A SERIES OF EXPERIMENTS ON THE SUBJECT OF PHOSPHORI, AND THEIR PRISMATIC COLOURS:

IN WHICH ARE DISCOVERED,

SOME NEW PROPERTIES OF LIGHT.

ALSO,

A Tranſlation of Two Memoirs of the late J. B. BECCARIA, Profeſſor of Philoſophy at BOLOGNA; taken from the BOLOGNA ACTS.

BY

B. WILSON, F. R. S. & AC. R. UPS. SOC.

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MDCCLXXVI.

PREFACE.

SINCE my firſt edition of EXPERIMENTS UPON PHOSPHORI, *and their Priſmatic Colours*, appeared, ſeveral material alterations have been made, and new experiments added; which rendered it neceſſary to print a Second Edition.

In the purſuit of theſe new Experiments, I was not a little ſtruck with ſome ſingular phoſphoric appearances, produced by ſolar-rays, after they had been refracted by a priſm. From which I was led to ſuppoſe, that the different-coloured rays may be poſſeſſed of certain properties which have hitherto eſcaped the obſervation of philosophers. Upon this foundation, I flatter myself, no impropriety will appear in the title prefixed to this book.

Those new Experiments are placed at the end, under the article of ADDITIONS, as they could not be inserted with propriety in the body of the Work.

Having in my First Edition of this Book had occasion to make some remarks upon Dr. Priestley's History, so far as relates to the subject of *Phosphori*, I find that he has since, in a late Publication *, plainly shewn, he has taken offence on that account. For in that Piece, a conversation is introduced between himself and Dr. Higgins; wherein the latter is represented as having thrown out some reflections against the Author of those Experiments, for the liberty he had taken in pointing out Dr. Priestley's inaccuracies. It is not my intention to enter into disputes with any one: and for that reason I de-

* *Philosophical Empiricism.*

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cline publiſhing a letter I lately received from Dr. Higgins, wherein he declares that the part of the converſation which relates to me has been *miſrepreſented.*

The matter might have reſted here, had not Dr. Prieſtley *himſelf*, in another of his † Publications, plainly diſcovered ſome degree of warmth againſt me, upon the ſame account. For he does not ſcruple in his Preface to that Work, to accuſe me of *negligence* ; aſſerting that, when " I had animadverted upon him " for ſpeaking, in his Hiſtory of light, &c. " of paper being *red hot* and *cooled again*, " I ought to have looked into the printed " *errata* of his book, where I ſhould have " found, for *red hot*, read *very hot*." To this perhaps, it may be a ſufficient anſwer to ſay, " that in the copy I had, which " was purchaſed of his own bookſeller " in *boards*, ſome time *after* it was pub- " liſhed, (in which *condition* it ſtill re- " mains) there were no printed *errata*;" ſo far as I could *then* perceive. But, leſt I

† Upon *Air*.

might be miſtaken, I have again, upon Dr. Prieſtley's allegation, carefully examined the ſame copy of his book, and *ſtill* find that there are *not* any *errata* to be found in any part of it.

Beſides this, the Doctor charges me with a want of candour, in not correcting, what he himſelf had left *un*corrected: affirming, that if my copy of his books wanted the printed *errata*, I ought to have ſupplied the place of it with a little *candour*.— Had I made alterations in the expreſſions of ſo ſenſible and accurate a writer as Dr. Prieſtley, would not this have been taking a liberty, which neither he himſelf perhaps, nor the world, would have thought juſtifiable?——But, though my copy of his book wanted the printed *errata*, yet will not the Doctor allow, that I have *ſupplied* the place of it with ſome degree of candour, when I aſſure him, that, in this ſecond edition of my *Experiments*, (previouſly to the appearance of his late Publication) I had left out the *remark*, which ſo much diſpleaſed him, merely upon a repreſentation, made to me me ſome time ago, by a Friend, who is alſo a Friend of his, that the paſſage in his book, upon which I had animadverted, *was* an error of the preſs?

Before I diſmiſs this diſagreeable ſubject, I am obliged, in duty to myſelf, to obſerve further, that Doctor Prieſtley, in the ſame Preface, perhaps contrary to that ſpirit of *candour*, which he expects from others; at leaſt contrary to that law of charity, which thinketh *no evil*, is pleaſed to aſſert, that "I take every "opportunity of *cavilling* at his *Hiſtory*." Upon which I muſt obſerve, "that I "deſire to cavil at no man," but, ſo far as is conſiſtent with a *free*, *candid*, and *impartial* enquiry into *truth*, to live peaceably with all men. Wherever I have found myſelf obliged to differ in opinion, either from Dr. Prieſtley, or Father Beccaria, I have always expreſſed my diſſent from them with all the reſpect due to their literary merit: nor has any thing elſe been the occaſion of my differing from either, but the reſult of certain facts and experiments. I agree with Doctor Prieſtley, "that Father Beccaria is a philoſo-"pher of ſuch a claſs, as entitles him "to the greateſt reſpect;" but cannot acquieſce in the inference he would draw from it; "That Beccaria's concluſions "ſhould not have been controverted:" becauſe, if Dr. Prieſtley's opinion of the preſent age be juſt, that PHILOSOPHERS are making ſuch haſty ſtrides towards perfection, in every branch of ſcience, that it is to be hoped every year will produce diſcoveries equal to all that were made by a *Newton*, or a *Boyle*; -----why may not the diſcoveries and concluſions of Father Beccaria, which were made ſome years ſince, be ſuppoſed capable of being *controverted*, (according to the Doctor's own ſanguine, and pleaſing expectations) by *ſome* philoſopher in a *ſubſequent* year of improvement? But then, according to the Doctor, it muſt not be *every* philoſopher, to whom the *honor* of controverting F. Beccaria's concluſions, is reſerved: for I obſerve, he

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aſſerts in the ſame place §, " that theſe " concluſions of F. Beccaria's ſhould *not* " have been controverted, but upon much " better grounds than Mr. Wilſon's."— Indeed Mr. Wilſon (who never loved to make any oſtentatious parade of the little knowledge he poſſeſſes) cannot ſay, that his ſtrict attention, in *his* more obſcure philoſophical retirement, to induſtrious reſearches after truths, has given him opportunities of boaſting ſo *extenſive* and *brilliant* an acquaintance, as it ſeems the Doctor has been *diſtinguiſhed* by. Nor does he conceive what intrinſic merit he could have derived as a philoſopher, from the *extent* and *brilliancy* of ſuch a *diſtinction*, had it fallen to his lot: nevertheleſs, as a *plain man*, and a *plain philoſopher*, he has, in the beſt manner he was able, induſtriouſly applied himſelf to *one* capital object, namely, a ſearch after philoſophical truths, in an unwearied attempt to make what diſcoveries he could, for

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the promoting uſeful knowledge. Now, why may not ſuch a plain philoſopher (with the good Doctor's gracious leave) be ſuppoſed capable of, at leaſt *ſtumbling* upon *diſcoveries*, which had eſcaped the obſervation of preceding Philoſophers, even of the higheſt and moſt reſpectable characters? For it is well known, that it is not always men of " *vaſt* and *comprehenſive* " *underſtandings*," that have been favoured by Providence with making diſcoveries ſometimes the greateſt, and moſt uſeful to the world: but on the contrary, (to allude to the words of an eminent writer with whom Dr. Prieſtley is intimately acquainted) the Great Author of Nature hath frequently choſen " weak things," in the philoſophical, as well as the ſpiritual world, " to confound the mighty, and " things that are not, to bring to nought " the things that are."——Beſides, the concluſions of Beccaria, which this plain and obſcure philoſopher has contradicted, will I hope appear, not only to Dr. Prieſtley, but Father Beccaria himſelf, to be *indeciſive*;

decisive ; if they will be at the trouble to examine this my Second Edition upon *Phosphori* with attention. The result of which examination (on the part of Dr. Priestley) I humbly hope, from the liberal and *ingenuous* spirit he professes, of which he has already given us a taste, in that *History of Experiments* which he "*ventures to exhibit as a model of the kind*, "the like to which, *there is not in the whole* "*compass of philosophical writing* :" I say, I hope, the result will be a candid acknowledgement from the learned Doctor, perhaps much in the same words, as those in page 18 of his Preface, (where he himself intimates the *possibility* of his mistake)—*In* THIS, however, *I* HAVE BEEN MISTAKEN.

It remains only to assure the Learned, that my experiments were made with the utmost fairness and impartiality. This, it is hoped, will manifestly appear, not only from the apparatus itself, employed on that occasion; and from the consideration of a neutral and an entirely disinterested person's having repeated the same experiment,

ment, in the preciſe manner of Beccaria himſelf, with no better ſucceſs than I had done: but likewiſe from this further and moſt material conſideration, namely, the *properties* above ſuppoſed to belong to *light*. Theſe, I apprehend, are *plain facts*; which are not to be ſhaken, or overturned, by *mere ſurmiſes*.

My Experiments, therefore, muſt remain as eſtabliſhed facts at leaſt, until they are contradicted by ſomething *ſtronger* than ſurmiſe. And when they are *ſo* contradicted, I ſhall not be aſhamed to own that I, too, have been miſtaken.

December 30th, 1775. B. WILSON.

A SERIES

A SERIES OF EXPERIMENTS ON THE SUBJECT OF PHOSPHORI, AND THEIR PRISMATIC COLOURS.

THE Bologna ſtone, which has been ſo much ſpoken of for it's property of ſhining in the dark, and which was firſt diſcovered by accident, about the year 1630, engaged the attention of many excellent chymiſts, and experimental philoſophers, to examine the nature of it's compoſition, and ſearch for the moſt advantageous method of calcining it, and making it luminous.

In conſequence of their diligent reſearches, we find very different and ingenious proceſſes to make

phoſphori,

phoſphori, in various learned works: particularly in the Berlin acts, by the celebrated M. Marggraaf; who in analyſing the heavy fuſible ſpar, and others of the like kind, produced ſome very fine phoſphori. But no one, that I have yet met with, has been more ſucceſsful, or has made a greater figure in experiments upon this ſubject, than the late learned Beccari of Bologna.

This excellent obſerver, by great patience and induſtry, joined to very great abilities in philoſophical enquiries, made many new diſcoveries, which he publiſhed, in two memoirs, in the Bologna acts for the years 1744 and 1747, from which it appears, that the family of phoſphori has been by him extended to an amazing number. Some ſhining with a greater, and others with a leſs, light, after having been expoſed to the ſun for a few ſeconds, and then removed ſuddenly into the dark.

The moſt brilliant phoſphori which he diſcovered, were linen, paper, ſome earths, ſtones, gums, and even the human ſkin: beſides others, when they were properly dried, or roaſted. Thoſe memoirs containing ſo many curious facts, and this undertaking of mine being a kind of ſequel to his diſcoveries, I thought it not improper to publiſh a tranſlation of them at the end of this work: as they may ſerve to illuſtrate each other, and render the whole of this enquiry more eaſy to be underſtood.

The apparatus, employed by Beccari at firſt, for bringing bodies ſuddenly from the light into the dark, did not, as he acknowledges, anſwer the end required ſo well as the ſecond he made uſe of.

I have had the ſatisfaction of trying both methods, and found that the latter was, on many accounts, the more eligible, and better calculated for the purpoſe. But as the duration of the phoſphoric light, mentioned by him, was ſhorter conſiderably than what I have obſerved, I am inclined to think that one difference, at leaſt, aroſe from a difference in the degree of darkneſs obtained, between the kind of box he made uſe of, and the ſmall cloſet I employed; the dimenſions of which were about ſix feet by five and a half: and the heighth about nine feet.

It was painted black, or covered with black baize in every part; and had two doors that were five or ſix inches broader and longer than the ſpace to enter at. There were two curtains of black cloth over the hole where the hand was occaſionally put out, to expoſe bodies to the light; the outer one conſiſted of three doubles, and the inner one of as many. All theſe were conſiderably larger than the hole, which was about fifteen inches diameter, and opened to the South. There were ſmall leaden weights faſtened to the bottom of each curtain, to preſerve them in their places when the hand was drawn into the room. And, that I might breathe the air freely, and continue longer in the cloſet without any inconvenience, there were fixed two curving pipes, about three inches diameter; one communicating with the external air from the top of the room, and the other with the external air from the bottom. The closet, in which I made these experiments, was in the house of Edward Delaval, Esq; which stands in an open situation on the side of the Thames, at Westminster.

In my first experiments I adhered strictly to the rule laid down by Beccari, of preparing the eyes for observation, by continuing in the dark a full half hour before an experiment was attempted to be made. But I now find that a much shorter time is sufficient, at least, for my purpose, and with my eyes; unless the bodies to be examined afford only a very feeble light. Before I begun to make any new experiments, I repeated many of those made by Beccari, and found that they answered, in general, as he hath related them. In the course of these trials my attention was chiefly engaged by such of the phosphori only as appeared most brilliant: they being the best calculated for my purpose to make further experiments upon; not only on account of their great splendor, but the duration of it. Of those luminous appearances the experiments upon paper are not the least considerable.

For Beccari having observed, that paper would shine pretty well in the dark, and with an equable light throughout, after it had been exposed to the sun, tells us, that he laid a sheet of it upon a gridiron,

gridiron, and underneath it placed ſome burning coals; that, when the paper had been heated enough, he expoſed it to the light, and then removed it into the place of obſervation, where he found the paper ſhining with an uncommon light: and that, on thoſe parts of the paper which had been covered with the bars, there was an appearance of darkneſs, which exhibited the outlines of the bars terminated with the greateſt preciſion.

The ſame ingenious author mentions another experiment, in which he laid upon the paper a pretty thick plate of braſs, which had been previouſly made ſo hot, that it could hardly be held in the hand; but not ſo hot as to hurt the paper: when the plate had made the paper ſufficiently warm, he expoſed it to the light as before, and afterwards removed it into the dark. That part of the paper, which the plate had touched, appeared to him of an extraordinary brightneſs; and ſhewed the figure of the plate perfectly defined: the other part exhibited the uſual light, evenly diffuſed all over it.

This extraordinary effect of heat upon paper induced me to purſue the experiment further. Accordingly, I had recourſe to what I thought a more convenient method, as it ſeemed better calculated to vary the degrees of heat. This was nothing more than the common ſmoothing iron, ſuch as women employ upon linen, into which there was put, occaſionally, a red hot heater.

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One day, in the month of Auguſt, when the ſun was ſhining, I placed the ſmooth ſurface of the heated iron on a quarter of a ſheet of white paper, which was laid upon a table, and continued it there for three, or four, ſeconds. After that, the paper was placed in the cylinder of the firſt apparatus deſcribed by Beccari, and in ſuch a manner, that the light might ſhine fully upon it when the cylinder was properly turned. On expoſing this paper to the ſun for two or three ſeconds, and then removing it into the dark ſuddenly, by turning the cylinder back again, a light appeared ſo very bright, that I thought ſome accident had happened to the apparatus; and that, in conſequence of a fracture, the light of the ſun had ſhone through it upon the paper. After examining every part of the apparatus, and finding that all was ſecure, I repeated the experiment many times, without warming the paper afreſh; when I was ſoon ſatiſfied, that the extraordinary light on the paper was phoſphoric, and exhibited the exact ſhape of the iron. The other part of the paper ſhone alſo, but ſo faintly, that it looked like a ſhade compared with that ſplendid appearance.

Upon warming the paper again with the ſame iron, and repeating the experiment, by expoſing it to the light, I placed the heated iron upon a part of the luminous appearance for about two ſeconds. By this treatment, as much of the phoſphoric light as the iron covered, and no more, was totally extinguiſhed. This extraordinary effect, which does not appear to have been observed by Beccari, excited my attention; and still the more so, upon finding, that, when the paper was exposed again to the sun, the brightness was restored.

The experiment was therefore varied, by trying the effect of the same iron, when it was cold. Accordingly, immediately after the light upon the paper was renewed as before, I placed the cold iron upon the luminous part, and continued it there, in different trials, from two or three, to eight or ten seconds, without being able to perceive that it had any effect upon the light.

After this, the effects of other substances were tried, when they were in the same states both as to heat and cold. And first with respect to heat.

Having heated a small piece of gold, I laid it in the middle of the luminous part of the paper, and continued it there for two or three seconds; after that, on removing it, a dark spot appeared upon the paper, exactly the size of the gold: this dark spot became luminous again by exposing it to the light.

Silver, and other metals, when they were made equally hot, produced the same effect.

I then heated a piece of marble; after that, a stone of the grit kind; also glass, and two or three different sorts of wood; and observed, that each of those produced the same effect as the heated iron, &c.

The experiment was repeated with all these sub-

ſubſtances, when they were in a cold ſtate, but not one of them had any effect upon the luminous appearance.

Now, as all thoſe bodies were applied in contact with the paper, the experiment was varied again, by ſuffering thoſe bodies to approach no nearer to it than about the 30th part of an inch.

For this purpoſe I provided myſelf with a circular rim of braſs; this I laid upon the phoſphoric light of the paper, and then placed the heated iron upon the braſs rim: and though the times of continuing the iron upon it, were varied, yet the heat had no effect upon the luminous part; except where the braſs rim was in contact with the paper, and there it appeared more, or leſs, dark in proportion to the heat which the different parts of the braſs had acquired from the iron.

Having ſeen the effects of heated ſolid bodies upon the phoſphoric light, I was ſollicitous to obſerve the effects of fluids, when in the ſame ſtate, upon the ſame luminous appearance.

To this end, whilſt the paper continued ſhining, a part of it was dipped in boiling water. This treatment preſently extinguiſhed the luminous appearance. Upon expoſing the paper again to the ſun, though in it's moiſtened ſtate, the ſplendor revived: but it was not then quite ſo bright, as before it's immerſion in the water.

In very hot oil, the phoſphoric light did not diſappear ſo ſoon: on expoſing the paper again to the

ſun,

ſun, it ſhone ſomething brighter, than when it was moiſtened with water.

Theſe different fluids, in their cold ſtate, had but little effect upon the luminous appearance, or upon the duration of it. And I find ſince, that Mr. Du Fay had made the ſame obſervation, by dipping other ſubſtances in cold water.

In the making of theſe experiments, I was a little ſurprized with a ſingular and unexpected appearance. For when the hot oil was brought into the cloſet, where I had continued for a conſiderable time, I obſerved that the oil, as well as the baſon which held it, were both diſcernible; and likewiſe ſeveral objects that happened to lie near, without the aſſiſtance of any light from without.

Beſides this unexpected appearance I muſt mention another which the heated iron produced: though I have found ſince, that M. Marggraaf had obſerved the ſame effect with the fuſible ſpar, when he expoſed it upon a hot plate of iron.

It was this: after the heated iron had made the luminous part of the paper dark, I continued the iron upon it for eight or ten ſeconds more. In conſequence of this, a new phoſphoric light was exhibited, without expoſing it afreſh to the ſun. The brightneſs of this light was about equal, in degree, to half of that phoſphoric light which the ſun produces on paper. Upon examining the paper in the open day, there appeared only a very tender yellowiſh tinge in thoſe parts with which the iron had been in contact. And perhaps it may be worth obſerving, that this paper, notwithſtanding the tender tinge it had acquired, would ſhine very well in the dark, upon preſenting it again to the ſun.

But I found, that whenever the paper happened to be more burnt, ſo that it's colour was conſiderably darker, and bordered upon blackneſs, it would not produce any light by being expoſed to the ſun.

Linen, treated in the ſame manner as the paper, in all the preceding experiments, exhibited, very nearly, ſimilar phoſphoric appearances: except that ſome fine linen ſhone conſiderably brighter than a coarſer kind; and that the duration of the light, from linen in general, was very near twice as long as that which paper exhibits. Yet I have met with ſome inſtances where very coarſe linen has afforded a very bright light, which laſted full as long as the light produced from a finer ſort of linen.

I now laid aſide the cylinder, upon finding ſeveral inconveniencies ariſing from the uſe of it: and in it's place I put the other apparatus, conſiſting of the black curtains, &c. This I continued to employ during the whole time of my future reſearches: but with this difference, that I was obliged to ſhut my eyes at all times whilſt I expoſed the bodies to the ſun, on account of the

light

light which entered the closet by the unavoidable opening of the curtains.

Loaf sugar not only shines very well in the dark, but may be made a much finer phosphorus by a proper application of heat. I found this, by exposing it in contact with the smooth part of the heated iron; which was not hot enough to melt the sugar, but only sufficient to leave a whitish gloss upon it's surface.

This improved light, which the sugar exhibited, I endeavoured to increase further, by a variety of means; and particularly by melting very small quantities of it upon a card, with the assistance of the heated iron; but these trials did not answer my expectations. However, my frequent attempts produced a very singular effect, which in it's consequences was of considerable use in my researches.

The smoothing iron, by repeated experiments, and those for several days together, was grown foul with the sugar, and perhaps other matter, sticking to it; some of which were melted, and others not. I nevertheless continued to use it in the state in which it was. One day, on pressing the iron upon a card, where a little powdered sugar had been previously laid, and which was melted by the heat of the iron, I observed, after drawing in the card from the light, a most beautiful *green appearance* in several parts of the card, and exceedingly brilliant; it resembled the colour of a very fine emerald in the light of the ſun. Perhaps the novelty of it might prejudice me, and induce me to think it brighter than it really was. The duration of it was as long as that of the white light in other parts of the card. When the green colour vaniſhed, I renewed it many times, and during four or five days, by only expoſing the card again to the ſun.

Several times afterwards, the ſame experiment was repeated upon two, or three, other cards with equal ſucceſs. But I ſoon found, that it was not always in my power to produce it, notwithſtanding every means almoſt, that could well be deviſed, were made uſe of in numberleſs attempts. All theſe trials ſatisfied me, that the green, produced above, muſt have depended upon ſome circumſtances which I am not yet acquainted with.

Beccari has obſerved, that ſugar would ſhine, not only upon it's ſurface, but within it's ſubſtance throughout. I found the obſervation true, by breaking it, and even pounding it; for all the parts continued equally luminous.

I then plunged in cold water ſome freſhly enlightened lumps, without being able to obſerve any alteration in their ſplendor. The duration of this light, in the water, was nearly as long as that from other lumps which were out of the water.

In this place I muſt obſerve, that the phoſphoric light ceaſes before the ſugar is intirely diſſolved: and though hot water diſſolves ſugar much ſooner than cold water, I could nevertheleſs perceive a light for two, or three, ſeconds after it's immerſion: and this appearance, I conjecture, was owing to the water, and perhaps the heat, not being able to penetrate it immediately.

White ſugar-candy ſhines alſo in cold and hot water, after it has been expoſed to the light, and for a longer time reſpectively. And though loaf-ſugar, and ſugar-candy, are immerſed in cold water for a few ſeconds, they will afterwards, even in their moiſt ſtate, ſhine afreſh upon expoſing them again to the ſun.

Among a variety of experiments upon vegetables, Beccari diſcovered that bread, when it is a little toaſted, ſhines very well. I do not find that the light thereof penetrates farther than the ſurface of it; becauſe, on breaking it, the new ſurfaces are totally dark. On trying bread, which was not toaſted, I obſerved that it would ſhine, but the light it exhibited was rather weak; and that may be the reaſon why Beccari did not obſerve it.

The light which gum arabic affords, anſwers fully Beccari's deſcription of it. Gum copal ſhines leſs than gum arabic, except when it is very well dried by a fire, and then it is a very good phoſphorus. Both theſe gums ſhine nearly as well in cold water, and almoſt for as long a time, as in air.

Cotton, is no inconſiderable phoſphorus; at leaſt, it is as good as paper without the application of heat.

IN the animal kingdom, I find that joiners glue ſhines conſiderably better than bread untoaſted; and if it be expoſed to the fire, till it is extremely well dried, ſo as to ſwell and change it's colour to a lightiſh yellow, it will exhibit a very fine yellowiſh light, which will ſhine in cold water for a conſiderable time, and nearly as bright as in air.

Some bones ſhine better than others, eſpecially if they are properly dried: at the ſame time there are others which do not afford any light, at leaſt, that I could perceive.

Hair, and wool, are alſo ſubject to ſhine, though not in any great degree; but even theſe differ alſo in that reſpect; for ſome hair, as well as ſome wool, ſhine better than others: but wool in general ſhines conſiderably brighter than hair.

Feathers, give a little phoſphoric light; the white appear the beſt; the yellow ones next; red and green the leaſt; but the quill part ſhines, in general, better than even the white of the feathers.

There are few ſhells but what ſhine, and ſome ſo conſiderably, that they exceed the light which paper exhibits: for example, oyſter-ſhells, cockles, and what is called the ſcuttle-fiſh.

I have tried a great many other ſubſtances in this diviſion, but I decline giving any account of them, as Beccari hath obſerved the ſame in a very able manner; beſides many more that are exceedingly curious.

BECCARI has given us a great variety of fine phoſphori,

phoſphori, in the mineral kingdom. His diligence has been ſo great in this part of nature, that I had very little hopes of adding any thing conſiderable, which might be worthy of notice. However, I was determined to purſue my obſervations upon ſuch ſubſtances as were the moſt phoſphoric: but not without ſome expectations that chance, and induſtry, would in time diſcover ſomething more deſerving my attention, than barely ſeeking for the moſt brilliant phoſphori.

Contrary to Beccari's plan, I examined the gems firſt; notwithſtanding they had been diligently attended to by him, Du Fay, and others.

Among thoſe that are phoſphoric, there are many diamonds, and ſome of different colours, which ſhine very well, and appear lucid throughout. There are others, and thoſe very fine, that give little or no light; which agrees with the obſervations made by Mr. Boyle, Du Fay, Beccari, &c.

However, I had a ſingular opportunity, by the favour of Lord Pigot, of examining, in the dark, his exceeding fine diamond; it being the moſt valuable one in this kingdom from it's water and ſize, and weighing two hundred grains. The great luſtre and magnitude of it, though *unſet*, were flattering circumſtances that it would exhibit a conſiderable phoſphoric light. But our expectations were greatly diſappointed upon making the experiment. For inſtead of perceiving a brilliant phoſphoric appearance, we only were able to obſerve

ſerve light enough from it to pronounce it's preſence, after it had been expoſed to the ſun. The degree of this light did not exceed that produced from the red and green feathers mentioned before.

The ſame nobleman, who was extremely obliging and ready to promote the enquiry I had in hand, produced a large brilliant drop, which was alſo unſet. This diamond gave no better light than the former.

But a large yellow diamond, *ſet* in a ring, and which belonged to Lord Pigot, exhibited a very good light, that laſted ſeveral minutes.

Some diamonds of leſs value, which were alſo ſet, and ſome of different colours, were phoſphori: but ſome of thoſe ſhone better than others.

Two rubies, a ſaphire, topaz, and aqua marine, which were very large and fine of their kind, gave no light.

The opal gave a pretty good light, but a fine emerald only an indifferent one.

The cat's eye made as indifferent an appearance: but a very large pearl in Lord Pigot's collection ſucceeded better: the light of which being nearly equal to that which paper gives without heat, and the duration of it was about twelve ſeconds.

Beſides theſe gems, I was favoured with ſeveral more of conſiderable value by Lord Seaforth: and particularly, a yellow diamond that was ſet tranſparently, which gave a moderate light, though

it

it rained at the time of obſerving it. I mention the circumſtance of tranſparency, becauſe the yellow diamond, belonging to Lord Pigot, which is deſcribed above, had a *foil underneath it*.

MARBLES, have been obſerved by Beccari to ſhine with a conſiderable degree of light: but I find that Portland ſtone, and a whitiſh ſtone of the grit kind, which is very common, and found in England, ſhine a great deal better than marbles, or even alabaſter, or lime-ſtone.

Some mortar, from an ancient part of the Tower of London, exceeded the grit ſtone, and even chalk, which had been burnt in the fire for ſome time. The light diffuſed over thoſe bodies did not appear to penetrate any farther than their ſurfaces: for, upon ſcraping, or ſcratching them, there was a total darkneſs underneath.

Flints, differ very much in the degrees of phoſphoric light they afford; the cleareſt kind having the advantage: and thoſe ſeem to ſhine internally as well as externally. I have made the ſame obſervation upon many ſpars, and ſome cryſtals.

A very fine ſpecimen of ſpar from Derbyſhire, of a cubic form, ſhone as bright as ſugar in it's improved ſtate by heat. And when it was plunged in cold water, the light continued, very nearly, as long as in air. But in hot water this was not the caſe; for the light diſappeared in a quarter of the time. This ſpar, beſides many others, as well as various other ſubſtances, and eſpecially if they happen to be of an inflammable nature, ſhine in the dark, merely by the application of heat. It is the ſame with a kind of greeniſh ſpar, or fluor, from Sweden. This fluor, expoſed to the light, ſhines full as well as ſugar in it's common ſtate. But what appears remarkable, is, that though it is not one of the moſt brilliant phoſphori, yet the light it affords continues above ſix minutes. This obſervation ſeems contrary to Beccari's rule, namely, that bodies ſhine the longer, the brighter they are: now ſome of the brighteſt phoſphori that I have hitherto obſerved, do not laſt above thirty ſeconds.

