unglazed pots, and one in tolerable preservation with two handles, were turned up at the same time. Perhaps upon or very near this place was the pottery to the Roman station near adjoining. Verstigan in his "Restitution of

* About the year 274.

decayed Intelligence in Antiquities," when mentioning the different fossils, shells, bones, &c. found in Britain says: "Moreover potters in "working their clay, which is gotten in some "espetial places, do fynd in it certain things "which are as hard as stone, and of the very "forme and shape of the toungs of some sortes "of fishes, each with the root unto it, to make "it the very markable and right proportion of "such a kynd of toung in all respects, some "being more than two inches long, and some "lesse than one inche, and they that thus find "them do not otherwise call them but the toungs "of fishes, which being so, and turned into very "hard stone is a strange thing in nature." Whether this same article be of the sort Verstigan alludes to, cannot at this day be determined; however, from the shape and its being found in the scite of an ancient pottery, it may be supposed to be one of those he describes.

No. 12 is a ring of brass found in Castlefield in 1796, with a bluish sort of a bead of pot ware upon it and ribbed, each rib terminating at the hole through which the ring passes; another of the same sort, supposed to belong to the other, was so broken as not to be worth gathering. I am inclined to think this ring was the bracelet of a British or Roman lady, and the beads upon it the amulet or charm to protect the wearer from misfortunes or injuries, and to procure the favour of lovers and superiors. This idea I think to be the origin of precious stones being afterwards set in rings. Several authors as late as the fifteenth century wrote upon the virtues of stones, and ascribed to them qualities which at this day every one is not inclined to believe they possess.

1



2

Antiquities

in the possession of Mr. T. Barrett of Manchester.
drawn half the size of the originals.

Eight of these Celts were found in the
River Ribble 1800.

9

10

Drawn by T Barrett Novr. 1801.


1
2
3
4
5
6
7
8
Antiquities
in possession of M.r T. Barrett of Manchester
9
12
10
11
A Roman Inscription
found in Castle field Manchester
in the possession of C. White Esq.r F.R.S
COH I
The size 13 In.s by 11 found 1790

EXPERIMENTAL ESSAYS

On the Constitution of mixed GASES; *on the Force of* STEAM *or* VAPOUR *from Water and other Liquids in different temperatures, both in a Torricellian Vacuum and in Air; on* EVAPORATION; *and on the Expansion of* GASES *by Heat.*

BY JOHN DALTON.

READ oct. 2, 16 AND 30, 1801.

THE progress of philosophical knowledge is advanced by the discovery of new and important facts; but much more when those facts lead to the establishment of *general* laws. It is of importance to understand that the descent of falling bodies is the same every where on the surface of the earth; but from that and some other particular facts to infer the law of gravitation, or that all matter attracts with a force decreasing as the square of the distance, is a much higher attainment in science. In the train of experiments lately engaging my attention some new facts have been ascertained, which with others, seem to authorise the deduction of general laws, and such as will have influence in various departments of natural philosophy and chemistry.

As the detail of experiments will be best understood and their application seen, if the laws of principles alluded to be kept in view, it may be proper here to state them; though it must not be understood that they were proceeded upon hypothetically in the direction of those experiments. On the contrary, the first law, which is as a mirror in which all the experiments are best viewed, was *last* detected, and after all the particular facts had been previously ascertained.

1. When two elastic fluids, denoted by *A* and *B*, are mixed together, there is no mutual repulsion amongst their particles; that is, the particles of *A* do not repel those of *B*, as they do one another. Consequently, the pressure or whole weight upon any one particle arises solely from those of its own kind.

2. The force of steam from all liquids is the same, at equal distances above or below the several temperatures at which they boil in the open air: and that force is the same under any pressure of an other elastic fluid as it is in vacuo. Thus, the force of *aqueous* vapour of 212° is equal to 30 inches of mercury; at 30° below, or

182°, it is of half that force; and at 40° above, or 252°, it is of double the force; so likewise the vapour from sulphuric ether which boils at 102°, then supporting 30 inches of mercury, at 30° below that temperature it has half the force, and at 40° above it, double the force: and so in other liquids. Moreover, the force of aqueous vapour of 60° is nearly equal to ½ inch of mercury, when admitted into a torricellian vacuum; and water of the same temperature, confined with perfectly dry air, increases the elasticity to just the same amount.

3. The quantity of any liquid evaporated in the open air is directly as the force of steam from such liquid at its temperature, all other circumstances being the same.

4. All elastic fluids expand the same quantity by heat: and this expansion is very nearly in the same equable way as that of mercury; at least from 32° to 212°.—It seems probable the expansion of each particle of the same fluid, or its sphere of influence, is directly as the quantity of heat combined with it; and consequently the expansion of the fluid as the cube of the temperature, reckoned from the point of total privation.

Having now stated the chief principles which seem to be established from the following series of facts and observations, I shall proceed to treat of them under the several heads.

ESSAY I.

On the Constitution of mixed Gases: and particularly of the Atmosphere.

Ever since the discovery of the atmosphere consisting of two distinguishable elastic fluids of different specific gravities, it has been a subject of insurmountable difficulty to explain clearly the mode of their combination. Two opinions have been given respecting it: the one supposes that the two fluids are merely mixed together, without any chemical combination; but assigns no reason why they do not separate and the heaviest take the lowest place. The other supposes a true chemical union to exist between the two, and thus obviates the difficulty arising from the consideration of specific gravity; but this produces others of no less magnitude. Why does no change of bulk, of temperature, or of any of their distinct properties take place, which is usual on all other chemical combinations? Why do not oxygenous and azotic gases taken in due proportion, and mixed, constitute nitric acid gas, another elastic fluid, totally distinct in its properties from either of the ingredients? To these questions, and many others that might be proposed, no satisfactory answer has ever been given. Indeed this hypothesis is much the most untenable of the two; the notion of chemical affinity connecting elastic particles mutually *repelling* each other is plainly an absurdity; and if we suppose the particles to have no repulsion to each other, but instead of it to coalesce, then it is impossible to conceive why they do not form nitric acid gas, and why a change of bulk, or temperature, or both does not occur. All these difficulties are entirely removed by the subsequent theory, for the understanding of which it will be needful to premise certain propositions.

Prop. 1. The density of elastic fluids is exactly as the compressing force, all other circumstances alike.

This is a physical proposition and depends upon experiment for proof. All experiments agree that if a quantity of air be pressed with two, three, &c. times the force of the atmosphere, it will occupy $\frac{1}{2}$, $\frac{1}{3}$, &c. of the space occupied before; and when the pressure of the atmosphere is taken off it expands accordingly. This I find is not *strictly* true with regard to atmospheric air; which, when condensed with a double force, occupies a space something *less* than what the above ratio assigns; because aqueous vapour, one of the elements of atmospheric air, loses its form, or becomes partly converted into water by pressure. If air confined by sulphuric acid be tried, it accords very exactly with the above law. If air confined by water be tried, the condensed air is always something *less* and the rarefied air something *more* than what the theory assigns, which is entirely owing to the destruction or formation of a quantity of aqueous vapour. My method of experimenting is very simple; it consists in condensing or rarefying the air by a column of mercury in a long straight tube, divided into equal portions; the tube must be $\frac{1}{12}$ or $\frac{1}{15}$ of an inch internal diameter, and then it may be inverted without losing its contents, if the mercurial column be less than 30 inches, when the rarefaction of air is the object.

Prop. 2. Homogeneous elastic fluids are constituted of particles that repel one another with a force decreasing directly as the distance of their centres from each other.

This proposition is a mathematical one, and its demonstration founded upon the fact of the density being as the pressure. The proof may be seen in the Principia, B. 2. Prop. 23. It follows too that the distances of the centres of the particles, or which is the same thing, the diameters of the spheres of influence of each particle, are inversely as the cube root of the density of the fluid.

The proposition applies to *homogeneous* elastic fluids only; how far it may apply to mixed fluids remains to be considered.—With regard to the constitution of such there may be several hypotheses; some of which we shall now consider.

1. *The particles of one elastic fluid may repel those of another with the same force as they repel those of their own kind.*

In this case, if m measures of A were mixed with n measures of B, in the pneumatic apparatus, and under the atmospheric pressure of 30 inches of mercury, the two would occupy $m+n$ measures of space. If they were of the same specific gravity, they would remain in the situation they were left, of intimate mixture or of separation, as it happened. If they were of different specific gravities, the lightest would rise to the top of the vessel:—The pressure on each particle of the mixture would be equal to 30 inches of mercury.

Now with regard to the application of these principles; as we know of no two elastic fluids, which when mixed, obey the laws of their specific gravities; this hypothesis is inconsistent with the phenomena.

2. *Particles of one elastic fluid may repel those of another with forces greater or less than what they exert upon their own kind.*

Here again *m* measures of *A*, with *n* measures of *B* would occupy $m+n$ measures, and the pressure on each particle of the mixture be the same, and equal to 30 inches of mercury. But the fluids in this case could not remain mixed or diffused intimately through each other; that fluid of the greatest specific gravity must take the lowest place.—This therefore is equally inconsistent with the known phenomena, and must be rejected also.

3. *The particles of one elastic fluid may have a chemical affinity or attraction for those of another.*

Here if *m* measures of *A* and *n* measures of *B* were mixed, a union of particles ensues, and the new compound may assume the solid, liquid or aëriform state according to its nature. If the compound be of the solid or liquid form, the two elastic fluids may wholly disappear; if it be aëriform, then a diminution of bulk, an increase of temperature and of specific gravity may be expected—Several facts in chemistry accord with this hypothesis.—When muriatic acid gas and ammoniacal gas are mixed together in due proportion, a solid substance, muriate of ammonia, is formed, and the gases wholly disappear. When ammoniacal gas and aqueous vapour are mixed, the two unite and a portion of the compound becomes liquid. When nitrous gas and oxygenous gas are mixed in due proportion, the two unite and form a new elastic compound of greater specific gravity and consequently of less bulk, nitric acid gas.—But there are other cases of mixtures of elastic fluids, some of which have been mentioned, where no signs of chemical affinity are discoverable; in regard to such this hypothesis fails equally with the other two. We must therefore have recourse to another.

4. *The particles of one elastic fluid may possess no repulsive or attractive power, or be perfectly inelastic with regard to the particles of another: and consequently the mutual action of the fluids be subject to the laws of inelastic bodies.*

According to this hypothesis if m measures of A be mixed with n measures of B, the two will occupy $m+n$ measures of space. The particles of *A* meeting with no repulsion from those of *B* further than that repulsion which as obstacles in the way they may exert, would instantly recede from each other as far as possible in their circumstances, and consequently arrange themselves just the same as in a void space; their density, considered abstractedly, becoming $\frac{m}{m+n}$, (that of the compound being supposed unity). In like manner the particles of *B* must recede from each other, till they become of the density $\frac{n}{m+n}$; thus the two gases become rarefied to such degree that their united forces only amount to the pressure of the atmosphere.—Here the particles of one fluid not pressing at all upon those of the other, the consideration of specific gravity does not enter. That part of the atmospheric pressure which the fluid *A* sustains, will be $\frac{m}{m+n}$; and the remainder, $\frac{n}{m+n}$, is the part that the fluid *B* sustains. The weight or pressure upon any one particle of any fluid mixture of this sort will arise solely from the particles of its own kind.

It is scarcely necessary, I think, to insist upon the application of this hypothesis to the solution of all our difficulties respecting the constitution of mixed gases where no chemical union ensues.

That moment we admit it every difficulty vanishes, and every fact appears a simple and immediate consequence of it. The atmosphere, or to speak more properly the compound of atmospheres, may exist together in the most intimate mixture, without any regard to their specific gravities, and without any pressure upon one another. Oxygenous gas, azotic gas, hydrogenous gas, carbonic acid gas, aqueous vapour, and probably several other elastic fluids may exist in company under any pressure and in any temperature, whilst each of them, however paradoxical it may appear, occupies the whole space allotted for them all. For, the space with them all in it, is little more comparatively than a vacuum; such is the great tenuity of all elastic fluids.

I shall now proceed to make a few observations on that collected mas of elastic fluids constituting our atmosphere, the principal of which are the *azotic* atmosphere, the *oxygenous* atmosphere, the *aqueous vapour* atmosphere, the *carbonic acid* atmosphere and the *hydrogenous* atmosphere.

Before the modern discoveries in chemistry, the atmosphere was considered as *one* simple elastic fluid, *sui generis*, containing in it, by some means or other, certain foreign substances not essentially but accidentally mixed with it. Lavoisier taught us there were *two* essentially distinguishable fluids to be found in it, and certain other substances accidentally or chemically combined with them; it now appears there are at least *four* distinct elastic fluids found in every portion of atmospheric air subject to examination. And these, for aught that appears, are totally independent one of another; so much that if any one of them was wholly withdrawn from the surface of the earth, the rest would not at all be affected by the circumstance, either in their density or situation; or if an atmosphere of another kind were added to them, they would still retain their respective stations and densities, provided that added had no chemical affinity for any one of them in the common temperature.

The *azotic* atmosphere is by far the largest and densest of them all: it supports the mercury in the barometer at a medium nearly 21. 2 inches: it is the same in quantity all over the surface of the earth; because not condensible into a liquid form at any temperature found there.

The *oxygenous* atmosphere is the next in quantity; its pressure on the surface of the earth amounts to about 7. 8 inches at a medium; it is the same nearly in quantity every where, because it preserves its elasticity in all observable temperatures.

The *aqueous vapour* atmosphere is variable in quantity according to temperature; in the torrid zone its pressure on the surface of the earth is equal to the force of ,6 and from that to one inch of mercury. In these parts it rarely amounts to a pressuse of ,6, but I have frequently observed it above half an inch in summer; in winter it is sometimes so low as to be of no more force than ,1 of an inch of mercury, or even half a tenth, in this latitude, and consequently much less where the cold is more severe.* This want of equilibrium in the aqueous vapour atmosphere is a principal cause of that constant inundation of it into the temperate and frigid zones, where it be comes in part condensed in its progress by the cold, like the vapour of distillation in the worm of a refrigeratory, and supplies the earth with rain and dew.

The *carbonic acid* atmosphere has not perhaps been accurately ascertained in quantity; it is found every where in a small proportion, not being condensible into a liquid by the usual degree of cold; its pressure may probably amount to half an inch of mercury.

* The means of ascertaining its quantity or pressure will be given hereafter.

The *hydrogenous* atmosphere is so small in quantity as scarcely to be at all appreciable; yet, as various processes on the surface of the earth disengage this gas, and as it mixes with all the other gases constituting the atmosphere without combining with any, or rising above them, we ought to find a proper *hydrogenous* atmosphere. Perhaps we have got no tests for ascertaining very small quantities of it.

Lavoisier describes the atmosphere to be "a compound of all the fluids which are sus- "ceptible of the vaporous or permanently elas- "tic state in the usual temperature, and *under* "*the common pressure.*" This last limitation should be omitted; he seems moreover to conceive that atmospheric pressure is the cause why water retains its liquid form at the common temperature: this notion is certainly wrong; were every atmosphere, except that of aqueous vapour, instantly annihilated, little addition would be made to the aqueous atmosphere, because it already exists in every place, almost entirely up to what the temperature will admit; the evaporation of water would be essentially the same in that case as it is at present; only the full effect would take place in less time. In short this notion of pressure preventing the evaporation of liquids, which seems to have been taken as an axiom by modern philosophers, has been the cause of more error and perplexity perhaps than any other ungrounded opinion.

Lavoisier thought that in the higher regions of the atmosphere a stratum of inflammable fluid exists in which the aurora borealis and other fiery appearances are produced; this opinion is plausible enough; but that fluid cannot be hydrogenous gas, because its particles are not repulsive of those of the other atmospheric gases, as appears by its intimate and almost instantaneous diffusion amongst them.

There may be gases in the higher regions of which we have not the principles below, the whole stock of matter being spent in their formation; and being constituted of particles repulsive of those of the atmosphere, according to the first or second hypothesis, and of less specific gravity than the other gases, they must float upon the surface of the common atmosphere, and consequently for ever elude the investigation of philosophy. He observes that even *metallic* substances may be found within the regions of the atmosphere: I have myself shewn, in my Meteorological Essays, page 180, that a fluid possessing *magnetic* properties constantly holds a place in the higher regions of the atmosphere, and which therefore we cannot help considering of a *ferruginous* quality; but it will probably ever be beyond the reach of philosophical research to ascertain the nature of so subtile and distant a fluid.

ESSAY II.

On the Force of Steam or Vapour from Water and various other Liquids, both in a Vacuum and in Air.

SECTION I.

On Vapour in Vacuo.

The term *steam* or *vapour* is equally applied to those elastic fluids which, by cold and pressure of certain known degrees, are reduced wholly or in part into a liquid state. Such are the elastic fluids arising from water, alkohol, ether, ammonia, mercury, &c. Other elastic fluids that cannot be reduced, or rather that have not yet been reduced, into a liquid state by the united agency of those two powers, are commonly denominated *gases*. There can scarcely be a doubt entertained respecting the reducibility of of all elastic fluids of whatever kind into liquids; and we ought not to despair of effecting it in low temperatures and by strong pressure exerted upon the unmixed gases. However unessential the distinction between the gases and vapours may be in a chemical sense, their *mechanical* action is very different. By increasing the quantity of any gas in a given space the force of it is proportionally increased; but increasing the quantity of any liquid in a given space does not at all affect the force of the vapour arising from it. On the other hand, by increasing the temperature of any gas a proportionate increase of elasticity ensues; but when the temperature of a liquid is increased, the force of vapour from it is increased with amazing rapidity, the increments of elasticity forming a kind of geometrical progression, to the arithmetical increments of heat.—Thus, the ratio of the elastic force of atmospheric air of 32° to that at 212°, is nearly as 5 : 7; but the ratio of the force of aqueous vapour proceeding from water of 32° and 212°, is as 1 : 150 nearly.

The object of the present essay is to determine the utmost force that certain vapours, as that from water, can exert at different temperatures. The importance hitherto attached to this enquiry has arisen chiefly from the consideration of steam as a mechanical agent; and this has directed the attention more especially to high temperatures. But it will appear from what follows that the progress of philosophy is more immediately interested in accurate observations on the force of steam in low temperatures. Different authors have published accounts of their experiments on the force of steam: I have on a former occasion (Meteorological Essays, page 134) given a table of forces for every 10° from 80° to 212°. The author of the article "Steam" in the Encyclopedia Britannica, has done the same from 32° to 280°: and M. Betancourt, in the "Memoirs des sçavans etrangeres" for 1790, (see Hutton's Math. Diction. page 755) has given tables on the subject, both for vapour from water and spirit of wine, also from 32° to 280°. But these two authors, having assumed the force of vapour from water of 32° to be nothing, are essentially wrong at that point and in all the lower parts of the scale; and in the higher part, or that above 212°, they determine the force too much; owing as I apprehend to a quantity of air, which being disengaged from the water by heat and mixing with the steam, increases the elasticity.—In a question of such moment it seemed therefore desirable to obtain greater accuracy.