Beccari obſerves, that tutty will ſhine on the convex, and not on the concave part: this laſt circumſtance is mentioned, as I find the concave part does give a light; but then it is a feeble one compared with the other.

White arſenic affords a light almoſt as bright as paper in it's common ſtate.

Corroſive ſublimate gives a weaker light conſiderably.

The flowers of zinc produce a better light than corroſive ſublimate: and ſometimes the colour of them was inclinable to the orange.

IN examining a variety of calcareous bodies, I obſerved in general, that they exhibited about a middle degree of light between the brighteſt, and fainteſt, phoſphori I have yet met with. The light of moſt of them, however, was increaſed by a partial calcination of them in a crucible; and in ſome, more

more than in others: particularly lime-ſtone, chalk, and oyſter-ſhells. Among the laſt, I ſometimes imagined there were faint appearances of colours. In two, or three ſpecimens, I am certain of having obſerved an orange colour, and a very tender blue. But not meeting with the ſame appearances on others, after repeated experiments, the purſuit was given over for that time.

AMONG the neutral ſalts, Beccari mentions borax as ſhining with a conſiderable degree of light; but he has not told us, whether it was refined or not. The phoſphoric light from either the native, or refined borax, I have not found the leaſt remarkable. The latter hath the advantage over the other: but the light even from borax refined, is not brighter than that which ſugar exhibits without the application of heat.

When borax is calcined, the light it affords, is greatly improved: but not near ſo much as when it is calcined in contact with chalk, though the chalk itſelf is not calcined. The ſplendor given by this compoſition, when it is properly made, is very great; and very near equal to the moſt brilliant phoſphori I have yet met with.

It was after a variety of trials, and without ſucceeding, that I thought of putting a lump of borax into an iron ſhovel, and afterwards the ſurface of a large lump of chalk upon the borax, while it was melting. In a little time, part of the chalk incorporated with the borax, and formed a kind of calx, which ſpread from under the chalk in large feather-like forms, curling upwards. The heat was continued only till all the borax was reduced to a calx. The upper ſurface of this calx was the part that ſhone ſo bright; the under ſurface being conſiderably leſs bright: and the chalk itſelf, that was untouched by the borax, gave only the light that is common to it; this appeared as a pretty ſtrong ſhade, compared with the very great ſplendor of the calx on it's upper ſurface.

After this, I combined borax with many other ſubſtances, ſuch as gypſum, oyſter-ſhells, limeſtone, ſulphur, &c. but the light they exhibited was not near ſo bright.

I then examined the ſedative ſalt, made both by calcination, and ſublimation: as likewiſe the foſſil alcali. Each of thoſe gave a light nearly equal, in point of brightneſs: and very near equal to borax calcined alone.

In the courſe of theſe experiments I was obſerving the phoſphoric effects of different pieces of amber, ſome of which were poliſhed, and others not. The poliſhed ones gave a much better light than the others; but even thoſe were not quite ſo bright as paper without the application of heat. Having the heated iron at hand, I placed one of the poliſhed pieces upon it. In a very little time, a reddiſh intenſe light appeared to dart from one end of the amber to the other: but this light ſeemed inſtantaneous, and reſembled very much the

electric

electric light. At the ſame time there was a kind of crackling noiſe attending it: which created a ſuſpicion that the amber had been injured by the heat. But, upon examining it in the open day, I could not perceive the leaſt alteration.

I repeated this experiment with a large lump of amber, which was not poliſhed, and obſerved an appearance very like the former; but then this light did not extend ſo far: owing, I ſuppoſe, to the irregular ſurface not permitting the iron to be in contact with it to ſo great a length.

WHILST I continued buſy in making theſe kind of experiments, and with other ſubſtances that were of an unctuous and ſulphureous nature, I ſtill kept in my remembrance the extraordinary green light which had been obtained from ſugar. The weather being ſettled, and very fine, it was an inducement to try, once more, whether the green light might not be produced by ſome other means. And as I knew that copper, in certain circumſtances, would make a green colour, recourſe was had to that ſubſtance; notwithſtanding the experiments and obſervations which had been made by Beccari, and others, upon metals in general, diſcouraged me from it: and though I had no pretenſion to any ſkill in chemiſtry, I had a mind to try what a ſolution of copper in aqua fortis would do, when united with a calcareous ſubſtance.

Accordingly,

Sol. of Copper. Accordingly, a ſmall quantity of aqua fortis, not exceeding five drops, being put upon a piece of copper, and continued there near twenty ſeconds, I waſhed the ſolution off ſuddenly with half an ounce of freſh aqua fortis. Upon one ounce and a half of calcined oyſter-ſhells, part of which happened to be in powder, and part not, I poured the aqua fortis thus impregnated with copper, and let it ſtand about ſixteen hours; not for any particular reaſon, but that it happened to be moſt convenient on account of other buſineſs. I then poured off what the oyſter-ſhells had not taken up, and put the ſhells, which were of a paſte-like conſiſtence, into a crucible, and preſſed the maſs down very cloſe. After this, the crucible was put into a ſea-coal fire, that was pretty hot, and continued there near forty minutes. On taking it out, and letting it cool, the whole maſs came eaſily out of the crucible, and appeared of a dirty greeniſh hue externally. This maſs I preſented to the light ſeveral times, and drew it into the dark as often, without obſerving any thing more than a weak, greyiſh, dirty light; though every part of the ſurface was properly expoſed, and carefully examined. I then broke a little off from that end which had been next the bottom of the crucible, and obſerved in the open day, a largiſh lump of oyſter-ſhell, that was more white than any other part of the maſs. Upon preſenting this bottom part to the external light, and bringing it back again ſuddenly into the dark, I was exceedingly ſurprized with a

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general appearance of colours, reſembling thoſe in the *rainbow*, only far more vivid. The red appeared the fineſt, and not unlike that which we meet with in old painted windows when the ſun ſhines upon it. The yellow was not near ſo intenſe, but very bright, and lay next to the red. The green next; but this was rather weaker than the yellow; and yet it was bright, though conſiderably inferior to the green which I produced from ſugar. The blue appeared two, or three, degrees fainter than the green; and not near ſo bright as the other colours. In regard to the purple I had ſome doubt, and therefore will not inſiſt upon it's being there.

The whole ſpace, thoſe colours occupied, was more than half an inch in length, and about one quarter in breadth.

In conſequence of this curious and accidental appearance of the colours, I was very induſtrious to diſcover the cauſe of it.

To this end, I firſt examined, with a magnifying glaſs, the white lump of oyſter-ſhell where the colours appeared, as likewiſe the greeniſh hue in the other parts produced by the ſolution of copper, and particularly thoſe parts next the confine of the red. Upon doing this, I obſerved, that there was a manifeſt difference in the appearance, between the greeniſh hue in general that was diffuſed over the whole maſs, and that which was neareſt to the edges, or ſurface of the ſhell; for the parts of the copper, which I ſuppoſe occaſioned the

the greenifh hue, feemed to lye in greater abundance next the red than in any other part. In making my obfervations, fome parts of the oyfter-fhell that gave the phofphoric red, crumbled away by the flighteft touch: for thefe obfervations were made on the fixth day after the colours appeared; and probably this would not have been the cafe on the firft day, as the whole mafs was then firmer and more compact.

Some of thefe colours continued vifible fix days at leaft, but not near fo diftinct as on the 1ft and 2d days; for the yellow, green, and blue, were confiderably fainter than the red. On the 8th day they were all vanifhed, and a white, yellowifh light occupied their places; and this light appeared rather brilliant than otherwife. I have been very circumftantial in defcribing this experiment, becaufe I imagined fomething material would depend upon it. On the lofs of this curious fpecimen I was very induftrious to replace it by repeating the experiment. But further trials foon taught me that it was not fo eafy a matter as I at firft apprehended.

However, in my fecond attempt, I prepared an equal quantity of the fame folution, and poured it upon an equal quantity of oyfter-fhells, which were taken from the former parcel. The whole mafs, after being preffed down in the crucible as before, was placed in the fire for an equal time, and then expofed to the light; after having broke a little off from the bottom of it, as in the other experiment; but no colours appeared: and what light there was ſeemed rather faint, and of a greeniſh duſky white. I then broke the whole maſs into a number of parts, but they ſtill exhibited the ſame greeniſh hue throughout.

The experiment was varied many ways without ſucceſs. Sometimes the light, from different maſſes, in the different experiments, appeared in colour like the new ſurfaces of broken ſteel. At laſt I begun to imagine, that *larger* ſolid parts of the ſhells might be neceſſary to exhibit colours. But before the trial was made, I had an inclination to ſee what effect other metals would have upon the ſhells when treated in the ſame manner as I had done the copper.

In one ounce of aqua regia I diſſolved one leaf of gold, and poured the ſolution upon ſome of the ſame calx. The maſs was preſſed down in a crucible, and expoſed to a pretty ſtrong fire for ten minutes. On taking it out of the crucible, the whole lump appeared of a purpliſh hue by day light, and gave only a very little phoſphoric light, rather inclining to purple. The lump was then broken into ſeveral pieces; notwithſtanding which, every piece had the ſame purpliſh hue in the open day, except two or three ſolid lumps, about the ſize of a pea; and thoſe appeared more white, and freer from the purple tinge. On expoſing thoſe to the light, and drawing them back into the dark, a yellowiſh white light appeared; and round about them, to a ſmall diſtance, the light was fainter; but it had a little red and blue- Sol. of gold.

iſh hue. The general effect upon my eyes, at a greater diſtance, was a kind of tender flame colour.

Theſe pieces becoming moiſt in three or four days, I expoſed them again to the fire, in a crucible, for ten minutes more. By this treatment, thoſe ſolid parts exhibited a very good red throughout; no traces of the other colours being left. But this red was not near ſo intenſe as the former one with copper.

Upon expoſing thoſe ſpecimens to the fire in a crucible for ten minutes more, all the red appearance vaniſhed, and a paliſh white light ſucceeded.

Sol. of ſilver. A ſolution of a leaf of ſilver in aq. fortis, mixed with powdered ſhells as before, produced, in a ſolid part of the ſame ſhells, a faint reddiſh purple with a little yellow, and a larger quantity of blue. The general effect of thoſe colours at a greater diſtance, was not unlike that of the opal: but it did not laſt above twelve ſeconds. After that, a pale bluiſh white light took place.

Sol. of copper with ſhells. Theſe laſt experiments encouraged me to try *larger* pieces of ſhells. Accordingly, having procured ſome calcined ſhells that were pretty thick, I broke them into convenient pieces, for the purpoſe of putting them into a ſmall crucible. I preferred having *ſeveral* of thoſe pieces at the ſame time in the crucible, that I might have the greater chance of obtaining the colours I was in ſearch of. Upon theſe pieces of ſhells I poured a ſimilar ſolution

tion of copper, and then placed the crucible with the ſhells in the fire for about twenty minutes. The ſeveral pieces were afterwards examined in the dark, after they had been expoſed to the ſun. In conſequence of employing thoſe ſolid ſhells, I obtained, at laſt, a view of the colours I ſo induſtriouſly had ſought after: for there appeared a variety of fine ſpecimens of different colours on moſt of thoſe ſolid pieces. The red and blue appeared moſt lively; next to them the yellow and green: as to the purple, it was ſtill very doubtful.

That I might not be led into any error in thoſe appearances of colours, from the ſhells themſelves poſſeſſing that property, without the addition of any foreign mixture, but merely by calcination; I examined a large parcel of them, that had been properly calcined in a crucible with a view to other experiments. Among thoſe were found a few, as I have obſerved before upon another occaſion, that did exhibit a faint appearance of priſmatic colours; but the red, which was moſt vivid, rather inclined to the orange. Theſe I laid aſide, and made uſe of ſuch only as gave a white light. Cal. ſhells.

It may not be improper to add, in this place, an obſervation, or two, that I made, reſpecting the crucibles in which the ſhells were calcined; and upon the ſhells themſelves after calcination. By ſome accident, or perhaps by the fire itſelf, the crucibles were ſometimes cracked. The ſhells, in this caſe, were generally more coloured than when the crucible was not injured. Another obſervation was, that thoſe edges and corners of the ſhells, which happened to touch the ſide of the crucible, frequently exhibited an orange-coloured phoſphoric light. The novelty of thoſe curious appearances did not, however, interrupt the other experiments I had in view.

I gave a preference to calcined oyſter-ſhells for the baſis of my work, in conſequence of a great variety of experiments, which had been previouſly made upon many calcareous bodies.

N. B. Wherever I ſpeak of ſhells as having been calcined, I wiſh to be underſtood, that I do not mean a *perfect* calcination, but only a certain approximation to that ſtate.

Sol. of gold. A ſolution of half a leaf of gold, in one ounce of aq. regia, was put upon ſome pieces of ſhells in a crucible, and continued in the fire for twenty minutes. The ſhells acquired the purpliſh hue mentioned before, and exhibited a very little phoſphoric light in the dark, except in a few parts, which had not partaken of the purple hue; and on thoſe parts there appeared a little red, and bluiſh light. I was ſomewhat diſappointed in this experiment, as I expected to have found the colours, not only broader and brighter but, more in number. I had therefore the curioſity to remove the purple hue, by *ſcaling* the ſhells tenderly with the point of a penknife. After this, the new ſurfaces were expoſed to the light, one, by one; and then examined in the dark: when a bright red, yellow, green, and blue, appeared on ſome

ſome of thoſe ſurfaces, and only red and blue on others. Two of them were faint, and exhibited a kind of flame colour.

This laſt experiment was repeated, but with a ſolution of ſilver in aq. fortis. The colours here were more vivid than thoſe produced by gold; particularly the red and blue. Whether any difference in the degree of heat, employed in the two experiments, (the duration of which being nearly the ſame) was the cauſe of the difference in their colours, I cannot tell; or whether it might not ariſe from ſomething elſe which eſcaped my obſervation; theſe are circumſtances, however, which may ſome time or other be aſcertained by others: my deſign, in theſe reſearches, being only to relate the facts as I really found them. Sol. of ſilver.

A ſolution of iron in aqua fortis with the ſhells produced the colours alſo; but the blue appeared by far the moſt brilliant. Sol. of iron.

A ſolution of tin in aqua fortis gave but a faint appearance of the colours, excepting the green, which was rather vivid, and broader, on ſome ſpecimens than I had ſeen it before. Sol. of tin.

One tea-ſpoonful of water, ſaturated with green vitriol, being put to about ſix or eight ounces of a ſolution of pot-aſh, I poured ſome of the clear liquor, from which a great part of the vitriol had precipitated, on to the ſhells, and expoſed them to the fire as before; and when they were examined in the dark, no colours appeared, except a white light. Sol. of pot-aſh with vitriol.

A ſo-

Sol. of vitriol. A ſolution of green vitriol only, gave no light. The vitriol having changed the colour of the ſhells, in appearance, to a reddiſh kind of iron ore; and this internally, as well as externally.

Cor. ſub. Corroſive ſublimate with the ſhells produced the colours. The red and yellow were very bright. The green and blue were not ſo vivid.

Experiments upon ACIDS.

AS I had entertained ſome doubts, whether theſe colours were actually produced by the ſeveral metals in their ſolutions, or by the acids themſelves; or that an union of both were abſolutely neceſſary to cauſe thoſe effects upon the ſhells; and leſt I ſhould be led imperceptibly into any miſtake, by making general deductions from facts that ſeemed to require a further inveſtigation, it was the more proper to examine the effects of each of thoſe materials, ſeparately. But before I begun the experiments with the metals, I was led, by degrees, into a ſeries of other experiments, in conſequence of examining the aqua fortis alone, which was the firſt experiment I ſet about.

I poured an equal quantity of this acid, (as I had done before) upon another parcel of ſhells in a crucible, and then expoſed the crucible to the fire for the ſame time; the heat being pretty nearly the ſame as in the former experiments. When the crucible was removed from the fire, and

and the ſhells were cold, I examined them ſeparately in the dark, after having expoſed them to the light; when the priſmatic colours appeared, but ſomething broader than before; though not ſo vivid as with the ſolutions of ſilver, copper, and iron. The parts of the ſhells, that exhibited a white light, were conſiderably brighter than in thoſe produced by the ſolutions of metals.

Sp. nitre.

With ſpirit of nitre, the ſhells gave a broad red light, ſomething like a burning coal, and brighter than the red above. The orange colour lay next to it, and at a little diſtance from that, a blue, ſomewhat brighter than the orange. In two of the ſhells there was a greeniſh light, and the white parts were more intenſe than thoſe produced with aqua fortis.

From ſeeing that the colours were exhibited upon the ſhells, in conſequence of the application of aqua fortis, or ſpirit of nitre, I tried the effects of the other acids upon the ſhells.

Oil of vitriol.

Half an ounce of oil of vitriol, with the ſhells, gave but a very *feeble* whitiſh light, many degrees darker than what the ſhells alone exhibit of themſelves by calcination: except in a few parts, and even thoſe were not to be called bright. The greater parts of the ſhells were totally dark; and when they were ſcaled or broken, the new ſurfaces, even in that caſe, gave only a very feeble light in general.

Spirit of ſalt.

An equal quantity of ſpirit of ſalt, treated in the ſame manner with the ſhells, ſhewed much the ſame appearances; but if there was any difference in the effects, between the two acids, it was in favour of ſpirit of ſalt.

Spirit of vinegar. Concentrated ſpirit of vinegar, produced all the colours: the red and blue were the moſt vivid: but even thoſe were not ſo bright as what were produced by ſpirit of nitre.

Common vinegar gave the colours alſo: theſe however were fainter than the laſt.

By comparing the whole of theſe experiments together, and finding that oil of vitriol, and ſpirit of ſalt, gave no colours to the ſhells; and that thoſe acids are conſidered by the chymiſts as having very little, if any, phlogiſton in them; I begun to entertain an idea, that the principal cauſe why the oyſter-ſhells produce colours, depended upon a phlogiſtic or inflammable principle, which the ſhells received from the metals and acids as have been here related.

To examine this matter further, I ſet about making other experiments; and begun the enquiry with obſerving the effects which the alcaline ſalts produced upon the ſhells.

Experiments upon ALCALINE SALTS.

Oil of tartar. THE firſt which I tried, was oil of tartar. With this, all the colours were produced. The red and green were the fainteſt; the blue was more bright, and occupied the greater part of the ſhells.

A ſmall quantity of pearl-aſhes, with the ſhells, gave the colours alſo, but not very vivid; yet pearl-aſhes I underſtand, contain much more phlogiſtic matter than oil of tartar does. But, perhaps, a difference in the heat which I employed, in the two experiments, occaſioned the difference in the vividneſs of their colours. Pearl-aſh.

Foſſil alcali, produced a great quantity of fine green light, which was exceedingly bright: the blue was next in degree, as to ſplendor: the red and yellow were very faint, compared with the other colours. Some parts of the ſhells gave a very intenſe ſilvery white light, which continued to ſhine a full half hour. Foſſil alcali.

Spirit of ſal ammoniac, gave an exceeding bright white light in every part of ſome of the ſhells: and in two, or three ſmall parts, there was an appearance of bright tender colours. Moſt of theſe ſhells continued to ſhine near half an hour. Spirit of ſal ammoniac.

Experiments upon Neutral Salts.

A quarter of an ounce of nitre applied to the ſhells, in a crucible as before, produced a very ſtrong red light, not unlike a burning coal; a little yellow, ſome green, and more blue; but the blue was brighter than the green. Nitre.

Borax refined, gave very good colours; but not quite ſo vivid as nitre, excepting the red and blue. Some of the calx ſticking to one of the Borax.

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ſhells, I removed it, and then a good red appeared underneath.

Sedative ſalt.

Sedative ſalt, with the ſhells, produced the colours but faintly; excepting the red, and that was rather bright than otherwiſe. I uſed but a very ſmall quantity of ſedative ſalt; perhaps a larger quantity, and a proper degree of heat, would have made the colours more vivid.

Tartar of vitriol.

Tartar of vitriol produced, on one ſhell, a very broad red light, and ſome blue on the confine of it. Other ſhells gave a white and blue light; and one ſhell gave an orange, yellow, blue, and green, colour; but thoſe appeared rather tender upon the whole, though very bright; and continued viſible for a conſiderable time.

There are two or three experiments, which I made upon other ſubſtances, that ought not to be omitted in this place.

Swediſh acid.

The Swediſh acid, or green fluor, put between the ſhells, produced the colours: but the red was moſt brilliant; and thoſe parts that gave a white light were very vivid.

During the calcination of this greeniſh fluor, and ſome time before it fell into pieces, I fancied that there appeared upon it's ſurface a dull purpliſh light, ſomewhat inclining to a mazarine blue; but, to be more certain, I ſhut the windows in the room (for the experiment was made about noon) and then I obſerved the purpliſh light very diſtinctly: this light ſeemed, to extend to ſome diſ-

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tance from the ſurfaces of the fluor, and to be of the phoſphoric kind.

The Derbyſhire ſpar, calcined with the ſhells, gave the colours alſo, and full as bright as the fluor deſcribed above. Spar.

A whitiſh ſpar, or quartz, out of which I took a conſiderable piece of ſilver, gave the colours; but thoſe were not near ſo ſtrong as in the two laſt experiments. Quartz.

It was now time to make experiments upon the metals themſelves, and as they are known to contain no ſmall quantities of phlogiſton, I was the more ſolicitous to examine them.

Experiments upon METALS.

A piece of iron, about two inches long and one broad, was laid between two oyſter-ſhells, and expoſed in a crucible, to a moderate fire for twenty minutes. The ſhells, in conſequence of this treatment, exhibited a red light; and what appeared to me the moſt curious, was, that the reds were in thoſe parts only where the iron had been in contact with the ſhells. The other parts of the two ſurfaces were white. On breaking the ſhells the light was conſiderably brighter than on their ſurfaces; for the red appeared to have penetrated almoſt through the whole thickneſs of each ſhell. Iron.

Round two large ſhells I wound two yards of iron Iron wire.

iron wire. Theſe were then put into a crucible, with a ſoft lump of the calcined ſhells, and continued in the fire for twenty minutes. On theſe ſhells I obſerved, in the dark, a fine red, and blue, light: there were other colours alſo, but then they were very faint. The ſoft lump, which was about the ſize of a large filbert, happened to lie at the bottom of the crucible: this exhibited a fine crimſon red on one part; on another, a very fine blue, three or four times larger than the red. In a remoter part a little green appeared; and a faint yellow in other places. On the back part, of the ſame piece, it was all red from end to end. I muſt here obſerve that, part of the wire, which paſſed round the ſhells, was in contact with the ſoft lump; and the parts of the ſhells, at their edges, where thoſe wires had been more cloſely in contact, and had cut the ſhells next the bottom of the crucible, exhibited the bright red and blue light mentioned above.

It may be proper to take notice in this place, that moſt of the ſhells, employed in the preceding experiments, were put into the crucible in a vertical poſition. And as that method anſwered my purpoſe, I generally continued to obſerve it, though it is not always expreſſed.

Iron crucible.

The laſt experiment was varied, by putting the ſhells into an iron crucible. The colours produced by this alteration, were very diſtinct on ſome ſpecimens. The red and orange were on thoſe parts of the ſhells that lay next the iron: other parts

parts gave a brilliant bluiſh white light, and ſome green. I then broke one of the ſhells, and obſerved, that the red had penetrated one eighth of an inch deep, and that a bright green appeared on the confine of it. Another ſhell, which had been purpoſely laid horizontally at the bottom of the crucible, on being broken, exhibited an intenſe, ſilveriſh white light only. According to my uſual method of examining the ſhells, I ſeparated the firſt ſcale from one of them, and expoſed the new ſurface to the ſun; after this, I was ſurpriſed to find the white light much more beautiful than before; and that there was a little appearance of colours in one part of it. In the making of this experiment, I had previouſly laid a ſhell on the top of the vertical ones, in an horizontal poſition alſo, but with a view to obſerve how it would be affected, in regard to it's giving colours, when compared with the other ſhell which lay at the bottom of the crucible: but this afforded a very little light, and no colours whatſoever.

Canton's phoſph.

I had frequently made the phoſphorus deſcribed by Mr. Canton in the Philoſophical Tranſactions, conſiſting of powdered ſhells and ſulphur, without having ever been able to obſerve any other phoſphoric colour than a fine yellowiſh white light. However, I had an inclination to try, whether even in it's powdered ſtate, joined with a ſolid lump or two of ſhells, it would not afford colours. To this end, having mixed the powdered ſhells with flowers of ſulphur, in their proper proportions, I put

put half of the quantity into the iron crucible, and then laid upon it two pieces of ſhells, about half an inch in diameter, one upon the other, horizontally. Upon thoſe I put the other half of the mixture, and preſſed the whole down very cloſe. The iron veſſel was then expoſed to a good fire for thirty minutes; and when it was cold, the maſs was broken into two parts, where the ſolid ſhells had been put. On expoſing thoſe parts to the light, a breadth of tender colours appeared in the middle, above one inch in diameter, extending a little beyond the confines of the ſolid ſhells; but thoſe colours were not very bright. The red occupied the centre, and was by much the broadeſt: a faint orange colour lay next to it; and a little yellow appeared beyond the orange. The other parts of the whole maſs exhibited only a very tender, yellowiſh white light.

Since the firſt edition of this work I have found, that all the colours and thoſe very fine, may be produced by the common preparation of this phoſphorus, and without the aſſiſtance of any other circumſtance, than a proper degree of heat.

Iron-filings. To equal parts of iron-filings and nitre, I put ſeveral large fragments of ſhells: theſe materials were expoſed to the fire, for fifteen or twenty minutes. After this, the ſhells exhibited a faintiſh red, and other colours; but the white light was exceedingly bright. When I came to view the ſhells in the open day more attentively, I obſerved that the calx of the iron had formed a kind of cruſt

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upon them in ſeveral parts. This calx was carefully removed by a pen-knife, without ſcaling the ſhells: and then they were expoſed again to the light. Upon this, a very fine red appeared in thoſe parts where the calx had been removed, as likewiſe a good green, and an intenſe blue very near it. That part of the ſhell where the calx was removed ſeemed firmer, and more compact, than any of the other parts.

This latter part of the experiment, in which the calx upon the ſhells had, as it were, been encruſted, turning out ſo ſatisfactorily, brings to my mind the firſt experiment I made with the ſolution of copper, and which exhibited the colours, reſembling thoſe in the rainbow. I mention this circumſtance the rather, as the two experiments appear to illuſtrate each other. For in that experiment I had obſerved, with the aſſiſtance of a magnifying glaſs, that a larger quantity of the calx of copper lay next the red, than there appeared to be in any other part of the whole maſs.

Upon ſucceeding ſo well in the experiments with iron, it was ſuggeſted to me by Mr. Delaval, to try what the difference would be with ſteel: on account of the greater quantity of phlogiſton it is known to contain.

Steel and Iron.

For this purpoſe, I procured ſome watch-ſprings, and broke them into ſixteen pieces: each of thoſe were two inches long. I procured alſo an iron-plate of the ſame thickneſs with the watch-ſpring, or very near it. This plate was cut into four pieces

pieces, two inches long alſo: and each breadth correſponded nearly with four of the ſprings, when they were laid cloſe and parallel to each other; for in this ſituation I diſpoſed them between the ſhells: I then put ſeven ſhells into a crucible vertically, and cloſe to one another. On one ſide of the middle ſhell were placed four of the ſprings; and between each of the other ſhells and the crucible, on the ſame ſide, I put four more; making in the whole ſixteen pieces. On the other ſide of the middle ſhell I put one of the iron plates, and another plate between each of the ſhells and the crucible on that ſide; being in all four. In this ſituation the crucible was put in a clear ſea-coal fire for twenty minutes: and during the latter half of the time the heat was increaſed by blowing. Before each ſhell was examined in the dark, particular care was taken to obſerve the direction in which the metals were expoſed to the ſhells. I began my obſervations with the middle ſhell: and preſented to the light that ſide of the ſhell firſt where the iron had been in contact. The colours were very diſtinct, but not ſo brilliant as I have ſometimes ſeen them in other experiments: the red, yellow, and blue, particularly. I then expoſed the other ſide of the ſame ſhell to the light, where the ſteel had been in contact, when the colours appeared conſiderably brighter; for in thoſe parts of the ſhell where the ſteel had touched, the red appeared very intenſe and lively; beyond that, lay the other colours; but not in their order: for the green was next the red,

red, then a little orange yellow; and beyond that, at a little diſtance, a very fine blue. In the next ſhell, which had ſteel on both ſides, the colours were pretty near the ſame, eſpecially the red. The ſecond ſhell, from the middle, exhibited the ſame colours; but the red, though leſs in quantity, was more intenſely bright; eſpecially in that part of the ſhell which had been neareſt the bottom of the crucible. This difference was probably occaſioned by the pieces of ſteel having changed their ſituation accidentally, becauſe, on taking this ſhell out of the crucible, I found their ends lying one upon another, towards the bottom. The third ſhell had the ſame colours on that ſide next the ſecond ſhell, and nearly as intenſe as thoſe upon the neighbouring ſurface, which the ſecond ſhell produced. But the other ſide of this third ſhell which was next the crucible, where four pieces of ſteel had alſo been placed, and which continued in the ſituation I had laid them, exhibited an equal quantity of red light, together with ſome other colours; as was obſerved in the firſt ſhell, reckoning from the middle.

Having completed the obſervations where ſteel was employed, I examined the other ſhells, where the plates of iron had been put. The firſt ſhell, next the middle one, as likewiſe the ſecond and third, exhibited the ſame colours with thoſe that the iron produced on one ſide of the middle ſhell; and their degrees of brightneſs were nearly the ſame when compared with one another, but much inferior to thoſe produced by the ſteel.

From this laſt experiment I collected, that the colours produced by the ſteel, being ſo much brighter, and more intenſe, than thoſe produced by the iron, the difference between them muſt have been occaſioned, by the difference in the quantity of phlogiſton they reſpectively contained. Now ſteel, I underſtand, is made by giving to iron a greater quantity of phlogiſton than it contained before. And the method of attaining this end is, generally, by expoſing the iron for a long time in a charcoal fire: for charcoal, according to the chymiſts, abounds with phlogiſton. After that operation, nothing more is required to make ſteel, than to quench the iron in water. From theſe conſiderations it ſeemed proper to try the effects of the charcoal itſelf, upon the ſhells.

Charcoal.

Accordingly, between ſeveral ſhells, I put ſome broad pieces of charcoal: they were made flat for the greater convenience of bringing their ſurfaces nearer to a contact with the ſhells themſelves. Theſe were put into a crucible, and expoſed to a ſeacoal fire for twenty minutes. After that, the ſhells exhibited exceeding fine colours: but the red and blue were the moſt intenſe; next to them the green had the preference; and in two ſpecimens the colours were ſo finely intermixed, yet at the ſame time ſo diſtinct, that they reſembled, in ſome degree, the tail of a peacock; but then they

they were very far beyond it in point of brilliancy.

The connection that Pruſſian blue is known to have with ſteel, from its being alſo compoſed of iron and phlogiſton, encouraged me ſufficiently examine the effects of that ſubſtance upon the ſhells.

Pruſſian blue.

A quantity of Pruſſian blue was therefore put into a crucible among ſeveral ſhells, and expoſed to the fire for twenty minutes. Theſe ſhells afterwards exhibited a phoſphoric yellowiſh white light, which was not very bright. From my former obſervations I had reaſon to apprehend, that the calx of the iron ſticking to the ſhells, obſtructed other appearances; I therefore ſcaled the ſhells, and then expoſed them again to the light: on doing this, the red, yellow, and green, were very diſtinct and bright: there was alſo a blue, but it was very faint, compared with the other colours. This blue, however, appeared brighter on removing one more of the ſcales.

It was now time to examine the other metals with the ſhells.

Gold.

Upon conſidering the nature of gold, which ſo obſtinately reſiſts the moſt violent attacks of fire, without ſuffering the leaſt diminution of it's weight, a ſuſpicion had been entertained by ſome, that it would produce no colour when applied to the ſhells. But this conjecture did not correſpond with the experiment; for notwithſtanding I took particular care to have ſtandard gold from the refiner, and was very attentive in the management of it with the ſhells, the colours were produced by it: but then thoſe colours were rather tender, upon the whole, when compared with one another, and not very bright. The quantity of red equalled the ſize of the piece of gold, and that was nearly equal to the ſize of a guinea. It weighed about thirteen penny-weights; and was beaten flat for my purpoſe with an iron hammer. This circumſtance is mentioned, as I would have done any other, had it occurred, which might even cauſe a ſuſpicion of it's being the leaſt unfavourable to the experiment.

Grain gold. However, as I was not perfectly ſatisfied with this laſt experiment, I procured two ſmall parcels of *grain* gold from the refiner. One of thoſe parcels was put into a crucible; and whilſt it continued in fuſion, ſix ounces of powdered nitre were added, but at different times. The gold, before it was melted, weighed three penny-weights. On taking it out of the crucible, it weighed two grains leſs. This difference aroſe, perhaps in part, from ſcraping away the ſcoriæ with a piece of glaſs; for a few particles of gold ſeemed to accompany the ſcoriæ. The gold ſo prepared was put (without beating it flat) between two ſhells; and thoſe were then placed near the bottom of a crucible in an horizontal poſition. In another crucible, and between other ſhells, for there were ſeveral in it, I put the other parcel of *grain* gold, and which was nearly of the ſame weight with the former, and then placed the two crucibles in a wind

wind furnace for ten minutes. The ſhells were afterwards examined in the dark, and within two hours after they were taken out of the fire; but, before this was done, I obſerved, in the open day, that moſt of the ſhells, from both crucibles, had acquired a reddiſh purple hue, but particularly in thoſe parts where the gold had been in contact. On expoſing them to, and removing them from, the light, ſeparately, I was ſurpriſed at ſeeing not only all the colours on the ſhells from both crucibles, but at finding them far more vivid, than in the former experiment. The red, however, was by far the moſt predominant in thoſe ſhells that were expoſed to the gold where no nitre had been put. For one of the ſhells exhibited a bright red all over it's ſurface, and on both ſides of it; ſo that the general appearance ſeemed as if the red light had actually penetrated through the whole ſubſtance of the ſhell. Upon removing the upper ſcale, which I did with ſome difficulty, on account of the firmneſs it had acquired, as likewiſe a ſecond, and a third, the red appeared a little brighter; and, when I broke it, the ſame colour then evidently appeared as having penetrated quite through the ſhell. Half of another ſhell appeared red, and the other half green. Upon removing one of the laminæ, a very fine blue diſcovered itſelf under the green, and the red appeared more intenſe. A third ſhell exhibited a red on one ſide, and a kind of opal colour on the other; and a fourth was red in every part. One of the ſhells from the other

 crucible

crucible exhibited a fine red at one end, and next to it a yellow, then a great quantity of fine green, and a good blue beyond it. The other ſhell exhibited much the ſame appearances, except that it was more motled with the colours. In all thoſe ſhells there was no appearance of any white light whatſoever. The red in general was not ſo intenſe as in ſome of the preceding experiments. However, I think this red was very like the colour of the ſun when ſeen through a ſmoaked glaſs, or a moderate fog in the ſpring or autumn.

Leaf gold. As I found it inconvenient to procure grain gold, I had recourſe to another expedient, and that was to try whether a much leſs quantity would not anſwer the purpoſe full as well. To this end, I covered ſome calcined ſhells with ſingle leaves of gold, and then laid other ſhells upon them. Theſe were put into a crucible, and expoſed to a good fire for fifteen minutes. When they were cold, I obſerved that the pieces of the leaves of gold, upon different ſhells, were a little changed in their colour: theſe were bruſhed off; and, after expoſing the ſhells to the light, the colours appeared, but then they were exceedingly faint; the yellow was broadeſt and brighteſt, the green next, and the red the leaſt ſo. Perhaps, if, inſtead of ſingle leaves of gold, I had put four or five leaves upon one another, they might have exhibited the colours more diſtinct and lively.

Quickſilver. An ounce of quickſilver was put to three ſhells in a crucible, and expoſed to a moderate fire about eight

eight minutes: whether the ſhells were continued too long in it, or not long enough, I cannot tell: for on examining them in the dark, I obſerved only a very fine blue in different parts of them, and thoſe parts were the moſt remote from the bottom of the crucible. The breaking, or ſcaling, of the ſhells made no difference in the appearances.

Not being ſatisfied with this laſt experiment, I repeated it again two months afterwards, but with a quarter of an ounce of quickſilver; and with the ſame number of ſhells. The crucible was now put into the fire with the ſhells in it, and when they were red hot, I dropped the quickſilver into the crucible, and continued the heat for three minutes more. After this, the colours, in two of the ſhells, were very diſtinct and bright; particularly the red, yellow, and blue. The other ſhell exhibited a little red, ſome green, and a great deal of white light.

Lead, produced the colours, but the red, orange, and yellow, were the brighteſt. Lead.

In another experiment, but with a leſs quantity of lead, the colours appeared more vivid, eſpecially the green and blue.

Thirteen penny-weights of the fineſt ſilver I could procure were put between two ſhells, and expoſed in a wind furnace for eight or ten minutes; by which the ſhells acquired a dirty purpliſh hue, and a firmer texture in ſome parts. In the ſame ſtate they were expoſed to the ſun: but no kind of light Silver.

light appeared upon thoſe purpliſh parts. I then removed the purpliſh hue, by ſcaling the ſhells with a penknife as before: in this ſtate they were expoſed again to the light; the colours now appeared, and to great advantage. The red and blue were the moſt intenſe and beautiful; the green next, but the yellow and purple were rather faint. Other parts of the ſhells were white, and exceedingly brilliant: thoſe continued luminous a full half hour. I do not mean by this, that the colours laſted ſo long, but only that ſome of the whitiſh light was perceptible at the end of half an hour.

Grain ſilver. I repeated the laſt experiment with about five penny-weights of ſilver, in grains, from the refiner: theſe grains were put between *ſeveral* ſhells in a crucible. And after the uſual treatment of ſcaling, &c. one of the ſhells exhibited a fine red and blue only; another a little red, with a great quantity of blue, and a little purple; a third exhibited a green and blue, with a conſiderable quantity of a ſilvery, white light, but very intenſe; and a fourth ſhewed red, yellow, and blue colours, that were rather vivid than otherwiſe. Two other pieces exhibited only a white light, but then this light was exceedingly brilliant.

Native ſilver. A piece of native ſilver, from the collection of Guſtavus Brander, Eſq; who favoured me with it, produced the colours: they were not, however, near ſo lively, as thoſe which appeared in the two laſt experiments.

Common copper. Two ſmall plates of common copper were applied to the ſhells, and after being expoſed in the fire

fire for fifteen minutes, thoſe ſhells exhibited the colours alſo, though they were inferior to them that are mentioned in the *firſt* experiment, where a ſolution of the ſame metal was made uſe of.

A very fine ſpecimen of copper, from the ſame collection, produced the colours nearly the ſame with the laſt experiment; except that the red and yellow were conſiderably brighter and more intenſe. Very fine copper.

In the courſe of thoſe laſt experiments, where colours have been produced by the application of metal to the ſhells, different degrees of heat were employed: I was obliged therefore to proportion the time of continuing the crucibles in the fire accordingly; but not without giving ſome attention to the nature of the different materials concerned in each experiment. Neverthelefs, even though I obſerved theſe precautions, I would not be underſtood, that I am at all acquainted with the degree of heat that is neceſſary for producing the colours to the greateſt advantage.

With Tin, the experiment ſucceeded pretty well: the red and yellow were but faint, compared with the green and blue that the ſame ſhells exhibited. Tin.

Having finiſhed the experiments upon ſhells with the metals, I was deſirous of ſeeing the effects of one of the ſemi-metals, which is ſaid to contain a great deal of phlogiſton: what I mean is zinc. I had no ſooner gone through the proceſs with it, but I examined the ſhells, even before they were quite Zinc.

cold, without being able to discover any appearance, but a little white light. I succeeded no better when they were cold. The outward appearance of the shells in the open day, was a little yellowish; and from what had been observed before, it seemed very necessary to remove this yellowish tinge: I therefore scaled the shells, and exposed them again to the light; upon examining them afterwards in the dark, there appeared very large breadths of fine prismatic colours on every shell; but the red was by far the most intense and lively. I then broke those shells into several pieces: and on observing them separately, after their being exposed to the light, there appeared, in many parts, all the colours; and in others, a most beautiful intense silvery white light, which continued to shine a full half hour.

After this, it seemed time to put an end to those kind of experiments, notwithstanding there were many other substances that I had not tried, which might have been brought into this class with propriety: such as reg. of cobalt, bismuth, marcasites, cinnabar, &c. but these were considered as being unnecessary.

In the course of the preceding experiments I sometimes suspected, that it was necessary to clean, and scrape, the shells very well before they were put into the crucible; in consequence of my having obseved that a few of them, so treated, seemed to exhibit the colours more lively. Whether it was really the case or not, I am still ignorant: however, in

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my future experiments, theſe precautions were not unattended to.

I HAVE now gone through the experiments propoſed, in which the priſmatic colours were produced with the ſhells, by the ſolutions of metals, by ſome of the acids, (for the other acids did not produce the colours) and by all the alcalies; as well as by ſeveral of the neutral ſalts: and having extended my reſearches to the metals themſelves ſo ſucceſsfully, where no other ſubſtance was introduced but the common baſis I made uſe of, namely, the calcareous earth: and finding that, among other ſubſtances, ſteel, as alſo charcoal, and Pruſſian blue, which are allowed to contain a conſiderable ſhare of phlogiſton, produce the priſmatic colours in an eminent degree; there appears to me a further foundation for entertaining more than a conjecture, that the priſmatic colours, as well as the light in general of all phoſphori, depend, in ſome degree at leaſt, upon that inflammable principle which ſeems to be diſengaged, by the force of fire, from the ſubſtances, the ſeveral ſhells were in contact with, in the crucible.

Nothing more remains, therefore, to eſtabliſh the principle and make it general, than to examine the effects of other ſubſtances upon the ſhells, in the ſame circumſtances, which are known to abound with phlogiſton. And if ſimilar appearances are to be obtained from them, we may then, I

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think, ſafely pronounce the principle general; at leaſt, till exceptions are brought againſt it.

Experiments upon INFLAMMABLE BODIES.

Linſeed Oil. ONE tea-ſpoonful of linſeed oil was put to the ſhells in a crucible, and expoſed to the fire for twenty minutes. The ſhells, afterwards, exhibited a feeble white light, and a very intenſe blue. In ſome ſpecimens a little orange and green, but no red. From obſerving the reſult of the ſecond experiment with quickſilver, I ſuſpected that theſe ſhells were not continued long enough in the fire.

Olive Oil. Olive oil, gave a good blue and green, and a faint orange colour.

Oil of turpentine. Oil of turpentine, produced a fine red and blue. Some other parts were green and white, but thoſe appearances were fainter than what are mentioned in the two laſt experiments.

Wax. A lump of wax, the ſize of a very ſmall nut, burnt with the ſhells, gave the colours alſo; and rather bright than otherwiſe.

Reſin. Reſin, produced the red, yellow, green, and blue, better than wax.

Pitch. Pitch, gave the colours much the ſame, except that the blue was brighter than the reſt.

Gum arabic. Gum arabic, gave a very intenſe bright red and blue, with a little yellow, and a dirty browniſh hue in other parts.

Copal,

Copal, produced the colours, but not near ſo bright as gum arabic. Copal.

Gum tragacanth, afforded colours alſo; the red exceeded in brightneſs the red produced by copal; the yellow and blue appeared rather better than the green. Tragacanth.

Flame, from a large wax candle, and alſo from ſpirit of wine, produced very fine colours; the red and yellow were the brighteſt, the green and blue next; the violet was leſs vivid. Flame.

Theſe colours were not only produced by burning the calcined ſhells in a crucible after they had been expoſed to the different flames for two or three minutes; but alſo, without expoſing thoſe ſhells to any other heat than the flame itſelf, it being fully ſufficient for the purpoſe. In theſe experiments the colours, produced by the flame of ſpirit of wine, were rather weaker than thoſe produced by the flame of wax.

A table-ſpoonful of lamp-black, or ſoot, put into the crucible, and expoſed to the fire for fifteen minutes, gave very fine colours alſo; the yellow, green, and blue were the moſt brilliant, the red next, but very broad; and the purple fainter. Soot.

A quarter of a ſheet of white paper, being cut into ſeveral pieces; I put them into a crucible along with ſeveral ſhells: and then expoſed the crucible to a fire, where I continued it twenty minutes. On examining the ſhells afterwards in the dark, moſt of them had acquired the colours; and thoſe colours appeared rather brilliant than otherwiſe. Paper.

otherwife. Two of the fhells however, fhewed only a fine blue light. Thefe were expofed again to the fire for ten minutes more (but in the crucible) in confequence of this, they exhibited all the colours; but the blue was weakened confiderably. From the effects produced by this fecond application of heat, I fufpect that the fhells in fome of the preceding experiments, which only exhibited green or blue, were not continued long enough in the fire.

Linen. Linen, treated in the fame manner, (except that it was continued in the fire for thirty minutes) gave the colours rather ftronger than the paper.

Cotton. I put a little cotton, along with fome fhells, into a crucible, and after thefe had been in the fire for fifteen minutes, two of the fhells exhibited three colours full and lively, viz. red, green, and blue; the other fhells gave a blue and white light only.

Sugar. A lump of fugar, the fize of a hazle nut, expofed, with the fhells in a crucible, to a fire for twenty minutes, produced fome exceeding fine fpecimens of colours. In three of the fhells there were great quantities of fine red and green, a little yellow, and an intenfe blue, but lefs purple. In one of the fhells the colours lay regularly, and in their order.

Wheat flour. Flour of wheat (about three tea-fpoonfuls) put into a crucible with the fhells, produced the colours; but then they were rather faint, excepting the red and blue, and thoſe were a little brighter than the reſt.

Some of the ſame flour, with a ſolution of pot-aſh, made into a cake and toaſted a little, gave all the colours without the application of the ſhells; but the red was a heavy dirty colour. This was not the caſe with the other colours, they being rather vivid, and the parts that gave a white light were very bright. Flour with ſolution of pot-aſh.

Oil of vitriol, with the ſhells, (in a former experiment,) producing no colours, and ſcarce any light, I put ſome powdered charcoal to them, and then expoſed the ſhells again to the fire for fifteen minutes. After this there appeared two or three of the colours, but thoſe colours were rather faint. However, the whitiſh appearance was conſiderably improved. Charcoal, &c.

Spirit of ſalt, with the ſhells, (in a former experiment) likewiſe producing no colour, I put ſome powdered charcoal to thoſe alſo: in conſequence of which, the colours appeared, and nearly of the ſame ſtrength as in the laſt experiment.

The experiment, I had made before with oil of vitriol, was varied, by mixing ſome powdered charcoal with it, and making a kind of paſte thereof. This paſte was put into a crucible, and the ſhells were then forced into the paſte. The crucible ſo circumſtanced, was put into the fire for fifteen minutes. By this treatment the colours were improved, and the blue particularly; it being very fine. Other parts exhibited a very lively white light.

Tartar

Tart. vit. Tartar of vitriol with charcoal, gave a fine white light to the ſhells, and a faint appearance of red and yellow.

Sp. wine. Spirit of wine, produced only, a lively greeniſh, and bluiſh, light. I made but one trial with it. Perhaps a different quantity of it, and a different heat, would have occaſioned more colours.

Fat. A quarter of an ounce of animal fat with the ſhells exhibited a fine red, and blue, on two ſhells; and a purpliſh ſilvery white light all over the other parts. The other ſhells were very bright, and of a ſilvery hue.

Butter. Freſh butter, produced the colours, and rather vivid than otherwiſe; but the blue was the brighteſt: and the parts that were white, appeared not quite ſo intenſe as thoſe in the laſt experiment.

Glue. A piece of glue, the ſize of a ſhilling, broken into ſmall pieces, and put between two ſhells, produced one very fine brilliant ſpecimen of colours, without ſcaling any part of the ſhell. The red was rather brighter than that which I obtained from nitre; and this colour was at that end of the ſhell which was neareſt the bottom of the crucible. The yellow lay next to it, then the green; but the green was much broader, and more intenſe than the yellow: the blue lay next to the green, and a faint purple appeared at the top of the ſhell. I now removed the upper ſcale of this ſhell, and expoſed it again to the light. On doing this, two ſeparate reds appeared where one only had been viſible before;

before; but theſe were conſiderably brighter: and what ſeemed the moſt curious to me was, an exceeding brilliant *purple light*, which lay in the interval between the two reds. The yellow green and blue colours, were a little improved: but the purple at the top of the ſhell had totally diſappeared. In this ſpecimen there was no appearance of any white light whatſoever. There were three more ſhells in the ſame crucible at the ſame time; but not one of them exhibited any of the colours that were ſo remarkably vivid, as the ſpecimen I have now been deſcribing.

Wool, put between two ſhells, in a crucible, and expoſed to the fire for ten minutes, and then to the light, exhibited, in the dark, a red, green, and blue light on one of the ſhells, and only a blue light, on the other ſhell. **Wool.**

The bone of the ſcuttle-fiſh, which had been thrown by the ſea upon the beach, and which contains a conſiderable ſhare of oil, (even in it's dried ſtate,) was put into a common ſeacoal fire. In a very little time it was properly calcined in ſome parts, and not in others; but being of an exceeding looſe texture (after this its partial calcination) it was with ſome difficulty I could preſerve three or four pieces from breaking into ſmaller parts. Theſe were expoſed to the light as uſual; and when I examined them in the dark, there appeared a red, and blue, colour on thoſe parts which were rendered white by the calcination: the other parts, that were not calcined, had a black appear- **Scuttle-fiſh.**

ance, and afforded no colours, or any phoſphoric light.

Hair. Hair, burnt with the ſhells, produced a red and blue light; thoſe colours were rather bright than otherwiſe.

Amber. Amber being exceedingly inflammable, I was not a little ſolicitous to try what effect it would have upon the ſhells. Having procured, therefore, three flat pieces of amber, about the ſize of a ſhilling, and near the thickneſs of a crown piece, they were put between four ſhells in a crucible; and after expoſing them to a good fire for twelve minutes, I took an opportunity, when the ſun was ſhining, for it was in December, to expoſe each ſhell to it's rays. The colours thoſe ſhells exhibited, in the dark, were very fine: the orange and yellow predominated greatly over the other colours upon one of the ſhells. Upon another, the red and green appeared deeper and more intenſe. One of the middle ſhells exhibited all the colours exactly in their order, and thoſe were very ſplendid. The red part of the ſhells, in all theſe ſpecimens, had been next the bottom of the crucible; and what proved rather a rarity to me was, the purple colour, which appeared full as bright as that which the glue produced. None of theſe colours were ſo vivid as when the upper ſcale was removed. But it muſt be remembered, that this is not always practicable, on account of a certain degree of hardneſs which the ſhells acquire in ſome of theſe experiments; yet ſcraping the ſhells does not anſwer ſo well at leaſt with me. In this caſe, I generally have recourſe to breaking the ſhells into ſeveral parts; by which means I have an opportunity of ſeeing, from their edges, how deep the different colours penetrate the ſhells, after they have been expoſed to the ſun.

The moſt inflammable ſubſtance I tried, was the phoſphorus of Kunkel, made by the late Dr. Hadley, who favoured me with it many years ago.

Between two ſhells that were placed horizontally in a crucible, I put a ſmall piece of this phoſphorus, about the ſize of a very ſmall pea, and expoſed the crucible, ſo circumſtanced, to a moderate fire; aſter a little time a fine bright flame appeared from between the two ſhells: in five or ſix minutes more, this flame expired. I continued the crucible in the fire, three or four minutes longer: and, after having expoſed the ſhells to the ſun, I examined them in the dark: and obſerved that the ſplendor of their light, in general, was very great. The blue appeared the ſtrongeſt and brighteſt, the green and yellow were rather tender, but very brilliant. Upon another ſhell there was a tender orange colour and green, which were alſo very fine; beſides theſe, there was a faint appearance of the other colours, excepting red. Theſe ſhells continued luminous above thirty minutes. **Solid phoſ.**

From the many obſervations made upon theſe colours, during ſuch a variety of experiments, I had ſufficient encouragement to try whether the yellow, green, and blue, colours, in ſome of my **Changing of colours.**

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beſt ſpecimens, might not be *changed* to red, by bringing the inflammable ſubſtances into contact with thoſe parts that gave ſuch colours.

Accordingly, I put ſeveral of thoſe fine ſpecimens again into the fire, in five different crucibles: after having marked, with the point of a penknife, the places on the ſhells, which exhibited thoſe colours only. To theſe were applied the inflammable ſubſtances, ſuch as paper, ſugar, gum arabic, amber, and charcoal, ſeparately. And though I found it difficult to bring moſt of theſe inflammable bodies into contact with all the parts of the ſhells that exhibited the yellow, green, and blue colours, on account of their irregular ſurfaces; yet with a little induſtry theſe difficulties were, in a conſiderable degree, removed. For after expoſing the ſeveral crucibles to the fire, but at different times, and then examining the ſhells in the dark ſeparately, many of the colours were entirely vaniſhed; and in their room the red had taken place; particularly in three ſpecimens, where very broad, and intenſe, blues had predominated; the greateſt parts, in general, of that colour upon the three ſhells, were, by this treatment, changed to a very fine red.

Moſt of thoſe ſhells ſeemed harder and firmer, after this ſecond proceſs, than before; I mean in thoſe parts where the new reds were produced.

All theſe trials, in the five crucibles, are ſet down as one experiment; becauſe it was apprehended,

hended, that the mentioning of them ſeparately might be thought too tedious.

One of the ſhells, which gave no other phoſphoric light than what was cauſed by calcination, I expoſed, for ten minutes, to the rays of the ſun, collected by a mirror, which was about fourteen inches diameter, and then removed it into the dark. On that part of the ſhell where the rays fell, the light appeared conſiderably brighter, but without exhibiting any colours. It did not laſt long: neither was the brightneſs of the light to be reſtored by expoſing the ſhell, in my uſual way, (through the curtain) to the open day. Light.

I now expoſed one of the fine ſpecimens of the ſhells to the collected rays, as before, for two or three minutes; and by moving the ſhell a little, the greater part of it's ſurface was illumined by thoſe rays. This ſhell, in the dark, exhibited the priſmatic colours very lively, and I think rather more ſo than I had ever ſeen them on any of my former ſpecimens.

With the ſun's light, collected in a focus by a concave mirror that was about two feet diameter, I calcined ſome oyſter-ſhells; with a view to try, whether, by that treatment only, they might not be made to exhibit phoſphoric appearances, and particularly the priſmatic colours.

When the inſide, of one of the ſhells, was expoſed to the collected rays, there iſſued from it a

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vapour, which was ſometimes accompanied with pretty loud exploſions: this vapour continued to paſs out for a while, frequently burſting forth in great quantities, and throwing ſeveral ſmall parts of the ſhell along with it. At the ſame time there eſcaped from this ſhell (as well as the other ſhells that I afterwards tried) a watery kind of matter, which fell upon the floor in drops. When I thought that part of the ſhell, to which the rays were applied, had been ſufficiently calcined, I ſhifted the ſituation of the ſhell, to calcine a different part. By repeating this operation, I at laſt accompliſhed the calcination of four-fifths of the ſhell at leaſt; I mean on the inſide thereof. After this, I expoſed the back part of the ſhell to the rays in the ſame manner, but not for ſo long a time. The longeſt time this ſide of the ſhell was expoſed to the rays, in all theſe trials, did not exceed three minutes; and the ſhorteſt about twenty ſeconds. I obſerved the ſame rule nearly with all the other ſhells, that were calcined afterwards. Being thus prepared, I took the ſhells into the dark cloſet, and after waiting a proper time, they were expoſed to the light of the ſun. On examining them in the dark, there appeared, on the inſide of one ſhell, a very good red, about half an inch in diameter; a little yellow, and a great deal of tender blue. In two or three parts there was a faint appearance of purple. And at the back of the ſame ſhell I obſerved two broad tender blues, and a little purple between them.

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On the inſide of the ſecond ſhell, the red and yellow were very vivid; there was alſo a good green; but the blue was very faint; and if there was a purple, I did not obſerve it. This, however, was not the caſe on the oppoſite ſide, or back, of the ſhell; for the purple predominated over a moderate red, and blue, light.

The colours which the other ſhells exhibited, were pretty nearly the ſame with thoſe I have juſt now deſcribed; except that, in two of them, there was no appearance of purple; and in one, no green; but then there was ſome red, and blue, with a great deal of white light in various parts.

It may be proper to obſerve, in this place, that moſt of the preceding experiments were made in the winter, and in various kinds of weather. However, I would not be underſtood to mean by this, that a thick fog, or rain, or ſnow, are ſeaſons ſo advantageous for expoſing the ſhells to the light, in order to ſee the colours; as a clear ſun, or even a cloudy day: and yet I have been able to obſerve the priſmatic colours pretty diſtinct, even in thoſe unfavourable ſeaſons.