My method is this: I take a barometer tube perfectly dry, and fill it with mercury just boiled, marking the place where it is stationary; then having graduated the tube into inches and tenths by means of a file, I pour a little water (or any other liquid the subject of experiment) into it, so as to moisten the whole inside; after this I again pour in mercury, and, carefully inverting the tube, exclude all air: the barometer by standing some time exhibits a portion of water, &c. of $\frac{1}{8}$ or $\frac{1}{10}$ of an inch upon the top of the mercurial column; because being lighter it ascends by the side of the tube; which may now be inclined and the mercury will rise to the top manifesting a perfect vacuum from air. I next take a cylindrical glass tube open at both ends, of 2 inches diameter and 14 inches in length; to each end of which a cork is adapted, perforated in the middle so as to admit the barometer tube to be pushed through and to be held fast by them; the upper cork is fixed two or three inches below the top of the tube and is $\frac{1}{2}$ cut away so as to admit water, &c. to pass by; its service being merely to keep the tube steady. Things being thus circumstanced, water of any temperature may be poured into the wide tube, and thus made to surround the upper part or vacuum of the barometer, and the effect of temperature in the production of vapour within can be observed from the depression of the mercurial column. In this way I have had water as high as 155° surrounding the vacuum: but as the higher temperatures might endanger a glass apparatus; instead of it I used the following:—

Having procured a tin tube of 4 inches in diameter and 2 feet long, with a circular plate of the same soldered to one end having a round hole in the centre, like the tube of a reflecting telescope, I got another smaller tube of the same length soldered into the larger, so as to be in the axis or centre of it: the small tube was open at both ends, and on this construction water could be poured into the large vessel to fill it, whilst the central tube was exposed to its temperature. Into this central tube I could insert the upper half of a syphon barometer, and fix it by a cork, the top of the narrow tube also being corked: thus the effect of any temperature under 212° could be ascertained, the depression of the mercurial column being known by the ascent in the exterior leg of the syphon.

The force of vapour from water, between 80 and 212° may also be determined by means of an air-pump; and the results exactly agree with those determined as above. Take a Florence flask half filled with hot water, into which insert the bulb of a thermometer; then cover the whole with a receiver on one of the pump plates, and place a barometer gage on the other: the air being slowly exhausted, mark both the thermometer and barometer at the moment ebullition commences, and the height of the barometer gage will denote the force of vapour from water of the observed temperature. This method may also be used for other liquids. It may be proper to observe the various thermometers used in these experiments were duly adjusted to a good standard one.

After repeated experiments by all these methods, and a careful comparison of the results, I was enabled to digest the following table of the force of steam from water in all the temperatures from 32° to 212°.

Two important enquiries still remained: the first, to determine the force of steam from water above 212° and below 32°; the second, to determine the comparative forces of vapour from other liquids. These enquiries seemed independent of each other; notwithstanding which I found them in reality connected.

Upon examination of the numbers in the table within the limits just mentioned, there appears something like a geometrical progression in the forces of vapour; the ratio however, instead of being constant, is a gradually diminishing one: thus, the force at 32° = ,200 inch.

thus, the force at 32° =	,200 inch.		
		17. 50	Ratios
—— at 122° =	3. 500		
		8. 57	
212° =	30. 000		

If we divide these ratios, according to observation, they will stand thus:

Force at	32° =	,200 inch.	
			4. 550
	77° =	,910	
			3. 846
	122° =	3. 500	Ratios
			3. 214
	167° =	11. 250	
			2. 666
	212° =	30. 000	

If we divide these again, they become:

Force at	32° =	,200 inch.	
			2. 17
	54°½ =	,435	
			2, 09
	77° =	,910	
			2. 00
	99°½ =	1. 820	
			1. 92
	122° =	3. 500	Ratios
			1. 84
	144°½ =	6. 450	
			1. 75
	167° =	11. 250	
			1. 67
	189°½ =	18. 800	
			1. 59
	212° =	30. 000	

By another division we obtain the ratios for every $11^\circ\frac{1}{4}$ of temperature from 32° to 212° as under:

Force at		Ratios
32° =	,200 inch.	
		1. 485
$43\frac{1}{4}$ =	,297	
		1. 465
$54\frac{1}{2}$ =	,435	
		1. 45
$65\frac{3}{4}$ =	,630	
		1. 44
77 =	,910	
		1. 43
$88\frac{1}{4}$ =	1. 290	
		1. 41
$99\frac{1}{2}$ =	1. 820	
		1. 40
$110\frac{3}{4}$ =	2. 540	
		1. 38
122 =	3. 500	
		1. 36
$133\frac{1}{4}$ =	4. 760	
		1. 35
$144\frac{1}{2}$ =	6. 450	
		1. 33
$155\frac{3}{4}$ =	8. 550	
		1. 32
167 =	11. 250	
		1. 30
$178\frac{1}{4}$ =	14. 600	
		1. 29
$189\frac{1}{2}$ =	18. 800	
		1. 27
$200\frac{3}{4}$ =	24. 000	
		1. 25
212 =	30. 000	

Thus it appears that a ratio having a uniform decrease nearly takes place; and we may therefore extend the table of forces at both extremes, without the aid of experiment, to a considerable distance. Thus, assuming the ratios for each interval of 11°¾ below 32° to be, 1. 500, 1. 515, 1. 530, 1. 545, &c. and for each interval above 212° to be 1. 235, 1. 220, 1. 205, 1. 190, 1. 175, 1. 160, 1. 145, 1. 130, &c. we can extend the table many intervals of temperature, and determine all the intermediate degrees by interpolation. This method may be relied upon as a near approximation; however it does not supersede the expediency of determination by experiment; though that is much more difficult above 212°, and below 32°, than in the intermediate degrees: because it is dfficult to procure a steady heat above 212°; and below 32° the variation of force becomes so small as to elude minute discrimination. It will appear from what follows that the extension of the table by this method above 212° is in all probability accurate, or very nearly so, for 100° or more.

TABLE

Of the Force of Vapour from Water in every temperature from that of the congelation of Mercury, or 40° below zero of Fahrenheit, to 325°.

Temperature.	Force of Vap. in inches of Mercury.	Temperature.	Force of Vap. in inches of Mercury.	Temperature.	Force of Vap. in inches of Mercury.
°		°		°	
-40	,013	15	,108	34	,214
-30	,020	16	,112	35	,221
-20	,030	17	,116	36	,229
-10	,043	18	,120	37	,237
		19	,124	38	,245
0	,064	20	,129	39	,254
1	,066	21	,134	40	,263
2	,068	22	,139	41	,273
3	,071	23	,144	42	,283
4	,074	24	,150	43	,294
5	,076	25	,156	44	,305
6	,079	26	,162	45	,316
7	,082	27	,168	46	,328
8	,085	28	,174	47	,339
9	,087	29	,180	48	,351
10	,090	30	,186	49	,363
11	,093	31	,193	50	,375
12	,096			51	,388
13	,100	32	,200	52	,401
14	,104	33	,207	53	,415

TABLE CONTINUED.

Temperature.	Force of Vap. in inches of Mercury.	Temperature.	Force of Vap in inches of Mercury	Temperature.	Force of Vap. in inches of Mercury.
54°	,429	82°	1. 07	110°	2. 53
55	,443	83	1. 10	111	2. 60
56	,458	84	1. 14	112	2. 68
57	,474	85	1. 17	113	2. 76
58	,490	86	1. 21	114	2. 84
59	,507	87	1. 24	115	2. 92
60	,524	88	1. 28	116	3, 00
61	,542	89	1. 32	117	3. 08
62	,560	90	1. 36	118	3. 16
63	,578	91	1. 40	119	3. 25
64	,597	92	1. 44	120	3. 33
65	,616	93	1. 48	121	3. 42
66	,635	94	1. 53	122	3. 50
67	,655	95	1. 58	123	3. 59
68	,676	96	1. 63	124	3. 69
69	,698	97	1. 68	125	3. 79
70	,721	98	1. 74	126	3. 89
71	,745	99	1. 80	127	4. 00
72	,770	100	1. 86	128	4. 11
73	,796	101	1. 92	129	4. 22
74	,823	102	1. 98	130	4. 34
75	,851	103	2. 04	131	4. 47
76	,880	104	2. 11	132	4. 60
77	,910	105	2. 18	133	4. 73
78	,940	106	2. 25	134	4. 86
79	,971	107	2. 32	135	5. 00
80	1. 00	108	2. 39	136	5. 14
81	1. 04	109	2. 46	137	5. 29

TABLE CONTINUED.

Temperature. °	Force of Vap: in inches of Mercury.	Temperature. °	Force of Vap. in inches of Mercury.	Temperature. °	Force of Vap. in inches of Mercury.
138	5. 44	166	10. 96	194	20. 77
139	5. 59	167	11. 25	195	21. 22
140	5. 74	168	11. 54	196	21. 68
141	5. 90	169	11. 83	197	22. 13
142	6, 05	170	12. 13	198	22. 69
143	6. 21	171	12. 43	199	23. 16
144	6. 37	172	12. 73	200	23. 64
145	6. 53	173	13. 02	201	24. 12
146	6. 70	174	13. 32	202	24. 61
147	6. 87	175	13. 62	203	25. 10
148	7. 05	176	13. 92	204	25. 61
149	7. 23	177	14. 22	205	26. 13
150	7. 42	178	14. 52	206	26. 66
151	7. 61	179	14. 83	207	27. 20
152	7. 81	180	15. 15	208	27. 74
153	8. 01	181	15. 50	209	28. 29
154	8, 20	182	15. 86	210	28. 84
155	8. 40	183	16. 23	211	29. 41
156	8. 60	184	16. 61	212	30. 00
157	8. 81	185	17. 00		
158	9. 02	186	17. 40	213	30. 60
159	9. 24	187	17. 80	214	31. 21
160	9. 46	188	18. 20	215	31. 83
161	9. 68	189	18. 60	216	32. 46
162	9. 91	190	19. 00	217	33. 09
163	10. 15	191	19. 42	218	33. 72
164	10. 41	192	19. 86	219	34. 35
165	10. 68	193	20. 32	220	34. 99

TABLE CONTINUED.

Temperature.	Force of Vap in inches of Mercury.	Temperature.	Force of Vap. in inches of Mercury.	Temperature.	Force of Vap. in inches of Mercury.
°		°		°	
221	35. 63	249	57. 31	277	85. 47
222	36. 25	250	58. 21	278	86. 50
223	36. 88	251	59. 12	279	87. 63
224	37. 53	252	60. 05	280	88. 75
225	38. 20	253	61. 00	281	89. 87
226	38. 89	254	61. 92	282	90. 99
227	39. 59	255	62. 85	283	92. 11
228	40. 30	256	63. 76	284	93. 23
229	41. 02	257	64. 82	285	94. 35
230	41. 75	258	65. 78	286	95. 48
231	42. 49	259	66. 75	287	96. 64
232	43. 24	260	67. 73	288	97. 80
233	44. 00	261	68. 72	289	98. 96
234	44. 78	262	69. 72	290	100. 12
235	45. 58	263	70. 73	291	101. 28
236	46. 39	264	71. 74	292	102. 45
237	47. 20	265	72. 76	293	103. 63
238	48. 02	266	73. 77	294	104. 80
239	48. 84	267	74. 79	295	105. 97
240	49. 67	268	75. 80	296	107. 14
241	50. 50	269	76. 82	297	108. 31
242	51. 34	270	77. 85	298	109. 48
243	52. 18	271	78. 89	299	110. 64
244	53. 03	272	79. 94	300	111. 81
245	53. 88	273	80. 98	301	112. 98
246	54. 68	274	82. 01	302	114. 15
247	55. 54	275	83. 13	303	115. 32
248	56. 42	276	84. 35	304	116. 50

TABLE CONTINUED.

Temperature.	Force of Vap. in inches of Mercury.	Temperature.	Force of Vap. in inches of Mercury.	Temperature.	Force of Vap. in inches of Mercury.
°		°		°	
305	117. 68	312	125. 85	319	133. 86
306	118. 86	313	127. 00	320	135. 00
307	120. 03	314	128. 15	321	136. 14
308	121. 20	315	129. 29	322	137. 28
309	122, 37	316	130. 43	323	138. 42
310	123. 53	317	131. 57	324	139. 56
311	124. 69	318	132. 72	325	140. 70

On Vapour from Ether, &c.

We come now to the consideration of vapour from other liquids. Some liquids are known to be more evaporable than water; as, liquid ammonia, ether, spirit of wine, &c. others less; as, quicksilver, sulphuric acid, liquid muriate of lime, solution of potash, &c. and it appears that the force of vapour from each in a vacuum is proportionate to its evaporability. M. Betancourt maintains that the force of vapour from spirit of wine is in a constant ratio to that from water at all temperatures; namely, as 7 to 3 nearly. My first experiments with spirits of wine led me to adopt this conclusion, and naturally suggested that the force of vapour from any other liquid would bear a constant ratio to that of water. The principle however is not true, either with regard to spirit of wine or any other liquid. Experiments made upon six different liquids agree in establishing this as a general law; namely, *that the variation of the force of vapour from all liquids is the same for the same variation of temperature, reckoning from vapour of any given force:* thus, assuming a force equal to 30 inches of mercury as the standard, it being the force of vapour from any liquid boiling in the open air, we find *aqueous* vapour loses half its force by a diminution of 30° of temperature; so does the vapour of any other liquid lose half its force by diminishing its temperature 30° below that in which it boils; and the like for any other increment or decrement of heat. This being the case, it becomes unnecessary to give distinct tables of the force of vapour from different liquids, as one and the same table is sufficient for all.—But it will be proper to relate the experiments on which this conclusion rests.

Experiments on Sulphuric Ether.

The ether I used boiled in the open air at 102°.—I filled a barometer tube with mercury, moistened by agitation in ether. After a few minutes a portion of ether rose to the top of the mercurial column, and the height of the column became stationary. When the whole had acquired the temperature of the air in the room, 62°, the mercury stood at 17. 00 inches, the barometer at the same time being 29. 75. Hence the force of vapour from ether at 62° is equal to 12. 75 inches of mercury, which accords with the force of aqueous vapour at 172°, temperatures which are 40° from the respective boiling points of the liquids. By subsequent observations I found the forces of the vapour from ether in all the different temperatures from 32° to 102° exactly corresponded with the forces of aqueous vapour of the like range, namely from 142° to 212°: the vapour from ether depresses the mercury about 6 inches in the temperature of 32°.

Finding that ether *below* the point of ebullition agreed with water below the said point, I naturally concluded that ether *above* the point would give the same force of vapour as water above it; and in this I was not disappointed; for, upon trial it appeared that what I had inferred only from analogical reasoning respecting the force of aqueous vapour above the boiling point, actually happened with that from ether above the said point. And ether is a much better subject for experiment in this case than water, because it does not require so high a temperature.

I took a barometer tube of 45 inches in length, and having sealed it hermetically at one end, bent it into a syphon shape, making the legs parallel, the one that was close being 9 inches long, and the other 36. Then conveyed two or three drops of ether to the end of the closed leg, and filled the rest of the tube with mercury, except about 10 inches at the open end. This done, I immersed the whole of the short leg containing the ether into a tall glass containing hot water; the ether thus exposed to a heat above the temperature at which it boils, produced a vapour more powerful than the atmosphere, so as to overcome its pressure and raise a column of mercury besides, of greater or less length according to the temperature of the water. When the water was at 147° the vapour raised a column of 35 inches of mercury, when the atmospheric pressure was 29. 75: so that vapour from ether of 147° is equivalent to a pressure of 64. 75 inches of mercury; agreeing with the force of aqueous vapour of 257°, according to the preceding estimation: in both cases the temperatures are 45° above the respective points of ebullition. In all the temperatures betwixt 102 and 147° the forces of ethereal vapour corresponded with those of aqueous vapour, as per table, betwixt 212° and 257°. I could not reasonably doubt of the equality continuing in higher temperatures; bnt the force increases so fast with the increase of heat, that one cannot extend the experiments much farther without tubes of very inconvenient lengths. Being desirous however to determine the force of the ethereal vapour experimentally up as high as 212°, I contrived to effect it as follows:—Took a syphon tube such as described above, only not quite so long, and filled it in the manner above mentioned, with ether and mercury, leaving about ten inches at the top of the tube vacant; then having graduated that part into equal portions of capacity, and dried it from ether, I drew out the end of the tube to a capillary bore, cooled it again so as to suffer the internal atmospheric air to be of the proper density, and suddenly sealed the tube hermetically, thus inclosing air of a known force in the graduated portion of the tube. Then, putting that part of the tube containing ether into boiling water, vapour was formed which forced the mercurial column upwards and condensed the confined air, till at length an equilibrium took place. In this way I found 8. 25 parts of atmospheric air of the force 29. 5 were condensed into 2. 00, at the same time a perpendicular column of 16 inches of mercury in addition pressed upon the vapour. Now the force of elastic fluids being inversely as the space, we have 2. 00 : 29. 5 :: 8. 25 : 121. 67 inches = the force of the air within; to which adding 16 inches, we obtain 137. 67 = the whole force sustained by the vapour, measured in inches of mercury. The force of aqueous vapour, at the same distance beyond the boiling point, or 322°, is equal to 137. 28, per table. Thus it appears that in every part of the scale on which experiments have been made, the same law of force is observable with the vapour of ether as of water.

Experiments on Spirit of Wine.

By boiling a small portion of the spirit I used (about one cubic inch) in a phial, the thermometer stood at 179° at the commencement; but by continuing the ebullition it acquired a greater heat. The reason is, the most evaporable part of the spirit flies off during the process of heating, and the rest being a weaker compound, requires a stronger heat. The true point of ebullition, I believe, was nearly 175°.—The force of the vapour from this spirit at the temperature of 212°, I found both by an open syphon tube and one hermetically sealed with atmospheric air upon the mercurial column, as with ether, to be equal to $58\frac{1}{2}$ inches of mercury. This rather exceeds the force of aqueous vapour at an equal distance from the boiling point; but it is no more than may be attributed to unavoidable little errors in such experiments. In a barometer tube the spirituous vapour at 60°, over the mercury, depresses the column about 1. 4 or 1. 5 inches; which is something less than the due proportion; one cause of this may be the evaporability of spirits, which in operating on small quantities, quickly dissipates part of their strength.

Experiments on Liquid Ammonia.

Liquid ammonia or volatile alkali, the specific gravity of which was .9474, boiled near 140°; in the barometer a small quantity depressed the mercury 4.3 inches in the temperature of 60°. In higher temperatures it did not produce a proportional depression; because the most volatile part of the compound, expanding in the vacuum of the barometer, leaves the rest more watery, and consequently its vapour must be weaker; especially when the portion used is confined to a drop or two.

Muriate of Lime.

Put a portion of liquid muriate of lime over the column of mercury in a barometer. The boiling point of the muriate was found by experiment to be 230°. At 55° the depression was ,22 of an inch:

at 65°—.30
— 70°—.40
— 95°—.90

all which nearly agree with the forces of aqueous vapour 18° below the respective temperatures.

Mercury and Sulphuric Acid.