I HAVE now gone through all the experiments, upon inflammable bodies, that ſeem moſt eſſential for eſtabliſhing *one* of the principles at leaſt, upon which the priſmatic colours, in the ſhells, appears to depend. As to the nature of this principle,

principle, whether it be perfectly ſimple or otherwiſe, I muſt leave to be determined by thoſe who have ſuperior abilities for inveſtigating ſuch difficult queſtions: I content myſelf with having ſhewn that phlogiſton is eſſentially neceſſary towards producing priſmatic colours in phoſphoric ſubſtances. And becauſe calcined ſhells were generally employed in moſt of the preceding experiments, I ſhall make a few obſervations in this place, that may contribute to the better underſtanding of this curious ſubject.

Calcined ſhells, from the weak coheſion of their parts, are eaſily broken or ſeparated, into numberleſs ſcales: and even thoſe again are found capable of being further ſeparated and reduced, ſo as to become inviſible to the naked eye.

From ſeveral of the preceding experiments it appears, that the coheſion of thoſe parts may be conſiderably increaſed, in many caſes, by only adding a quantity of the inflammable matter: for a firmneſs, and compactneſs, frequently takes place, in thoſe parts only, which exhibit a red light.

And as I have conſtantly found, that any part of the ſhell, may be diſpoſed to give a red light, by only bringing the part propoſed to have that colour, into contact with the ſubſtance that gives the phlogiſton, we have a proof, that there is no particular diſpoſition in the ſhell to exhibit that colour only, without the aid and aſſiſtance of the inflammable principle.

By this red happening in conſequence of the

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contaſt, it appears, that more phlogiſton muſt have penetrated the ſhell in the part where the red was exhibited, than in any other. This colour, therefore, depended in ſome degree at leaſt, upon the quantity of phlogiſton received in that particular part.

And this reaſoning received the fulleſt confirmation from the experiment where the other colours were ſeverally changed to red, by putting the ſeveral parts of the ſhell, which exhibited them, into contaſt with the ſubſtance that imparted the phlogiſton *.

According to theſe obſervations then, it ſhould ſeem, that *different quantities* of phlogiſton are united, in ſome degree, with the ſhells which exhibit priſmatic colours; and that the ſeveral parts of the ſhells exhibit ſuch colours as correſpond with the different quantities of phlogiſton they reſpeſtively contain. The parts, for example, which contain the greateſt quantity of phlogiſton (though even that, perhaps, may be limited) emit, as I ſuppoſe, no other colour than red; and thoſe which contain a leſs quantity (but next in degree) emit no other colour than yellow; and ſo on of the reſt.

When, therefore, the whole ſubſtance of any ſhell happens to be uniformly prepared, ſo that the quantity of phlogiſton combined with it, is

* Page 59.

not more in one place than in another; ſuch a ſhell, I apprehend, according to this manner of reaſoning, ſhould emit but one uniform colour throughout. And if we conſult experiment, this obſervation is not without ſome foundation: for I have frequently found that ſome ſhells would exhibit a green phoſphoric light in every part; ſome a blue light only; and others a yellowiſh light, inclining a little to the orange.

That different quantities of phlogiſton are neceſſary to prepare the ſeveral parts of the ſhells properly for exhibiting different colours, appears from each of the colours being frequently changed, and rendered fainter at the end of a few days only, unleſs the weather happens to be very dry; merely by the ſhells imbibing moiſture from the air: for moiſture, by uniting with the ſhells, diſengages the phlogiſton, (or part of it at leaſt) that is lodged within their ſubſtance.

That phlogiſton is conſtantly diſengaged from particular ſubſtances by the reception of moiſture, appears from the following inſtance. When ſulphur and iron filings are mixed together, they remain in a quieſcent ſtate; but upon moiſtening them with water, the whole maſs is entirely decompoſed, by the eſcape of the phlogiſton which made part of the compoſition of the ſulphur and iron: and the ſubſtance which remains behind, after the eſcape of the phlogiſton, that is, the calx of the iron, and the vitriolic acid

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of the sulphur, is found united in the form of green vitriol.

It appears, likewise, that many other changes frequently happen in bodies by a similar operation: such as, fermentation, putrefaction, &c. for those changes depend upon the inflammable parts flying off, in consequence of their cohesion being rendered weaker by this principle of moisture; because, without the mediation of water none of those effects are observed to take place. The calcined shells, we are treating of, are particularly subject to this change, as we find they are in a great measure, converted into quicklime; the cohesion of which to water is known to be exceeding great.

WITH regard to the shells exhibiting prismatic colours in the dark, and yet affording no other appearance in the open day, than the same white light they acquired by calcination; a very little consideration may, perhaps, explain this matter, as we find it so exactly consonant with other phænomena in the natural world.

The stars, and even the moon itself, are totally obscured when the sun is in the meridian, by the flood of light that is diffused over the whole of our atmosphere. And yet, when that superior light is withdrawn, how very splendid do the moon and stars appear! Now the darkness we produce by art, exceeds this natural one by many degrees. No wonder then, that the difference between the external light, and the closet we have described, is far greater than that between noon and midnight; and more especially whilst the moon continues shining. It is, therefore, entirely owing to this very great difference, that the prismatic colours in the shells become so visible in the dark, when the eye is properly prepared. So likewise, those colours, though they actually continue to exist in the open day, cease to appear, immediately on being exposed to the sun, by the very great quantity of reflected light with which they are surrounded.

And however strange it may appear, I have some foundation for apprehending, that the splendor of the prismatic colours in the open day, could we perceive them, is far greater than that we observe in the dark. This notion arises from many observations that were made during the whole course of the preceding experiments. For when the different phosphori were brought from the light into the dark, the splendor of the colours was exceedingly more vivid in the first instant, than it appeared even in the third of a second afterwards. The disappearing of this great degree of brilliancy was so sudden, that I could seldom move the substances, which exhibited it, a few inches from the curtains, before a considerable alteration in it's vividness had taken place: notwithstanding the time in moving the phosphori from the curtains to the convenient distance for observing, (it being

being about 12 or 15 inches) was not more than one ſecond.

IN deſcribing the preceding experiments, I have ſet down the ſeveral colours which were obſerved in each ſhell; the order in which they happened to lie; and generally their degrees of brightneſs, as they appeared at 12 or 15 inches from the curtains. Nevertheleſs, I ſhall not be ſurpriſed, if another obſerver was to find the colours, in all theſe reſpects, conſiderably different, even at that diſtance, on account of a variety of circumſtances which accompany each experiment; and which may be materially neceſſary in producing the colours as I found them. Such as, the quantity of matter, or materials, that is, or are, to part with the phlogiſton; the manner and diſtance of their application to the ſhells; the uneven and irregular ſurfaces of thoſe ſhells in general; and the great uncertainty of the heat employed in each experiment, &c. Theſe are conſiderations of ſome moment with me, and, in my opinion, render it very difficult, nay, perhaps, impoſſible, to make any two experiments with the ſame materials, ſucceed in every reſpect alike: and as I had no exact rule to guide me in the great number of experiments I have made, it will be no wonder if more able obſervers ſhould produce the colours in greater perfection than I have done: for I am ſo young in theſe reſearches, that I do not know whether I have yet ſeen any one of the priſmatic colours,

colours in the greateſt perfection poſſible. However, notwithſtanding all the conſiderations and difficulties I have mentioned above, no one ought to be diſcouraged from making the experiments, as he will certainly obtain the priſmatic colours in ſome degree or other, even if his attempts ſhould happen to be made rather careleſsly than otherwiſe.

To ſhew how eaſy it is to produce the priſmatic colours, I will give an example, where nothing more is required than a few oyſter-ſhells, and a good fire of any kind. For if thoſe ſhells are thrown careleſsly into the middle of the fire, and continued there for a proper time, (which may be from ten minutes, a quarter, half, or three quarters, of an hour, to one, two, or three hours, according to the thickneſs and compactneſs of the ſhells, and the degree of fire they are expoſed to) they will exhibit the priſmatic colours very lively, after they are removed from the ſun into the dark ſuddenly; and the eyes have been previouſly prepared a little to receive them.

My ſucceſs of late has been ſo conſiderable in this reſpect, that a general account of it ought not to be omitted.

Half a dozen of thoſe ſhells I forced into the middle of a ſea-coal fire, (which was then flaming, in the part where they were put) and continued them there one hour. Upon taking the ſhells out, the ſurfaces had acquired a dirty yellowiſh hue. When they were ſcaled, and expoſed to the light, the colours, in the dark, exceeded all thoſe which I had obſerved before. In one, the green predominated in ſo extraordinary a manner, that I thought it was the brighteſt and moſt beautiful colour I had ever beheld. In another, the yellow and red were very broad, and lively. In a third, all the colours were exhibited to very great advantage. The fourth ſhewed a very fine red, and blue; but the yellow, and green, were fainter. The fifth and ſixth exhibited very fine colours alſo. Theſe colours did not only excel in their ſplendor of light, but in the continuance of their colours; for I was able to obſerve them near three minutes. In all theſe ſpecimens there was no inſtance where the colours lay exactly in their order.

Upon expoſing ſome of thoſe ſhells to the fire again for fifteen, or twenty, minutes more, the colours were varied in many places, and new ones appeared in others.

From this laſt experiment, and ſome former obſervations, I ſuſpect, that the ſhells, during their calcination, are continally undergoing ſuch a variety of changes in their component parts, by the inflammable principle they receive, that they are almoſt every moment differently diſpoſed, in different parts, to exhibit different colours. It is therefore probable, that there is a time for taking the ſhells out of the fire, when they are diſpoſed to exhibit the colours in the moſt favourable manner; but to ſeize upon that moment, when the

ſhells

ſhells are ſo diſpoſed to give the colours to the greateſt advantage, is a circumſtance beyond the reach of my abilities: and indeed to ſay the truth, I have always found that chance did more for me, in this reſpect, than judgment; as for example. At one time I have obſerved the green to predominate over the other colours in an extraordinary manner, I mean, as to breadth and brilliancy; at another, the blue excelled; and ſo on with reſpect to the other colours; excepting the purple, which I very rarely was able to obſerve; there being but five or ſix inſtances where that colour appeared very bright. However, I found upon the whole ſuch an endleſs variety of fine colours to be produced in this manner, that it hath amply repaid me for all the trouble I have had in theſe curious reſearches.

Candle-light. To ſhew how excellent ſome of theſe ſpecimens were as phoſphori, the flame of a candle, at eight or ten feet diſtance, was ſufficient to make them exhibit a little phoſphoric light, in the dark; and this light would continue for ſome time, but without exhibiting any of the colours.

Leafgold. In treating of the priſmatic colours exhibited by gold, I mentioned the effects which a ſingle leaf of that metal produced: and having ſince obſerved that the red appearances are occaſioned by the contact of metal, or other ſubſtances, which impart their phlogiſton to the ſhells, and the great difficulty there is in bringing a large ſurface of metal into contact with thoſe calcareous bodies,

bodies, which are generally very uneven and irregular at their ſurfaces; I had a deſire of trying, whether the whole ſhell might not be made red, by laying a number of gold leaves, one upon another, all over it, and expoſing the ſhell afterwards to a proper degree of heat. Upon making the experiment with ſix of thoſe leaves I was not diſappointed: as they were made to lie pretty cloſe to the ſhell by blowing upon them gently. In this ſituation the ſhell was put horizontally into a crucible, but with the gilded ſide upwards, and upon that, I laid another ſhell. Theſe were put into the fire for ten or twelve minutes, and after expoſing them to the light, the whole ſurface of the ſhell, where the gold had been in contact, exhibited a red light that was rather brilliant than otherwiſe, excepting a very ſmall part which had a faint greeniſh hue. The upper ſhell exhibited a red light alſo, but this red appearance was ſomething weaker, and only obſervable in two or three parts on that ſurface which lay next to the gold. This experiment ſeems to confirm my former ſuppoſition, where it was obſerved, that when the whole ſubſtance of any ſhell happens to be uniformly prepared, ſo that the quantity of phlogiſton combined with it, is not more in one place than in another, it ought to emit but one uniform colour throughout.

Mother-of-pearl.

After trying a few experiments upon ſome ſmall pieces of a ſhell, known by the name of mother-of-pearl, I had encouragement to calcine the whole of one of thoſe ſhells. It was very large, and required ſome care in the calcination to prevent it's breaking, and ſcaling, into ſmall parts, which it is extremely ſubject to, by the effect of fire. This ſhell I expoſed to a moderate heat, and continued it in that ſituation, more than twelve minutes. But, notwithſtanding all my care, on taking it out of the fire, the ſhell broke into two pieces, and many ſcales ſeparated from it. Thoſe pieces I expoſed to the light at different times; upon obſerving them in the dark, both ſides of thoſe pieces exhibited the colours very finely. The red, yellow, and blue, appeared very diſtinct and lively, all over both fragments: there was a little green, but it was rather faint; and a few ſmall parts gave no colour at all. There was no appearance of any white light whatſoever on either of thoſe pieces. Upon the whole, I do not think that the colours exhibited by this ſhell, of mother-of-pearl, were finer than thoſe which the oyſter-ſhells produce: yet the magnitude of theſe pieces, afforded ſuch a variety, and breadth, of colours as might prejudice a common obſerver to give them the preference.

Neverthel{e}ſs, the ſhells of mother-of-pearl are not recommended in preference to oyſter-ſhells, for making experiments with, on account of their being ſo eaſily broken, and ſeparated into ſmaller parts by calcination.

Some oyſter-ſhells, however, are, from their nature, far better than others for theſe kind of experiments: at leaſt I have found them ſo in the courſe of my little experience. The beſt ſhells are thoſe, which from their thickneſs and compactneſs, require the longeſt time to calcine properly. I mention, properly, becauſe they muſt be firm enough, after their calcination, to admit of being ſcaled without much difficulty. I have known ſome ſhells, in a moderate fire, require two or three hours, nay ſometimes more, before they were properly calcined: whilſt others, of a different thickneſs, and compactneſs, although expoſed to the ſame degree of fire nearly, were frequently reduced to a fine powder in leſs than half that time. In my experiments therefore, when I was obliged to make uſe of the inferior kind of ſhells, I endeavoured to proportion the time accordingly, that they might retain the neceſſary degree of compactneſs, which would admit of their being eaſily ſcaled.

DURING the making of all the preceding experiments, I took every opportunity in my power to learn whether the priſmatic colours, in phoſphoric ſubſtances, had been diſcovered by thoſe who have written upon the ſubject of phoſphori. Hitherto I have not been able to find that any other colour, than a *red* light has been obſerved, and that, in two or three different phoſphori; particularly in one, by the celebrated Marggraaf, which is mentioned in a *memoir* of his, printed in the Berlin Acts for 1750, vol. vi. p. 156.

" La solution d'écailles d'huitre faite dans l'esprit " de nitre, précipitée par l'esprit de vitriol, & cal- " cinée avec less charbons, donnera une lumiere " rouge."

When I first read the account of this experiment, it appeared somewhat singular to me, that the phosphoric preparation, therein described, gave only a red colour in the dark, notwithstanding it had been exposed to a charcoal fire: I have since suspected, that the heat M. Marggraaf employed, was either too violent, or too long continued; for I have frequently observed, that the colours, which the shells exhibit, may be entirely destroyed, in either of these cases, as a white light, afterwards, generally occupies the places of the colours: nay, I even suspect, that a *less heat* is required to occasion a *purple* colour in the shells, than appears necessary for producing the other colours.

Had I met with this curious experiment of M. Marggraaf's, before I observed the prismatic colours at first, it is not improbable but that this undertaking of mine would have been shortened considerably, by falling upon a different train of experiments. However, since the method here taken hath been productive of some material information, it may, perhaps upon the whole, be as well as it is; for in these curious researches, where a series of experiments were my guide, many circumstances conspired to throw more light upon

upon the ſubject, than would probably have been found by proceeding upon a different plan.

In conſequence of my ſteadily purſuing thoſe experiments, ſtep by ſtep, with great application and induſtry, and obſerving every circumſtance, in each experiment, with all the care and attention I was able, in hopes of tracing out ſome ſecret principle upon which the phoſphoric power depended, *Accident* threw into my way ſo extraordinary an object, I mean, the colours reſembling thoſe in the rainbow, mentioned in page 23, that I conſidered it as a conſiderable acquiſition in experimental philoſophy to exerciſe my moderate talents upon. No day paſſed, from the time I made that experiment, to the completion of theſe obſervations, without making ſome new experiments, or obtaining further information; and, after having extended my reſearches ſo far as to be able at all times, by the ſimpleſt means, to produce the priſmatic colours ſo very intenſe and beautiful, without ever failing in my attempts; it now appears a matter of ſome ſurprize, how I avoided making the diſcovery, immediately after I had obſerved the effects upon the ſhells that were calcined in the cracked crucibles, mentioned before in an early part of theſe experiments. What a deal of fatigue and trouble would the putting of an oyſter-ſhell into the fire have prevented! Who would have thought that ſo ſimple an operation could have produced ſuch extraordinary appearances? and that thoſe appearances, ſimple as the operation is, ſhould have

eſcaped

eſcaped the obſervations of philoſophers, and thoſe particularly who have intereſted themſelves in theſe matters for ſo many years paſt? But experience teaches us daily, that, during an over eagerneſs in our purſuits to attain ſome favourite point, we generally overlook others of a more extraordinary nature. Perſeverance, in the end, however, ſometimes makes great amends for thoſe overſights, by diſcloſing gradually the object we are in ſearch after; but it muſt be allowed, that Accident is frequently more liberal, by removing the veil at once, when we the leaſt think of it. For my part, I have been greatly obliged to Accident upon this, and many other occaſions; and, had it not been frequently taken notice of before, I ſhould have enumerated ſome of the greateſt diſcoveries that have been made by others entirely through accident.

IT now remains to ſay ſomething concerning the *nature* of the *light* by which phoſphoric bodies are ſeen to ſhine in the dark.

The few experiments and obſervations which I have to offer upon this ſubject, are thrown out merely for the conſideration of more able obſervers, who may hereafter be diſpoſed to inveſtigate the cauſe of this luminous property; as I know very well, from experience, that even diſtant hints are ſometimes attended with very good conſequences.

In the firſt memoir by M. Beccari, there are other ideas reſpecting this matter, which to me appear highly deſerving of attention. But to enter upon thoſe curious ſpeculations in ſuch a manner as they deſerve, requires greater abilities than I am maſter of. The little knowledge I have in theſe matters is founded upon a few experiments of my own, and Sir Iſaac Newton's Obſervations upon the nature and action of light and fire, and the ſulphureous, or rather inflammable, principle deſcribed in his Optics.

All ſubſtances ſeem to contain the inflammable principle more or leſs; and thoſe ſubſtances which abound with it moſt, are obſerved to grow hot, and kindle into fire, and flame more readily, by the action of light, or other violent motions, than other bodies do.

This

This principle, however, does not, in all caſes, appear to be equally diſtributed through, or equally united to, the ſeveral parts in all bodies.

In phoſphoric bodies, for example, the inflammable principle appears to be ſo weakly united, (and even in thoſe inſtances where ſome parts contain more of it than others, as in the ſhells we have experimented upon,) that it is more liable to be diſengaged, and emitted from them, in conſequence of the action of light, or other violent motions, than in other inſtances where it is more ſtrongly united.

And that the phoſphoric light depends in a conſiderable degree upon this inflammable principle, and not entirely upon the ſolar rays which the ſhells imbibe, appears from ſome experiments I made in conſequence of reading a curious one, related by the celebrated M. Beccari, profeſſor of natural philoſophy, of Turin, in the Tranſactions of the Royal Society for the year 1771 *; and which I have repeated many times in the manner in which it is deſcribed; and alſo with ſome conſiderable variations. But, whether from not

* Thecas plures confici curavi ex lamina ferrea cylindraceas intus nigerrimas. Operculum late pertuſum cryſtallo occluditur, colore in theca quaque diverſo. Singulis thecis offas immiſi ex phoſphoro calcareo-ſulphureo ſingulas omnia pares. Hæ clauſæ ſoli objiciuntur ſimul omnes; aſportatas in tenebras aperio, atque offam, quæ per cryſtallum viridem, video vireſcere; rubeſcere, quæ per rubram; flaveſcere, quæ per flavam cryſtallum lucem imbuit: videlicet conſit hoc experimento jam non quantum ſolum lucem ebiberit phoſphorus, ſed et qualem, eam ipſum unice emittere.

knowing

knowing ſome material circumſtance which he, poſſibly, may have omitted, or that there may remain ſomething eſſential which I have overlooked, the experiment, in my trials, did not ſucceed as he hath deſcribed it.

This learned author tells us, that he procured ſeveral cylindrical boxes of braſs, which were black on the inſide. The top of each box was covered with glaſs of a different colour. In each of theſe boxes he put cakes of ſulphureo-calcareous phoſphorus which were like each other in all reſpects; and after they were incloſed, he expoſed them all to the ſun at the ſame time: and, after having brought them into the dark, he then opened the boxes. Upon which he perceived, that the cake, which had imbibed the light through the green glaſs, was green; that through the red, was red, and that through the yellow was yellow. From this experiment he not only obſerved how much light the phoſphorus had imbibed, but that it emitted light of ſuch colour only as it imbibed.

Dr. Prieſtly, after giving an account of M. Beccaria's experiment, proceeds in theſe words: "This therefore ſeems to be deciſive in favour of "the opinion, that the phoſphorus emits the very "ſame light that it receives, and no other, and "conſequently that light conſiſts of real particles "of matter, capable of being thus imbibed, re- "tained, and emitted; and I doubt not but that "Zanottus, (if he be now living) will *retract* his "concluſion *."

* Prieſtly *on Viſion*, &c. p. 365.

Now Zanotti's conclufion, by the fame author, (for I have not feen the original) is, "that phof-"phori fhine by their own *native light*, after they "have been *kindled* by foreign light."

I am inclined to think that the learned Zanotti is confiderably nearer the truth than Dr. Prieftly apprehends: for it feems highly probable, that (if he be now living) he will have no reafon to *retract* his conclufion, after reading the following experiments and obfervations; or at leaft till they are contradicted by other experiments.

1. My firft attempt, in repeating M. Beccaria's experiment, was with cylindrical glaffes of different colours, that were open at one end, and covered, occafionally, with three pieces of black leather. The depth of each glafs was about fix inches, and the width about two inches. Into thefe glaffes I put, at different times, the fame calcined fhell; which had been previoufly prepared to exhibit, upon occafion, a fine white light that would continue for a confiderable time. I then tied the leathers over the aperture of the glafs with which the experiment was made; and, as each glafs had a little bending border round the top, which projected outwards, it was the more convenient to exclude the light, by tying the leathers clofe below this border. The advantage gained by having glaffes, inftead of metal boxes, was, that the fhell within was more expofed to the fun than in M. Beccaria's method. And, that the leaft time might be loft by untying the leathers, I tied them with a broad

piece

piece of tape, and in ſuch a manner, that one motion was ſufficient to leave the leathers at liberty. In all the trials I made with thoſe glaſſes, and with the ſame prepared ſhell, there was no other appearance than a white light: but that light was fainter than the phoſphoric light produced by expoſing the ſhell to the open day.

2. I then had recourſe to cylindrical boxes, that were black on the inſide; into which I put the ſame ſhell in each experiment, and then covered the boxes with glaſs, in the manner M. Beccaria has deſcribed. After this, they were expoſed to the ſun, but at different times. When I uncovered the box, by removing the red glaſs, (which was the firſt experiment) the ſhell exhibited the ſame kind of white light I had obſerved with the cylindrical coloured glaſſes. So likewiſe there was no difference in the appearance of this light, when the green, yellow, or blue-making rays were tranſmitted ſeparately through the reſpective coloured glaſſes, (on the other boxes) upon the ſame ſhell.

3. But leſt any objection might be made to the nature of the phoſphoric ſhell employed in the laſt experiment, I repeated it, and the others, with the ſulphureo-calcareous phoſphorus which Mr. Canton gave an account of to the Royal Society, and which ſeems to have been the ſame that M. Beccaria made uſe of, without being able to obſerve any alteration in the appearances from thoſe deſcribed above.

Seeing ſo great a difference in M. Beccaria's obſervations, compared with theſe; and leſt any doubt ſhould remain between our experiments, I had a mind to try, whether that doubt might not be removed, by varying the experiment.

4. To this end, inſtead of employing the ſhell which exhibited the white light, I put in it's place another ſhell, which would exhibit the priſmatic colours intenſely bright, after it had been expoſed to the ſun. By this alteration I imagined, that if the coloured glaſs made any material difference, it would be perceptible upon ſome one, or other, of the colours. The firſt experiment was made with the cylindrical box, and a fine piece of red glaſs, which was well ground and poliſhed on both ſides, and tinged equally with red throughout. On removing this glaſs in the dark, after the ſhell in the box had been expoſed to the ſun, the priſmatic colours appeared; and I could not perceive any alteration in them, except that they were fainter.

The ſame effect of colours, nearly, was produced, when each of the glaſſes was put in the place of the red glaſs.

AFTER theſe experiments, I made ſome others upon phoſphoric ſhells, by the application of *heat*.

5. Having prepared ſeveral fine ſpecimens of oyſter-ſhells, that would exhibit priſmatic colours, I laid three of thoſe ſhells flat, and at a little diſ-

from one another, upon a tin caſe; in which there was ſhut up a red hot maſs of iron, about five inches ſquare, and two inches thick. The iron was thus encloſed in the caſe, to prevent the light from the heated iron diſturbing my eyes. But this precaution proved of little uſe, becauſe the tin caſe itſelf, before I begun the experiment, acquired a red heat in two or three different places which were neareſt to the iron. In a very ſhort time, after the ſhells were laid upon the tin, a feeble whitiſh light appeared at the edges of the ſhells; this afterwards ſpread upon their upper ſurfaces; and it increaſed in brightneſs, for a little time, in almoſt every part, but particularly at the edges. The whole time taken up by this increaſe, was not leſs than half a minute. Soon after, the brightneſs began to decreaſe, and continued to do ſo, till it entirely vaniſhed. But this decreaſe of the light was three, or four, times longer than the increaſe. The greateſt ſplendor did not exceed that excited by the external light, upon the ſame ſhells, in a cloudy day. In this experiment I perceived ſome of the priſmatic colours, but they appeared more obſcure, and conſiderably fainter, than thoſe which the open day excited upon the ſame ſhells, particularly the red.

6. When theſe ſhells were become quite cold, I laid them, as before, (but without having expoſed them again to the ſun) upon the ſame heated tin: and though I waited ſeveral minutes, there was only ſo ſmall an appearance of a whitiſh light from the

the ſhells, that it was with ſome difficulty I could perceive it.

7. One of the ſame ſhells, which had been placed upon the heated tin for a few minutes, and whilſt it remained hot, I expoſed to the open day as uſual; upon bringing it ſuddenly into the dark, the priſmatic colours appeared to great advantage; they being ſomething livelier, and more intenſe than thoſe produced in the experiment with paper, mentioned in page 53.

8. I laid the ſame ſhell upon the heated tin again, and when it had continued there the proper time, it was then taken off to cool. This ſhell, in its cold ſtate, was expoſed to the light as before, and after bringing it into the dark, I obſerved the ſame priſmatic colours: they appeared intenſe and lively alſo, but not quite ſo brilliant as when the ſhell was hot. The ſixth, ſeventh, and this experiment, were repeated many times with the ſame ſhell, and others of the like kind, with the ſame ſucceſs reſpectively. Phoſphoric ſhells therefore, after they have been heated properly, or while they continue hot, are not rendered unfit to receive light, as Dr. Prieſtly ſeems to apprehend *.

* "I would beg leave to obſerve, that the *ſame ſtate of heat*, which "diſpoſes the phoſphorus to throw off the light after it has been "imbibed, muſt likewiſe *render it indiſpoſed to receive it*; ſo that in "thoſe circumſtances *it will in reality imbibe leſs light*, as I doubt "not would be found to be the caſe, if the head of the obſerver was "cloaked up, and the phoſphorus preſented to him immediately "after being expoſed to the light."

PRIESTLY ON VISION, &c. p. 337.

9. The ſame ſhells, but in a cold ſtate, were expoſed again to the open day; and whilſt they continued ſhining, I placed them upon the hot tin as before, and with that ſide upwards, on which the colours appeared moſt vivid. This took up ſo much time, that the colours became ſomewhat fainter. However, the heat the ſhells were now expoſed to, revived the colours conſiderably; eſpecially the red, yellow, and blue. And when the colours diſappeared, a whitiſh light occupied their places, which laſted about one minute before it entirely vaniſhed.