Mercury boils by my thermometer at 660°, and sulphuric acid of the specific gravity 1. 83, boils at 590°. It is very difficult to determine the precise force of vapour from these liquids in any temperature under 212°; because at such great distance from the boiling point the vapour is so weak as to be in effect almost imperceptible. Following the general law, the vapours of these fluids ought to be of the force .1, mercury at 460°, and sulphuric acid at 390°.—Col. Roi makes the expansion of 30 inches mercury by 180° of heat = .5969 or .5651; and in a barometer the expansion in the same circumstances is .5117; the differences are .0852 and .0534 which should measure the effective force of mercurial vapour of 212°, nearly. This is in all probability too much; as it is next to impossible to free any liquid entirely from air; and if *any* air enter the vacuum, it unites its force to that of the mercurial vapour.

That the force of vapour from sulphuric acid, in low temperatures, is exceedingly small, will appear from the ensuing section.

SECTION II.

On Vapour in Air.

The experiments under this head were made with manometers, or straight tubes of different lengths, hermetically sealed at one end, of $\frac{1}{15}$ inch internal diameter, and their capacities divided into equal portions. A drop or two of the liquid, the subject of experiment, was conveyed to the bottom or sealed end of the tube; the internal surface was then dried by a wire and thread, and atmospheric, (or any other air) was admitted into the tube, upon which a column of mercury was suspended of $\frac{1}{10}$ of an inch, or of 30 inches, less or more, according to the nature of the experiment. By immersing the end of the manometer, containing the air thus circumstanced, into a tall glass vessel containing water of any temperature, the effect of the vapour in expanding the air could be perceived. It was first indeed necessary to determine the increase air unaffected by any liquid (except mercury) would obtain by increase of temperature: that was done, as will be particularly shewn in the next essay. The expansion of all elastic fluids, it seems probable, is alike or nearly so, in like circumstances; 1000 parts of any elastic fluid expands nearly in a uniform manner into 1370 or 1380 parts by 180° of heat.

It will be unnecessary to repeat in detail the numerous experiments made on the various liquids in all temperatures from 32° to 212°; as the results of all agree in one general rule or principle, which is this: let 1 represent the space occupied by any kind of air of a given temperature and free from moisture; $p =$ the given pressure upon it, in inches of mercury; $f =$ the force of vapour from any liquid in that temperature, in vacuo; then, the liquid being admitted to the air, an expansion ensues, and the space occupied by the air becomes immediately, or in a short time $= 1 + \frac{f}{p-f}$; or which is the same thing, $= \frac{p}{p-f}$.

Thus in water for instance:

Let $p = 30$ inches,

$f = 15$ inches, to the given temp. 180°.

Then, $\frac{p}{p-f} = \frac{30}{30-15} = 2$, for the space; or the air becomes of twice the bulk.

If the temperature be 203°, $f = 25$, and the space becomes 6 times as large as at first.

If $p = 60$ inches

$f = 30$ inches to the given temperature

212°; then the space $= \frac{60}{60-30} = 2$; or water under the pressure of 60 inches of mercury, and at the temperature of 212°, produces vapour which just doubles the volume of air.

If ether be the instance: let the temperature be equal 70°; then $f = 15$; and suppose $p = 30$; in this case the colume of air is doubled; that is, ether of 70° being admitted to any portion of air, dcubles its bulk.

The expansion of hydrogenous gas and atmospheric air by the vapour of water is the same for every temperature.

Sulphuric acid does not expand atmospheric air to any sensible amount by the heat of boiling water.

The theory of these facts is evident upon the principles laid down in the former essay: for instance; let it be required to explain the experiment with water of 212° under a pressure of 60 inches. Here the air was condensed into the space 1 by the pressure of 60 inches; but being exposed to water of 212° a vapour arose from it equal in force to 30 inches; the air therefore expanded till its force also became = to 30 inches, which was effected by doubling its volume: then the vapour pressing with 30 inches force and the air also with 30 inches force, the two together support the pressure of 60 inches and the equilibrium continues.—In short, in all cases the vapour arises to a certain force, according to temperature, and the air adjusts the equilibrium, by expanding or contracting as may be required.

The notion of a chemical affinity subsisting between the gases and vapours of different kinds, cannot at all be reconciled to these phenomena. To suppose that all the different gases have the same affinity for water might indeed be admitted if we could not explain the phenomena without it; but to go further, and suppose that water combines with every gas to the same amount as its vapour in vacuo; or in other words, that the elasticity of the compound should be exactly the same as if the two were separate, is certainly going far to serve an hypothesis.

Besides, we must on this ground suppose that all the gases have the same force of affinity for any given vapour; a supposition that cannot be admitted as having any analogy to other established laws of chemical affinity.

ESSAY III.

On Evaporation.

When a liquid is exposed to the air, it becomes gradually dissipated in it: the process by which this effect is produced, we call *evaporation.*

Many philosophers concur in the theory of chemical solution: atmospheric air, it is said, has an affinity for water; it is a menstruum in which water is soluble to a certain degree. It is allowed notwithstanding by all, that each liquid is convertible into an elastic vapour in vacuo, which can subsist independently in any temperature; but as the utmost forces of these vapours are inferior to the pressure of the atmosphere in ordinary temperatures, they are supposed to be incapable of existing in it in the same way as they do in a torricellian vacuum: hence the notion of affinity is induced.—According to this theory of evaporation, atmospheric air (and every other species of air for aught that appears) dissolves water, alkohol, ether, acids, and even metals. Water below 212° is chemically combined with the gases; above 212° it assumes a new form, and becomes a distinct elastic fluid, called *steam:* whether water first chemically combined with air, and then heated above 212°, is detached from the air or remains with it, the advocates of the theory have not determined.—This theory has always been considered as complex and attended with difficulties; so much that M. Pictet and others have rejected it, and adopted that which admits of distinct elastic vapours in the atmosphere at all temperatures, uncombined with either of the principal constituent gases; as being much more simple and easy of explication than the other; though they do not remove the grand objection to it, arising from atmospheric pressure. It has however been made to appear in these essays, I presume, that the objection to it from pressure, is itself founded upon an ungrounded hypothesis.

Leaving the theory of evaporation for the present, we shall proceed to the experiments.

The following positions have been established by others, and need therefore only to be mentioned here.

1. Some fluids evaporate much more quickly than others.

2. The quantity evaporated is in direct proportion to the surface exposed, all other circumstances alike.

3. An increase of temperature in the liquid is attended with an increase of evaporation, not directly proportionable.

4. Evaporation is greater where there is a stream of air than where the air is stagnant.

5. Evaporation from water is greater the less

the humidity previously existing in the atmosphere, all other circumstances the same.

The objects in view in this essay, are,

1. To determine the precise effect that a variation of temperature has upon the quantity evaporated.

2. To determine the ratio of evaporability of different fluids.

3. To find a rule by which the quantity and effect of previous humidity in the air may be ascertained.

4. From these and other facts to obtain a true theory of evaporation.

On the Evaporation of Water at 212°.

I took a small cylindrical vessel of tin, its diameter $3\frac{1}{4}$ and depth $2\frac{1}{2}$ inches; and having fixed three pieces of wire to equidistant points of the circumference, they were fastened together at the top and the extremities bent into a hook, by which the vessel might be suspended from the end of a balance, &c. This done, the vessel was nearly filled with water, which was then made to boil over a small red fire in different circumstances: it was held in the hand and removed nearer to or further from the fire, so as to be kept just at the point of ebullition. In this state the vessel and water were weighed true to a grain, and the instant of time noted by a watch; then kept as above at 212° for ten minutes or more and again weighed: and the loss of water by evaporation, per minute, was thus ascertained. The experiments were repeated several times in the same as well as in different circumstances; and the results in no instance differed materially when obtained in the same circumstances.

The least evaporation per minute was 30 grains: this was when the fire, or lamp, was in the middle of a room, the doors and windows shut, and the air calm.

The next degree was 35 grains per minute or thereabouts: this was when the evaporating vessel was over a small fire in the usual fire-place; there being a moderate draught of air, and the room close.

A brisker fire, causing a stronger current of air up the chimney, gave from 35 to 40 grains per minute.

When the windows of the room were open, and a strong wind prevailed, the draught over the fire was proportionally increased, and the evaporation was from 40 to 45 grains per minute.

The extremes that have thus been noticed are 30 and 45 grains per minute: but were the experiment tried in the open air in high winds, I am inclined to believe from a comparison of the observations, that an evaporation of 50, 55 or even 60 grains per minute might be observed.

On the Evaporation of Water below 212°.

I have frequently tried the evaporation at all the temperatures below 212°: it would be tedious to enter into detail of all the experiments, but shall give the results at some remarkable points. In all the high temperatures I used the vessel above mentioned, keeping a thermometer in it, by which I could secure a constant heat, or at least keep it oscillating within narrow limits.

The evaporation from water of 180° was from 18 to 22 grains per minute, according to circumstances; or about $\frac{1}{2}$ of that at 212°.

At 164° it was about $\frac{1}{3}$ of the quantity at the boiling temperature; or from 10 to 16 grains per minute.

At 152° it was only $\frac{1}{4}$ of that at boiling; or from 8 to 12 grains, according to circumstances.

The temperature of 144° afford $\frac{1}{5}$ of the effect at boiling; 138° gave $\frac{1}{6}$; &c.

Having previously to these experiments determined the force of aqueous vapour at all the temperatures under 212°, I was naturally led to examine whether the quantity of water evaporated in a given time bore any proportion to the force of vapour of the same temperature, and was agreeably surprised to find that they exactly corresponded in every part of the thermometric scale: thus the forces of vapour at 212°, 180°, 164°, 152°, 144° and 138° are equal to 30, 15, 10, 7½, 6 and 5 inches of mercury respectively, and the grains of water evaporated per minute in those temperatures were 30, 15, 10, 7½, 6 and 5 also; or numbers proportional to these. Indeed it should be so from the established law of mechanics, that all effects are proportional to the causes producing them. The atmosphere, it should seem, obstructs the diffusion of vapour, which would otherwise be almost instantaneous, as in vacuo; but this obstruction is overcome in proportion to the force of the vapour. The obstruction however cannot arise from the *weight* of the atmosphere, as has till now been supposed; for then it would effectually prevent any vapour from arising under 212°: but it is caused by the *vis inertiæ* of the particles of air; and is similar to that which a stream of water meets with in descending amongst pebbles.

The theory of evaporation being thus manifested from experiments in high temperatures, I found that if it was to be verified by experiments in low temperatures, regard must be had to the force of vapour actually existing in the atmosphere at the time. For instance, if water of 59° were the subject, the force of vapour of that temperature is $\frac{1}{60}$ of the force at 212°, and one might expect the quantity of evaporation $\frac{1}{60}$ also; but if it should happen, as it sometimes does in summer, that an aqueous atmosphere to that amount does already exist, the evaporation, instead of being $\frac{1}{60}$ of that from boiling water, would be nothing at all. On the other hand, if the aqueous atmosphere were less than that, suppose $\frac{1}{2}$ of it, corresponding to 39° of heat, then the effective evaporating force would be $\frac{1}{120}$ of that from boiling water; in short, the evaporating force must be universally equal to that of the temperature of the water, diminished by that already existing in the atmosphere. In order to find the force of the aqueous atmosphere I usually take a tall cylindrical glass jar, dry on the outside, and fill it with cold spring water fresh from the well; if dew be immediately formed on the outside, I pour the water out, let it stand a while to increase in heat, dry the outside of the glass well with a linen cloth, and then pour the water in again; this operation is to be continued till dew ceases to be formed, and then the temperature of the water must be observed; and opposite to it in the table (page 559) will be found the force of vapour in the atmosphere. This must be done in the open air, or at a window; because the air within is generally more humid than that without. Spring water is generally about 50°, and will mostly answer the purpose the three hottest months in the year: in other seasons an artificial cold mixture is required.—The accuracy of the result obtained this way I think scarcely needs to be insisted upon. Glass, and all other hard, smooth substances I have tried, when cooled to a degree below what the surrounding aqueous vapour can support, cause it to be condensed on their surfaces into water. The degree of cold is usually from 1 to 10 below the mean heat of the 24 hours; in summer I have often observed the point as high as 58° or 59°, corresponding to ½ an inch of mercury in force, and once or twice have seen it at 62°: in changeable and windy weather it is liable to considerable fluctuation; but this is not the place to enlarge upon it.

For the purpose of observing the evaporation in atmospheric temperatures I got two light tin vessels, the one 6 inches in diameter and ½ inch deep, the other 8 inches diameter and ¾ inch deep; and made to be suspended from a balance, like the former one. When any experiment designed as a test of the theory was made, a quantity of water was put into one of these (generally the 6 inch one, which I preferred) the whole was weighed to a grain; then it was placed in an open window or other exposed situation for 10 or 15 minutes, and again weighed to ascertain the loss by evaporation; at the same time the temperature of the water was observed, the force of the aqueous atmosphere ascertained as above, and the strength of the current of air noticed. From a great variety of experiments made both in the winter and summer, and when the evaporating force was strong and weak, I have found the results entirely conformable with the above theory. The same quantity is evaporated with the same evaporating force thus determined, whatever be the temperature of the air, as near as can be judged; but with the same evaporating force, a strong wind will double the effect produced in a still atmosphere. Thus, if the aqueous atmosphere be correspondent to 40° of temperature and the air be 60°, the evaporation is the same as if the aqueous atmosphere were at 60° of temperature and the air 72°; and in a calm air the evaporation from a vessel of 6 inches in diameter in such circumstances would be about .9 of a grain per minute, and about 1. 8 grains per minute in a very strong wind; the different intermediate quantities being regulated solely by the force of the wind.

The following table exhibits the ratios and quantity of water evaporated in each temperature, derived from the preceding theory, and confirmed by experiments, as far as they have been extended. The first column expresses the temperature; the second, the corresponding force of vapour taken from the preceding table; the other three columns give the number of grains of water that would be evaporated from a surface of 6 inches in diameter in the respective temperatures, on the supposition of there being previously no aqueous vapour in the atmosphere. These columns present the extremes and the mean of evaporation, likely to be noticed, or nearly such: for, the first is calculated upon the supposition of 35 grains loss per minute from the vessel of $3\frac{1}{4}$ inches in diameter; the second, 45 and the third 55 grains per minute.

TABLE

Shewing the force of vapour, and the full evaporating force of every degree of temperature from 20° to 85°, expressed in grains of water that would be raised per minute from a vessel of six inches in diameter, supposing there were no vapour already in the atmosphere.

Temperature. 212°	Force of Vap. inch: 30	Evaporating Force in Grains. 120	154	189
20°	.129	.52	.67	.82
21	.134	.54	.69	.85
22	.139	.56	.71	.88
23	.144	.58	.73	.91
24	.150	.60	.77	.94
25	.156	.62	.79	.97
26	.162	.65	.82	1. 02
27	.168	.67	.86	1. 05
28	.174	.70	.90	1. 10
29	.180	.72	.93	1. 13
30	.186	.74	.95	1. 17
31	.193	.77	.99	1. 21
32	.200	.80	1. 03	1. 26
33	.207	.83	1. 07	1. 30

TABLE CONTINUED.

Temperature. 212°	Force of Vap. inch. 30	Evaporating Force in Grains. 120	154	189
34°	.214	.86	1. 11	1. 35
35	.221	.80	1. 14	1. 39
36	.229	.92	1. 18	1. 45
37	.237	.95	1. 22	1. 49
38	.245	.98	1. 26	1. 54
39	.254	1. 02	1. 31	1. 60
40	.263	1. 05	1. 35	1. 65
41	.273	1. 09	1. 40	1. 71
42	.283	1. 13	1. 45	1. 78
43	.294	1. 18	1. 51	1. 85
44	.305	1. 22	1. 57	1. 92
45	.316	1. 26	1. 62	1. 99
46	.327	1. 31	1. 68	2. 06
47	.339	1. 36	1. 75	2. 13
48	.351	1. 40	1. 80	2. 20
49	.363	1. 45	1. 86	2. 28
50	.375	1. 50	1. 92	2. 36
51	.388	1. 55	1. 99	2. 44
52	.401	1. 60	2. 06	2. 51
53	.415	1. 66	2. 13	2. 61
54	.429	1. 71	2. 20	2. 69
55	.443	1. 77	2. 28	2. 78
56	.458	1. 83	2. 35	2. 88
57	.474	1. 90	2. 43	2. 98
58	.490	1. 96	2. 52	3. 08
59	.507	2. 03	2. 61	3. 19
60	.524	2. 10	2. 70	3. 30
61	.542	2. 17	2. 79	3. 41

TABLE CONTINUED.

Temperature. 212°	Force of Vap. inch. 30	Evaporating Force in Grains. 120	150	182
62°	.560	2. 24	2. 88	3. 52
63	.578	2. 31	2. 97	3. 63
64	.597	2. 39	3. 07	3. 76
65	.616	2. 46	3. 16	3. 87
66	.635	2. 54	3. 27	3. 99
67	.655	2. 62	3. 37	4. 12
68	.676	2. 70	3. 47	4. 24
69	.698	2. 79	3. 59	4. 38
70	.721	2. 88	3. 70	4. 53
71	.745	2. 98	3. 83	4. 68
72	.770	3. 08	3. 96	4. 84
73	.796	3. 18	4. 09	5. 00
74	.823	3. 29	4. 23	5. 17
75	.851	3. 40	4. 37	5. 34
76	.880	3. 52	4. 52	5. 53
77	.910	3. 65	4. 68	5. 72
78	.940	3. 76	4. 83	5. 91
79	.971	3. 88	4. 99	6. 10
80	1. 00	4. 00	5. 14	6. 29
81	1. 04	4. 16	5. 35	6. 54
82	1. 07	4. 28	5. 50	6. 73
83	1. 10	4. 40	5. 66	6. 91
84	1. 14	4. 56	5. 86	7. 17
85	1. 17	4. 68	6. 07	7. 46

The use of this table will appear from the following problems:

PROBLEM I.

Having given the temperature at which the aqueous atmosphere begins to be condensed into water, and the temperature of the air, to find the quantity of water that would be evaporated in a minute from a vessel of 6 inches diameter.

Solution. Subtract the grains opposite to the lower temperature from those opposite to the higher one, in the first, second or third column of grains, according to the strength of the wind, and the remainder will be the quantity evaporated in a minute, under those circumstances, nearly.

Example. Let the point of condensation be 52°, the temperature of the air 65°, with a moderate breeze.

The number opposite 52° in the second column of grains is 2. 06, and that opposite 65° is 3. 16; the difference, 1. 1 grain, is the evaporation per minute.

PROBLEM II.

Having given the quantity evaporated in a minute, found by experiment, and the temperature of the air, to find the force of the aqueous atmosphere, and the point of condensation.

Solution. Subtract the observed evaporation from that opposite the given temperature in the table; and look above for the number nearest to the remainder in the same column of evaporation, opposite to which will be found the force of the aqueous atmosphere, and the point at which it begins to be condensed.

Example. Finding the evaporation from a vessel of 6 inches in diameter to be 1. 7 grain per minute with a brisk wind, air 62°; what is the weight of the aqueous atmosphere, and the temperature at which it begins to be condensed into water?