10. I then applied the edge of a ſhell (which was rather ſmooth than otherwiſe) to the heated tin. In a few ſeconds, a pale whitiſh light appeared at the edge next to the tin; which ſpread by degrees, to the top of the ſhell. This light increaſed in brightneſs at the bottom, and extended itſelf upwards one inch or more: beyond that diſtance, the light decreaſed gradually; and in a little time totally diſappeared. During the decreaſe of this light, and ſome time before it became very weak, a darkneſs took place, next the edge, in contact with the tin; which ſpread upwards upon the light, more than half an inch. In a little time after this, the whole phoſphoric light diſappeared by degrees. I continued this ſhell in the ſame poſition, but upon a heated iron, for ſome minutes longer; when there appeared a new light, cloſe to the ſame edge that had been next the tin, and was now next the iron; but this ſecond

light was very weak, and did not ſpread farther than the twentieth part of an inch.

11. Theſe experiments were repeated with other ſhells, of different thickneſſes, in order to obſerve the appearances on both ſides of them. Upon trying thoſe, I found, that one of their ſides ſeemed generally brighter than the other: but the whitiſh light, and the feeble colours that appeared, (for they were barely diſtinguiſhable) did not ſpread over both ſurfaces entirely, at leaſt over ſome of them; there being many places on one ſide that did not give any light, and which I ſuppoſe aroſe chiefly, from a different texture of their parts.

During the making of theſe experiments, the edge of the ſhell itſelf, which was in contact with the tin, was frequently examined. This edge appeared luminous, before any other part of the ſhell; and afterwards dark, before the half inch of the ſhell became dark, as deſcribed above.

Upon the whole of theſe experiments reſpecting heat, it appears, that phoſphoric light, whether it conſiſts of priſmatic colours or otherwiſe, is not generated by heat; but only excited, and that occaſionally, by the action of it. The power, therefore, which generates phoſphoric appearances, muſt depend upon ſome other principle.

BY Sir Iſaac Newton's experiments upon light, it ſhould appear, that each of the different coloured rays, after refraction, is from it's nature, incapable

incapable of any the leaſt change by further refractions: ſo that the red, or yellow, or green, or blue, or violet rays, continue always the ſame entire colour as at firſt.

BY Profeſſor Beccaria's experiment, phoſphori, in general, emit the ſame light they receive.

According to that idea, the phoſphori that, by means of the open day, exhibit a white light, ought, when expoſed to any refracted ray, to exhibit the colour of that ray: but it is found from experiment *, that ſuch phoſphori expoſed to any refracted ray, do ſtill exhibit the ſame kind of white light; without any other variation in it's appearance than that of being more or leſs faint, according to the colour of the ray to which the phoſphori are expoſed.

So phoſphori, that exhibit priſmatic colours, (by the effect of mixed light) ought (according to the ſame idea, when expoſed to any refracted ray) to exhibit only the colour of that ray. But it is here likewiſe found from experiment †, that ſuch phoſphori expoſed to any refracted ray, do, nevertheleſs, exhibit colours; (that is, the ſame priſmatic colours they did at firſt in mixed light) and thoſe colours are more or leſs faint, according to the colour of the ray they are expoſed to: for inſtance, the red-making parts exhibit a red light many degrees brighter, after

* See Experiment with the priſm, N°. 5. p. 84.
† Sixth and ſeventh experiments, p. 107, 108, &c.

having been expoſed to violet, blue, or green refracted rays, than after they have been expoſed to red refracted rays. The phoſphoric colours, therefore, do not appear to depend upon mixed light, but upon the matter itſelf of which light conſiſts, (whatever original properties the different rays may have,) combined with the inflammable principle. For, without the matter of light, or without the inflammable principle, circumſtanced as it is *, thoſe phoſphoric appearances do not take place. And the experiments, where heat was employed, ſeemed to elucidate this doctrine: becauſe in one inſtance, a white light, not very bright, and a weak appearance of colours, was exhibited §: and in another, ſcarce any light, and no appearance of colours †. From which it appears, that the heat, (in the firſt inſtance,) by it's action upon the combined phoſphoric matter in the ſhells, made them luminous while the application of the heat was continued: or, however, during ſuch time, as the latent matter in that combination, or part of it at leaſt, was expending itſelf: and when it was expended, or very nearly ſo, as in the ſecond inſtance where the ſame degree of heat was continued, the ſhells of courſe could no longer exhibit phoſphoric appearances, without receiving a further ſupply of light. And though the time, employed to acquire that ſupply, was not more than two ſeconds, the priſmatic colours appeared afreſh,

* Page 65.
§ Fifth experiment, p. 84.
† Sixth experiment, p. 85.

and

and full as lively, as if no ſuch heat had ever been applied: nay, theſe colours are renewable again and again; for the experiment may be repeated a whole day together, ſo far as I have obſerved; whether the light, to which they are expoſed, conſiſts of mixed rays; or only of the red, or yellow, or green, or blue, or violet refracted rays. This being the fact, as I have ſince found from repeated experiments, the nature of phoſphoric light ſhould ſeem, by the doctrine above, to be better underſtood.

It appears ſomewhat difficult to account for the power by which this, and other phoſphoric lights are emitted, without having recourſe to an *exceeding ſubtile and elaſtic fluid*, or *æther*, in which vibrations are ſuppoſed to be excited by the action of light, or heat, or other violent motions: for the effect of ſuch vibrations muſt be an agitation of the phoſphoric parts; and when they are ſufficiently agitated, I ſee no reaſon why theſe phoſphoric parts, during their luminous appearances, may not be conſidered as *actually*, though *inſenſibly*, in a burning ſtate, and conſequently emit light, and ſhine, according to the ſeveral circumſtances which accompany them.

Whether air is eſſential to theſe luminous appearances, I have had no opportunity of examining. The difficulty, hitherto experienced, of procuring what is generally underſtood to be a good *vacuum*, was one diſcouragement to my attempting any enquiry of that nature: but there was another rea-

 ſon,

ſon, which I had found, reſpecting the different appearances, by different approximations to a *vacuum*. For, from ſome experiments I made many years ago, in company with Mr. Smeaton, and with his excellent air pump, which, when it was in very good order, rarefied the air above two thouſand times, we obſerved, in general, that very ſmall differences of air occaſioned very material differences in the luminous effects produced by the electric fluid; inſomuch, that when all the air was taken out of the receiver, which this pump, at that time, was capable of extracting, no electric light was perceptible in the dark: upon letting in a little air by a ſtop-cock, a faint electric light was viſible, and the letting in of more air increaſed the light conſiderably. But this light began to decreaſe on the letting in of more air, till, at laſt, on letting in greater quantities, it entirely vaniſhed. By this experiment it appeared, that a certain limited quantity of air was neceſſary to occaſion the greateſt luminous effects.

This curious fact induced me to ſuſpect, that the *vacuum* in the Torricellian tube, as it is generally made, is not a perfect one; becauſe, when the tube is properly electrified, a ſimilar light, to the brighteſt deſcribed above, is viſible in the dark through the whole length of the *vacuum*.

Theſe obſervations are made only to ſhew, how cautious we ought to be in drawing general concluſions, from experiments, that are liable to many exceptions.

The

The mentioning of electric effects, gives me an opportunity of introducing an obſervation I made many years ago concerning that fluid. And it is the more proper to take notice of it in this place, as it relates to the inflammable principle. The obſervation is this: upon making a variety of experiments relative to the electric fluid, I found, that the *luminous* appearances ſeemed to depend upon a ſulphureous or inflammable principle, that is driven off from bodies, by the violent action of that electric and elaſtic fluid; and ſo, by mixing with the acid particles in the air, I apprehend, is kindled into flame.

From obſerving how eſſential the inflammable principle is, in occaſioning phoſphoric appearances, I was naturally led to make ſome experiments with the electric fluid upon the ſhells; in order to try whether the ſhells might not be properly phlogiſticated by that method only, for the purpoſe of exhibiting priſmatic colours: and, though I found ſome encouragement to apprehend, that the matter, which occaſions thoſe luminous effects in electricity, depends upon this inflammable principle, yet I forbear giving any account of thoſe experiments, till ſome others have been tried, which, from their nature, may prove more concluſive than thoſe I have already made.

ADDITIONS.

SINCE the firſt edition of the preceding experiments was publiſhed, I have been informed, that M. Bourriot, one of the canons of Bazas in France, had likewiſe failed in attempting to make M. Beccaria's experiment with the coloured glaſſes, and the ſulphureo-calcareous phoſphorus: and that, in conſequence of being diſappointed therein, he entertained ſome doubts, whether all the circumſtances accompanying that experiment, had been fully ſtated in M. Beccaria's letter to the Royal Society. He therefore deſired M. Magellan, F.R.S. a gentleman well known in the literary world, and a correſpondent of M. Beccaria's and M. Bourriot's, to procure from Turin, an account of every particular relative to that experiment.

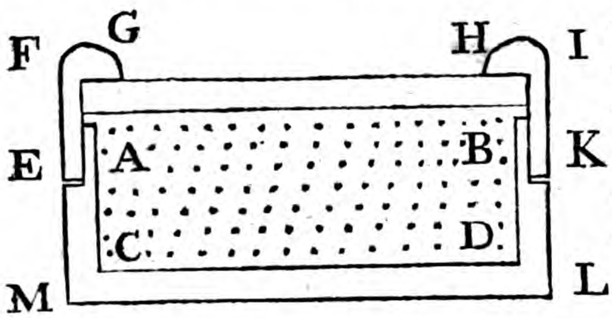
The anſwer from M. Beccaria, which M. Magellan received, is dated in September, 1772: and as it contains ſome circumſtances that were not communicated before, which ſeem to be conſidered by him, as material and neceſſary to be obſerved in the trying of that experiment, I ſhall give an exact copy of that part of the letter which relates to the ſubject; and then deſcribe the experiment as I myſelf repeated, and varied it, with all thoſe additional circumſtances.

To obviate any objection that might be made to the apparatus employed on this occaſion, I requeſted the favour of M. Magellan, as being entirely diſintereſted, to provide ſuch a one, as would fully anſwer the deſcription in M. Beccaria's letter; and would, at the ſame time, ſatisfy himſelf. Accordingly, he gave directions to his own workmen; and, in a ſhort time, procured all the particulars requiſite to that experiment, excepting the phoſphorus, which I provided myſelf. It was very fine of the kind, and exhibited a white light only, after having been expoſed to the open day. In ſome of the experiments, another phoſphorus was made uſe of, which was alſo fine; but this exhibited a whitiſh light, inclining a little to the yellow.

The part of M. Beccaria's letter which relates to this ſubject, is ſolely confined to his method of making the experiment, and is as follows.

" Metto nelle ſcattolette de ottone fosforo elettiſ-
" ſimo; nella ſtanza buia metto tali ſcattollette
" in centro ad anelli di ferro molto maſſicci,
" e quaſi roventi; e coſi ſpoglio il fosforo di ogni
" luce: copro le ſcattolette con coperchi di ottone
" al buio; e coſi coperte, una perſona le reca al
" ſole, dove il fosforo bee la luce attraverſo al
" vetro colorato per alcuni ſecondi. Apro al buio
" le ſcattolette, che mi ſi recano coſi chiuſe: e
" allora

" allora vedo il fosforo a rendere una luci tanta di
" quel colore, che ha il vetro incaſtrato nel co-
" perchio. Perciochè la luce, che paſſa per tali vetri
" è pure compoſta; epero la luce che rende il
" fosforo è ſolamente tinta di quel colore, che
" paſſa più copioſo per i criſtalli. La differenza,
" ſe bene mi ſovengo, è più diſcernevoli uſando
" un coperchio di colore roſſo intenſo, ed uno di
" colore giallo.



" Ho ſperimen-
" tato lo ſteſſo
" con diamanti
" fosforici. AB
" C D fosforo.
" AMBL ſcat-
" toletta di ot-
" tona. GFE,
" HIK coperchio di ottona. GH vetro colorito
" incaſtrato nel coperchio.

" Giambatista Beccaria, d.S.P.

" Mondori, 1 Septembre, 1772."

" Into ſmall boxes of braſs I put the choiceſt
" ſelected phoſphorus. Theſe boxes I afterwards
" put into thick rings of iron that are made
" red hot in a dark room, by which means the
" phoſphorus is diſcharged of the whole light it
" had received before. The boxes are there co-

" vered each with a cap of metal: and a ſecond " perſon carries them to the ſunſhine, where the " phoſphorus abſorbs it's beams, during ſome " ſeconds, through the coloured glaſs. I uncover " them as ſoon as they are brought to me in the " dark room. And then I perceive that the " phoſphorus gives a light tinged with the colour " of the glaſs which formed the cover of the " box. And though the light tranſmitted through " the glaſſes is mixed with other colours, that " light however, given by the phoſphorus, is only " tinged with the colour which paſſes more copi- " ouſly through the reſpective glaſs. The differ- " ence, if I recollect well, is more diſtinctly per- " ceived when an high red glaſs covers the top of " the box; and likewiſe when a yellow glaſs is " employed. I have found the ſame reſult in " making ſuch experiments with phoſphoric " diamonds. A B C D, repreſents the phoſpho- " rus, A M B L, the box of braſs. G F E, H I K, " the cap (or rather the rim) of braſs. And G H, " the coloured glaſs which forms the cover of " the box."

EXPERIMENT I.

HAVING prepared ſome pieces of phoſphorus, according to M. Beccaria's method, by expoſing them to the heat of a red hot iron in the dark; I put one of thoſe pieces, after it was cold, into a ſmall japaned box, which was black on the inſide. Over this box I put the cover; in the middle of which was fixed a round piece of red glaſs, (above one inch in diameter) that the light of the ſun might be immediately tranſmitted to the phoſphorus. The phoſphorus, thus circumſtanced, was, according to my uſual way, firſt of all expoſed, for ſome ſeconds, to the ſolar rays: and then ſuddenly removed into the dark. On taking the cover off, the phoſphorus exhibited a white light; but this light was fainter than that which the open day had excited in the ſame phoſphorus, without the glaſs: and notwithſtanding this experiment was repeated many times, the effect was conſtantly the ſame, without the leaſt appearance of red. Nor did the making uſe of different-coloured glaſſes cauſe any difference in the effect: for the phoſphoric light appeared equally white, or very nearly ſo, in each experiment.

EXPERIMENT II.

INTO the ſame japaned box I put another piece of the prepared phoſphorus, in the place of the other; and then encloſed the box with the cover, which had the red glaſs fixed in it, as before. This box, ſo covered, I put into another japaned box, and ſhut it down cloſe with the cover belonging to it. In this ſituation the boxes were carried out of the cloſet, by an aſſiſtant, into the light of the ſun, (while it continued ſhining) and then the cover of the large box was taken off. After a few ſeconds, and while the leſſer box remained in the ſame ſituation, the ſun's rays were ſuddenly excluded by the lid of the large box. In that ſtate both boxes were brought quickly into the dark cloſet; where I had continued the whole time, to avoid having my eyes diſturbed by the external light. Upon uncovering them immediately, the phoſphorus exhibited the ſame kind of white light, as was obſerved in the firſt experiment, without the leaſt appearance of red in any part of it.

EXPERIMENT III.

INSTEAD of the japaned boxes employed in the firſt and ſecond experiments, I made uſe of the braſs boxes that were procured by M. Magellan. In this experiment, however, the phoſphorus was not prepared, as had been done in the two former trials, by laying it upon the hot iron, but was put unprepared, and in a powdered ſtate, into one of the braſs boxes. This braſs box, after being filled with the phoſphorus, was laid upon a large maſs of red hot iron in the dark, and continued there till all appearances of light ceaſed. I then covered the box with the red glaſs, and expoſed the phoſphorus contained therein, to the ſolar rays, by putting it out, with my hand, through the hole in the cloſet. But I could not perceive that, even by this variation in the circumſtances, there was the leaſt difference in the reſult of the experiment: for no other appearance was exhibited by the phoſphorus in the box, than the ſame kind of white light which had been obſerved before. Nor did any of the other coloured glaſſes, when put in the place of the red glaſs, make any alteration in the general appearance of the light that was exhibited by the phoſphorus; unleſs that, in ſome of them, the light might appear a little fainter than in others.

However, that M. Magellan ſhould be thoroughly ſatisfied in this matter, I deſired him to make the experiment himſelf, with the braſs boxes, and examine carefully every circumſtance which accompanied it.

Accordingly, ſome time after this, (the day being very fine) M. Magellan repeated the experiment different ways, and, according to my judgment, conducted the whole with great care and attention: for I was preſent at the time, along with a friend who was curious to ſee the event. The reſult of all his trials (together with the copy of M. Beccaria's letter which hath been given already) he, at my requeſt, communicated to me in writing; a copy of which I ſhall now inſert.

" —— Agreeable to your deſire, I have ſent " that part of the letter I received from the re- " verend M. Beccaria, profeſſor of natural philo- " ſophy at Turin, which is an anſwer to my

" enquiry

" enquiry about the practical method for making
" his curious experiment that is already published
" in the Philosophical Transactions of the Royal
" Society for the year 1771. The occasion of
" this enquiry, took it's rise from my good friend
" the Abbè Bourriot's (canon of the cathedral of
" Bazas, in Gascogne) not being able to succeed
" in repeating that experiment at Paris. And
" notwithstanding the author has furnished me
" with further particulars, I am, as yet, very sorry
" to observe, that something is still wanting in
" the method he has prescribed to have any suc-
" cess in it: because, I am sure I did take all
" proper care to perform the trials agreeable to his
" instructions, but without finding that they an-
" swered my expectations. I have already ac-
" quainted Professor Beccaria with the whole of
" my attempts: and have desired him to favour
" me with further instructions how to make this
" experiment. I will not fail of communicating
" them to you the moment I receive his answer.

" I am, &c.

" J. H. du Magellan.

" Sept. 6, 1775, Fetter-Lane, London."

To satisfy myself, that the different-coloured glasses, employed in M. Beccaria's experiment, were incapable of transmitting the respective rays of their own colour, unmixed with other rays, I made the following experiment.

Experi-

EXPERIMENT IV.

IN the dark closet, at a round hole, about one quarter of an inch in diameter, made in a thin plate of brass, I placed the same red glass which was made use of before, and secured it in it's place with a rim of putty*; I preferred putty for this purpose, because, with a very little management, it prevented the entrance of any foreign light at the edge of the glass; which, otherwise, might have disturbed the experiment. Immediately after the coloured glass I placed a prism, but in such a position, that a beam of the sun's light, which came in at that hole and through the coloured glass, was refracted by it towards the opposite side of the closet. The refracted image I received upon a sheet of white paper at different distances from the prism: and observed all the colours painted upon the paper in their regular order; and these colours were, in general, nearly as full, intense, and lively, as those, which are usually painted with a prism by a beam of the sun's light, after it hath past through a clear piece of uncoloured glass, placed in a similar situation with the red glass in this experiment. Except that the red rays appeared a little more full, intense, and lively than the yellow; the yellow, a

* Made of linseed-oil and whiteing.

little more than the green; and the green, a little more than the blue, or violet refracted rays.

On repeating the experiment

With yellow glafs, in the place of the red; the yellow refracted rays appeared a little more full, intenfe, and lively than the red; the red, a little more than the green; and the green a little more than the blue, or violet refracted rays.

With green glafs; the green refracted rays appeared a little more full, intenfe, and lively than the yellow; the yellow more than the blue; and the blue more than the violet, or red refracted rays.

With blue glafs; the blue refracted rays appeared a little more full, intenfe, and lively than the green; the green more than the violet; and the violet more than the red, or yellow refracted rays.

I did not make any experiment upon violet glafs, as I could not procure any that was good enough for the purpofe.

The degrees of different-coloured light tranfmitted by each of the different-coloured glaffes mentioned in thofe experiments, are particularized, merely to give fome idea of the fulnefs, intenfenefs, and livelinefs, of the feveral refracted rays as they appeared to my eye upon comparing them together. Although, from more accurate obfervations, thofe differences may poffibly turn out to be a little more in fome of the colours, or a little lefs in others, than are here fet down. Had the afcer-

taining

taining of ſuch differences been abſolutely material in the preſent caſe, they would perhaps have been more diligently attended to. My deſign in making theſe experiments was ſolely to ſhew, that each of the different-coloured glaſſes, tranſmitted (not only the reſpective colours belonging to them, but likewiſe) all the other colours; and thoſe in a far greater proportion than hath been generally apprehended.

UPON finding that theſe glaſſes were incapable of tranſmitting the reſpective rays of their own colour, without being mixed in a very great degree with the other different-coloured rays, I laid them aſide, in favour of another method; which, from it's ſimplicity, ſeemed more likely to determine the queſtion whether phoſphori, in general, do actually emit the very ſame light they receive.

In an experiment of ſo much conſequence, as this appears to be, it ſeemed proper to have it tried in a manner which would be liable to the feweſt objections. For this purpoſe, therefore, I had recourſe to a beam of the ſun's light, ſeparated by a priſm in the dark cloſet; by which means it was eaſy to let fall, upon the phoſphorus, more intenſe, and leſs-compounded colours than the different-coloured glaſſes were capable of tranſmitting. And notwithſtanding the cloſet, on account of it's ſmallneſs, would not admit a lens, and priſm, to be placed at the diſtances recommended by Sir Iſaac Newton, as the moſt advantageous for intercepting the foreign light which diſturbs the colours, by rendering them more compound and impure; yet, the diſtance which the cloſet allowed of, with the priſm only, anſwered the end far better than the other method of M. Beccaria's; becauſe each colour of the refracted light, circumſtanced as it was, approached conſiderably nearer to purity, than that light did which was tranſmitted through the coloured glaſſes, in the preceding experiments.

Before I enter upon the experiment propoſed, it may be proper to obſerve, that ſuch phoſphori only were employed, as were very brilliant of their kind, and had (according to M. Beccaria's method) been previouſly expoſed to a red hot iron in the dark for a few minutes; or, however, till ſuch time as they had ceaſed to exhibit phoſphoric appearances. Theſe phoſphori ſo treated (and which for the future I ſhall call the *prepared phoſphori*) were not expoſed afterwards to any other light than what was neceſſary to the experiment.

It is to be obſerved further, that as it was found material, in trying the following experiments, to intercept the beam of the ſun's light ſuddenly as it entered the cloſet, a curtain (made of black cloth, and loaded at the bottom with a little lead) was fixed over the hole where the light entered; which, with the aſſiſtance of a cord and pulley, might be drawn up or let down readily, juſt as the circumſtances of the experiment required. And, that I might be the better prepared to make obſervations in the dark, my eyes were previouſly ſhut for a conſiderable time: for which reaſon, it became neceſſary to have part of the experiment conducted by an aſſiſtant.

EXPERIMENT V.

AT the ſame round hole, mentioned in the preceding experiment, I placed a very fine priſm *, whereby a beam of the ſun's light, which came into the cloſet at that hole, was refracted on the oppoſite ſide of the room. Then, upon a ſheet of white paper, I received the coloured image at different diſtances from the priſm: and having marked the diſtance at which I thought the image appeared moſt diſtinct, the beam of light was then intercepted by letting down the curtain; and in the place which I had marked, the prepared phoſphorus was put, but in ſuch a ſituation, as that the rays might fall perpendicularly upon it's ſurface. Before the beam of light was ſuffered to enter the cloſet again, the phoſphorus was covered with a cap made of black cloth. The aſſiſtant then drew up the curtain to let the beam enter, in order that he might have an opportunity to intercept, at a convenient diſtance from the priſm, all the rays that were refracted by it, except the red. When this was done, he removed the cap which covered the phoſphorus, that the red light might fall directly upon it: and after a few ſe-

* Made by Mr. ADAMS, in Fleet-ſtreet.

conds, he let the curtain down ſuddenly. At that inſtant, nearly, I obſerved the phoſphorus ſhining: but the light it exhibited was not red; nor the leaſt inclining to it: on the contrary, it appeared white, though faint; yet it was ſufficiently diſtinct, and continued viſible for a conſiderable time.

This experiment was repeated, not only with the yellow rays, but alſo with the green, blue, and violet rays. In none of thoſe trials was there any appearance of colour exhibited by the prepared phoſphorus; for the ſame kind of white light was viſible in every experiment, which was obſerved before with the red: except that this white light appeared more or leſs faint, according to the colour of the rays to which it was expoſed.

EXPERIMENT VI.

I NOW ſuffered the red, yellow, green, and *ſome* of the blue rays, (for the phoſphorus was not large enough to take them all in) to fall ſeparately, and at the ſame time, upon the prepared phoſphorus; notwithſtanding which, the phoſphorus ſtill exhibited the ſame kind of white light in every part: though the light was here alſo, more or leſs faint, where the different-coloured rays had fallen upon it.

Experiment VII.

NOR could I find that any of the prepared ſhells (which were previouſly diſpoſed to exhibit the priſmatic colours), on expoſing any one, or more of the colour-making parts, in thoſe ſhells, to any one of the different refracted rays, ſuffered the leaſt alteration in the reſpective colours which thoſe parts exhibited; excepting that theſe colours, which were exhibited by the different refracted rays, appeared more or leſs faint, than when they were expoſed to the open day.

IN purſuing theſe enquiries, however, I remarked a very ſingular appearance, namely, that the ſame parts of the ſhells which exhibited an intenſe red colour, equal for inſtance, to *twenty degrees*, by the effect of mixed light, as it entered the cloſet at the ſame round hole, did, after being expoſed to the red refracted rays only, exhibit ſo *faint* an appearance of that colour, as not to exceed *one* or *two degrees:* and, on the other hand, ſtrange as it may appear, the very ſame red-making parts of the ſhells, after being expoſed to the *violet* refracted rays only, exhibited a red light *ten* or *twelve degrees* more intenſe, and lively, than that which was excited by the red refracted rays. The ſhape of this red light upon the phoſphorus was round, correſponding with the hole in the intercepting plane,

plane, through which the red rays only were ſuf-
fered to paſs.

Obſerving ſo extraordinary a difference here, I proceeded to examine the effects which the other different-coloured rays would have upon the ſame, and ſimilar parts of other ſhells; and I found that, with the blue rays, the red phoſphoric light was only a little leſs intenſe and lively than with the violet rays; and that, with the green rays, the red light exhibited, was ſtill ſomewhat leſs intenſe and lively than with the blue rays, and conſequently ſeveral degrees brighter than with the red rays.

But the difference did not take place ſo regularly with the yellow rays; for the effect of thoſe rays upon the ſame parts that exhibited the red phoſphoric light, was ſeveral degrees leſs intenſe and lively than that which the green rays produced. In this account, the orange, and purple rays are included in the yellow, and violet rays; as I found it difficult to ſeparate them from the colours which lay next to them: that is, the yellow rays were more mixed with the orange rays than with the green rays; and the violet rays of courſe were alike mixed with the purple rays.

Theſe experiments were repeated over and over, and always with the ſame ſucceſs.

The red light of the phoſphorus, excited by the violet rays, being ſo many degrees more intenſe and lively than that excited by the red rays, put me upon trying what effect the red rays would have

upon

upon the red phoſphoric light itſelf, immediately after it had been excited by violet rays.

Accordingly, when the phoſphorus had been thus expoſed to the violet rays, for a few ſeconds, and I had juſt time enough to obſerve in the dark, that it exhibited a lively red; I expoſed this phoſphoric red, to the red refracted rays, for two or three ſeconds; in conſequence of which, contrary to what might have been expected, the red light of the phoſphorus was conſiderably weakened.

Upon repeating this experiment ſeveral times, not only with the red, but yellow rays, and finding that the effects in both caſes, were conſtantly the ſame, or very nearly ſo, I had ſome foundation for ſuppoſing that red and yellow rays, when mixed with green, blue, and violet, rays, were not of any great advantage in exciting many phoſphoric appearances.

To try this, I intercepted, by a plane, the red and yellow rays at the ſame time; and behind the intercepting plane, but near the hole through which the green, blue, and violet, rays were ſuffered to paſs, I placed a lens in ſuch a manner, as to receive thoſe different rays. And in, or near the focus of this lens, where the rays appeared to be moſt mixed, I placed the ſame red-making phoſphorus for a few ſeconds, and then cloſed the hole at which the ſun's light entered the cloſet. The phoſphorus, in this caſe, exhibited a red light ſeemingly more full, intenſe, and lively, than that which the open day excited in the ſame ſpecimen.

The

The collected ſtate of thoſe rays was one different circumſtance indeed, that might have contributed to the intenſeneſs of the red colour, more than the light of the open day.

The ſpecimens of red-making phoſphoric parts employed in thoſe laſt experiments, had this conſiderable advantage, that they exhibited that colour more intenſe and lively (after being expoſed to the open day) than the other colour-making parts which exhibited yellow, green, or blue light. But notwithſtanding theſe laſt were inferior in point of brightneſs, yet they furniſhed me with a few general obſervations that may ſerve as hints, at leaſt, for others to purſue, who poſſeſs ſuperior abilities: for having not made a ſufficient number of experiments upon thoſe colours, I am not, as yet, ſufficiently ſatisfied about them.

When the yellow-making parts, for inſtance, had been expoſed in one experiment to the red, in another to the yellow, and in another to the violet rays, the effect ſeemed ſtronger than when they were expoſed to the green or blue rays: that is, they ſeemed to exhibit in the three caſes, a more intenſe and lively yellow, but particularly with the red rays.

So the effect of red rays, and violet rays, but particularly the latter, upon the green-making parts, ſeemed ſomewhat ſtronger (that is, they ſeemed to exhibit a more intenſe and lively green) than even the green or blue rays themſelves.

In like manner, the effect of the red rays and yellow rays, but particularly the former, upon the blue-making parts, ſeemed ſtronger (that is, they ſeemed to exhibit a more intenſe and lively blue) than even the blue or violet rays themſelves.

To account for theſe very ſingular appearances, ſo contrary to the generally received ideas of light, may be rather difficult, without ſuppoſing different original properties in the rays of light hitherto not attended to.

However, upon the whole, it appears from theſe experiments, that the ſolar rays, whether they are ſeparated, or compounded, do not make any ſenſible alteration in the colour of the light which thoſe phoſphori are found to emit: for example, the red-making parts of thoſe phoſphori conſtantly exhibit a red light only, whatever different-coloured rays thoſe parts are expoſed to; and without any other difference taking place, in conſequence thereof, than that of their being rendered more or leſs faint, according to the nature of the different rays: and ſo on, of the other colour-making parts. And though the ſeveral refracted rays in theſe experiments, were not ſo ſimple, or ſo advantageouſly circumſtanced as they might have been by other methods, yet, according to M. Beccaria's experiment, even in this their diſadvantageous ſtate, compared with the coloured glaſſes, they ought at leaſt to have made ſome difference in the colour of the light exhibited by thoſe phoſphori. But, as no ſuch difference has hitherto appeared, and as it is found, that the effects of violet and blue and

and green rays, upon the ſame and ſimilar colour-making parts of phoſphori, excite a red light more intenſe and lively than the effect of yellow, or even red rays, upon the ſame parts; we may now, I think, with ſome degree of certainty conclude, " That phoſphori do not emit the ſame light which " they receive." And ſince it has been fully proved, by the preceding experiments, that the *matter of light*, (indued, probably, with certain original properties,) is eſſentially neceſſary towards rendering thoſe phoſphori luminous, we have, I apprehend, a further confirmation of what has been before advanced, in page 91, reſpecting the *nature of the light* by which phoſphoric bodies are ſeen to ſhine in the dark.

TO theſe experiments, ſeveral others might be added, where the electric fluid, or rather, the *light*, produced by it, was applied to phoſphoric ſhells, in the dark, with great ſucceſs. But as I wiſh to avoid being thought too tedious, I ſhall content myſelf with relating only two of thoſe experiments which appear to be the moſt deſerving of attention: and thoſe perhaps, from the hints they afford, may induce others to purſue them further than I have done: for I conſider the whole of my experiments as merely a beginning of a very extenſive and intereſting enquiry.

In a dark room, where there was an excellent electrifying machine, and with which a ſurface of glaſs, about one foot ſquare, was properly charged, I placed upon a ſtand of metal, which was rounded at the top, and about half an inch in diameter, one of the prepared ſhells that would exhibit priſmatic colours very lively. On the upper ſurface of this ſhell, and near the middle where the colour-making parts predominated, I brought the end of a metal rod, (which was rounded alſo,) and then connected the two metals, properly, with the metal covering on each ſide of the glaſs, for the purpoſe of diſcharging the electric fluid. In this circuit there was left, deſignedly, an interval of about three inches, unoccupied by metal, and next one ſide of the glaſs. Being thus circumſtanced, the diſcharge was made by completing the circuit with metal where the interval was left. The ſhell at that inſtant was lighted up to an exceeding great advantage; ſo that all the colours appeared perfectly diſtinct, and in their reſpective places, anſwering to their different colour-making parts.

Theſe colours continued viſible ſeveral minutes; and when they ceaſed to appear, a white purpliſh light occupied their places, which laſted for a conſiderable time. And notwithſtanding this experiment was frequently repeated with the ſame, and other ſhells, the colours continued in their reſpective places, and nearly of the ſame degree of brilliancy: excepting that in or near thoſe parts, where the exploſion took place, a few ſcales were driven off by the natural effect of the fluid itſelf, whilſt under theſe circumſtances.

By

By repeating this experiment with ſome other ſhells, I found there was no occaſion to ſcale them before-hand, becauſe, after a few trials, the diſcharge of the fluid ſeparated the unfavourable ſcales far better than I had been able to do with the point of a penknife: and the colours, excited on the new ſurfaces, frequently appeared brighter than ever. Beſides, by this other method of exciting priſmatic colours, I obtained another advantage, which appears to me not inconſiderable. For here, the colours may be ſeen at the inſtant, after the exploſion hath taken place: and, agreeably to a former obſervation, they appear much more full, intenſe, and lively, than in three or four ſeconds afterwards; or even after they have been expoſed to the light of the ſun.

That theſe colours are excited merely by the *light* produced in diſcharging the glaſs of it's electric fluid, ſeems evident from the following experiment.

I placed ſeveral pieces of ſhells (which would exhibit colours,) at different diſtances from that part of the circuit where the exploſion was propoſed to be made; and in ſuch a manner, that they had very little chance of making part of the circuit. The leaſt diſtance was not more than half an inch, and the greateſt diſtance, not leſs than ſix inches. At the ſame time I held in my hand a prepared ſhell, at the diſtance of ſix or ſeven inches from the ſame part. The charged glaſs had near four feet ſurface, properly coated on each ſide.

When the discharge was made in a dark room, all the shells, at that instant, exhibited their respective colours very lively: but the colours, in some of those shells, were far more lively than in others*: for those shells that were nearest to the light, exceeded the other colours considerably in point of brilliancy and duration. In some instances, these colours continued visible more than five minutes; which is a longer time than I ever found, when similar shells were excited by the solar rays.

From the result of these last experiments, I conjectured, that any *light*, provided it was rather *sudden and sufficiently intense*, would excite the prismatic colours in phosphoric shells that were properly prepared. In order to be satisfied of this, I made use of three different parcels of gun-powder; each of which was mixed with different materials, such as steel, zink, orpiment, amber, &c. From one of those parcels I then took half an ounce or more, and placed that quantity upon a table, at two or three inches distance from several prepared shells, that were disposed round it, and vertically, with their flat sides towards this mixture, pressing it down a little with my fingers. In this situation (the room being dark) the mixture was exploded;

* Many years ago, Mr. LANE, who has obliged the world with several curious experiments in Electricity, shewed me one, where, if I remember well, he produced a fine whitish phosphoric light, upon a kind of limestone, and other phosphoric substances, by the same method, nearly, as I have above described.

and although the light was not ſo great as might perhaps be contrived by other methods, yet the ſhells exhibited the priſmatic colours: but then thoſe colours were not near ſo lively, as the colours excited by electric light.

The other mixtures were afterwards tried in the ſame manner, with the ſame ſhells, and very nearly with the ſame ſucceſs.

F I N I S.

AN

ACCOUNT

OF A GREAT NUMBER OF

PHOSPHORI,

DISCOVERED BY

J. B. BECCARI,

PROFESSOR OF PHILOSOPHY IN *Bologna*.

PUBLISHED IN THE BOLOGNA ACTS, 1744.

1. OF bodies that give light in the dark, there are ſeveral kinds. For ſome bodies throw out light *ſpontaneouſly*, and others, upon being *excited*. Of the former kind, ſome ſhine with a *natural light*; as glow-worms, dates, and a good many aquatics: others enjoy an *adventitious light*, as rotten woods, and the fleſh of ſome quadrupeds and birds. Theſe laſt are not naturally phoſphori, but owe that property to ſome particular cauſe; which, generally, is *putrefaction*,

and, ſometimes, an inſenſible change introduced into the natural conſtitution of the parts.

2. The other kind of bodies, which become phoſphori upon being *excited*, or that are at leaſt aſſiſted by that means, may be diſtributed into different ſpecies, according to the different modes of *excitation*. Theſe modes are *attrition*, *heat*, the *free admiſſion of the air*, and laſtly, *being expoſed to the external light*.

3. Bodies of every kind become phoſphori by *attrition*, provided they can bear that force of friction which is ſufficient to produce the reluctant light that is hid in their ſubſtance. For the ſame reaſon almoſt all bodies grow hot, and often ſo violently, as to take fire. There are ſeveral modes of *attrition*: in the firſt place, *preſſure*, whereby, as Du Fay obſerved, ſome diamonds became phoſphori: in the ſecond place, *colliſion*, which is the vulgar method of ſtriking fire out of ſtones: *friction*, and *contrition*; which laſt is the well-known method whereby the coats of many animals are made to emit ſparks. In the laſt place, and not to be too minute, we muſt reckon *agitation*, which agrees moſt particularly with *liquid* ſubſtances: as the ſea water, when ſtruck with oars, and agitated in a dark night, is always obſerved to ſhine.

4. The emerald phoſphorus, and many gems, and, amongſt theſe, not a few diamonds, together with many other ſtones of ſmall value, as the lapis lazuli, the falſe emerald, and a great part of the

the mountain cryſtals, become phoſphori by the application of *heat*.

5. But the *free admiſſion of air*, in uſing the phoſphorus of Konkel or rather of Kraft, not only produces light, but even a blaze of fire, where friction is *uſed*. The phoſphorus of *Homberg*, and a great many others formed upon the ſame principles from *allum*, or any other animal or vegetable ſubſtance, according to Lemmery's method, as well as Le Fevre's preparation from iron and ſulphur, burn very furiouſly upon the approach of the air. In the laſt place, we muſt mention the *pulvis fulminans* of Godofred, which exceeds every thing of this kind, and has obtained that name from the thundering report it makes, and the quantity of ſparks it emits upon the admiſſion of air.

6. In this ſecond kind of bodies the *Bologna ſtone* deſerves the principal place. From this the philoſophers were firſt taught this wonderful faculty of *attracting the external light*, which was before entirely unknown; by which means they learned to inveſtigate a ſimilar power in other bodies. But their attempts were long without ſucceſs: and if accident had not aſſiſted them, perhaps our Bologna ſtone had been the only phoſphorus known at this day.

7. For though *Helmont* has told us, in his writings, that he had in his poſſeſſion a *flint* ſo prepared as to receive the light, and to preſerve it viſible for ſome time in a dark place: yet nobody

ſince Helmont has been curious enough to find out his preparation. Whence it has happened that the ſecret of making this phoſphorus, and almoſt the memory of it, have died with the inventor.

8. In conſequence of this, the Bologna ſtone maintained for ſome time a conſiderable reputation, until *Chriſtian Adolphus Baldwyn*, in ſearching after ſome ſtrange chymical menſtruum, diſcovered a phoſphorus which perhaps he was not looking for, and certainly had no hopes of finding. This phoſphorus was like *our* ſtone in it's faculty of attracting the external light, and not inferior to it in the brilliancy of it's ſplendor. It was therefore received by the philoſophers with ſingular admiration and pleaſure. For as there was not enough of the Bologna phoſphorus to furniſh every body with ſpecimens who wanted them, (which was not owing to the loſs of the art of making them, as is generally believed, but becauſe this ſecret was confined to a few families at Bologna, and principally to the family of Zagony) this new phoſphorus came very opportunely to ſupply it's place, and was eaſily procured by all that wanted it, and was as fit for making the experiments as the Bologna one. Though after all, it is uncertain, whether the phoſphorus which is now generally aſcribed to Baldwyn, and compoſed of chalk, diſſolved in ſpirit of nitre, be the ſame phoſphorus which that author ſeems to have

have hid by a number of dark paſſages, rather than diſcloſed.

9. After the diſcovery of *Baldwyn*'s phoſphorus, no addition was made to this claſs for near ſixty years. But in the year 1730 Du Fay introduced an incredible number. For this ingenious perſon found, that whatever ſubſtances might be turned to a calx by burning only, or being diſſolved in a nitrous acid, and then brought into a ſolid form again, that was hard enough to be burnt to a white heat, were proper to imbibe the external light. By this means the phoſphorus, which was before a ſingle genus, and confined to the *Bologna* ſtone, was much increaſed, and divided into two claſſes; in one of which our phoſphorus obtains the firſt place, and in the other the phoſphorus of *Baldwyn*.

10. From theſe fortunate beginnings he imagined and expected, that all bodies might be made capable of this diſtinction, either by the proceſſes before mentioned, or perhaps by ſome one more obvious. True it is that Du Fay uſed his utmoſt endeavours to bring into this claſs of phoſphori, metals, gems, chryſtals, flints, and other bodies of that ſort which could reſiſt the force of fire, or of corroding liquors.

11. But what he endeavoured to reach with ſuch great labour, he found four years after effected by Nature herſelf in the moſt noble of all bodies. Du Fay was at that time examining electric ſubſtances; and obſerving in moſt of them, that not

only an attractive power, but even light was excited by friction, he made a more diligent enquiry into the wonderful differences of that light. Upon this occasion he made many experiments of his own invention, and at the same time repeated those which *Boyle* had formerly made with *Clayton*'s diamond. Amongst these he distinguished that experiment in which the gem when moved towards a *lighted candle* continued to shine for some time with the splendor that remained. *Boyle* had been of opinion that this light was owing to the heat occasioned by the vicinity of the flame: and Du Fay, upon first making these experiments, thought the same. But he afterwards exposed some diamonds to the rays of the sun, and though they acquired very little heat, yet they had just the same splendor: upon which he changed his opinion, and determined that this light did not proceed from the heat, but from the lighr of the sun. And here he found himself engaged in a very large field of enquiry. Therefore he began in the same manner to make experiments first upon the other kinds of gems, and afterwards upon other stones, as well precious, as otherwise. But these pursuits did not repay his labour, or answer his expectation. He found indeed that some diamonds, and a certain rough emerald, together with certain crystalline concretions resembling emeralds, amethysts, and topazes in their colour, but much inferior to them in hardness, would shine in the dark after being expoſed to the light. To theſe he added the lapis lazuli, the Bern ſtone, and a few leaden ores. Except theſe bodies he could diſcover none that might be reckoned amongſt gems for their hardneſs and brilliancy, or that might be deemed related to cryſtals, or talks, or ſelenites, or chalks, or even to the numerous claſſes of marbles, that had this property.

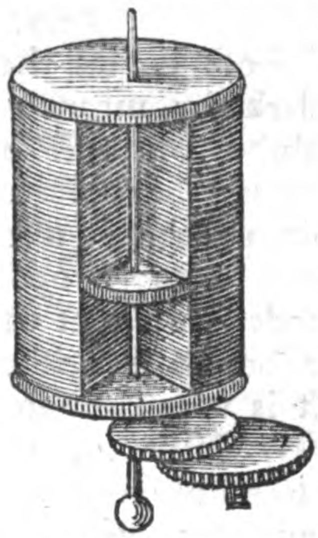
12. Whilſt this was doing at Paris, I accidentally diſcovered the ſame wonderful power in a diamond which I happened to wear in my ring. It appeared to me a ſubject well worthy the attention of a philoſopher. I immediately began to try all ſorts of experiments, in different kinds of lights, upon my own diamond, and upon as many others, of different ſizes, waters, tranſparencies, and colours, as I could procure; in order to diſcover what difference might ariſe in this variety of ſtones, either in the mode, force, or continuance of the ſplendor. I communicated theſe enquiries, and the whole fruit of them, ſuch as it was, to the academy. And I received from that ſociety the moſt favourable marks of their approbation. However many things were left for further enquiry, and it was indeed impoſſible even to touch upon ſome parts of the ſubject. In particular, no enquiry was made into the cauſe why ſome diamonds become phoſphori, whilſt others in all reſpects ſimilar to them, and expoſed to the ſame light, do ſtill remain dark.

13. This new inveſtigation brought with it

more labour than profit, with reſpect to the principal point; but afforded a great deal of matter which was little expected from it. For being obliged in the courſe of theſe experiments to adopt a very nice method of obſerving, inaſmuch as the ſmalleſt difference of light would have very material conſequences: it followed from this accuracy that ſome things were obſerved, which had before eſcaped not only me, who am a pretty diligent obſerver, but likewiſe other perſons of great ſagacity and diſcernment. I found for inſtance, with reſpect to diamonds, upon account of which bodies this enquiry was undertaken, that there are amongſt them a much greater number of phoſphori than any perſon of leſs attention would have imagined. It appeared too, that beſides this gem, and the ſmall number of ſtones above-mentioned by Du Fay, that many other bodies are capable of the ſame diſtinction without any artificial means, and by the mere liberality of Nature; and that not a few other ſubſtances become phoſphori by undergoing a very ſmall alteration. Being therefore entered into this large field of enquiry, I thought it my duty to conduct with the utmoſt diligence and care what I had undertaken by accident. For this reaſon I employed upon theſe experiments all the time that could be ſpared from more ſerious buſineſs. And finding that I had ſpent as much time upon this ſubject, with a view of extending my enquiries, as could be expected or required from a perſon who had many other engagements; and moreover that it would take a much longer ſpace to bring them to perfection: for theſe reaſons I reſolved not to keep theſe obſervations private, but to communicate them, how trifling ſoever, to the learned. And I hope that ſuch as are judges of the ſubject, if they find my obſervations to be founded upon truth, will not altogether deſpiſe the preſent undertaking; eſpecially as they muſt know, that in phyſical learning many great and uſeful diſcoveries have ariſen, or at leaſt have been advanced, and perfected, from circumſtances apparently of ſmall moment.

14. But before we ſpeak of thoſe bodies that are obſerved to ſhine in the dark by means of the light they have before imbibed, it will be proper to explain in what manner the obſerver ſhould prepare himſelf for this purpoſe; ſince the whole affair depends almoſt entirely upon ſuch preparation. It muſt be underſtood that the light imbibed by moſt of theſe bodies is neither very bright, nor very laſting. It is very clear that a weak light does not ſufficiently ſtrike the eye that has been before accuſtomed to a ſtronger; and it is as certain, that a tranſient and ſhort-lived light will be extinguiſhed before the commotion of the eye, occaſioned by the precedent ſtrong light, can have time to ſettle. In order to prevent theſe inconveniencies, it is proper in the firſt place, before we begin our obſervations, to remain in a very dark place until every veſtige of the light before

received into the eye is entirely obliterated: and in the next place that the bodies, upon which we obſerve, ſhould be conveyed from the full light into the dark place of obſervation with the utmoſt ſwiftneſs; which cannot be performed unleſs the place of obſervation be very cloſe to the full light. But in this caſe, how ſhall we avoid admitting ſome portion of light ſufficient to diſturb the obſerver's eye, together with the body? However, I manage this point in the following manner: others muſt ſuit their own fancy and convenience, but this contrivance anſwered my purpoſe. I had a



ſmall box made, but large enough either to ſit or ſtand in, and at the ſame time of a convenient ſize to be moved about. There was a door on one ſide, which ſhut very cloſe; on the other ſide was a window cut out, to which was fixed a cylinder or *tympanum*, ſo cloſely joined as to leave the leaſt ſpace poſſible between the joints, namely juſt enough to give room for the cylinder to turn round it's axis, which was perpendicular. To the borders of the window were fixed two wings, and to the upper and lower borders were fixed two liſts: all of which were of ſuch breadth, and ſo adapted to the curvature of the drum, as to intercept ſuch portion of the external light, as would otherwiſe have

have paſſed through the joinings. In the laſt place there was an aperture from the top to the bottom of the cylinder, being in breadth about the ſixth part of it's circumference. Through this opening the ſeveral bodies were placed upon the bottom of the cylinder, and by that means they were eaſily expoſed to the light or to the obſerver, by only turning the cylinder. And thus were thoſe bodies conveyed in the ſwifteſt manner from the light into the dark; and the obſcurity was at the ſame time defended entirely from the light. Nothing therefore remained but that the ſpectator ſhould have his eye as free as poſſible from every preceding impreſſion. And this precaution requires a conſiderable time to be paſſed in the dark: which circumſtance would be very diſagreeable even to a man at perfect leiſure, and to a perſon anxious to try new experiments, muſt be exceedingly diſagreeable. Therefore Du Fay is very right in recommending the firſt part of the morning for theſe obſervations, ſince the reſt and darkneſs of the night have then refreſhed the eye, and given it it's full ſtrength. As ſoon therefore as a perſon wakes, he ſhould immediately apply himſelf to theſe obſervations without any previous preparation. But if it ſhould happen that the day-light has firſt fallen upon the eyes, and the obſerver ſhould be unwilling to go through the previous preparation of darkneſs and ſolitude; in that caſe, Du Fay recommends the keeping of one eye open and the other ſhut. By this means the eye that is ſhut will, by it's reſt and the excluſion

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of light, be fit for obſerving; and the perſon may uſe his other eye to prepare what is neceſſary for the experiment. Theſe preparations will take up half an hour, or ſomething longer; for in theſe matters delay cannot hurt, but diſpatch may. When this time is over, let him get into his box and prepare for obſervation; for which he may know himſelf to be ready, when he is able in the dark to perceive the form and whitiſh hue of a ſheet of white paper.

15. Thus much for the manner in which theſe obſervations ſhould be conducted. In which, if I have been too long, I hope to be excuſed, eſpecially by ſuch as have upon other occaſions entered upon a work of this ſort, without any body to direct them. And now we ſhall proceed to the obſervations themſelves.

In order to treat of theſe in proper method, we ſhall divide the whole family of phoſphori into *two grand diviſions*; and ſhall treat, in the firſt place, of ſuch as are naturally phoſphori, and, in the ſecond place, of ſuch bodies as become phoſphori, by means of ſome proceſs, which however is ſlight and does not alter their nature. For the phoſphori that become ſuch by elaborate operations, namely, by the intire diſſolution of their natural conſtitution, have been fully treated of by Mr. Du Fay. With reſpect therefore to our firſt diviſion, namely, the natural phoſphori, we muſt conſider the following ſubdiviſions. In theſe we ſhall follow that order which Nature herſelf, the only producer of theſe ſubſtances, hath followed in the arrangement of terreſtrial

terreſtrial bodies; that is, we ſhall refer all the phoſphori to the claſſes of *foſſils*, *vegetables*, or *animals*.

Amongſt the foſſils, we muſt give the firſt place to thoſe bodies which ſeem to conſtitute the ſtructure of the earth, being it's principal parts, or at leaſt that have a ſtrong relation to it's general diſpoſition; and which bodies we find either thrown together in vaſt maſſes, or ſpread through great tracts; or laſtly diſpoſed in ſtrata. Some of theſe bodies conſiſt of parts that are ſcarcely, if at all, connected together; which is the caſe of collections of earths or ſands: the parts of other bodies are connected and joined together, as marbles, and other ſtones of great bulk.

16. I have already ſeen many earths that are phoſphori, and likewiſe not a few that are dark. The dark ones are for the moſt part of dark colours. Amongſt theſe I reckon in the firſt place black, and dark; and then the following deep colours, firſt red, then purple, then green, and laſtly orange; but the white, the aſh colour, the green, and the yellow, being faint colours, are neither favourable to the phoſphoric quality, nor the contrary. For example, *nuceriana*, the white bole of the gilders, and likewiſe the bole that is called the Great Duke of Tuſcany's, are white bodies and phoſphoric. But at the ſame time there are many other bodies, as white, that are not phoſphoric; and amongſt theſe the Vicetina fullonia. There is the ſame ſort of uncertainty with reſpect to the other qualities. There

is for inſtance a great variety in the nature of earths, ſome of which are of a looſe texture, and others thick and cloſe; for which reaſon the former are light, and the latter more ponderous; ſome again are hard, and others on the contrary ſoft; ſome are tough, and others brittle; ſome are lean, and others fat; ſome are rough, and others ſmooth; amongſt which latter ſome are ſo ſmooth as to appear greaſy to the touch. But in this great variety I have not been able to diſcover any certain criterion for diſtinguiſhing which are phoſphori. But in moſt earths the light is weak, and eaſily eſcapes obſervation, unleſs the eye be very clear and attentive. The earth that is brought from the Argentarian mountains, and what is brought from Sicily under the name of the mineral Bezoar, ſeemed to excel other earths in the force and duration of their light. The earth that is called the golden virgin earth by Bocconi, exceeds the foregoing not in it's ſplendor, but in it's duration.

17. After the earths we muſt conſider the ſands, not becauſe they are of the ſame nature with the earth, according to the vulgar opinion, but becauſe they occupy large ſpaces, and conſequently make a conſiderable part of the earth's ſurface. But in truth ſands are very minute fragments of ſtones. It is therefore probable that they follow the nature of the bodies from whence they are broke in the circumſtance of being phoſphori or not, as well as in others. For which

which reaſon I gave myſelf but little trouble about ſands that are black, or of other deep colours, having known by other obſervations that ſuch colours did not admit the light. I therefore applied myſelf to white and other pallid colours; and in fact I think they appear well adapted to the admiſſion of light; for though I found one or two that were otherwiſe, yet I am doubtful whether this was owing to their natural conſtitution, or to the adheſion of ſome foreign ſubſtance. For that ſand, which is dug out of the hills near Bologna, and is of an orange colour, will not become a phoſphorus ſo long as it continues tinged with the ocre by which it is coloured; but, when that is waſhed off, the ſand becomes white, pellucid, and a very fine phoſphorus.

18. Next to the ſands and earths we muſt place ſuch ſtones as are of a great bulk. Theſe, being in a great meaſure diſtributed in ſtrata, by their quantity and ſolidity, ſupport and embrace other bodies that are looſe and unconnected, and by that means give a certain firmneſs and ſolidity to this globe.—Some of theſe ſtones are of a looſe texture, and a rugged ſurface; others are of a firmer compoſition, and not altogether rough; and, in the laſt place, ſome are of ſo firm a texture as to receive a ſhining poliſh. The ſandy ſtones, which are in the firſt claſs, are pale and languid phoſphori; the Coſolearia, which is in the ſecond claſs, is ſomewhat brighter; and marbles, which are in the laſt diviſion, are almoſt all phoſphori. But amongſt theſe, ſuch as are the whiteſt and the ſofteſt, are the

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moſt eminent. Theſe two qualities concur in the alabaſter. But the moſt beautiful of this kind is that *alabaſter* which our ſtatuaries call *cotogni.* Next to this are certain marbles which are as tender as the alabaſter, but are of a deeper colour in particular parts, and have conſequently dark ſpots intermixed with their light; for in theſe bodies, as in others, colours obſtruct the phoſphoric quality, and the more, the darker they are; which was before obſerved with reſpect to earths. But with faint colours it is otherwiſe; and indeed there is a marble of a tender red, which I have obſerved to be a good phoſphorus, and eſpecially ſome pieces of Spaniſh marble of that fine ſort which is called *brocatelli.* Porphory, ophites, granates, and other hard and dark marbles of that ſort, are either not phoſphori, or their light is ſo uncertain as to be hardly diſtinguiſhable.

19. Next to theſe large bodies we muſt reckon chalk (or limeſtone) which in ſome countries, and eſpecially about Bologna, does not lie upon the ſurface of the earth, or in ſmall quantities, but is found in great maſſes. To the ſouth of our city are very high hills, conſiſting entirely of limeſtone; and from thence very large veins thereof run through the neighbouring country. The limeſtone that is dug from thence for building is naturally a pretty ſtrong phoſphorus, but becomes much ſtronger by burning. But this adventitious quality goes off in a few days; which agrees with Du Fay's obſervation with

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respect to this ſtone, and to marbles, and other ſtones. But when this artificial light is gone away, the natural phoſphoric quality ſtill remains, and is not deſtroyed by age, or by the injuries of the air, or other ſurrounding bodies. I ſhould not have ventured to affirm this, if I had not found a piece of lime mortar which was taken out of a wall above one hundred years old, and that had preſerved this power quite unhurt by the force of time, and that perpetual ſtreſs of weather to which it was always expoſed.

20. Next to the ſtones which occupy large ſpaces of the earth, by their own great bulk, we muſt ſpeak of ſuch as are in themſelves ſmall, and are not diſpoſed in continued maſſes, but are found intermixed with the ſtrata of other bodies. Some of theſe are not harder than marble, and others are harder than any marble. Of the former kind, ſome have no determinate exterior form, and no particular and certain diſpoſition of the internal parts, which is the caſe of moſt lime-ſtones: the ſecond ſort are informal on the outſide, but have a certain particular diſpoſition of parts on the inſide, which in ſome of them exhibits a grain that runs in parallel lines, as in the ami-antus, and the alumen plumoſum: in others it produces thin, flexible, and parallel laminæ, as in talc, and lumps of gold or ſilver: in other bodies it exhibits incruſtations one over another, as in the ætites: and laſtly we muſt reckon ſuch ſtones as have a certain regular formation, both

internal and external. To this claſs are referred by *Woodward*, who is our authority for this diviſion of the ſelenites, the lapis ſpecularis, the belemnites, the ſtalactites, and oſteocolla.