The number opposite 62° in the third column of grains is 3. 52, being the whole evaporating force at that temperature in a perfectly dry atmosphere; from which take 1. 7 grains, the real evaporating force observed, and the remainder, 1. 82, corresponds, as per table, to the force .294 inches of mercury, the weight of vapour, and to 43° of temperature*.

* It may be proper to remind the reader that all the experiments on evaporation are understood to be made in the open air, or in a window with a current inward; also it may be observed the evaporation in a close room is much less and is besides irregular, being greater proportionably from a less surface, evidently from the stagnation of the air.

Evaporation of Spirits, Ether, &c.

If the law of evaporation above given apply to water in every part of the scale of heat, no reasonable doubt can be entertained respecting its application to other liquids. I have notwithstanding made several experiments on others, the results of which are conformable to the same law. Some of them follow:—

1. Spirit of wine.—Evaporated from a surface of 4 inches in diameter, 54 grains in 25 minutes: air 53°; aqueous atmosphere at 49°, and beginning to rain with a moderate breeze. It would proportionally have been 121 grains from a vessel of 6 inches in diameter. This gives nearly 5 grains per minute. The same spirit boiled at or near 180°.

Now from the *data*, water of 83° is equivalent in force to spirits of 53°: and it may be seen that the evaporating force of water of 83° is nearly 5 in the first and second columns of grains of the table. It seems probable that the aqueous atmosphere does not diminish the evaporation of spirits as it does that of water.

2. Ether. 1. Put a phial containing ether, and a small tin vessel of 1¾ inch diameter into a scale and balanced them exactly: then poured the ether into the evaporating vessel and put

the phial into the scale again; took out 40 grains from the opposite scale, and waited till the equilibrium was restored: this was in 8 minutes 6 seconds. The air was 50°, and the ether at first 50°; but it rapidly sunk, as was found by dipping a very small bulbed thermometer into it, to 28°. In a window with a moderate breeze.

——— 2 and 3. Repeated the experiment in the same circumstances, except the evaporating vessel, which was now porcelain, and 2½ inches diameter. Lost 40 grains in 3 minutes. Thermometer sunk from 50 to 30°. The two experiments made this way did not differ above one or two grains.

These results reduced to a vessel of 3¼ inches in diameter give

1st. Experiment, loss 17 grains per minute;
2 & 3 ——— — 22½ ———

The reason why the result in the first experiment was something less than in the other two, was evidently owing to the circumstance of its longer duration, by which the ether was the greater part of the time in a low temperature, and consequently evaporated less.—The ether used boiled at 102°. At 50° it was therefore in the capacity of water at 160°. But water at 160°, at most loses only 17 or 18 grains per minute, and less

20° below that temperature. At first view therefore it should seem that ether evaporates quicker than the general law assigns.—But it must be allowed that the temperature of the *air* has some effect upon evaporation, though it has certainly very little. Now ether in the above experiments is acted upon by a current of air of an equal or higher temperature than itself; but water of 160° is usually acted upon by air 100° lower than itself, which is every moment precipitating the vapour formed, and thus obstructing its circulation. This appears to be a sufficient cause for the small difference observed.

With respect to mercury, sulphuric acid, muriate of lime, &c. there can be no doubt but they experience a real evaporation like those above; but it must be very small in proportion as their boiling points are high. And it would be difficult to make experiments upon such of these as have an affinity for aqueous vapour; because their acquisition from the aqueous atmosphere would far exceed their loss by evaporation.

Since writing the above essay, opportunities have occurred to ascertain whether the evaporation from ice is conformable to the same law as that from water. Every one, who has tried the experiment, admits the fact that ice is evaporable.—I have lately made several observations on this subject, the results of which, as far as they go, support the conclusion that the general law of evaporation continues the same below the point of congelation as above it. All the experiments were made in the tin vessel above described of 6 inches in diameter; a quantity of water was suffered to freeze in it, so as to form a circular cake of ice; the vessel and ice were then weighed together, and exposed in the open air for a certain time, after which being again weighed, the loss was found; the force of the aqueous atmosphere was sometimes determined during the experiment by a mixture of pounded ice and salt, in the manner already described.

	Grs.	H.	Grs.		Wind.	Air.
Nov. 5. In the night lost	110 in	9	; or, .20	per m.	N. E. brisk.	28° to 31°
—— at 10 A. M. —	25 in	1¼	; or, .33	——	N. E. mod.	32°
—29. at 1 P. M. —	24 in	1¾	; or, .23	——	calm.	31°
P. M. —	84 in	9¼	; or, .15	——	——	30°
—30. in the night —	94 in	9	; or, .17	——	N. E. mod.	31°
Dec. 19. P. M. —	75 in	8	; or, .16	——	N. E. calm.	26°—28°
In the night —	33 in	11	; or, .05	——	calm.	29°
—20. A. M. —	21 in	2	; or, .175	——	W. mod.	31°

Some of these being made in the night, and of long duration, neither the temperature of the air, nor the force of the aqueous atmosphere could be fairly determined: the second experiment was made under every favourable circumstance, and the aqueous atmosphere found at 22°. By problem 2, at page 588 it would have been determined at $21\frac{1}{2}°$, using the second column of grains in the table.*

* On the subject of evaporation it may be considered as unpardonable not to advert to De Saussure's valuable Essays on Hygrometry.

That excellent philosopher determined, by a well conceived experiment, that dry air of the temperature of 64° or 66°, imbibed aqueous vapour so as to increase its elasticity $\frac{1}{54}$ of the atmospheric pressure; and that a cubic foot of such air required 11 or 12 grains of water to produce the effect. By the table above at page 560 it appears the force of vapour at $61° = .54 = \frac{1}{54}$ of 29 .5 inches, nearly. It is probable this difference is occasioned in part at least by the want of perfect dryness in the air he operated upon, which caused the increase of elasticity to be less than otherwise.—It was, I think, unfortunate that he attached so much importance to and confidence in his hygrometer; and that he adopted the theory of chymical solution of water in air, contrary to the facts he discovered, which seemed more reconcileable to the notion of aqueous vapour being a distinct elastic fluid. Indeed he is forced to acknowledge in the 1st. chap. of his Essay on the Theory of Evaporation, that in the ordinary temperature of the atmosphere, aqueous vapour is formed in the first instance a distant elastic fluid, and *after it has been converted into an elastic fluid*, it is dissolved by the air; "Je crois qu'il ne la dissout que lorsque l'action du "feu l'a convertie en vapeur elastique." Now if it can for a moment exist independently under the pressure

ESSAY IV.

On the Expansion of Elastic Fluids by Heat.

The principal occasion of this essay is another on the same subject by Messrs. de Morveau and du Vernois in the first vol. of the Annales de Chimie. It appearing to them that the results of the experiments of De Luc, Col. Roi, de Saussure, Priestley, Vandermonde, Ber-

of the atmosphere, why may it not continue to exist in that state?

His table of the weight of aqueous vapour in a cubic foot of air at different degrees of the thermometer, being derived from experiments with his hygrometer, except the standard one of 66° (15° Reaumur), is far from accurate; and the inaccuracy increases with the distance from the standard, which, as has been observed, appears to be nearly correct: in the higher temperatures he makes the water dissolved too little, and in the lower temperatures too much.—He says (§ 93) that the lowest he has seen the hygrometer in the open air, is 40; and that it indicated a reduction of temperature in the air amounting to 34° .7 (78° of Fahr.) was necessary in order to deposit dew. This observation alone is sufficient to render his hygrometer suspected; for, few who have attended to the formation of dew will admit the probability of so large a reduction being necessary in any climate or season: I believe it rarely requires 40° reduction in temperature in any part of the world to produce the effect.

thollet and Monge did not sufficiently accord with each other; and that it would be of importance to determine not only the whole expansion of each gas from two distant points, such as the freezing and boiling, but likewise whether that expansion be uniform in every part of the scale, they instituted a set of experiments expressly for those purposes. The result of which was, that betwixt the temperatures of 32° and 212°, the whole expansion of one gas differs much from that of another, it being in one instance about $\frac{4}{10}$ of the original, and in others more than 12 times that expansion; and that the expansion is much more for a given number of degrees in the higher than in the lower part of the scale. These conclusions were so extremely discordant with and even contradictory to those of others, that I could not but suspect some great fallacy in them, and found it in reality to be the fact: I have no doubt it arose from the want of due care to keep the apparatus and materials free from moisture.

My method of experimenting on this subject is simple, and therefore less liable to error. A straight manometer tube, such as has been mentioned, is duly divided into equal portions of capacity; it is then dried by a wire and thread, and the open end inserted through a cork into a phial containing sulphuric acid, in order that the aqueous vapour may be drawn out of the tube; this is essential if we operate in temperatures lower than that of the atmosphere, otherwise not. For want of this attention, Col. Roi, in his valuable paper in the Philos. Trans. vol. 67, has been led into some erroneous conclusions.—A small column of dry mercury is then let down to a proper point in the manometer, and it is ready for experiment with common air.

It requires some address to fill the manometer with any other gas.—I succeeded best as follows: filled the tube with dry mercury; then pushed down a wire with thread, so that when the wire was got to the end of the tube, a thick covering of thread just entered the open end, and held the mercury like a cork, so that the tube could be inverted without losing the contents; then having a glass funnel with a perforated cork over the water apparatus, containing the gas, I slipped the manometer through the hole in the cork, and putting my hand into the water under the funnel, drew the wire out of the manometer, and with it the mercury; upon which the gas entered the manometer. For carbonic acid gas, I opened the sealed end of the manometer, drew it out to a capillary bore, and forced a stream of the gas through the tube; then putting my finger on the other end, sealed it again by a blow-pipe, and let down a small column of mercury to the proper point.

When the manometer was to be exposed to a heat of 212°, I used a Florence flask, with a long glass tube corked into it, in such sort that as much of the manometer as was necessary to be exposed to the temperature might be in the tube; then water at the bottom of the flask was made to boil violently, so that a constant stream of vapour issued out of the top of the glass tube, which was found to raise the thermometer to 212°. Small specks of white paint were put upon the divisions of the manometer together with numbers which were discernible through the containing tube. For lower temperatures a deep tin vessel containing hot water was used, in which the manometer was immersed, the water being well agitated previously to each observation.

From a great many experiments made in this way on common air, and likewise upon hydrogenous gas, oxygenous and nitrous gases, and carbonic acid gas, I can assert that the conclusions of De Luc, Roi, Saussure, Berthollet, &c. are nearly accurate throughout, and that those of de Morveau and du Vernois are extremely inaccurate in the higher temperatures.

I have repeatedly found that 1000 parts of common air of the temperature 55° and common pressure, expand to 1321 parts in the manometer; to which adding 4 parts for the corresponding expansion of glass, we have 325 parts increase upon 1000 from 55° to 212°; or for 157° of the thermometric scale. As for the expansion in the intermediate degrees, which Col. Roi's experiments shew to be a *slowly diminishing* one above the temperature of 57°, but which de Morveau's on the contrary shew to be a *rapidly increasing* one in the higher part of the scale; I am obliged to allow that Col. Roi is right, though it makes in some degree against an hypothesis I have formed relative to the subject; he has certainly however made the diminution too great from 72° downwards, owing to his not perceiving that he actually *destroyed* a portion of the elastic fluid he was operating upon (aqueous vapour) in reducing its temperature so low; if his air had been previously dried by sulphuric acid, &c. he would not have found so remarkable diminution below 72°. My experiments give for $77\frac{1}{2}$° above 55°, 167 parts; for the next $77\frac{1}{2}$° only 158 parts: and the expansion in every part of the scale seems to be a gradually diminishing one in ascending.

The results of several experiments made upon hydrogenous gas, oxygenous gas, carbonic acid gas and nitrous gas, which were all the kinds I tried, agreed with those on common air not only in the total expansion, but in the gradual diminution of it in ascending: the small differences observed never exceeded 6 or 8 parts on the whole 325; and differences to this amount will take place in common air, when not freed from aqueous vapour, which was the situation of all my factitious gases.

Upon the whole therefore I see no sufficient reason why we may not conclude, that *all elastic fluids under the same pressure expand equally by heat*—and that *for any given expansion of mercury, the corresponding expansion of air is proportionally something less, the higher the temperature.*

This remarkable fact that all elastic fluids expand the same quantity in the same circumstances, plainly shews that the expansion depends *solely* upon heat: whereas the expansion in solid and liquid bodies seems to depend upon an adjustment of the two opposite forces of heat and chemical affinity, the one a *constant* force in the same temperature, the other a *variable* one, according to the nature of the body; hence the unequal expansion of such bodies. It seems therefore that general laws respecting the absolute quantity and the nature of heat, are more likely to be derived from elastic fluids than from other substances.

In order to explain the manner in which elastic fluids expand by heat, let us assume an hypothesis that the repulsive force of each particle is exactly proportional to the whole quantity of heat combined with it, or in other words to its temperature reckoned from the point of total privation: then, since the diameter of each particle's sphere of influence is as the cube root of the space occupied by the mass we shall have $\sqrt[3]{1000} : \sqrt[3]{1325}$ (10 : 11, nearly) : : the absolute quantity of heat in air of 55°: the absolute quantity in air of 212°. This gives the point of total privation of heat, or absolute cold, at 1547° below the point at which water freezes. Dr. Crawford (On Animal Heat, &c. page 267) deduces the said point by a method wholly different to be 1532°.—So near a coincidence is certainly more than fortuitous.

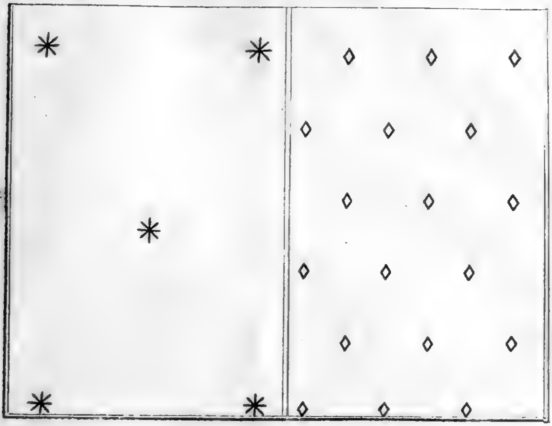
The only objection I see to this hypothesis is, that it necessarily requires the augmentation of elastic fluids for a given quantity of heat to be greater in the higher temperatures than in the lower, because the cubes of a series of numbers in arithmetical progression differ more the larger the numbers or roots: but it has just been shewn that in fact an augmentation of a contrary kind is observed. This refers us to the consideration whether the mercurial thermometer is an accurate measure of the increments of heat; if it be, the hypothesis fails; but if equal increments of heat cause a greater expansion in mercury in the higher than in the lower temperatures, and that in a small degree, the fact noticed above instead of being an objection will corroborate the hypothesis.—Dr. Crawford determines the expansions of mercury to be very nearly in proportion to the increments of heat; M. De Luc makes them to be less for a given quantity of heat in the lower than in the higher part of the scale; and in a ratio that agrees with this hypothesis. Now as every other liquid we are acquainted with is found to expand more in the higher than in the lower temperatures; analogy is in favour of the conclusions of De Luc, that mercury does the same.

EXPLANATION OF THE PLATE.

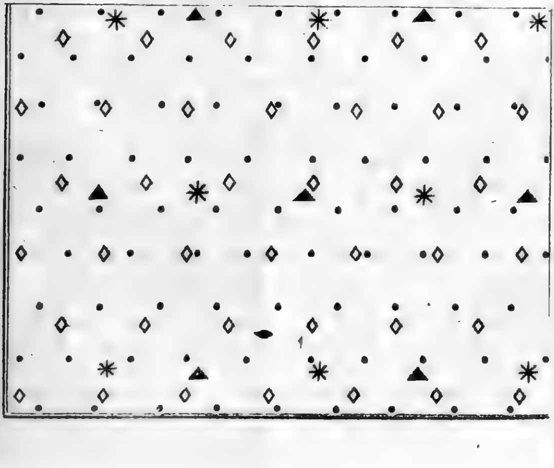
The annexed plate is intended to illustrate the author's conception of the constitution of the atmosphere. The different marks or characters of the particles of the gases are merely arbitrary, and intended for distinction; the simple atmospheres are given nearly on their real densities, and the particles are arranged at equal distances from each other. In the compound atmosphere the same arrangement is made of each kind of particles as in the simple; but the particles of different kinds do not arrange at regular distances from each other; because it is supposed they do not repel each other.

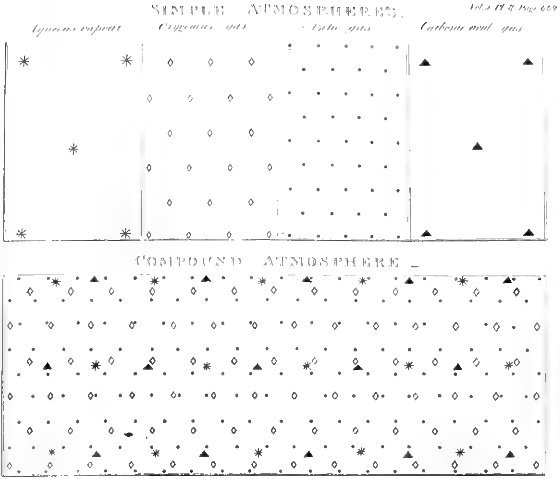
SIMPLE AT

Aqueous vapour — *Oxygenous gas.*



COMPOUND A




SIMPLE ATMOSPHERES.
Aqueous vapour
Oxygenous gas
Azotic gas
Carbonic acid gas
COMPOUND ATMOSPHERE.

A REVIEW *of some* EXPERIMENTS, *which have been supposed to* DISPROVE *the* MATERIALITY *of* HEAT.

BY WILLIAM HENRY.

READ JUNE 5, 1801.

The following remarks, on the subject of heat, were written soon after the publication of Count Rumford's Inquiry concerning the Source of the Heat evolved by Friction; and of the interesting essays of Mr. Davy, which appeared in Dr. Beddoes's West Country Contributions. They were transmitted to Dr. Beddoes, for publication, about the close of the year 1799; but circumstances, with which I am unacquainted, have, I believe, induced the Doctor to decline the continuation of his periodical work. These circumstances, I deem it necessary to state; because, had the essay been written nearer the period of its publication, it would probably have assumed a very different form. At present, I have not leisure to review the subject, or to attempt any material alteration; and still less, to examine, whether I have been anticipated by any of the authors, whose essays have been published, during the two last years.

A Review of some Experiments, which have been supposed to disprove the Materiality of Heat.

It has long been a question among philosophers, whether the sensation of heat, and the class of phenomena arising from the same cause, be produced by a peculiar kind of matter, or by motion of the particles of bodies in general. The former of these opinions, though far from being universally admitted, is now most generally received; and the peculiar body, to which the phenomena of heat are referred, has been denominated by M. Lavoisier, caloric. Against the doctrine of the French school, some forcible arguments have lately been advanced by Count Rumford and by Mr. Davy, both of whom have adopted that theory respecting heat, which assigns, as its cause, a motion among the particles of bodies.

The method of reasoning, employed by Mr. Davy, in proving the immateriality of the cause of heat, is the *reductio ad absurdum*, i. e. the oppugned theory is assumed as true, together with its applications; and facts are adduced, directly contradictory of the assumed principles.

I shall take the liberty of offering a statement of the argument, rather different from that of Mr. Davy; though I trust without misrepresentation, or any material omission.

Let heat be considered as matter; and let it be granted, that the temperature of bodies depends on the presence of uncombined caloric. Now, if the temperature of a body be increased, the free caloric, occasioning that elevation, must proceed from one of two sources; either 1stly. it may be communicated by surrounding substances; or 2dly. it may proceed from an internal source, i. e. from a disengagement of what before existed in the body, latent or combined. But the temperature of bodies is uniformly increased by friction and percussion, and, necessarily, in one of the foregoing modes.

I. Mr. Davy found, by experiment, that a thin metallic plate was heated, by friction in the exhausted receiver of an air pump, even when the apparatus was insulated, from bodies capable of supplying caloric, by being placed on ice. This experiment he considers as demonstrating, that the evolved caloric could not be communicated by surrounding bodies.

To the inference deduced from this experiment, it may be objected, that the mode of insulation was by no means perfect. Admitting the vacuum, produced by the air pump, to have been complete, still the supply of caloric could not thus be entirely cut off; since it has been shewn by Count Rumford, that caloric passes even through a torricellian vacuum. If, therefore, friction produce in bodies some change, which enables them to attract caloric from surrounding substances, this attraction may be equally efficient in an exhausted receiver, as in one containing an atmosphere of mean density. It would be an interesting subject of experiment, to determine the influence of atmospheres of various densities, as conductors of caloric; for, since effects are proportionate to their causes, and it is ascertained that common air conducts caloric, better than it is conveyed through a vacuum, as 1000 is to 702, it may be expected that the ratio will hold in all intermediate degrees.

In Count Rumford's masterly experiment, the metal, submitted to friction, was encompassed by water; and air was carefully excluded from the surfaces in motion. Yet the water became hot, and was kept boiling a considerable time. In this case, the only obvious source of caloric, from without, was through the borer, employed in producing the friction; if it be true, as the Count has observed, that the water could not, at the same instant, be in the act of giving out and receiving heat. The same objection to the communication of heat, from an external source, exists, also, in thus explaining Mr. Davy's experiment: but I cannot admit that the argument is demonstrative, in proving the evolved caloric not to be derived from external substances; for no absurdity is implied in supposing, that a body may be receiving caloric in one state, and giving it out in another. We have an example of the simultaneous admission and extrication of a subtile fluid, the materiality of which is admitted by Mr. Davy, in an excited electric, which, at the very same instant, receives the electric fluid from without, and transfers it to the neighbouring conductors. In an ignited body, also, the two processes of absorption and irradiation of light, are, perhaps, taking place at the amse moment.

II. Another cause of the increase of temperature in bodies, is the liberation of their combined caloric; and, if this be a source of temperature, the absolute quantity of caloric in a body must be diminished by friction. That no such diminution really takes place, we have the evidence of two experiments—the one of Mr. Davy, the other of Count Rumford. Mr. Davy, by rubbing together two pieces of ice converted them into water. Now water, *ex hypothesi*, contains more caloric, than the ice, from which it was formed; and, on the same hypothesis, the absolute quantity of caloric in ice is diminished by friction and liquefaction, which is absurd. Count Rumford, also, ascertained that the specific heat of iron was not diminished, when converted by a borer into turnings, and consequently when it had been the source of much temperature. In explanation of these facts, we may be allowed to assume the communication of caloric from surrounding bodies, till this communication has been demonstrated to be impossible. But even were the impossibility established, it would yet remain to be proved, that the evolved caloric does not proceed from an internal source; and this can only be done, by an accurate comparison of the quantity of caloric in bodies, before and after friction. Now, in instituting this comparison, it is implied, that we possess means of determining the absolute quantity of caloric in bodies, and that we can compare quantities of caloric, with as much certainty, as we can obtain from an appreciation by weight or by measure. Such perfection, however, does not, I apprehend, belong to the present state of our knowledge respecting heat; for I have always been distrustful of that part of the doctrine, which assigns the ratio of heat latent in bodies. The grounds of this distrust I shall state pretty fully—for, if it can be proved that we have no accurate conceptions of quantity, as appertaining to heat, all arguments against its materiality, derived from supposed determinations of its quantity, must be inconclusive.

The only clear conceptions, which the mind has of quantity, are derived either from a comparison of the magnitude, or of the gravity, of bodies. In the instance of caloric, both these modes of mensuration fail us. We cannot estimate the bulk of a substance, which eludes our grasp and our vision; nor have we yet succeeded in comparing its gravity, with that of the grosser kinds of matter, which it surpasses in tenuity, beyond all comparison. Our notions of the quantity of caloric are derived, not from such simple judgments, but from complicated processes of reasoning, in the steps of which, errors, fatal to the whole, may, perhaps, sometime appear.

Whatever be the nature of caloric—whether it be a body *sui generis*, or a quality of other bodies,—its effects are peculiar and appropriate; and, like all other effects, bear a proportion to the energy of their cause. Expansion, for example, it is proved by experiment, keeps pace with the actual increments of heat; and on this principle is founded the thermometer, the great agent in the acquirement of all our ideas respecting heat, both absolute and relative. The competency of this instrument, however, to afford information of the quantity of caloric, is limited by the following circumstances.

1. The mercury of the thermometer indicates only the quantity of heat, which it has itself acquired, and by no means that contained in surrounding bodies. 2dly. The scale of expansion is wholly arbitrary, commencing far from the absolute privation of heat, and falling far short of its maximum. 3dly. The caloric, latent in bodies, or chemically combined with them, has no effect on the thermometer. 4thly. The experiments of Dr. Crawford, though sufficient to shew that the expansion of the mercury of the thermometer bears a ratio to the actual increments of heat, in any temperature between the boiling and freezing points of water, by no means prove that this proportion holds universally.

Equal weights of heterogeneous bodies, it is presumed, contain unequal quantities of caloric; and the ratio of these quantities, is approximated, in the following manner.

Equal weights of the same body, at different temperatures, give, on admixture, the arithmetical mean: but equal weights of different bodies, at different temperatures, afford a temperature, which varies considerably from the mean. Thus a pound of water at 100 degrees, and a pound at

200°, give the temperature of 150°; but a pound of water at 200° and a pound of mercury at 100° afford, not the mean, but a temperature considerably higher. Hence it follows that a pound of mercury has not the power of fixing and retaining so much caloric, as a pound of water: and the fixation of more heat, by the water than by the mercury, is ascribed to the superior energy of a power, inherent in both, and termed capacity for caloric.

From an extensive series of experiments Dr. Crawford infers, that the capacities of bodies are permanent, so long as they retain their form. Thus, the capacity of water has to that of mercury the ratio of 28 to 1, at any temperature between 32° and 212°. The difference of capacities of bodies, it is inferred, therefore, would continue the same, down to the absolute privation of temperature.—Imagine, then, two bodies, at this point of privation: they may still contain unequal quantities of combined caloric; for, when chemically combined, caloric does not produce temperature. On Dr. Crawford's hypothesis, these comparative quantities of combined caloric, in the two bodies, may be learned, by observing the ratio of temperature, produced, by the addition to each, of similar quantities of heat. This supposition, however, is manifestly gratuitous; and the contrary might be maintained, with equal or greater probability: for, it may be supposed, that at this assumed negation of temperature, one body renders latent more caloric than another, because it actually contains less; as certain dry salts attract more water from the atmosphere, than others containing much water of crystallization. The commonly employed mode of ascertaining the specific caloric of bodies, is founded, therefore, on an assumption, which is deficient in the character of a datum, and which itself requires proof.

If these objections be valid, they will apply also to shew the fallacy of the theorem, for finding the absolute zero of bodies. By this term some philosophers appear to understand the point of absolute privation of caloric, both free and combined. I apprehend, however, that in strict propriety it can only be used to signify the negation of *uncombined* caloric, or, as Dr. Crawford expresses himself, the point of absolute cold. As applied, however, to water, it is evident that the whole quantity of heat is understood.—In ascertaining the zero, say these calculators, the capacity of ice to that of water is as 9 to 10. It is plain, therefore, that when water freezes, it must give out $\frac{1}{10}$th. of its whole heat, and this 10th. part is found to answer to 146° of Faht. Consequently its whole heat is

10 times 146, or 1460°; and hence the natural zero is 1460 — 32 or 1428°. Now of this estimate it is a datum, that the capacities of ice and water have precisely the above ratio. But if the general formula, for ascertaining the specific caloric of bodies, be founded on erroneous principles, it cannot serve as the groundwork of any solid conclusions.

The materiality of caloric may, I apprehend, be maintained, without admitting that we have made any steps towards determining its quantity in bodies; and the arguments of Count Rumford and Mr. Davy are not demonstrative, because they assume, that this part of the doctrine of caloric cannot be relinquished, without abandoning it *in toto*. I may be permitted, therefore, to state my reasons for believing caloric to be matter; which would have been unnecessary, had the contrary been proved, with all the force of mathematical demonstration.

Avoiding all metaphysical reasoning on the nature of matter, and assuming the generally received definition, as sufficiently characterizing it, I shall examine how far this general character of matter applies to the individual—caloric. Caloric occupies space or is extended, because it enlarges the dimensions of other bodies; and, for the same reason, it is impenetrable, since if it could exist, at the same time, in the same place, with other bodies, their volume would never be enlarged by the addition of heat. Of form or figure, as only a mode of extension, it is unnecessary to prove that caloric is possessed; and indeed there is perhaps only one general quality of matter, that will not be allowed it, viz. attraction. That caloric is influenced by the attraction of gravitation, or by cohesive attraction, has never yet been proved. Yet the various experiments of Buffon, Whitehurst, Fordyce, Pictet, &c. cannot be alleged as proofs, that it is actually devoid of this property; since they only decide, that the small quantities, which can be artificially collected, are not to be set in the ballance against the grosser kinds of matter. One kind of attraction, *that* which has lately been termed chemical affinity, may, I think, after a full survey of phenomena, be fairly predicated of caloric—and if its possession of this quality be rendered probable, we shall thence derive a powerful argument, in favour of its materiality.

That chemical affinity has a considerable share in producing the phenomena of heat, appears probable from the following considerations.

1. All the characters, distinguishing caloric when separate, cease to be apparent, when it has contributed to a change of form in other

bodies; and the properties of the substances so changed are also materially altered. Now this is the only unequivocal mark of chemical union, that we can apply in any instance; and chemical union implies the existence and efficiency of chemical affinity.

2. The relation of caloric to different substances appears to observe that peculiar law, which, in other instances, is termed elective affinity. If a compound of two or more principles, a metallic oxyd for instance, be exposed in a high temperature, the caloric forms a permanent union with the one, but not with the other. In certain instances, caloric is evolved, when two substances, attracting each other more powerfully than they attract caloric, produce on admixture, an elevation of temperature. In other instances, caloric is absorbed, when it is attracted by the new compound, more strongly than by the separate components. Such facts warrant the deduction, that caloric is subject to the laws of chemical affinity.—But the precise order of its affinities remain to be decided, by future experiments.

3. Caloric seems, also, on some occasions, to bear a part in the operation of double elective affinities. In this way, it produces decompositions, which, by single affinity, it is incapable

of effecting.—Thus a most intense fire does not expel, entirely, the carbonic acid from alkalis. But when the affinity of an acid for an alkali concurs with that of carbonic acid for caloric, a decomposition ensues.—Again—Water may be submitted to the highest temperature, without imparting a gaseous form to the hydrogen which it contains; but the conspiring affinity of a metal for oxygen occasions the production of hydrogenous gas. On this principle, many chemical facts are resolved into the law of *double affinity*, which are, at present, explained by that of single elective attraction.

4. Caloric acts, sometimes, as an intermedium in combining bodies, which, without its aid, are not susceptible of combination. Thus carbon and oxygen do not evince any tendency to combination, at the ordinary temperature of the atmosphere; but caloric brings them into union, and constitutes, itself, part of the resulting compound. This, and a variety of other instances, have a striking resemblance to what is called *intermediate affinity*.

In the theory of Dr. Crawford, no influence is allowed to chemical affinity over the phenomena of heat; and indeed *that* philosopher expresses a decided opinion, that elementary heat is not capable of uniting chemically with bodies. Hence it appears, that the difference between the terms affinity and capacity is not merely a verbal one; but that they are actually expressive of different powers or causes: and the question, therefore, which of these terms shall be adopted, in the description of facts, is one involving the determination of causes.

The term capacity for heat is employed, by Dr. Crawford and others, to denote, in the abstract, that power, by which different kinds of matter acquire different quantities of caloric. But in the various applications, that are made of this theory, a more precise meaning is often affixed to it; and the term is applied, in much the same sense, which it has in common language. When thus understood, a difference of capacity necessarily implies a difference in the extent of the spaces, between the minute particles of bodies; and that these differences occasion the varieties, observed in the acquirement of heat by different bodies. On this theory, there is no active principle or power inherent in bodies, and more active in some than in others,—no tendency in the matter of heat to attach itself, in preference, to any one substance. The assigned cause of the phenomena of heat is not, I apprehend, adequate to produce the effects ascribed to it.

On the theory of capacities, a change of form is, in certain instances, antecedent to the absorption of caloric. Thus, when ether is converted into gas, on removing the pressure of the atmosphere, according to this hypothesis, the capacity of the ether is increased by its volatilization; and the change of form is prior to, and the cause of, the absorption of caloric. The order of events, then, in the volatilization of ether, is first an alteration of form; next a change of capacity; and lastly an absorption of caloric. On this hypothesis, ether may exist in the state of gas, without containing a greater absolute quantity of caloric, than in a liquid form. But such an interpretation of phenomena is directly contradictory to an established principle, admitted, even by those who prefer the doctrine of capacities, viz. that all bodies, *during* their conversion from a fluid to a vaporous state, absorb caloric. It is at variance, also, with observed facts: for if a thermometer be immersed in a portion of ether, confined under the receiver of an air pump, the temperature of the ether will be found to sink gradually, during the exhaustion of the air; and the evaporation becomes proportionally slower, till, at last, it is scarcely perceptible. We may, therefore, infer, that at a certain point of diminished temperature, the volatilization of ether would entirely cease, if the supply of caloric, from surrounding bodies, could be completely intercepted. But on the theory of capacities, the evaporation should proceed as rapidly at the close, as at the commencement, of the process—or, in other words, evaporation should be wholly independent of temperature, which every one knows is contrary to fact.

It may be considered, therefore, as extremely probable, that the tendency of ether to assume a gaseous form depends on its chemical affinity for caloric. But, (it may be asked) how is this affinity counteracted by an increased pressure, and augmented by a diminished one?

A circumstance, absolutely essential to the formation of gasses, is, that free space shall be allowed for their expansion.—Mechanical pressure acts as a counteracting force to this expansion; and either prevents it completely, or partially, according to the degree of its application. But from this fact, no argument can be drawn against the existence of chemical affinity, as an attribute of caloric. Two opposite forces in physics may be so balanced, that neither shall produce its appropriate effect. Thus a body, impelled in contrary directions, may remain at rest, yet the operation of the opposing forces, in this case, cannot be denied. Even in chemistry, we have unequivocal examples, in which the action of the affinities is suppressed by more powerful causes. Thus bodies, that have a strong chemical affinity, are kept perfectly distinct, even when placed in contact, by the affinity of aggregation. The only inference, then, that can fairly be deduced from the effects of pressure, in preventing the formation of gasses, is, that it is a power, sometimes superior, in energy, to that of chemical affinity.

Since, therefore, caloric is characterized by all the properties, except gravity, that enter into the definition of matter, we may venture to consider it as a distinct and peculiar body. Nor is its deficiency of gravity sufficient to exclude it from the class of material substances. Such nicety of arrangement might, with equal propriety, lead us to deny the materiality of light, the gravity of which has never yet been proved: for, besides the experiments of Mr. Michell, which failed in ascertaining this property of light, we have several chemical facts tending to the same conclusion. Thus Mr. Cavendish, after firing a mixture of hydrogenous and oxygenous gases, in a close vessel, a process during which much light is always emitted, found not the smallest diminution of weight.

To have completed this defence of the material nature of heat, it would have been proper to have pointed out the circumstances, in which the phenomena of heat differ from the known and acknowledged phenomena of motion. At present, however, I have not leisure to pursue the subject at much length; and, though several points of disagreement would doubtless be found, I shall mention only one of the most marked and decisive.

Motion is an attribute of matter, independently of which it cannot possibly subsist. If therefore, the phenomena of heat can be shewn to take place, where matter is not present, we shall derive, from the fact, a conclusive argument against that theory of heat, which assigns motion as its cause. Now, in the experiment of Count Rumford, before alluded to, heat passed through a torricellian vacuum, in which, it need hardly be observed, nothing could be present to transport or propagate motion. This experiment, in my opinion, decidedly proves, that heat can subsist independently of other matter, and consequently of motion—in other words *that heat is a distinct and peculiar body.*

An INVESTIGATION *of the* METHOD *whereby Men judge, by the Ear, of the* POSITION *of* SONOROUS BODIES *relative to their own Persons.*

BY MR. JOHN GOUGH.

COMMUNICATED BY DR. HOLME.

READ NOVEMBER 27, 1801.

The power of the ear to distinguish very slight variations of tone, has been observed long ago; and some experiments have been made to ascertain the degree of discrimination, which a good ear possesses in this respect. But there is *another* faculty of the auditory organs, which, as far as I know, has never been explained,—I mean the power of ascertaining, with some degree of precision, the bearings or relative positions of sonorous bodies. Every man is sensible, by constant experience and observation, of the existence of the faculty in question; for every person who can hear, knows, without the use of his eyes, from what quarter any particular sound proceeds; or, to speak in more definite terms, perceives, whether it comes from before or behind him, from his right hand or his left. His powers of discrimination in this respect are not confined to the general limits or points, which I have here mentioned by way of illustration; for, he can in most instances divide these principal angles, not indeed into degrees and minutes, but with so much accuracy as to know whether the sounding object lies in a line making a less angle with that imaginary right line, which may be supposed to join his ears; or with another right line, which, passing from the front to the back part of his head, bisects the former at right angles. This sort of perception is not confined to what passes upon the horizon; for, place the same person on a tower, or other steep eminence, and he will perceive whether a certain sound comes from a part below or above him: besides which, he immediately combines this sensation with the preceding, so as to form a judgment respecting the true situation of the sounding body, which is of great practical use in the common affairs of life.

The nature of the present essay seems to require, that the reader should conceive the human head to be divided by two fixed mathematical planes, with a view to assist his imagination while he peruses the sequel.