21. Lime-ſtones are all phoſphori more or leſs. The colour makes a great difference. The white ones are the beſt phoſphori, and the dark ones produce a pale weak light. Of this ſort are thoſe that the public ſtreets are paved with at Bologna. The amiantus, and the papers that are made from it, are not phoſphori, nor is the talc a phoſphorus; neither the common pellucid ſalt, nor that which is green and unctuous; neither is any of this quality in the ſpangles of talc wherewith the ſands in our mountains are mingled. This power is very weak in ſilver ſpangles; it is ſtronger in a certain kind of ætites which exhibits that aſh colour which is obſerved in the incruſtations of certain ſtones that look like a kind of calx; but there is nothing of it in another kind of ætites that is found in the neighbourhood of *Narni*, and which is a concretion of yellow ocre; ſo that it plainly appears, that in the ſame ſpecies of foſſils there is a great difference in the materials of their compoſition, and conſequently in their phoſphoric qualities.—But in that kind of ſtones which is diſtinguiſhed by a regular external and internal form, nature ſeems to have effected a wonderful ſingularity. In the ſtalectites ſhe amuſes herſelf with the moſt elegant variety of forms. She faſhions tubes, laminæ,

næ, trunks of trees, bones, clusters of grapes, and almost an infinite variety of figures and even, sometimes, human forms, out of the strong concretions that are found in water. But in the selenites, and the Iceland crystal, she is more laborious, and constructs their figures according to the severe laws of geometry. Our Bologna stone has been the object of her industry; for she has disposed therein certain striæ, pellucid as crystal, and which tend to it's axis in the most beautiful manner. Moreover, she has adorned all these bodies not only with the greatest beauty, but with the phosphoric power. Amongst the rest we must distinguish a certain *stalectites* that is dug in our neighbourhood, which is rather pellucid, of a yellowish colour, and of a beautiful form. Of the same kind are those stones which are perforated like sponges, and are therefore called in our language water sponges. Upon being burnt they afford a very white substance. Our waters too produce a crust on the sides of the pipes, and at the bottom of vessels; which crust is a very strong phosphorus. The same may be observed with respect to other concretions belonging to this kind. We need say nothing of the selenites and the lapis specularis, since we have said so much of our lime-stone, to which they have so much resemblance. We must not omit the Iceland crystal, and our Bologna stone; which last mentioned substances produce a light almost equal to those abovementioned. The belemnitæ, the aſtroitæ, with reſpect to their form, may be reckoned of this kind; bnt, with reſpect to their origin, they ſeem to be of a quite different one. But whatever kind they may belong to, I have obſerved that moſt of them are phoſphori, ſome brighter than others. And ſince we are talking of figured ſtones, we muſt obſerve that many dentritæ are phoſphori; but that ſort of dentiitæ, that is formed like a croſs and ſpeckled, is little or nothing of a phoſphorus.

22. I come now to conſider ſuch ſtones as are harder than marble. Theſe are all endued with a brightneſs that is altogether ſingular; for they are capable of ſo great a poliſh as to ſend forth light from every point. Many of them are likewiſe ſo perfectly tranſparent that there is no part of them that does not ſeem to be entirely penetrated by the light, and to be full of it. And yet in this kind of bodies, ſo noble and ſo numerous, but a few ſtones can be found that retain in the dark any veſtige of that ſplendor which is ſo great an ornament to them in the light. The lapis lazuli is the only ſtone amongſt the opaque ſtones which preſerves it's light in the dark. The malechitæ, and the whole family of the jaſpers, have not this property. But amongſt the ſtones that have a ſort of tranſparency, I have obſerved that flints have a certain light ſpread over them. And although this is but a weak light, yet it ſeems to penetrate the internal parts of the ſtone, and at the ſame time to aſſume it's colour.

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Many ſpecimens of agate exhibit a like appearance. In theſe was obſerved a very ſmall light diffuſed through the whole body, and of ſuch a horny colour as this ſtone has. I likewiſe obſerved a light ſomewhat ſtronger in ſeveral concretions of the agate kind; and in particular I ſaw one that was formed in a plough, and ſome others in the ſhells of cockles. All of which repreſented, in a beautiful manner, the ſame appearance as the true agate does. But there are a great number of agates that do not ſhew the leaſt appearance of this faint light. But the lapides Caledonii that I have examined are all phoſphori; and, amongſt the reſt, one was ſo bright that the interior parts of it ſhone very conſiderably. But I have found that Du Fay's obſervation, which is mentioned above, is perfectly true with reſpect to all tranſparent ſtones, whether coloured or colourleſs, namely, that diamonds alone are the only phoſphori amongſt the gems; nor does this extend even to to all diamonds, nor is the phoſphoric quality in any proportion to the other fine qualities of the ſtone. And with reſpect to cryſtals I have obſerved that their ſize, their tranſparency, or their colour, have nothing to do with their phoſphoric properties; nor are theſe properties affected by the oppoſite circumſtances. And theſe conſiderations have made me ſuſpect that certain concretions which are called cryſtalline, and ſo eſteemed, do in truth differ very much from the mountain cryſtal; ſince otherwiſe they would

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no more be phoſphori than the true cryſtals are. I have ſeen many ſpecimens of theſe concretions that are as hard as the true cryſtals, and are found in thoſe globular and hollow ſtones which are called *cryſtalline mothers*, and there appear in the form of very pretty ſmall gems. I ſaw likewiſe another cryſtalline concretion in a cockle-ſhell that incruſted it's cavity with exceeding ſmall drops that were moſtly ſexangular. Theſe all were very bright phoſphori.

23. As to thoſe metals which are of the next value to gems, they are farther removed from being phoſphori than gems are; for not only metals, but all ſubſtances that have any affinity to them, are excluded from that claſs of phoſphori into which ſome diamonds are admitted. Therefore cinnabar and lapis calaminaris, and biſmuth, and marcazites, and zinks, and other bodies of the ſame kind, are not phoſphori. But foſſil *orpiment* and white *arſenic* are phoſphori; but perhaps theſe bodies may not be metallic, or they may have ſome particles in their compoſition that are phoſphoric.

24. And if this ſhould be reckoned to be ſome terreſtrial juice, yet I cannot think it to be ſulphur; and yet it is a common opinion that the orpiment has a great deal of ſulphur in it. But I have obſerved that ſulphur, and all ſulphurious bodies and fat juices, not only ſuch as are liquid, as the *petroleum*, but likewiſe ſuch as are concretions, for inſtance, amber, jet, pit-coal, and all the

the claſſes of the bitumens, though they kindle by the application of fire, yet they are by no means phoſphori.

25. On the other hand, *ſalts*, which conſtitute the other claſs of the terreſtrial juices, are phoſphori: but then they muſt be perfectly free from all mixture, and eſpecially of metals: otherwiſe they will not be phoſphori, though they be as tranſparent as the cleareſt gem. I am warranted in this opinion by experiments made upon vitriol, both *Cyprean*, *Roman*, and *German*, and which had been made as tranſparent as a ſaphire, or an emerald, in order for the experiment. Other ſalts are phoſphori: ſome better than others. The rock ſalt and the ſparkling ſalt are very feeble: the opaque ſalt is ſomething better: the foſſil red ſalt not at all: the ſea ſalt is a pretty good one, provided it be cryſtalline and dry: the ſal ammoniac is not extraordinary, but not the worſt of all. The ſalt that is called Cathartic, and the modern preparations of nitre, are pretty ſtrong: the Egyptian ſalt ſomewhat weaker, and ſo is allum: but borax is an exceeding fine and bright phoſphorus.

26. Thus far of bodies which are naturally and truly foſſils. We muſt now ſpeak of ſuch as are reckoned foſſils, not upon account of their origin, but becauſe they are found in the earth. A great part of this claſs conſiſts of the ſhells of marine animals, which are found in ſuch plenty and variety all over the earth; and even at great depths

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and amongſt the moſt ſolid ſtrata. And theſe muſt needs be good phoſphori, ſince I obſerved that all of them had that quality more or leſs, except certain tubes that had had worms in them: and a body of that kind which is called *dentalia*, together with a certain nautilus of the foliatious kind. But none of theſe marine bodies had that quality ſo ſtrong, as ſome gloſſopetræ that I happened of. I have ſeen ſome fiſhes teeth that had acquired the hardneſs of ſtones, and were pretty good phoſphori. The remains of ſome land animals are not altogether without this quality: for I obſerved that the foſſil *ivory* was entirely ſuffuſed with this light; and that many *bones* of quadrupeds were phoſphori at different points of them. But I cannot ſay ſo much with reſpect to the parts of vegetables: if indeed thoſe ſtony pieces which were ſhewn to me under the name of foſſil wood, are properly to be called vegetables: for though they had the form of wood, yet their texture, hardneſs, and colour, were very various. For I ſaw nothing in any of theſe ſubſtances that was not quite dark: though I obſerved them with the greateſt attention.

27. And now we have gone through all the foſſils, and are to ſpeak of the vegetables. This claſs of bodies is as numerous and various in it's ſpecies as any that is known: and yet it ſeems to us to have but few phoſphori. But perhaps I am prejudiced by the incredible multitude of ſplendid minerals that may have dazzled my eyes: and

and which is ſo great, that I have hardly been able to ſet down their names. However, if I may hazard a conjecture in ſuch an abſtruſe queſtion, I ſhould think that vegetables are not in fact deſtitute of this power, but that they contain ſome mixture of foreign particles that hinder it's appearance. I ſhall give the reaſons for this opinion upon ſome other occaſion. At preſent I muſt obſerve that land plants, whilſt they are full of juice and in verdure, have nothing of the phoſphoric quality. But wood that is dry and without ſap affords a weak and tranſient light: and what may ſeem ſtrange, this light *flows* to the edges and angles in ſuch a manner that it ſeems to leave the reſt of the ſurface. When I firſt ſaw this, it brought to my mind the magnetic virtue which is likewiſe obſerved to flow to the edges of magnetic bodies. It is further remarkable that in ſome pieces of wood, and eſpecially in fir, there are ſmall ſpots about the ſize of a tare, which are of a remarkable brightneſs; and make their appearance in ſuch parts of the body as have nothing to diſtinguiſh them from the general darkneſs of the ſurface. After the woods I examined ſome *barks*, and found them to be middling phoſphori: but the fruits and the ſeeds, together with the kernels and the flour contained in them, had nothing of this quality. And in particular I found, that wheat meal, and the whiteſt flour of wheat, was as dark as any body whatſoever. But it is otherwiſe with that ſoft down which is contained in the fruit of the cotton-tree, and which is produced by it when it becomes ripe: this down is very well known, and is beautifully white. Salts that are the concretions of the juices of plants, are ſtill better phoſphori. Of this ſort are thoſe ſtones that are found growing to the inſide of wine caſks, and which are called tartar. But of all the ſalts in this claſs of vegetables, the brighteſt phoſphorus is *ſugar*. This is not only bright upon it's ſurface, but through it's whole ſubſtance, as if it imbibed the light. On the contrary, gums and reſins, as maſtic, myrrh, and frankincenſe, are not at all phoſphori: in which they agree with thoſe fat juices, which we before reckoned among the foſſils. But we muſt except wax out of this account of fat vegetables: it is indeed an oleaginous concretion, but of a ſingular nature. White wax is found to be a phoſphorus, but of a very weak and tranſient kind. As to marine plants, I have very little to offer that is worthy of notice: for I have examined but few, and thoſe not juicy ones, but rather ſtony concretions. I found that ſome white corals, and a madripora of the ſtarry kind, were phoſphori. But I ſaw another madripora of the fungus kind, that was dark.

28. We ſhall now, in the laſt place, conſider of animals. As this claſs of beings excels the other two in many particulars, ſo we may obſerve that it is equal to either of them in it's phoſphoric appearances. Indeed it produces ſuch a great

number

number of phoſphori, as one can hardly reckon. But at the ſame time there is ſuch a general reſemblance, and ſuch a natural correſpondence amongſt them, that it ſeems not difficult to compriſe them all under ſome general principles, and to treat of them in a narrow compaſs. It may be obſerved, that whatever animal ſubſtances are ſimply earthy concretions, hardened into a ſolid form, are at the ſame time phoſphori. For inſtance, moſt bones are phoſphori, and eſpecially the bones of quadrupeds; the teeth of animals, and particularly the human teeth, eſpecially in ſuch parts of them as have contracted a ſtony hardneſs, are the moſt phoſphoric amongſt the bones. In theſe bodies hardneſs, compactneſs, and whiteneſs, are circumſtances the moſt favourable to their phoſphoric appearance. For this reaſon almoſt all the ſhells of marine animals are phoſphori; and in the turbinated ſhells the light is the ſtrongeſt at the cavity of the orifice, and in the bivalve ſhells at the nodes of the joints; becauſe, in theſe places, they are the moſt compact, and the whiteſt. Muſcles are covered with a very thin ſhell, and ſo are the *pinnæ:* I have ſeen ſome of this latter kind brought from Amboyna; they had a tranſparent coat, but of the brittleſt kind. Theſe had no phoſphoric quality. The ſame may be obſerved of the unguis odoratus. It's coat is formed out of a bony ſubſtance that is ready to take fire, rather than out of any thing earthy or ſtony. And this makes me ſuſpect, that in ſuch bodies as are in a great meaſure earthy concretions, that the phoſphoric quality is certainly greatly leſſened, if not entirely extinguiſhed, by the mixture of any greaſy ſubſtance. And indeed the horns and hoofs of land animals, which have a great ſhare of oleaginous parts, have either no phoſphoric quality, or it is ſo weak as to be imperceptible. On the other hand, ſtones that are bred in the bodies of animals are pretty ſtrong phoſphori. Of this ſort are the ſtones which are found in the heads of ſome fiſhes, but which the *river aſtaci* have in their belly; ſuch likewiſe are what are called Bezoar ſtones; and laſtly thoſe ſtones that are bred in the kidneys and bladder of men. But perhaps theſe become phoſphoric for a contrary reaſon; for they are lean, and contain no more unctuous matter in them than ſuffices to connect the earthy parts whereof they conſiſt. We find likewiſe many examples of this ſort in other kinds of bodies. We ſhall take the birds as an inſtance. Their feathers, though they are ever ſo white, are not phoſphori. On the contrary, their eggs, or rather the ſhells of them, have a great deal of this quality. I obſerved it particularly in the oſtrich's eggs. But theſe indeed are larger, harder, and thicker, than any other eggs, though there are many as white. But it is more ſtrange, that in thoſe eggs of that foreign bird which they call an *emaw*, and which are thick and hard, but which at the ſame time have a

number

number of rough and dark ſpots upon their ſurface; and likewiſe that in the eggs of many other birds, which, though they are white, have yet a very thin ſhell, there ſhould be the like phoſphoric quality. But the truth is, that however thin they may be, and however ſpotted, yet their conſtitution is more earthy, and leſs oleaginous, than the feathers.

29. We have now finiſhed our account of ſuch bodies as are *naturally phoſphori*. But before we proceed to the ſecond branch of our general diviſion, namely, the *artificial* phoſphori, we ſhall anſwer a few queſtions that may ariſe upon what has been ſaid. And firſt it may be aſked, in what degree of light, and for how long a time, theſe phoſphoric bodies ſhould be expoſed to it? In the next place, what are the bounds beyond which theſe phoſphoric bodies cannot ſhine, and within which their qualities will not be perceptible? We ſhall next ſhew how long the light may be expected to continue in the dark place. And laſtly we ſhall enquire, whether there is any proportion or correſpondence obſerved amongſt theſe ſubſtances.

30. As to the firſt point I have tried experiments upon different phoſphori in different lights. They all of them are brighteſt in the light of the *ſun*; next to this, in the clearneſs of a ſerene air: a cloudy and dark day affords the weakeſt appearances. The air muſt be free and open; for many phoſphori that would afford a middling light in

the open air have been quite dark when ſeen in a light that paſſes through a glaſs window. And therefore *Homberg*, in directing the manner of making experiments upon the Bologna ſtone, ſays, very rightly, that it ſhould be placed in the open air. For, if the ſtones are of a dull kind, and not favourable to theſe experiments, the light that comes through the window will not excite them at all, or very weakly. Laſtly, it muſt be obſerved, that the moſt brilliant phoſphori hardly give any light at all, though expoſed to the brighteſt flame. For inſtance, alabaſter, which is perhaps one of the moſt ſplendid phoſphori, if it be expoſed to a large fire, will ſcarcely contract ſo much light as may be found in the common flint pavement of the ſtreets upon a cloudy day. By which we may underſtand what a great difference there is in the force of the ſplendor between theſe two kinds of light, which if conſidered ſeparately, and rather curſorily, may be thought to be a good deal alike, at leaſt in their extremities. For who is there that, in a dark night, when his eye is ſtruck with a bright flame, would not prefer that ſplendor to the cloudy light of a gloomy day, or would not at leaſt think it as good? And yet how many degrees muſt there be between thoſe two extremes of the ſolar light, and the light of the fire, ſince we find that the brighteſt light of the latter is ſo much inferior to what is almoſt the leaſt light of the former, that the objects expoſed to each are ſo differently enlightened?

enlightened? But in truth our ſenſes deceive us extremely in theſe matters, eſpecially when we conſider them ſeparately. Perhaps it would be ſafer to compare theſe gradations of light by the application of phoſphoric bodies. Thus for inſtance we might, without any great danger of error, determine that degree of day-light to be equal to the greateſt brightneſs of a fire, in which day-light, alabaſter, or any other body of the ſame kind, ſhall be found to ſhine as brightly as in the light of a furnace. But this degree of day-light is not to be found except towards the evening.

31. The time during which bodies muſt be expoſed to the light, in order to make them ſhine in the dark, is very ſhort. Four ſeconds, or five at the moſt, are generally ſufficient for the weakeſt phoſphori, and a longer time is uſeleſs. But to give a middling and tranſient light, it is enough in general to expoſe the bodies to the light for half a ſecond, or a third of a ſecond at moſt. And I muſt obſerve that this *greedineſs* in imbibing the light, which we find in phoſphori, that are not in the firſt, or even in the ſecond rank, is to my apprehenſion, much more ſurpriſing than the great phoſphoric vigour of the Bologna ſtone, which has been ſo much admired by many philoſophers.

32. Thus much ſhall ſuffice, as to the light in which we place our phoſphori, and the time during which they are to continue in it. We are now to ſpeak of the light which they bring with them into the place of obſervation. And as to this, it may be aſked, what is the ſtrength of it,

it, and how long it continues? But as this is a ſubject that depends entirely upon the judgment of our ſenſes, every body muſt ſee how difficult it is to reduce it to any known and certain ſtandard. For which reaſon we ſhall lay down but one general rule with reſpect to this ſubject, and which is the only one that admits of no doubt; namely, that the light of theſe phoſphori is weaker than that of the Bologna ſtone, and even than that of fine diamonds. For it is certain that this ſtone may be ſeen like a *burning coal* even in a moderate ſhade, and without any extraordinary preparation in the obſerver's eye. And it is as certain, according to Du Fay's obſervation, that diamonds, in order to be viſible, require no greater darkneſs, nor any other clearneſs of ſight, than what are ſufficient to perceive the light that iſſues from rubbed amber. But the phoſphori in queſtion, although they be of the moſt ſplendid kind, and at the height of their luſtre, do yet, to be diſcerned, require that the eye ſhould be more exquiſitely prepared.

33. The time of the duration of the phoſphoric light is, as we before obſerved, not the ſame in all bodies. Some loſe their light ſooner than others; but the longeſt duration of it is very ſhort: for I never ſaw any that kept it's light more than ſix or at moſt eight ſeconds. But there are ſome that do not keep it for two ſeconds. And it is eaſy for an obſerver that is not well aware of this, and ready in his obſervation, to miſtake theſe for dark bodies.

34. From what has been ſaid it may be inferred, that there exiſts ſome ſort of proportion between the ſeveral parts of this ſubject: the greater the light is, the greater and more durable will be the phoſphoric appearance; which is conſequently leſſened by a ſmall light. The ſame obſervation holds with reſpect to the length of time during which they are expoſed to the light. If this be too ſhort, the light will be faint, and ſoon go out; therefore as much time muſt be allowed as is neceſſary to ſaturate each body with it's juſt quantity of light. This, as we have ſaid, need not be long, but it muſt be enough: the duration of the light is proportionable to the force of the light received; conſequently bodies, that have received a brighter light, do not ſo ſoon become dark as thoſe bodies that have received a weaker light.

35. Having now finiſhed what I propoſed to ſay with reſpect to *natural phoſphori*, I ſhall now proceed to conſider ſuch bodies as become phoſphoric by *artificial* means, or ſome other accidental cauſe; and I ſhall begin with ſuch as are made phoſphoric without the application of fire. The firſt of theſe are ſuch ſubſtances as are produced by the maceration of certain *ſtalks*: and here I mean principally ſuch ſtalks as have barks; or ſuch ſtalks as are furniſhed with fibres that are long, ſolid, and flexible: the only art uſed in this caſe is to get rid of whatever is viſcous, tough, or fat. When theſe dirty parts are removed, the

fibres

fibres remain ſoft, flexible, and perfectly white: they are then pounded, carded, ſpun, and wove. And thus cloths are made, thoſe beautiful and uſeful inventions, ſo acceptable to all ages and degrees of human life. But to this uſual commendation of their utility, we muſt add their great merit as phoſphori of conſiderable rank. For it muſt be underſtood, that although flax, or hemp, undergoes ſuch a number of changes, by heat in the ſun's light, by being diſſolved in water, by putrefaction, by waſhing, by bleaching, and ſprinkling; I ſay, notwithſtanding all this, the natural conſtitution and mixture of theſe bodies is not at all corrupted or impaired. For every injury ariſing from ſo long and laborious a proceſs affects not the ſtamina of the plants, but their juices only. It is theſe that ferment, that putrify, and that are cleared away; but the ſtamina preſerve their ſtrength, and their innate qualities, againſt all theſe accidents; and come out poſſeſſed of that phoſphoric quality, which was before oppreſſed and weakened by the juices. But ſince Nature has formed not only theſe plants, but many, if not all the reſt, with the ſame ſort of fibres, I have no doubt but that, by a like proceſs, other plants may be brought to the ſame clearneſs and brightneſs. For ſince the compoſition and mode of mixture are the ſame in all, as chymical reſolutions ſhew us, why ſhould we not attribute to them all the ſame phoſphoric quality? And I have the following experiment to ſupport my conjecture.

I ma-

I macerated the fibres of the leaves of a garden willow in water, very ſlightly, and making up a maſs of that ſubſtance, and placing it in the day-light, I obſerved upon it a faint and tranſient, but at the ſame time a moſt manifeſt, light. And this reaſon made me obſerve formerly as to vegetables, that they ought not to be ſuppoſed to be otherwiſe than phoſphori.

36. From linen macerated, rubbed, pounded, and reduced to a ſort of liquid pulp, is made paper; a moſt uſeful production, and which wants nothing but ſcarcity to entitle it to the higheſt compliments. This is alſo a phoſphorus: Philoſophers ſhould therefore hold it in great eſteem: I am ſure it is entitled to my acknowledgments, for it opened the way to my diſcovery of this multitude of phoſphori. For when I was looking for ſomething elſe, I accidentally obſerved a piece of paper ſhining with a moſt beautiful light. At firſt I aſcribed it to the whiteneſs of it's colour, and therefore endeavoured to find the like appearances in a great number of other white bodies. But obſerving that ſome of theſe were phoſphori, and others not; and going on in this train of obſervations, I at length arrived at the knowledge of thoſe things which I have already delivered, and am now going to purſue. Paper then, after it has been vexed by this long proceſs, retains that phoſphoric faculty which it received from it's firſt origin, and is, therefore, a great proof of the ſtrength of it.

Theſe appearances are more wonderful in the paper, than they are in the linen out of which it is made; inaſmuch as the paper is more unlike the ſtamina of the plants from which theſe two curious productions are made; for who can find in paper the leaſt trace of the original form of flax or hemp? That whiteneſs, which is not exceeded by that of any other body; that denſity, which is at the ſame time not altogether impenetrable by wet, or light; that flexibility, which is at the ſame time tenacious of folds; it's ſmoothneſs, it's thinneſs, and all thoſe other qualities that diſtinguiſh it ſo much: what difference is there between theſe and that wet, ſoft texture, that verdure, fragility, and all the other qualities of a tender bark? But if we eſtimate things not by their outward appearance, but by their internal conſtruction, we ſhall find that human induſtry has made a very ſmall alteration, or perhaps none at all, in the real vegetable nature of the plants, but has only introduced a new diſpoſition of the ſenſible parts. For what do we find in the fibres of the ſtalks, from which linen, and afterwards paper, is made, which theſe two bodies do not, upon an accurate ſcrutiny, diſcover to the induſtrious chymiſt, whoſe art extends to the internal compoſition of bodies? It is well known that paper, and likewiſe the fibres of plants, from whence linen is made, do afford in the proper furnaces, firſt water, then another liquor of an acid nature, next oil; at firſt mild, afterwards

afterwards acrid and more burnt; laſtly, thick and blackiſh. At the end of the proceſs is left a ſort of coal, chiefly of an earthy nature, black, and poſſeſſing an acrid taſte, from the intermixture of oil and an alkaline ſalt. From whence it plainly appears, that in the whole ſeries of theſe changes, although the external form of the bodies is loſt, yet the inſenſible parts, and what conſtitutes their natural and internal mixture, remain entire.

37. However, whether this phoſphoric quality in the paper be derived from it's vegetable origin, or be occaſioned by it's laborious manufacture, it is yet ſo fine a phoſphorus as could not be paſſed over in ſilence without ſome loſs to natural philoſophers, eſpecially as theſe particulars have not been mentioned by any other writer. Much leſs can I omit what I afterwards learned by experiments made upon paper itſelf. Though paper be, as we obſerved before, a conſiderable phoſphorus, yet upon the application of a moderate heat, at leaſt ſuch a heat as does not at all hurt it, the paper will become ſo ſplendid as to be in a manner a new phoſphorus. I proved this by two experiments: in the firſt I expoſed the paper to the fire, without the interpoſition of any other ſolid body; in the ſecond I applied the fire by means of a ſolid heated body. The firſt experiment was in this manner: I laid a ſheet of paper upon a gridiron, and placed upon it a ſquare flat tyle ſo as to preſs the paper cloſe to the bars. Under-

neath were placed burning coals. When the paper was heated enough, the tyle was taken off, and the paper, hot as it was, expoſed to the light, and afterwards removed into the place of obſervation. It was there a very pleaſing ſight to obſerve the paper ſhining with an uncommon light, and to ſee the figure of the gridiron expreſſed by the moſt elegant mixture of ſhade and light. For in thoſe places which were freely expoſed to the action of the fire, there was the brighteſt light; but in the other parts that were covered by the bars, and which had only the uſual quantity of light upon them, there was the appearance of ſomething like darkneſs, which exhibited the outline of the bars determined with the greateſt preciſion. The duration of the light was equal to it's ſtrength; for it continued ten ſeconds at leaſt, ſo lively, that the repreſentation of the gridiron remained very diſtinct; and when the appearances ceaſed, the paper was again expoſed to the light, and being brought into the place of obſervation, the ſame appearances were again obſerved. This being often repeated, the brightneſs of the image went off by degrees, and at length, after being many times expoſed to the light, and taken into the place of obſervation, they at length entirely diſappeared, leaving the uſual light of the paper evenly diffuſed over every part of it.

38. The ſecond experiment was made in the following manner: there was a pretty thick plate of braſs, which was made ſo hot that it could hardly

hardly be held in the hand, but at the ſame time would not hurt the paper. Being placed upon the paper, and continued there till the paper was hot, it was taken away, and the paper removed, as uſual, into the place of obſervation. In the part that had touched the plate, it appeared of an extraordinary brightneſs, and ſhewed the figure of the plate perfectly defined: the reſt was as in the former experiment. Long ago I made another trial of the ſame thing, with a view of determining the duration of the light received from the fire. For this purpoſe, having heated the paper, by means of the plate, in the uſual manner, I expoſed it for a few ſeconds to the light, and preſently drew it back again into the dark. I repeated theſe movements fifty times, and found that I could clearly diſtinguiſh the figure of the plate every time. After that there was left a very ſmall light circumſcribed by the outline of the former figure; and ſoon after the whole appearance was gone off; and the paper, upon being expoſed to the light, produced only that moderate and equable appearance which was natural to it.

39. But I muſt mention one circumſtance, namely, that when the adventitious light began to go off, I ſeveral times expoſed the oppoſite ſide of the paper to the external light; and as often as I did ſo, I obſerved the figure of the plate to appear much brighter on that ſide than on the upper one; and yet the latter had been in contact

with the heated plate, and had been often refreſhed by the external light. From whence it may be inferred, that the phoſphoric quality, which is acquired by the applcation of fire, may in theſe kind of phoſphori be worn out and exhauſted by the repeated application of light: though this does not hold with reſpect to the natural phoſphoric power. This obſervation was made long ago in relation to the Bologna ſtone: as to which it has been often laid down as a rule, that in order to preſerve it for a long time fit for experiments, we ſhould not often produce it in the open and lucid air. But I think it may be proper to enquire, by means of accurate experiments, whether this property is obſervable in the other phoſphori which are made ſo by the application of fire: for if it be, we ſhall have another new criterion in both the kinds of phoſphori, that is the *natural* and *artificial*, which may perhaps aſſiſt us in our further enquiries.