The first of these planes lies parallel to the horizon, when the hearer stands in an erect posture; and contains in it the imaginary right line that joins the ears, which in future will be called, the *axis of hearing*. This plane, therefore, divides the head into two dissimilar solids, the one superior, and the other inferior in respect to itself. The other plane must be conceived to stand perpendicular to the last, and to bisect the axis of hearing at right angles. On these accounts, it evidently passes from the front to the back part of the head, dividing it vertically into two portions; which are so nearly alike in most men, that they may be considered, without danger of error, as two equal and similar geometrical solids. The preceding description, which bears the face of a mathematical construction, may perhaps assist my readers in conceiving what was meant above, when I spoke of combining the perception produced by the comparative elevation or depression of a sounding body, with the sensation which arises from its place on the horizon, relative to the axis of hearing: for every man judges, with some degree of accuracy, of the angles which are made with the two planes described above, as well as their common section, by the right line that joins his head and the place from which any particular sound proceeds.

Though the fact I have been describing is established by universal experience, the cause of it, I believe, has never been investigated; and indeed the question, when we first attend to it, seems to put on a very puzzling appearance. We know that when sound finds a free passage through the air, it takes the shortest path, leading from its source to the person who attends to it; and that the sensation of hearing is occasioned by a succession of waves or pulses of air, which fall upon the auditory organs, in the direction of a right line drawn from the sounding body to the head of the hearer. But after the enquirer has arrived at this point in his examination of the problem, his progress is hindered by the difficulty under consideration. The perplexity here alluded to arises from the obscurity of the principle, whereby men compute the angles, which a right line, drawn from the place of a sound to the head of a hearer, makes with the two planes described above, as well as their common section.—It is in vain to endeavour at an explanation of the phænomenon, by analogies borrowed from vision.—The spectator judges of the relative positions of visible objects, by knowing the situation which their images have on the retina, in respect to the axis of his eye;

but the person, who judges by the ear, has not the same advantage in measuring angles: for whatever may be the direction of a sound in the open air, as soon as it enters the auditory passage, it is compelled to follow the course of that duct, until it reaches the apparatus in which the sense of hearing resides.—In consequence of this restriction, all sounds whatever fall on the seat of sensation in the same direction; viz. in the ultimate direction of the auditory passage. The foregoing circumstance must, it should seem, unfit the ear for judging of the comparative positions of sounding bodies; because, if like effects follow like causes, we must conclude, that the ear is incapable of perceiving any angular variation, arising from the situations of sounding bodies in respect to itself: seeing the pulses, proceeding from any number of such bodies any how disposed, are forced, by the construction of the auditory organ, to strike the sensorium under a given angle; for, ultimately they all move parallel to a given right line.—The impossibility of explaining this problem by analogy, shews the necessity of examining the nature of hearing itself, in order to discover a proper way of investigating the difficulty.

The great sensibility of the ear is, in my judgment, the real cause of the phænomenon, which constitutes the topic of the present essay. But it will be proper to say something, in the first place, respecting the accuracy of this organ in distinguishing the difference of two sounds that are known to be nearly equal in force; because the truth of my opinion rests on this fundamental fact.—The want of a sure method of measuring the momentum of the air when agitated by a vibrating body, with the same certainty that the angles between rays of light are measured, appears to be the reason why the accuracy in question is so generally overlooked. But though it seems very difficult to give a general rule for measuring magnitudes of this description, the following experiment proves, in a very satisfactory manner, what a delicate faculty the sense of hearing is.—A bolt, driven by a spring against a fixed piece of metal, may be made to produce a succession of strokes of equal force; consequently the concussions given to the air, by any two of these strokes, will also be equal; and will therefore occasion like effects on the same ear, placed at equal distances from the spring, the state of the wind and weather being the same in both cases.—I caused an instrument of the preceding description to be struck repeatedly at the distance of 40 feet from my ear, care being taken to place it in the axis of hearing produced; after which, it was moved in the same right line sometimes two feet further from me, at other times two feet nearer to my person; and I could always distinguish distances thus varied. The range of the sound, or the distance at which it ceased to be audible, was 240 feet, or six times the interval made use of in the experiment. The sound which I employed was therefore of a moderate force, and perhaps the interval was a suitable one, being neither too great nor too little a part of the whole range. It appears then, that a good ear will discover a perceptible difference in the forces of two equal sounds; the one of which moves through one sixth part of its whole range, and the other through a space which differs from the distance of the former only the 120th. part of the range common to them both.—The foregoing instance affords a remarkable proof of the ear's accuracy in comparing slight variations in the momenta of sounds; and I have reason to believe that the delicacy of my organs, in this respect, surpasses the medium of sensibility; for, some ears which were tried in the same manner, did not perceive the effect in question, until the instrument had been moved four feet, or the 60th. part of my range. But either instance furnishes a proof sufficient for the present purpose, and shews the human ear to be a very delicate judge of comparative loudness.

The nice faculty of discriminating sounds, nearly equal in force amongst themselves, being established by the preceding experiment, it is to be applied in the next place to explain the phænomenon we are treating of. When we find that successive parts of the same sound preserve their force or loudness unaltered, we are taught by experience to conclude, that the sounding body also preserves its distance from us unchanged: on the contrary, when successive parts of the same sound grow stronger or weaker, we know with equal certainty, that the space interposed between our ears and the vibrating object is, for the most part, shortened and lengthened accordingly, by changing the place of the hearer or of the sounding body. Experience also teaches men to use the same nicety of perception, in ascertaining the positions of sonorous objects with respect to their own persons. In order to illustrate this question, it will be proper to begin with a simple example. Suppose then a sounding body, situated towards the front of the hearer, to lie in the horizontal plane, mentioned in the beginning of this essay, and to be placed on one side of the right line, which bisects the axis of hearing at right angles:— for instance, let it be to the right hand of it.

Moreover, we will take for granted, what will be afterwards proved; namely, that the pulses of air, proceeding from the vibrating surface, will strike the right ear, which is turned towards it, more forcibly than the left. Under these circumstances, two cotemporary currents of the same sound will strike the opposite sides of his head, at the same time, with unequal momenta: he is therefore left to form a judgment of the incidents; and the sensibility in question enables him to determine with certainty, which of his ears is most affected. A thousand similar cases have occurred in the course of his life, in which he has been convinced by the testimony of his eyes and hands, that the strongest impression is constantly made on that ear which is turned towards the sounding body; he therefore draws the same conclusion in those cases where he is not assisted by the evidence of sight and touch. In this manner every man acquires a practical rule, which, like all practical rules formed in infancy, becomes a mechanical action, and is therefore exercised by every body, while very few understand the nature of it. Some of my readers may suspect, that on the present theory, too much influence is ascribed to experience. But such a suspicion would in my opinion be ill founded; for, all the senses have their practical maxims, which they borrow mutually one from another. Opticians have demonstrated, that vision would prove but of little use, were not its natural imperfections corrected by touch: hearing also forms its judgment in one class of phænomena, from a kind of secondary knowledge afforded by the hands and eyes. Two sounds moving through unequal spaces may be equal in force, nay the remoter may prove the more powerful of the two: *loudness*, therefore, is not a sure indication of proximity; but every man has remarked, at an age when his memory was too weak to record incidents, that sounds lose more and more of their asperities the farther they move: every man, therefore, in his riper years, considers *roughness* to be a proof of proximity, and smoothness to be the criterion of remoteness. That this faculty is acquired, appears evident from the errors into which it leads the judgment under certain circumstances. I have known a person mistake the mellow tone, which is produced by rubbing the brim of a glass vessel with a wet finger, for the blast of a distant horn, though there was but the breadth of a table between him and the place of the sound, and almost every one falls into a similar deception the first time he hears the soft notes of an Æolian harp. If then the ear correct its imperfections in one instance, by means of information derived from the other senses, it will not fail to make use of the same aids as often as it can do so with advantage; it will therefore have recourse to this secondary species of knowledge, provided the pulses of air, proceeding from a sounding body placed not directly before or behind the hearer, can be proved to fall with a greater force on one of his ears than on the other. I am apprized that this supposition is not countenanced by the prevailing theory of the propagation of sounds: according to which, the pulses of air diverge from every point in the circumference of an obstacle towards all parts beyond that impediment. But this theory appears to be a mathematical conception rather than a fact; it is therefore better fitted to explain the outlines of hydrostatics, than to assist in the minuter parts of a physical enquiry. The laws and properties of a perfectly elastic fluid do not apply to the atmosphere without exception; and an occurrence, which is not unfrequent, seems to prove decisively, that the pulses of air which move parallel to the axis of hearing strike only one ear with effect, I mean that which opposes itself to their progress. If you happen to address yourself in an open area to a person who has the misfortune of being deaf on one side, having your mouth directed to his defective ear, he will perceive nothing, or at most but a very confused noise. This circumstance shews, that the pulses which pass immediately before and behind him, continue in their course without being deflected to the sound ear, which it screened by the interposition of the temples and face, from the action of those that strike the contrary side of the head. But the case would be otherwise, provided sounds diverged from all points indifferently; because on this supposition the ear could not be protected from their influence by its situation. Any person has it in his power to make the same experiment on himself; for let any one standing in the open air close one of his ears with moist paper, and cover the same side of his head with a folded napkin or cushion; if then a watch be held in the axis of hearing, at the distance of two or three inches from the napkin or cushion, he will not be made sensible of its presence by sound. Should the same experiment afterwards be repeated in a confined apartment, the person who makes it will perceive the clicking of the watch in the direction of his open ear, because the beats are reflected in their natural succession from the side of the room which is opposite to that ear. Opportunities sometime occur, that enable us to contemplate the same phænomenon on a larger scale. If a lofty and extensive building happen to intervene between a person in motion, and the place from which a loud noise proceeds, he no longer hears the sound in its proper direction, provided he passes very near the edifice, though at the same time he is convinced that the noise still continues, by hearing it reflected from another quarter. Let him, in the next place, advance in a right line perpendicular to the side of the building, and he will in a short time lose the echo, and recognize the original sound. Perhaps it may be imagined, that this change will not be observable before a right line, drawn from the head of the hearer to the sounding body, barely touches the top of the edifice: but this supposition, which is true when applied to the phænomena of shadows, cannot be relied on in the present instance. For as often as a sound meets with interruption, those pulses which pass nearest the obstructing body propagate themselves afresh into the air lying on the contrary side of the impediment, by means of the lateral communication of motion; on which account, an intercepted sound recurs at a less distance from the obstacle, than that which a geometrician would assign to the event, by drawing the right line specified above. By the way, we may observe, that the principle which Signor Venturi has lately denominated the lateral communication of motion to fluids, has so considerable a share in the various phænomena of sound, as to render geometrical speculations on the subject in a great measure useless. For instance, if a lofty obstacle interpose itself between a sounding body placed at the foot of it and a person standing at any distance on the opposite side, according to geometry, he will not be able to perceive any sound from it, because the pulses which should strike his ear with effect, will be intercepted in their progress by that obstacle; on the contrary, we are convinced by experience, that he will hear it, not indeed in the true direction, but as if it proceeded from some place elevated above the impediment. Sound, therefore, is propagated in a manner which neither coincides with the commonly received theory relative to the subject, nor with the phænomena of shadows; on the other hand, it seems to follow a law, that may be said to form a medium between the two.

The consequences of interrupting the pulses of sound, as well as the lateral communication of motion being now explained, the topic of the essay may be resumed. The annexed diagram will be found of use in explaining the phænomena which arise from the pulses of sound being obstructed by the hearers head, as they move in the horizontal plane passing through his ears, which case ought to be treated separately from the more complex one that comprizes the angle of elevation, along with the horizontal distance from the axis of hearing. When the sonorous object stands directly in front of the hearer, the semicircle ACB may be supposed to represent the horizontal section of his head, passing through the places of the ears, E and F, and the axis of hearing EF; also let G be the place of the sounding body, which, according to the conditions of the case, lies in the plane ACB produced, and likewise in the right line GS, which bisects EF at right angles: seeing then EF is bisected by the perpendicular SG, the arch ECF is also bisected by the same in the point C. Draw LG, GK, to touch the circle in T and P, then will the arcs TEC, and CFP be equal. Now all the pulses which do not move in right lines, contained in the angles TGS and SGP fly off without touching the circle; consequently they add nothing to the sound impressed on the ears by the body placed at G, whether the places E and F be supposed to lie in the arcs TC and CP, or without them. But the same number of pulses equal in force will fall in a given time, and in similar directions, on the arcs TEC and CFP as well as on the ears situated at E and F; and it is equally manifest, that the same reasoning will apply to

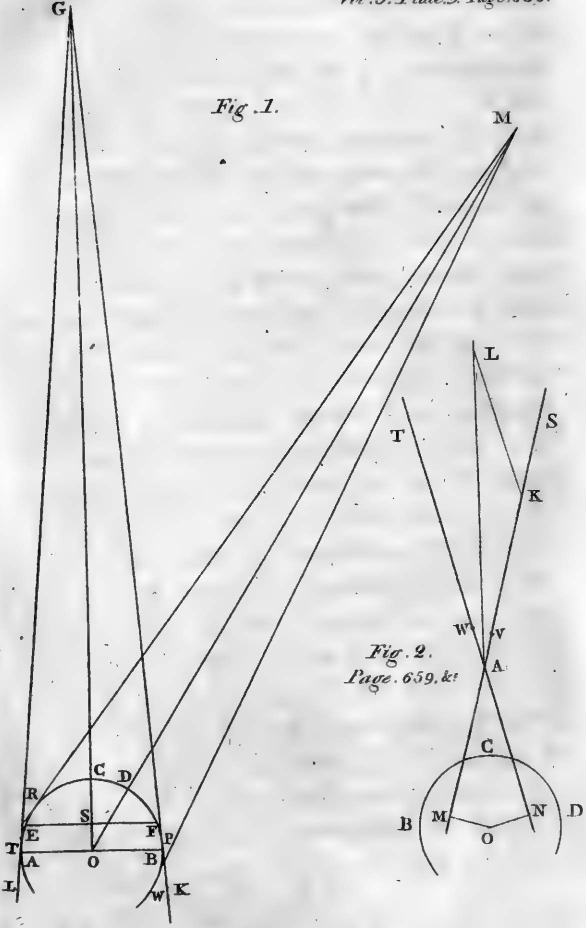


Fig. 1.

Fig. 2.
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two equal and similar solids, constructed upon the equal and similar planes, ECS and F CS. Now sound, though it be formed in the ears, is very much increased by the vibrations excited in the contiguous parts of the head by the pulses which fall upon them, as I shall endeavour to prove hereafter: therefore, as often as the two portions of the head, which are separated by the vertical plane perpendicular to the axis of hearing, are equally agitated by the pulses of the same sound, the ears are also equally affected from the same cause; which never happens, as we learn from the testimony of the other senses, unless the sounding body be placed somewhere in the right line that bisects the axis of hearing at right angles. In this manner men are taught by experience to draw a general inference from a general observation; they therefore conclude a body to be situated directly before or behind their persons, as often as the sound of it strikes both their ears with equal forces. The preceding demonstration elucidates the case of direct hearing, as far as this can be done by the assistance of a diagram; but the perception which determines the place of a sounding body to be in front of the hearer or behind him, requires a separate investigation; and I shall endeavour in the next place to ascertain the cause of it, by the following observations.

The head is a sensitive solid, and it perceives the impulses made on it by sounds much more exquisitely than men generally imagine. This sensibility is strongest in the auditory passages, and next to them in the parts immediately adjacent to the ears; nevertheless it diffuses itself more or less perfectly over the face, forehead, and temples, as well as all the external teguments of the skull. The sensation in question being of but little use independent of its connection with hearing, we for the most part mistake its true situation, and refer it to the organs of this sense, unless some circumstance, resembling the succeeding experiment, should happen to discover the nature of it to us. If any one will take the pains to close the orifices of his ears with wet paper, and will hold two slender rods of wood to his forehead or to one of his temples, taking care to keep the ends which are in contract with the skin separated by a small interval: and let another person at the same time touch the opposite ends of the rods with two watches, one of which does not move: the beats of the active watch will immediately pass along the stick, and make a sensible impression on the spot where its other extremity rests; which proves, that the bones of the head do not simply conduct sounds to the auditory nerves, but that the external teguments of this member also assist in discovering the directions of sounds by their sensibility. The same apparatus may be used to shew, that all parts of the head are not equally alive to the impulses of sounds; for a stick, which is of a proper length to impress the beats of a watch very faintly on the ear and parts adjacent, will prove too long to produce the same effect on the forehead, which is nevertheless much more exquisite in its feelings than the back part of the head.

The phænomena of oblique hearing remain to be explained; which case occurs as often as the sounding body is situated in the horizontal plane, but not in the right line that bisects the axis of hearing at right angles. Let M be the place of the sounding body, and draw MO to the centre of the circle; also let OC bisect the arc ECF, and take OG in it equal to OM: also draw WM, MR, PG, GI tangents to the circle. Now suppose a sound equal to that at M, to proceed from G, then the latter would have the same effect on the arc TCP that the former has on WDR, because the arcs are manifestly equal, and alike situated relative to the points M and G. But the sound proceeding from G is a case of direct hearing, consequently the ears placed at E and F receive equal impressions from it, which is not the case with the pulses that flow from M, For though the forces imparted to the two arcs, TCP, WDR are equal, they do not fall equally on the circle in respect to the points E and F, which represent the ears; the sound therefore coming from M strikes these organs with unequal forces, as may be easily inferred from the figure.

What I have just now demonstrated by help of a semicircle may be said with equal justice of any solid, such as the human head, that can be divided vertically into two equal and similar portions by the plane, to which the axis of hearing is perpendicular. This assertion is too evident to need a laboured demonstration. It was observed in a former part of this paper, that if a person stop one of his ears, and hold a watch near it in a close room, he will hear the sound of it in the direction of the open ear by reflection, which circumstance may be sufficient to establish the truth of the preceding theory: but, in order to diversify the proof, the following experiment was tried. I took a wooden fork made of a stick of ash, which was split more than two-thirds of its length for the purpose; the points of the two branches turned inwards, and were placed at a proper distance to receive the head between them; a watch was then suspended upon the haft, or undivided part of the fork, after which the ends of the branches were brought into contact with two plugs of wet paper that closed my ears; in consequence of which I made the following conclusive remarks. When both points pressed with equal forces on the plugs of paper, I judged the watch to be directly before me from its beating: but as often as the pressure of one of the points was augmented, the sound seemed to quit its station in front of me, and to incline towards that side where the greater force was applied; lastly, I discovered, by using only one branch of the fork, that an increase of the pressure increased the effect of the same sound. The two cases of horizontal hearing being by this time pretty fully considered, it is necessary to change the subject, that the method whereby we judge of *elevation* by the ear may be examined in its turn.

For this purpose, suppose a right line to join the sonorous object, and the centre of the axis of hearing; also conceive a plane to pass through the same centre, to which the same right line is perpendicular. Then it is evident that all the pulses, which are impressed by the sonorous object on the head, must fall on that part of it which lies between the plane and the place of the sound; consequently that portion of the head is the seat of the sensation excited by the sonorous object; because the head is a sensitive solid, and capable of topical irritation arising from the impulses of sounds. Seeing now we are convinced, by a delicate sort of touch, what part of the head is affected by the strokes of the sonorous object, and are at the same time well acquainted with the form of this member of the human body, we judge accurately what is the situation of the excited portion of it, relative to the centre of the axis of hearing: in consequence of this judgment, we also determine the position of the plane last mentioned, which forms the base of the portion under consideration, by dividing it from the rest of the head.

But the right line which joins the middle point of the axis of hearing and the sonorous body, points out the direction of the sound, which line is perpendicular to the given plane in a given point; for which reason it is given in position: or, in other words, the direction of the sound in respect of fixed points in the hearer's head is determined: and this is done with a degree of accuracy, which I believe very few people are aware of. In order to satisfy my own curiosity on the subject, I tried an experiment, with a view to discover what is the least sensible variation of a given sound from the perpendicular to the axis of hearing, supposing the deviation to be measured by an arc of the horizon; and I found the angle comprehended by this perpendicular, and the sound's path, did not exceed eight degrees.

The experiment was tried in an open plain, to prevent the intrusion of reflected sounds as much as possible. The instrument made use of was that which has been already described, as consisting of a bolt driven by a spring against a metal button; and the distance from my person to this sounding body was kept equal to forty feet, in the different parts of the experiment. I also discovered, by the assistance of the same apparatus, that the least sensible angle of elevation above the horizontal plane, was not more than ten degrees at the last mentioned distance; the sounding body being placed in the vertical plane, to which the axis of hearing is perpendicular. But the foregoing observations are not to be adopted and used as fixed rules, for much depends on the comparative sensibility of the auditory organ in different persons; and an alteration in the force of a sound will, without doubt, make a considerable change in the result of the experiment on elevation: because I have observed, that the feeble sound of a distant watch, though sufficient to shew whether it came from my right or left hand, was too weak to point out its situation, relative to the horizontal plane passing through my head. This imperfection in the sense of hearing, if it may be called one, is in all probability owing to the want of sensibility in the upper part of the head and lower part of the face, which defect renders them incapable of perceiving the impulses of feeble sounds.

The faculty of hearing which I have been investigating, betrays men under certain circumstances into errors, that appear the more surprizing, because the judgment relies on the admonition of the ears with the greatest confidence. The theory of these deceptions will therefore form a proper supplement to this essay. Mention has been already made of the sudden change that takes place in the sensible direction of a sound, as soon as the direct communication with the sounding body happens to be broken by the intervention of a lofty obstacle, provided the sound in question be loud enough to produce an echo from another quarter. Any person who has had occasion to walk along a valley obstructed with buildings, at the time that a peal of bells was ringing in it, will assent to the truth of the circumstance here alluded to. For the sound of the bells instead of arriving constantly, at the ears of a person so situated, in its true direction, is frequently reflected in a short time from two or three different places. These deceptions are in many cases so much diversified by the successive interpositions of fresh objects, that the steeple appears, in the hearer's judgment, to perform the part of an expert *ventriloquist* on a theatre, the extent of which is adapted to its own powers, and not to those of the human voice.

The phænomenon has often attracted my attention; and the similarity of effect which connects it with ventriloquism, convinces me every time I hear it, that what we know to be the cause in one instance is also the cause in the other: I mean that the echo reaches the ear, while the original sound is intercepted by *accident* in the case of the bells, but by *art* in the case of the ventriloquist. In order that the cause which gives rise to the amusing tricks of this uncommon talent may be pointed out with the greater clearness, it will be proper to describe certain circumstances that take place in the act of speaking, because the skill of the ventriloquist seems to consist in a peculiar management of them. Articulation is the art of modifying the sound of the larynx, by the assistance of the cavity of the mouth, the tongue, teeth, and lips. The different vibrations, which are excited by the joint operation of the several organs in action, pass along the bones and cartilages, from the parts in motion to the external teguments of the head, face, neck, and chest; from which, a succession of similar vibrations is imparted to the contiguous air, thereby converting the superior moiety of the speaker's body into an extensive seat of sound, contrary to general opinion, which supposes the passage of the voice to be confined to the opening of the lips. There are but few persons, I imagine, who have not some time or other witnessed an incident, which shews the vulgar notion to be erroneous in this particular. For if a man standing in a close apartment should happen to apply his face to a loop-hole, or narrow window, in order to speak to some person in the open air, a by-stander in the room with him will hear his voice, not indeed in its natural tone, but as if it were smothered by being forced to issue from a hollow case; but the circumstance of his words being heard distinctly, by one who cannot receive them from his mouth, proves the vibrations requisite for their production to be conveyed through the solid parts of the speaker's body, agreeably to the preceding assertion. The reason why we generally conclude the voice to be confined to the opening of the mouth, appears to be this. Those pulses which escape from the aperture are the strongest, they therefore surpass the weaker vibrations of the contiguous parts; for when a number of sounds moving in different directions strikes the ear at the same instant, the hearer does not notice their several places, but refers all of them to the quarter in which the most powerful is perceived. For instance, when a man stands at a sufficient distance from an extensive obstacle, his words are answered by an echo; but let him make a loud uninterrupted noise, neither he nor any body near him hears two voices whilst his continues, but as soon as the noise ceases the echo is perceived. This does not happen because the one begins the moment the other ends; but the reflected sound being the weaker of the two, it is smothered by that which precedes it.

We have seen in what manner secondary or reflected sounds are smothered by their principals; but though the places of such sounds are not recognized by the ear, their effects do not die away unnoticed: for the reverberated pulses mingle with those which come immediately from the sounding body, and thereby alter the sensation, which, without their interference, would be less compounded. This is the reason why the same musical instrument has one tone in a close chamber, where its notes undergo a multiplicity of reverberations, and another in the open air, where the reflections are few in comparison.

But it is time to apply the preceding facts to the subject in hand; and it will be proper to begin with a familiar example. When an orator addresses an audience in a lofty and spacious room, his voice is reflected from every point of the apartment, of which all present are made sensible by the confused noise that fills up every pause in his discourse; nevertheless every one knows the true place of the speaker, because his voice is the prevailing sound at the time. But were it possible to prevent his words from reaching any one of the audience directly, what then would follow? Undoubtedly a complete case of ventriloquism would be the consequence, and the person so circumstanced would transport the orator, in his own mind, to the place of the principal echo, which would perform the part of the prevailing sound at the instant. This he would be obliged to do, because the human judgment is bound, by the dictates of experience, to regard the person as inseparable from the voice; and the deception in question would be unavoidable, being produced by the same concurrence of causes which makes a peal of bells, situated in a valley, seem to change place in the opinion of a traveller. It is the business of a ventriloquist to amuse his admirers with tricks resembling the foregoing delusion; and it will be readily granted, that he has a subtle sense, highly corrected by experience, to manage, on which account the judgment must be cheated as well as the ear. This can only be accomplished by making the pulses, constituting his words, strike the heads of his hearers, not in the right lines that join their persons and his. He must therefore know how to disguise the true direction of his voice, because the artifice will give him an opportunity to substitute almost any echo he chuses in the place of it. But the superior part of the human body has been already proved to form an extensive seat of sound, from every point of which the two pulses are repelled, as if they diverged from a common centre. This is the reason why people, who speak in the usual way, cannot conceal the direction of their voices, which in reality fly off towards all points at the same instant. The ventriloquist therefore, by some means or other, acquires the difficult habit of contracting the field of sound within the compass of his lips, which enables him to confine the real path of his voice to narrow limits. For he, who is master of the art, has nothing to do but to place his mouth obliquely to the company; and to dart his words, if I may use the expression, against an opposing object, whence they will be reflected immediately, so as to strike the ears of the audience from an unexpected quarter, in consequence of which the reflector will appear to be the speaker. Nature seems to fix no bounds to this kind of deceptions, only care must be taken not to let the path of the direct pulses pass too near the head of the person who is to be played upon; for, if a line joining the exhibitor's mouth and the reflecting body approach one of his ears too nearly, the divergency of the pulses will make him perceive the voice itself instead of the reverberated sound.

The only ventriloquist I ever attended, acted in strict conformity to the preceding theory of this curious paradox in the science of acoustics. His audience was arranged in two opposite lines, corresponding to the two sides of a long narrow room. The benches on which they were seated reached from one end of the place to the middle of it, the other part remaining unoccupied. The feats exhibited by him were the three following. *First:* he made his voice come from behind his audience, but it never seemed to proceed from any part of the wall, near the heads of the people present; on the contrary, it was always heard resembling the voice of a child, who seemed to be under the benches. He stood during the time of speaking in a stooping posture, having his mouth turned towards the place from which the sound issued; so that the line joining his lips and the reflecting object, did not approach the ears of the company. *Second:* advancing into the vacant part of the room, and turning his back to the audience, he made a variety of noises, that seemed to proceed from an open cupboard which stood directly before him, at the distance of two or three yards. *Third:* he placed an inverted glass cup on the hands of his hearers, and then imitated the cries of a child confined in it. His method of doing it was this; the upper part of the hearer's arm laid close along his side; then the part below the elbow was kept in an horizontal position with the hand turned downwards, which was done by the operator himself. After taking these preparatory steps, the man bent his body forwards in a situation which presented the profile of his face nearly to the front of his hearer, whilst his mouth pointed to the cup; in which posture he copied the voice of a confined child so completely, that three positions of the glass were easily distinguished by as many different tones, viz. when he pressed the mouth of the cup close against the palm, when one edge of it was elevated, and when the vessel was held near the hand but did not touch it. The second and third instances of ventriloquism afford strong proofs, that this delusive talent is nothing more than the art of substituting an echo for the primary sound; for, besides the change perceivable in the direction of the voice, it was found to be blended with a variety of secondary sounds; such as we know by experience are produced as often as a noise of any kind issues from a cavity. I have already made some remarks on this species of knowledge; but it would be improper to dismiss the subject without noticing the accuracy, with which the ear recognizes the finer modifications of sounds, and their causes. I have frequently observed, that a certain waterfall makes a flatter and duller noise when the ground is covered with snow, than that which it affords at other seasons. The human voice also undergoes a similar change within doors, by striking a multiplicity of soft bodies, such as a number of piles of wool, or a crowded congregation in a church.

The method of preventing the vibration of the vocal organs from reaching the external teguments, is still wanting to complete this theory of ventriloquism; and I presume it can only be supplied by an adept in the art. I must therefore dismiss the subject unfinished, because I have no pretension to that character.

The THEORY *of* COMPOUND SOUNDS.

BY MR. JOHN GOUGH.

COMMUNICATED BY DR. HOLME.

Dr. Smith, author of the work on Harmonics, takes for granted in his theory of compound sounds, that the pulses which proceed at the same time from a number of sounding bodies, do not clash, or obstruct one another, in their passage through the air. According to this hypothesis, each set, of any number of cotemporary sets of pulses, strikes the ear without being confounded with the rest; in consequence of which, any number of sounds may be distinctly perceived at the same time. On this supposition, a compound sound is a sensation rendered variable by the irregular manner in which the pulses of the constituent sounds succeed to one another. For, if the intervals of time between two successive pulses of one of the constituent sounds be not equal to the same intervals belonging to the sound or sounds which accompany it, the secondary intervals, or small parts of time separating the pulses which fall in succession on the ear, will vary in magnitude; in the same manner that the distances between the figures upon the face of a barometer and its nonius vary, none on the slider coinciding with those on the fixed plate excepting the highest and lowest. I have chosen this familiar instrument to illustrate Dr. Smith's method of explaining the physical cause of compound sounds, because it affords a visible example of a cycle of pulses, according to his notion of the subject.

The sketch which I have exhibited of Dr. Smith's hypothesis shews, that he allowed that a number of simple sounds might exist in concert, and strike the ear in a distinct manner, without suffering any interruption in their motions from the interference of their pulses. But a late writer on sound rejects this axiom in Harmonics as a mathematical inconsistency; and substitutes the following theory of compound sounds in the room of it. If two musical strings, differing in their times of vibration, happen to vibrate in concert, they do not occasion two distinct sounds in the opinion of this gentleman, because the strings agitate the air in conjunction; consequently the pulses, which one of them would actually form in an undisturbed atmosphere, must unavoidably clash with those which the other string would produce in similar circumstances. Hence the waves of air belonging to both strings are interrupted in their natural progress, and are compelled by their mutual interference to coalesce, thereby producing a new succession of pulses, constituting a single sound in the place of two. This sound is of a peculiar kind; for, the pulses of which it consists, are separated by unequal intervals of time, and disposed in cycles.

The merit of the preceding theory, when compared with Dr. Smith's hypothesis, must be ascertained by contrasting it with a variety of facts, which are furnished by the phænomena of compound sounds, and make a part of every man's experience. For, if it be found upon examination to be repugnant to these facts, it will prove inconsistent with nature, and cannot fail of disappointing the inventor's expectations.

Were it possible for a number of sounds to coalesce, and form but one, the compound would acquire sensible properties peculiar to itself, and at the same time lose the distinguishing characters of its elements, some of which are incompatible with the qualities of an individual. On this supposition, the presence of the constituent sounds could not be detected by the ear in this newly created being: on the contrary, an experimental process would be required to analyse every compound sound the first time it attracted a man's attention, for the same reason that a chemist finds it necessary to analyse a substance with which he is unacquainted. The abstract term *coalescence* is used, in a physical sense, to signify any intimate union of bodies or the powers of bodies; and the introduction of the term into language proves the existence of the principle in nature, or more properly in the human mind. For, when a number of agents act in conjunction upon one of the senses, we have two ways of conceiving their mode of operation. If the sensible effects of each agent be distinctly perceived, we attribute a separate action to every member of the assemblage, and call the aggregate a mixture: this is the conclusion of a person who tastes an infusion of pepper in vinegar. On the other hand, when we know that certain agents are present without being able to recognize their distinguishing powers, in the room of which we find qualities of a different description, we pronounce the aggregate to be in a state of coalescence. This is the situation of the chemist, who tastes common salt, but cannot perceive the presence of soda and the muriatic acid. It is my business then to prove compound sounds to be *mixtures*, not aggregates by coalescence. This I shall endeavour to do, by shewing that they have properties which belong not to individuals, such as a number of tones, a variety of directions, and several sets of pulses.

First, the tones of a flute and violin are as distinct to sense as any two things can be when they are sounded separately; and I appeal to common experience to determine, if they are not equally distinct when heard in concert. Taking it for granted that the answer will be in the affirmative, I pronounce the aggregate to be a mixture of sounds in one case. Secondly, if a violin sound in front of the hearer, and a flute be heard at the same time in an oblique situation, the person thus circumstanced is able to determine the relative positions of the two instruments, which shews the aggregate to have two cotemporary directions. It is therefore a mixture of sounds, not a single sound. Thirdly, I have found by making the experiment, that any number of musical strings may be made to vibrate by a compound sound acting upon them, provided this compound be occasioned by an equal number of strings with the former, having one in the latter set in unison with each one in the preceding set: This is an experimental proof that there are as many sets of pulses in an aggregate of sounds as that aggregate contains elements, because no string whatever is in unison with a concord or discord. Lastly, if it were possible for sounds to coalesce, men would never hear any thing more than one noise at one time: The general hum would have varied perpetually from the extinction of existing sounds, and the intrusion of fresh ones; but the human mind would have had no conception of two cotemporary sounds; because the ear being in that case incapable of conveying the complex sensation, the idea of such an existence would have transgressed the sphere of human knowledge. The preceding arguments are drawn, for the most part, from common experience; and they shew, that the free passage of cotemporary sounds through the air may be safely admitted as an axiom in harmonies. I shall therefore proceed to prove the same proposition to be consistent with the doctrine of forces.

The propagation of sound through the atmosphere, and the nature of aëreal pulses are commonly explained in elementary books of natural philosophy; I shall, for this reason, enumerate only a few particulars, the recollection of which will be found useful.

Proposition I. Two contiguous particles of air which are agitated by a vibrating body, either directly or by the intervention of an elastic medium, receive two motions from each impulse; *first*, an absolute motion carries them to a greater distance from the sounding body, and afterwards brings them towards it again, both the progress and regress being performed in the time of a single vibration: *second*, a relative motion resulting from the former, compels the two particles to approach and recede alternately, which double motion is also accomplished in the time of a single vibration.

Proposition II. Both the absolute and relative motions are greatest amongst those particles which are nearest the sounding body, and they diminish as the distance from that body increases; but, in all cases, the change of place is too small to be perceived by the ear, on which account every particle preserves a fixed position in respect of this organ and its connections.

For each corpuscle is confined within the circumference of a physical right line, the diameter of which is determined by its own absolute motion.

Proposition III. If two sounding bodies, affording different notes, act in conjunction upon the same particle, through the media of two right lines of similar particles connecting them with it, this particle will be urged at the same instant in the direction of these lines, by two forces having an inconstant ratio.

For, let the particle A be urged, by the acuter sound, in the line SA, and by the graver, in the the line TA; (*vide* fig. 2, plate 9, page 636.); then the contiguous particle V, placed in SA, will approach to, and recede from A more frequently than W, similarly placed in TA, by Prop. I.: consequently the force of V upon A will vary in a quicker manner than the force of W upon A; but this variation of ratio is limited in time; because it evidently begins and ends with the cycle of the vibrations of the sounding bodies.

PROPOSITION IV. The coalescence of two sounds is impossible on mechanical principles.

For, suppose the thing possible; then the coalescence of two sounds requires, that a particle of air should possess a motion, compounded of the motions which the two sounding bodies would impart to it separately; and that this compound motion should act in a given right line, for an assignable part of time, otherwise it could not excite a similar motion in the elastic particles occupying that given right line. Let A be such a particle, and let the construction used in the last proposition, be retained; consequently (Principia, Prop. 23, Lib. 2.) VA and AW are in the ratio of the forces that act at any moment in the right lines TA and SA. Make AK as AW, and draw KL parallel to AW, and make it as AV; also join AL; then will the particle A be urged in the direction LA at that instant. But the ratio of AK to KL varies perpetually, by Prop. III.; therefore the species of the triangle AKL is equally inconstant; consequently the compound force does not act in a given direction for an assignable part of time. Now the production and propagation of motion in a given right line requires force to be combined with time, which combination is wanting in the present instance; wherefore the coalescence of sounds is impossible.

PROPOSITION V. It may be demonstrated from mechanical principles, that a number of distinct cotemporary sounds cannot do otherwise than produce distinct sensations.

In order to make the necessary diagram as simple as possible, let the directions of two cotemporary sets of pulses be represented by the right lines SM and TN, lying in the same horizontal plane, and intersecting in the point A; also, let BCD be the horizontal section of the hearer's head, made by the same plane; and suppose the centre of the axis of hearing to be at O; draw OM, ON perpendicular to SM, TN. Now I have shewn in the preceding paper, that if a set of pulses move in either of the right lines SM, TN, it will excite a sensation in that part of the head which is cut off by a vertical plane, passing through one of the perpendiculars OM, ON. It also appears from the last proposition, that the impulses of the vibrating bodies, acting in the lines SA, TA, do not compel the particle A to move in any given intermediate direction, as LA. But, according to the second proposition, the position of the particle A, is fixed in respect of the planes MO, NO; that is, though the corpuscle actually changes place, in respect of the geometrical point A, it is always found in the intersection of the physical right lines SM, TN. Now the two vibrating bodies continue to act in the directions of these right lines, consequently the particle A is constantly urged in these lines by two forces, which, though variable in magnitude, are combined with time; which circumstance enables the corpuscle to transmit the impulses of one body to M, and those of the other to N. What has been demonstrated of the particle A, may be affirmed of any other particle, which is the intersection of two right lines parallel to SM, TN; in other words, it may be affirmed of two sets of pulses; and the same demonstration may be extended to three sets, &c.

Corollary 1. The substance of this and the preceding proposition will apply to all elastic mediums; hence it happens, that a plate of glass &c. in a state of vibration, will conduct a foreign sound, whilst it produces one of its own; for the same reason, if light be considered as a vibrating medium, one particle of the luminous fluid may assist in transmitting two sensations.

Corollary 2. When the inclination of the planes MO, NO, is less than a given angle, the ear cannot distinguish the relative positions of the sounding bodies; it therefore refers them to the same place.

The first time I perused Dr. Smith's Harmonics, Dr. Young's objection occurred to me; but the preceding train of arguments removed the scruple, without discovering the author's reasons for treating this article of his work with so much brevity. Perhaps the demonstration, which cost me an effort of study, was an intuitive conclusion in his comprehensive mind. As soon as the proposition was established, I assented to his definition of an interval of sound, allowing it to be a quantity of a certain kind, terminated by a graver and an acuter sound. The demonstration of Prop. V. convinced me, that intervals of this sort may be subdivided by the interposition of one or more intermediate sounds, which concession formed the basis of my analysis of the human voice.* Speculative men may differ in opinion about the origin of the small intervals which form the tones of various voices; but they must exist, whether we ascribe them to an undulating motion like that of a stretched cord, or to the cotemporary vibrations of a system of elastic bodies. It does not appear, that Dr. Young was acquainted with my paper at the time he composed his own; but he found it necessary to allow the tone of the larynx to receive various modifications from the vibrations of the adjacent

* Page 58.

parts. His theory therefore differs from mine in this particular only: he pronounces the voice to be a compound by coalescence; I deny the possibility of such a compound, and call it a mixture of imperfect unisons. This mixture appears to be a single sound, because it has but one direction; for the proximity of the various parts contributing to the formation of it, disqualifies the ear, so that it cannot perceive their relative positions, and compels it to refer them all to one place, by Corollary 2 to proposition V.

A certain class of sounds, which, for the sake of brevity, were not noticed in my paper on the voice, deserve a place in the present communication. If a finely-toothed file pass slowly over a smooth elastic substance, such as a piece of horn, it makes a grating noise; but if the velocity of the instrument be sufficiently increased, a continued sound is produced, which becomes more or less acute, by giving a quicker or slower motion to the file. The grating noise is occasioned by a succession of short interrupted sounds, resulting from the united vibrations of the file and the body it scratches; but the quick succession of these sounds, caused by an increase of velocity, gives rise to a secondary sound resembling the harmonical notes, being produced by a like cause. Now this sound becomes a primary object with the ear, in all probability because the pitch of it may be varied; for the first sounds proceeding from the action of the file, evidently supply nothing but the tone. Many instances of the kind occur in art and nature: the notes of all reed-instruments are of this description, and the voice must be referred to the same class, because the larynx resembles a reed-instrument in structure.

METEOROLOGICAL OBSERVATIONS.

BY JOHN DALTON.

Observations on the Barometer, made at Manchester, for 1801.

	Mean.	Highest.	Lowest.
Jan.	29.59	30.05	28.98
Feb.	29.56	30.02	28.87
Mar.	29.61	30.20	28 68
April	29.86	30.20	29.10
May	29.65	30.00	29.20
June	29.88	30.11	29.53
July	29.65	30.10	29.13
Aug.	29.88	30.15	29.42
Sept.	29.73	30.12	29.11
Oct.	29.62	30.15	28.73
Nov.	29.55	30.07	28.68
Dec.	29.29	39.00	28.51
	29.66	Mean for the year.	

The observations were made three times a day.

It is now well known that the fluctuations of the barometer are not local, but extend over a considerable portion of the earth: the extremes, whether high or low, usually take place on the same day in Great Britain, France, Germany, and Russia, and seldom differ more than one day. If a number of barometers were stationed at equal distances over the surface of the globe, and cotemporary observations made on them for a year or more, we should then probably be in possession of facts from which a rational theory of the variation of the barometer might be derived. Observations made in different parts of the same province or country seem not now of much importance in this respect.

Observations on the Thermometer, made at Manchester, for 1801.

	Mean.	Highest.	Lowest.
Jan.	39°·3	52°	23°
Feb.	39·4	52	28
Mar.	42	57	27
April	46·5	68	28
May	51·9	67	38
June	56·3	73	40
July	58*	—	—
Aug.	62·1	80	53
Sept.	56	67	47
Oct.	49·2	60	35
Nov.	39·6	54	26
Dec.	34·6	45	20
Mean	48		

The observations were made three times a day; namely about 8 A. M. and 1 and 11 P. M. The mean obtained from them is probably below the true mean temperature.

* The observations in July were interrupted.

The mean of the morning observations through the year was 46°.5; at 1 P. M. 52°.5; at 11 P. M. 45°: the mean taken upon the 1 and 11 o'clock observations would be nearly 50°, which agrees with the temperature of most springs in this place.

Account of Rain at Manchester.

	1794, Inch.	1795, Inch.	1796, Inch.	1797, Inch.	1798, Inch.	1799, Inch.	1800, Inch.	1801, Inch.	Mean, Inch.
Jan.	1. 09	0. 95	3. 10	1. 58	2. 70	1. 76	2. 86	2. 42	2. 06
Feb:	3. 77	2. 46	2. 29	1. 21	1. 91	4. 16	0. 44	1. 85	2. 26
Mar.	2. 03	2. 02	0. 58	0. 94	1. 18	2. 16	2. 37	2. 89	1. 77
April	3. 91	3. 23	1. 60	2. 18	1. 37	2. 19	4. 10	0. 79	2. 42
May	2. 27	1. 01	5. 09	5. 96	1. 48	2. 13	2. 85	2. 60	2. 92
June	1. 25	2. 06	2. 35	4. 26	0. 85	2. 05	0. 71	0. 53	1. 76
July	3. 40	3. 01	5. 19	2. 52	4. 75	4. 08	0. 29	4. 85	3. 51
Aug.	4. 38	4. 28	1. 04	5. 49	4. 13	8. 74	1. 05	0. 73	3. 73
Sept.	4. 66	0. 46	2. 63	3. 86	3. 35	5. 35	6. 55	6. 41	4. 16
Oct.	5. 10	5. 49	2. 87	2. 31	3. 51	3. 77	4. 37	4. 27	3. 96
Nov.	3. 45	4. 16	2. 13	2. 99	3. 68	1. 90	3. 70	3. 15	3. 14
Dec.	2. 11	3. 61	1. 76	5. 50	2. 35	0. 35	3. 05	4. 56	2. 91
	37. 42	32. 74	30. 63	38. 80	31. 26	38. 64	32. 34	35. 05	34. 60

Hence the annual quantity of rain on an average of 8 years, is 34. 60 inches.

Mr. George Walker of Salford, makes the average rain for the same time 38. 5 inches. He seems to think his measure has somewhat over-rated the quantity; and it is not improbable mine may be too little: add to this, the gages are a mile and a half distant, and mine on more elevated ground.

There were 160 days in the year at an average, on which rain more or less fell.

I kept a rain-gage on the top of St. John's steeple, Manchester, from midsummer 1797 to the end of 1798; and another on the ground in the vicinity, about 50 yards perpendicularly below. In summer the ratio of the rain below to that above was 3 : 2 nearly; in winter it was 2 : 1 nearly.

Rain at Kendal, 1793—52. 74; 1794—69. 04; 1795—56. 25; 1796—45. 73; 1797—56. 83; 1798—54. 63; 1799—56. 93; 1800—48. 20; 1801—50. 612; uniting these observations with those for the five preceding years (see my Meteor. Essays) we obtain 58.1 inches the mean annual rain at Kendal for 14 years.

Observations on the Winds for 1801.

North, 44: North East, 277: East, 11: South East, 19: South, 22: South West, 412: West, 153: North West, 12. Total 900.

The South West and the North East winds, as usual, have been the most prevalent; they are in fact winds that properly belong to the northern temperate zone, arising from the two general currents of air tending from and towards the equator. (See my Meteorological Essays, page 91).

Account of the Evaporation from Water, in a Cylindrical Vessel of 10 *inches diameter, kept nearly full, at Manchester.*

	1799. Inch.	1800. Inch.	1801. Inch.	Mean Evap. Inch.
Jan.	———	———	———	1. 5 *
Feb.	———	———	———	2. 0 *
Mar.	1. 082	3. 700	———	3. 5 *
April	5. 398	4. 760	———	4. 5 *
May	5. 050	5. 228	4. 600	4. 959
June	7. 702	5. 207	6. 551	6. 487
July	5. 157	5. 679	6. 048	5. 628
Aug.	6. 000	6. 376	5. 798	6. 058
Sept.	4. 340	3. 986	3. 368	3. 898
Oct.	3. 337	1. 998	1. 718	2. 351
Nov.	2. 428	1. 600	2. 098	2. 042
Dec.	1. 384	———	———	1. 5 *
				44. 4 Ann. Ev.

N. B. Those means marked * are conjectural; the frost in the winter season usually damaged the gage.

The mean annual evaporation from soil covered with grass, or from green ground, exclusive of dew, was at an average for the three years above, 23½ inches. (See page 361 for the three years preceding.)

Observations relating to Hygrometry.

One important attainment in meteorology is the knowledge of the quantity of aqueous vapour existing in the atmosphere at any time. From this, and the observed temperature of the air, we can easily ascertain its disposition for the evaporation or precipitation of water; or in other words for *fair* or *rainy* weather. Various instruments have been used to determine the quantity of vapour in the air. The hygrometer of De Saussure seems to gain the most credit; but I have in a former essay assigned a reason for disallowing it. It is acknowledged that an hygrometer ought to indicate that degree of cold which is necessary to make the air begin to part with vapour, or to form dew upon the surfaces of bodies; now as this is at all times capable of being effected by a simple experiment, (see page 581) it almost totally supersedes the necessity of a dubious, very delicate, and consequently easily injured instrument.

In my journal for a year and a half past, I have had a column entitled, *vapour point*, in lieu of the hygrometer column. By vapour point I mean that degree of the thermometer at which dew begins to be formed at the time. The higher this point is, the greater is the quantity and force of vapour in the atmosphere, as is shewn by the table at page 559; and the lower it is with respect to the actual temperature of the atmosphere, the greater is the force of evaporation.

1800. *July.* Mean vapour point for 21 days =53°; highest 62°; lowest 40°.

Aug. Mean for 11 days =56°; but too high for the monthly mean: highest 60°.

Sept. Vapour point above 50° for 6 days; highest 60°.

Octo. Vapour point mostly below 52°; highest 59°.

1801. *May.* Vapour point above 50° for 4 days; highest 55°.

June. Mean for 10 days, 49½°; highest, 57½°; lowest, 30°*.

July. Mean for 8 days, 53°; highest, 56°.

Aug. Mean for 22 days, 54½°; highest, 61°.

Sept. Mean for 14 days, 54°; highest, 60°.

Octo. Vapour point for 5 days, above 50°; highest, 57°.

Nov. Highest, 54°; lowest, 22°.

Dec. Highest, 44°; lowest, 18°.

* On the 13th. great damage done to potatoes, &c. by the cold which accompanied this remarkably low state of vapour for the season. It was 46° on the 12th. and 40° on the 14th.

The mean monthly vapour point may also be inferred from the monthly evaporation and mean monthly temperature, according to the theory in the Essay, page 574.—Let August be taken for an instance: the evaporation was 5,798 inches= 1312 grains from my small gage, which is nearly 6 inches in diameter: but $1312 \div 1440$ (= min. in 24 hours) = .91 gr. per minute, the mean rate of evaporation. The mean temperature (taken from the mean noon and evening observations) was 63°, with which entering table at page 587, we obtain 3. 63 grains, the evaporating force at that temperature (taken from the third column, because the evaporating gage is in a very open situation). Subtracting .91, there remains 2. 72 grains, corresponding in the said table to 54°. 3, the mean vapour point. The mean derived from actual observation is stated above to be $54\frac{1}{2}$°.

AURORÆ BOREALES:

Observed since 1793*.

1794. January, 7 and 22. March, 8 and 29. December, 8 and 19.—Total, 6.

1795. September, 8 and 14.—Total, 2.

1796. None observed.

* For those observed by me preceding that period, see my Meteorol. Obs. page 54.

1797. January, 22. Feb. 1, 18, 27 and 28. March 2 and 10. April, 24. Nov. 18, 21, 22 and 23. Dec. 20.—Total, 13.

1798. None observed.

1799. September, 3. Octo. 25.—Total, 2.

1800. March, 18. Nov. 2 and 7. Dec. 10.—Total, 4.

1801. January, 4 and 25. Feb. 22. Aug. 18. Octo. 6.—Total, 5.

In all, 32.

The *Auroræ* have been much less frequent in the above period than for the same number of years before.

I observed 53 of them in 1788.

All the phenomena corroborated the notion maintained in my Essays abovementioned; namely, that the luminous beams of the *Auroræ* are cylindrical, magnetic, parallel to each other and to the dipping needle. The centre of each *Aurora* uniformly appeared to be in the magnetic north.

APPENDIX

I.

Explanation of a Roman Inscription by Mr. THOMAS BARRITT: *with a Note on the same subject by* Dr. HOLME.

The stone (see plate VII.) found in the year 1795 in the Castle-field Manchester, (the Mancunium of the Romans) from what appears of the inscription, seems to have been a votive one, dedicated to Jupiter, by the first Frisian Cohort stationed there, in the 24th year after their arrival.

Camden mentions two inscriptions at Manchester, found in the old Mancunium; one he says he saw himself, and another was copied for him by the famous mathematician, Dr. Dee, warden of Manchester College. They were placed there in memory of two Centurions, who in their turn had commanded the Frisians, under the Roman government, for 23 years.

The Frisian Cohort at Mancunium is supposed to have been part of, and to have belonged to the sixth Roman legion which was stationed at York, and stiled Victrix; but it may admit of a doubt whether this cohort did not belong to the 20th legion, stationed at Chester, and likewise stiled Victrix.

In Archæologia, vol. 3, page 236, the late learned and Rev. John Watson, rector of Stockport, exhibited a drawing and description of a stone similar in size, shape, and grit to this of Mr. White's; it was discovered at Melandra Castle, a little way from Mottram Longendale; the ornaments and two first lines correspond very much with Mr. White's; but upon viewing the stone I found the two last lines very imperfect: yet it must be confessed the superior judgment of Mr. Watson was alone of sufficient authority to establish the interpretation. He read it

Cohortis primæ Frisianorum Centurio Valerius Vitalis.

This Valerius perhaps might not only belong to the first Frisian Cohort, but be the first centurion, or commander of an hundred men, placed there at the erection of the castle, and the stone fixed up in memory thereof at his own request.

It appears likely the stone was fixed over the centre of the arched gateway of the castle, it being found in the ground where the principal entrance into the fortress was situated; and Mr. Watson's stone was discovered in the gateway at Melandra Castle. This at Manchester was probably thus placed when the gate was erected, or at least when it might undergo a repair in the time of Trajan or Hadrian, coins of both emperors being found at the place where the gate stood.

This castle was in a ruinous state about the year 900: history says that Edward the elder, king of the West Saxons and afterwards of the Mercians, sent an army of the latter into Northumberland, which then had a king of its own, to repair the castle at Manchester, and put a garrison into it, as a defence against the Danes, who were ravaging the kingdom with fire and sword.

Note by Dr. Holme.

The following is, I apprehend, a more correct transcript of the inscription, than that exhibited in plate VII. page 534. The characters I have ventured to supply in italics, are obscure in the original. In the engraved copy an O is substituted for the Q in the fourth line; and there is a member redundant in the complication that follows it.

CHOR. I.
FRISIAVO
N. Q. VI. MVNI.
M. P. XXIIII.

Probably:

Cohortis primæ Frisiavonum quæ viam munivit millium passuum viginti quatuor: which may refer to the construction of the military road between Mancunium and Condate; as the distance between these stations, fixed by Richard of Cirencester in his tenth *Iter* at twenty-three miles, measures, according to Mr. Whitaker, twenty-two English, which are nearly equivalent to twenty-three Roman miles and three quarters.*

The relic before us is of importance, as it enables us to restore the proper appellation of the cohort that garrisoned Mancunium: concerning which antiquarians have been misled by an ambiguous contraction in the inscription at Melandra Castle, and probably in that transcribed for Camden by Dr. Dee. It is farther valuable, as it may serve to vindicate the authority of Pliny and the purity of his text, in regard to a subject on which they have been questioned, in a work of great erudition

* Hist. of Manchester, I. 102.

published by an eminent scholar of the seventeenth century*.

The FRISIABONES, or adopting the reading of Harduin's MSS. FRISIAVONES, are twice mentioned by the elder Pliny: first as inhabitants of an island situated at the mouth of the Rhine, between the Maese and the Zuyder Zee; and secondly as a nation of Belgic Gaul†. The former are supposed by Harduin to have been a body of emigrants from the latter. The name is likewise preserved in an inscription found at Rome, of which I insert a copy from Gruter§.

D. M.
T. FL. VERINO.
NAT. FRISÆVONE.
VIX. AN. XX. M. VII.
T. FL. VICTOR.
EQ. SING. AVG. FRATRI.
DVLCISSIMO.
F. C.

Whether the Mancunian cohort was the same with the *Cohors I. Frixagorum* of the *Notitia*, stationed in the decline of the empire at *Vindobala*, is a question that must be decided by future discoveries; as no inscriptions occur at Rutchester, which is supposed by Mr. Horsley to coincide with that station.

E. H.

February 24, 1802.

* *Vid.* Cluverii German. Antiq. 561.

† Hist. Nat. Lib. IV. capp. 29. 31.

§ Inscript. Antiq. DXXXII. 7.

APPENDIX.

II.

Note to Mr. W. Henry's *Paper on Heat.*

The argument, at page 611, which is the basis of my objections to the commonly employed mode of ascertaining specific caloric, I fear is not so fully and clearly stated, as the abstruse nature of the subject requires.

Assuming two bodies, A and B, to be at the point of privation of temperature, or to possess no free caloric whatsoever, the quantity of combined caloric in each, according to Dr. Crawford's theory, is *directly* proportional to the quantities of heat, necessary to produce equal elevations of temperature in the two bodies. Thus, if to attain a given temperature, A require caloric as 20, and B only as 10, the combined caloric of A, before this addition, is inferred to have borne to that of B, the ratio of 2 to 1. But it might, with equal or perhaps greater probability, have been assumed, that the combined caloric of A and B is *inversely* proportional to the quantities of heat, required to produce a given temperature;—that A, for example, to attain a certain temperature, has absorbed more caloric than B, because in A less caloric existed, previously, in a state of chemical union.

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