40. Nor is it leſs remarkable that this bright appearance of the plate was very clearly diſtinguiſhable, though there was no heat in the paper perceptible to the touch. And from hence it is eaſy to conclude one of theſe two things, either that the corpuſcles of light, or of that body in which light reſides, continue much longer in bodies where they are fixed than the corpuſcles of heat: or if we attribute an equal power in this reſpect to each of them, then we may conclude that the ſight and the touch do not judge of their reſpective objects

with equal accuracy: from hence it may follow, that one may be ſtruck, when the other is little or not at all affected.

41. It will not be difficult to propagate this laſt opinion amongſt thoſe philoſophers who maintain that one and the ſame body of fire is both heat and light: that is, it is heat when the fire itſelf is greatly agitated with a variety of motion; and that it is light, if the corpuſcles of the fire are diſpoſed in a certain ſeries, and being driven forward in right lines, ſtrike upon the eye of the obſerver. Theſe gentlemen will judge for themſelves how far my experiments coincide with their opinion. It is certain that they can find nothing more likely to change the heat impreſſed upon the paper into light, according to their notion, than this application of the external light. This light being diſpoſed in right lines, and thrown from the ſhining body by quick and ſtrong vibrations, will impel, direct, and order the congenial matter which it finds upon the paper. By this means, that which only affected the touch, as heat, will, upon being excited by other light, ſtrike upon the eye. In this particular, therefore, they will find theſe experiments very agreeable to their notion. But I apprehend, they will not be able to reconcile to their opinion that certain and accurate definition of the figure which we have before related. For we do not ſee how that figure could be ſo exactly taken, or ſo firmly retained, by fiery corpuſcles. For they muſt all allow that it is the nature of heat, or fire, to endea-

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vour conſtantly to expand itſelf, and in fact to expand itſelf every way. How then can it be, that this reſtleſs and impatient ſubſtance ſhould remain quiet within the outline of the plate? And if it ſhould paſs that, and be diffuſed over the reſt of tbe paper, and if it ſhould then be expoſed to the external light, would it not become all over a phoſphorus; and either enlarge the image of the plate, or rather confound and obliterate it? Theſe things ſhould be well conſidered by thoſe philoſophers who chooſe to try their parts in this curious inveſtigation. They ſhould further be acquainted, that by the heat of fire, not only paper, but many other bodies, have their phoſphoric light encreaſed: ſome of them retain the ſame degree of light which they had before: others have their natural light ſo far weakened, that it would appear quite extinguiſhed if it did not revive upon removing the adventitious heat. But I have not yet had an opportunity to prepare ſuch a number of obſervations as might enable me to digeſt theſe bodies into certain kinds: ſo that by comparing them together, we might learn how far they agree with the nature of fire and heat, or how far they differ from it.

42. But it is now time to treat of thoſe phoſphori, that become ſuch by the operation of fire. And here it muſt be remembered, that ſuch a degree of fire only is required as may affect only the external texture of the bodies, and that in a ſlight degree; and not ſuch as may diſſolve or

deſtroy the internal conſtitution. Therefore let the fire be no more than ſuch as may leave ſome traces of it's operation upon the bodies; and may not change their form, but leave them, in fact and name, ſuch as they were before: for we do not mean to introduce cinders, aſhes, or calx's, but the bodies as they were. Moreover, theſe phoſphori are not to be produced by burning, but only by drying, or roaſting. And in mixed bodies we cannot roaſt the watery humour; for this eludes the force of the fire, and becomes vapour, and returns again to water, if properly received: nor do we roaſt the ſaline or earthy particles, for theſe vitrify, or they dry up, or become calx's: but the *oils*, and *fat* parts, which conſtitute the principal parts of the bodies, theſe are the objects of their roaſting. Vegetables and animals abound moſt with theſe parts. And there is great reaſon to ſuppoſe, that almoſt all animals and vegetables, if properly roaſted, may be brought into this claſs of phoſphori. But we muſt truſt to obſervations and not to conjecture. Therefore, in this place, I ſhall relate nothing that is not founded upon experiment; reſerving other conſiderations for further trial.

43. The fleſh of animals, if roaſted, is a phoſphorus. I tried the experiment only upon white meats, ſuch as poultry: for I was afraid to venture upon brown meats, well knowing that the light is rejected or loſt in them. Bones likewiſe, which are natural phoſphori, become brighter,

and of an unuſual colour, by burning. Likewiſe the *nerves*, upon being roaſted or dried by a moderate heat, become very ſplendid and durable phoſphori. And ſince almoſt the whole animal ſyſtem conſiſts of nerves, and ſince all nerves contain a viſcid, tough, and glutinous matter, I did imagine, that if animal glutens were roaſted, they might become good phoſphori: and I found myſelf not miſtaken; for the common glue uſed by joiners, and iſinglaſs, become phoſphori by roaſting. I was not leſs attentive to other animal ſubſtances. I roaſted many of them: ſome ſucceeded, and others did not. Feathers and hoofs, which, as was ſaid before, are naturally dark; and likewiſe the whites of eggs; though I have tried them ſeveral ways, yet I could never make them phoſphori. However, it is otherwiſe with the ſhells and the yolks: but theſe laſt do not become phoſphori till they are hardened and reduced to a ſolid and almoſt a dry maſs, by preſſing out the oleaginous part: for whilſt they have that juice, they will grow ſofter by the application of fire, and not acquire that dryneſs which is requiſite to make them bear roaſting without taking fire. But when they are made dry, or at leaſt leaner, by having the oil taken out by means of a very gentle heat, and ſuch an one as does not mark them at all, they then become phoſphori, and produce that light, which the abundance of the oily juice cauſed to adhere to the bodies, or oppreſſed and prevented from ſhining forth. But now, how comes it to paſs that one part of the egg ſhould become a phoſphorus by ſo ſlight a proceſs, and the other is not to be made ſo by any means? How comes it that when Lemery was endeavouring to make Homberg's phoſphorus, out of each part of the egg, he found that the yolk eaſily anſwered his purpoſe, but could do nothing with the white? Theſe are ſtrange effects in theſe bodies: what connection is there between theſe phoſphori of mine, and that of Homberg? Is it not ſtrange that the ſame ſubſtances, which remained dark, or become phoſphoric when expoſed to the light, ſhould, in his experiment, be not at all affected by the contact of the air, or ſhould, on the contrary, be ſet on fire by the air, and blaze? In truth, this is a new field of meditation and enquiry. But I have no time at preſent to enter into it, nor does it fall within the compaſs of this work: for I am now giving an account of experiments, and not debating upon philoſophical queſtions. To return; I muſt obſerve, that in this enquiry concerning animal ſubſtances, I made ſeveral experiments upon milk. For being perſuaded, that white bodies were in a ſingular manner adapted to theſe appearances, I could not help trying experiments upon milk and ſnow, and other very white bodies. But I had no ſucceſs in the firſt two. As to the milk, I aſcribe my ill ſucceſs to it's fluidity: and as to the ſnow, I thought that ſome other qualities, which need not now be mentioned, had diſappointed me: for in reaſoning upon abſtruſe and new ſubjects, we are apt to think in a very confuſed manner. However, whether this conjecture about the milk was well or ill founded, I ſtill continued to make experiments upon it. For I could not help thinking, that there were ſome qualities in milk, which, upon being properly managed, would make it become a phoſphorus. I knew, for inſtance, that it contains ſome very viſcid parts, which is a circumſtance well known to many artificers, who make a very ſtrong and uſeful glue out of cheeſe well waſhed and pounded with quick-lime. I therefore conjectured, that cheeſe well dried, and toaſted, would afford a good phoſphorus: and I found my conjecture to be right.

44. The ſame proceſs, which has ſo much increaſed the number of animal phoſphori, may be applied with the like ſucceſs to vegetables. The gums make the greateſt figure in this reſpect, the gum arabic in particular; after that, the gum tragacanth; myrrh likewiſe affords ſome light, though mixed with a dark hue; which laſt may be owing to the marks of the fire, for it is difficult to avoid them. However, the greateſt care muſt be uſed in applying the fire to prevent any mark of burning, and that the bodies may be dried only. It is uſual for the gums to ſwell upon being expoſed to the fire; which is cauſed by the obſtruction which the incloſed humour meets with from the viſcous and tough covering. For

which

which reaſon a very gentle heat muſt be kept up, ſo that the watery parts may be inſenſibly and eaſily evaporated. By this means the body will be left dry, and as it were cartilaginous, and will have it's phoſphoric qualities in proportion to it's whiteneſs. With the ſame attention we ſhould roaſt all kinds of nuts, pulſes, grains, and flour of wheat, if we would have good phoſphori. Theſe bodies in drying, if they be white, ſhould be brought to a very ſlight yellow; or, if they are naturally yellow, as many are, they muſt then be made of a faint red. This obſervation particularly relates to pulſes, which, if dried in this manner, become brittle, and are eaſily ground. And by theſe marks we may know when bodies are properly prepared. In ſhort, by this ſlight proceſs, I have found very fine phoſphori in almonds, in the fruit of cheſnut trees, beans, vetches, peas, oats, and wheat; in a word, in every kind of grain that I happened to think of: and ſince I have obſerved that all the experiments ſucceeded well, when tried upon all theſe bodies indiſcriminately, I thought proper to give over, for I ſaw there would be no end of the ſubſtances of this kind that are phoſphori. Amongſt other experiments, I muſt not omit thoſe that were made upon that grain that is now ſo faſhionable in Europe. I do this the rather, becauſe coffee, which is what is meant, does not belong to any of the claſſes of vegetables before-mentioned, but clearly a different one. For it is not a ſpecies of the bean, but, according to *Jeſſicu*, is the ſeed of a tree which belongs to the genus of jeſſamines: this ſeed, therefore, when moderately burnt, and not roaſted as it is for drinking, affords a reddiſh dark light, altogether unlike that which true pulſes afford: by which it clearly appears, that the natural conſtitution of bodies has a great ſhare in cauſing this difference of light.

45. I ſhall juſt mention ſome experiments that have been made upon bread. This body holds a conſiderable rank amongſt theſe phoſphori; nor does it require any other proceſs than that whereby it is made bread out of meal or paſte. For the kneading and the concoction, which make it bread, do likewiſe make it a phoſphorus. It ſhines moſt in thoſe places that are leaſt baked, and inclines more to white than red. The crumb of bread, not toaſted, is dark; but, upon being toaſted at a gentle fire, ſo as to be juſt yellow, it becomes bright, and for the moſt part brighter at the extremities and angles than in other places. Obſerve therefore by how eaſy a change meal, which is naturally dark, becomes phoſphoric. But there is another phoſphorus made from the ſame materials by a much ſlighter change; it is not indeed ſo ſplendid as bread, but it is a manifeſt phoſphorus. It is well known that *wafers* are made out of meal diluted with a great deal of water like a liquid glue, and dried upon hot iron plates. It is uſual to colour them with red, otherwiſe they are perfectly white, which ſhews

that

that the meal has ſuffered nothing by the heat of the plates. Theſe wafers are good phoſphori, but that quality is increaſed by heating them, as was before obſerved with reſpect to paper.

46. Hitherto we have treated of ſuch phoſphori as are produced by ſuch a force of fire as ſcarce makes any ſenſible alteration in the bodies. But a greater force of fire muſt be applied, even ſo great, as to diſſolve, and ſet at liberty, one or more of the elements of the body, in order to give this quality to thoſe ſubſtances of which we ſhall now treat. We ſhall mention in the firſt place certain reſins. Theſe muſt firſt part with an acid liquor; and then with that oil, which, on account of it's ſubtility, is called ætherial. After this proceſs there will remain a certain ſolid tranſparent brittle maſs, ſmooth as glaſs, and poſſeſſing the attractive quality of amber. This is called colophonia, upon account of it's likeneſs to the reſin of that name. This is a phoſphorus; but I cannot ſay that all reſins, when treated in this manner, do become phoſphori; ſince I have obſerved it only in the pine reſin, in amber, and in turpentine. But I do not deſpair of bringing many other reſins into this claſs, either by ſome particular proceſs, or perhaps by the proper application of fire. In truth, whenever I have been preparing the above mentioned reſins, I have conſtantly found, that the proper management of the fire was the great point of the work. For there is a certain meaſure in drying and concocting them, of which, if the operation falls ſhort, they

ſtill

ſtill remain dark: and if it be exceeded, they loſe the phoſphoric power. Therefore they muſt be treated with ſo gentle a heat as to concoct them; and not ſo great a one as to make them begin to burn. And I have found by experience that this meaſure may be known by the colour of the oil that iſſues out. For it is at firſt white, and when it begins to turn yellow, we may know that we muſt immediately ſtop; or the proceſs will not ſucceed. For this change of the colour ſhews, that the oleaginous part of the reſin is beginning to be too much burnt. And this agrees with what was before obſerved concerning the heating of other bodies: wherein we ſhewed that the *colour* was the rule to go by in the application of the fire. And perhaps, after all, there may be found out ſome one ſingle method for the preparation of all theſe phoſphori: nay, perhaps there is ſome principle in them, upon which the fire acts in the preparation, and the light, after they become phoſphori. Certainly, in mixed bodies, it is the oleaginous part that is principally concerned in bearing the fire whilſt they are roaſted or dried. But they cannot be dried unleſs the moſt ſubtile parts of them be evaporated; much leſs can they be roaſted. Therefore, if we allow that the turpentine reſin, whilſt we are concocting it, and condenſing it into a colophonia by means of a heat not greater than boiling-water, ſends forth an acid liquor, and a thin oil: if this is the caſe, may we not imagine that

that glutens, leguminous and frumentaceous grains, together with meal, and other bodies of that nature, when roaſted by a heat full as great, do likewiſe ſend forth certain very ſubtile vapours of the ſame kind: and may it not be imagined that the oil, which is left in theſe bodies, is condenſed, and made ſolid, in the manner of a very thin colophonia? Beſides, other arguments might be produced if there were occaſion, or if it had come within the compaſs of this work. But I ſhall reſerve theſe for another opportunity: for I am in hopes that, by ſome proceſs or other, all reſins, and every body of an unctuous nature, may be made phoſphoric. And if I do this, which has in fact already ſucceeded with reſpect to many ſubſtances that are not already phoſphoric, but have become ſuch upon being dried, we ſhall then find that our enquiries upon this ſubject will be much clearer and ſafer.

47. And now we muſt ſpeak of thoſe particulars wherein theſe artificial phoſphori differ from the natural. But we muſt firſt make proper diſtinctions amongſt the artificial phoſphori; otherwiſe we ſhall miſtake for general properties ſuch as relate only to a few of them. Some of them, as we have before obſerved, become phoſphori without the application of fire; as linen and hempen cloths and paper: there are others which have no ſenſible alteration by fire, as wafers: and a third ſort of bodies become phoſphori in conſequence of ſome change produced by fire. And this change either affects

the conſtitution of them, lightly and ſuperficially, or it affects their interior diſpoſition, and entirely deſtroys their conſtitution. The former of theſe happens with reſpect to bodies that are roaſted; and the latter is the caſe of ſuch bodies as are burnt, or, having been firſt diſſolved, after Baldwyn's method, by the force of corroding liquors, are then made very hot. Such bodies therefore as become phoſphori by maceration, or other like cauſe, and not by means of fire, are in every particular the ſame as natural phoſphori. But the phoſphori that are produced by fire have many things quite ſingular. This does not extend to the kind of light to be imbibed; for in this reſpect, and the eaſineſs of being excited, they reſemble natural phoſphori: but theſe particulars conſiſt in the nature of their phoſphoric power, which they derive from the fire, and in the kind of light which they exhibit.

48. With reſpect to the phoſphoric power, we muſt uſe ſome diſtinction in order to explain, and ſet in a proper light, the ſeveral varying circumſtances, which are conſiderable. It muſt be obſerved, in the firſt place, that theſe artificial phoſphori, which are prepared by roaſting, do in time ſo entirely loſe that phoſphoric power which they derived from the fire, as to return to their original darkneſs; this is entirely different from the conſtant and perpetual exiſtence obſerved in the natural phoſphori, which reſiſts all force, and perhaps will ſurvive after the entire diſſolution of

their fabric. But ſuch bodies as become phoſphoric by burning only, or by Baldwyn's proceſs, ſeem to contain a double phoſphoric power: one of theſe is lively, and in many reſpects like that which is found in roaſted bodies; the other is weaker, and not much unlike what we ſee in natural phoſphori. The former of theſe, which immediately after the proceſs was bright and vivid, does not long continue ſo; but, like the roaſted phoſphori, goes off by ſlow degrees, and is at laſt entirely loſt: but the ſecond kind of light, if we may gueſs at the future by what we have already obſerved, will always laſt. The aſhes, or rather the moſt ſubtile earthy particles that remain in the *lixivium* of burnt plants, have been found after many years to be very conſiderable phoſphori. I have likewiſe obſerved, that concretions of ſalts, formed ſpontaneouſly in theſe lixivia, by being kept a good while in a warm place, and which are equal to the famous Glauber ſalts; and likewiſe that the ſalts, which are extracted from aſhes burnt with common ſulphur to a white powder, are very bright phoſphori, after being kept for a great number of years. Therefore the phoſphoric power, ariſing from fire, does in burnt bodies end in a ſmaller degree of power, but ſuch a one, as appears likely to be perpetual; whereas in roaſted bodies, the power is not only leſſened in a much ſhorter time than in the foregoing, but ſeems to be altogether extinguiſhed. But the duration of it is not the ſame in all of them. The gum arabic continued for ſix days at moſt; bread did not laſt one day; and coffee but a few minutes.

49. And as this virtue is ſoon loſt, ſo it may be ſpeedily and eaſily reſtored. It is enough to warm theſe languid, or rather extinct, phoſphori; for there is no occaſion to roaſt them much. By this means the loſt virtue revives. In this reſpect they reſemble the Bologna phoſphorus, and other ſubſtances prepared in the ſame manner. But how often this may be repeated, and the phoſphoric power reſtored, I do not know, for I have not tried it; nor does it appear that it has been tried by any body upon our ſtone, or by Du Fay upon his phoſphori: for which reaſon I hope to be excuſed for the omiſſion of this particular, which is certainly of ſome conſequence: or rather I beg leave to defer it, together with many other particulars, to ſome other opportunity.

50. In the mean time, I muſt mention ſomething which I conſtantly obſerved with reſpect to the phoſphoric power of roaſted ſubſtances. They do not diſcover this quality immediately upon being taken from the fire. You muſt firſt ſuffer ſome part of the heat to go off; the virtue acquired from the fire grows ſtronger as the body grows colder: ſo that heat ſeems to be oppoſite to this quality. In this reſpect likewiſe theſe phoſphori reſemble that of Bologna, which, when hot, does not exert this quality: and even though

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it be not hot, yet, if it be newly prepared, and not uſed to the light, it will perform but indifferently.

51. And theſe are the principal points wherein the phoſphoric power acquired by fire differs from the natural; but the artificial phoſphori, and the natural, differ likewiſe in the appearance of the light itſelf: for the artificial exhibit a brighter light, but which is reddiſh, and ſometimes dark; they likewiſe retain the light ſomewhat longer; but the natural phoſphori have a light that is of a whiter kind, or tinged with a tender yellow, and they loſe their light ſooner. But theſe appearances are not ſo conſtant, nor ſo general, as not to afford a great deal of variety. For there are ſome natural phoſphori that have a very bright and durable light; and by this means firſt occaſion themſelves to be obſerved by the philoſophers. There are others, for inſtance, the lapis lazuli, which afford a reddiſh light; and laſtly, amongſt the artificial phoſphori, the colophonian turpentine (which, according to the nature of this kind of body, produces a light which is rather ſtrong and durable;) does likewiſe preſerve it's phoſphoric quality for many years; which in this ſort of body is ſingular. But there are very few, conſidering to what numerous kinds they belong; and, in ſo great a number, no regard is to be paid to the appearance of a few individuals.

52. Since therefore that phoſphoric power which is naturally produced, or at leaſt not by the force of fire, and likewiſe that phoſphorus

which

which is found in antient calxes, are endued with qualities differing from thoſe that are found in roaſted bodies, or bodies newly burnt; it may hence be eaſily ſuſpected, that there are two ſpecies of this power. This conjecture will be ſtrengthened, if it ſhall appear, that each ſpecies is founded upon certain and definite qualities. And, in truth, the phoſphorus in bodies roaſted or newly burnt, ſeems to be fixed in it's oily, or, as the chymiſts call it, it's ſulphureous principle. For we have already ſhewn, that this principle is more adapted to roaſting than any of it's other; and many bodies newly burnt, make this notion very probable, by the ſulphureous odour exhaled from them. But the phoſphorus of natural bodies, or of bodies that have only undergone *maceration*, or ſuch like change; and likewiſe the phoſphoric power of ancient calxes, if they reſide in any element in particular, may well be ſuppoſed to be fixed in the earthy element. For bodies, which are phoſphori naturally, and without any preparation, do, for the moſt part, upon being burnt, become calxes; and have therefore a great deal of earth and of lime, or rather limiſh matter, in their compoſition. By maceration the fatter parts of mixed bodies are preſſed out; conſequently the earthy parts remain drier, and more ſolid: moſt certainly they are relieved from that matter which principally obſtructs their phoſphoric qualities. Laſtly, in ancient calxes this earth ſeems to remain pure and uncorrupted; and being entirely fixed, and of the moſt unchangeable

able nature, it naturally affords, more than any other body, thoſe conſtant and perpetual appearances that are mentioned before. But it is quite otherwiſe with reſpect to ſalts and ſulphur. Salts very eaſily ſuffer damage by the air, by moiſture, and by the prevalence of oppoſite ſalts; ſulphur, on the other hand, very eaſily frees itſelf from any foreign mixture, and is eaſily diſſipated by any ſlight agitation, being naturally a very light ſubſtance. This being the caſe, it is not at all ſtrange that ſulphur does not continue long in bodies: but whilſt it does continue, the phoſphoric power, which adheres to it, and is by that means viſible in freſh calxes, remains entire and unchanged: but when the ſulphur flies off, and is at laſt deſtroyed by time, then the phoſphoric power is alſo by degrees weakened and conſumed.

53. For theſe reaſons it may be eaſily concluded that there are two ſpecies of the phoſphoric quality: and if we give them their names from thoſe bodies in which they are moſt remarkable, we ſhould call one of them the *phoſphoriſm of earthy bodies or lime-ſtone*; and the other, the *oleaginous or ſulphureous phoſphoriſm*: following in this reſpect the example of electricity, one ſpecies of which is for a like reaſon called *vitreous*, and the other *reſinous*. However, I have a ſuſpicion that both theſe phoſphoric qualities have one and the ſame principle, wherein they both agree, and which is not very complicated. And if I could ſuffer myſelf to

purſue

purſue this train of ſpeculation, I might perhaps imagine, that not only theſe forces, which in different ſubſtances ſeem ſo various, but likewiſe others which appear full as different, might all be properly reſolved into one general and common principle; or that at leaſt there is ſome ſecret connection and agreement amongſt all of them. In this I ſhould imitate ſeveral philoſophers of the greateſt ſagacity, who have entertained ſuſpicions concerning a like connection between many properties, that appear very different, founded upon reaſons not much ſtronger than mine. What, for inſtance, can have leſs connection with the electric power, or with light, than dew? And yet there are philoſophers who have ſuppoſed, becauſe, of all bodies, metals only cannot be made electric nor phoſphoric, nor can be wetted by nocturnal vapours of a ſerene air; that for theſe reaſons the three accidents above named either have one common nature, or at leaſt ſome connection not yet diſcovered. And why ſhould not this ſuſpicion appear probable to me, who have ſeen by experiment that metals cannot be made phoſphoric? But we muſt leave theſe conjectures: eſpecially ſince I have ſome hopes, that, at ſome time or other, they may be made phoſphoric. Perhaps this may be obtained by ſome common and obvious preparation; which, for that very reaſon, has never yet been thought of. And, indeed, I ſuſpect that the preparation of Helmont's flint, mentioned in the beginning, remains at this day unknown, for that reaſon

reaſon only : for who would imagine that by burning only, that ſtone might be made a phoſphorus, which bears the heat of the fire, ſo as to vitrify, rather than be turned into a true calx? And yet a ſtone of this ſort may be burned in the potters furnaces till it becomes white and brittle, and ſo good a phoſphorus, as to catch the light very eagerly, and keep it for a long time. And that no one may be miſled by that appearance of brittleneſs and whiteneſs, it is proper to inform them, that this ſtone, ſo burned, cannot be diſſolved by liquids, or acted upon by corroding liquors: by which it plainly appears, as was juſt now ſaid, that it differs very much from the nature of a true calx. Therefore perhaps metals, by ſome eaſy proceſs, which may at this time be not unknown, may have thoſe qualities which more laborious proceſſes have not yet produced. I have obſerved that the *ſalt*, which is a concretion of *cerus* diſſolved in ſpirit of vinegar, and is uſually called ſalt, or ſugar of *ſaturn*, is a weak phoſphorus, but a very viſible one: but for this purpoſe it muſt be kept dry, and not be ſo old as to have loſt it's tranſparency. To this claſs we muſt refer that orey ſubſtance which is found in ſmiths furnaces, and is commonly called tutty. This body is uſually ranked amongſt metallic ſubſtances, and very properly; for it ariſes from the vapour which many minerals and ſome metals throw out when they are fuſed. Theſe vapours flying upwards, adhere to iron rods fixed at the top of the furnaces:

covering them with incruſtations of an aſh colour, and ſometimes of an azure colour: theſe incruſtations are like the barks of trees, ſmooth and hollow on the inſide, but on the outſide rough and granulated, and with a convex ſurface: ſo that by the form, it is plainly ſeen that they have grown to oblong and round bodies. The tutty therefore, when expoſed to the light, is a ſtrong, red, and laſting phoſphorus. But, what is very wonderful, this quality is confined only to the convex part, which, as we ſaid before, is rough, and does not extend to the concave part, which is ſmooth, or at leaſt not altogether unpoliſhed. I do not mention in this place other refuſe of metals, ſuch as the flowers, as they are called, and the calxes; which, though they have ſome phoſphoric appearances, yet it is uncertain whether theſe are to be aſcribed to the metallic parts, or to ſuch earthy and ſaline mixtures, as theſe concretions are by every body ſuppoſed to contain.

54. I ſhall therefore for the preſent lay aſide all thoughts of making metals phoſphori, and ſhall apply myſelf to conſider that property of the tutty which was juſt now mentioned, namely, that it's rough ſurface is a bright phoſphorus, whilſt the ſmooth and hollow ſurface is for the moſt part dark. This circumſtance would not be very material, if it did not fall in with other obſervations: but, where that happens, it is not ſafe to overlook any thing. I have ſeen ſome ſpecimens of marble that would not be phoſphori, or

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at leaſt would throw off the light to the extremities ſo long as they continued poliſhed; but, upon loſing the brightneſs of the poliſh, became good phoſphori. If, therefore, we may have leave to venture a conjecture, we may ſay, that ſurfaces are either phoſphoric or not, according to their diſpoſition to reflect the light. And if this be the caſe with reſpect to *reflection*, why ſhould it not with reſpect to *refraction* alſo? the reaſon ſeems the ſame, but the queſtion muſt be determined by experiments: I intend to make many for this purpoſe; but, as I have not at this time proper opportunity, I muſt content myſelf with relating one or two that ſeem relative to the preſent purpoſe. I expoſed a glaſs bottle full of pump water to the light, and then removed it into the place of obſervation. I found it, as I expected, quite dark. I then put in a few drops of *oil of tartar*. The water immediately became turbid, and of a whitiſh colour; for this is the nature of ſpring water, which abounds with chalky earth. It was again brought into the place of obſervation. Appearances were then changed; for it exhibited a pale light, ſufficient to diſtinguiſh the form of the bottle. Inſtead of the ſpring water, was ſubſtituted rain water, wherein ſome lapis ſpecularis had been diſſolved: it is the property of this ſtone, that, being powdered and thrown into water, it diſſolves like ſalt, and does not hinder the tranſparency of the water. When the water was ſaturated with this powder, a quantity of *oil of tartar* was again put in, and the event of the experiment was the ſame. Thus we ſee, that earthy corpuſcles, whilſt they continue very ſmall, diſſipated, and perfectly tranſparent, and whilſt their ſurfaces perfectly coincide with the water wherein they float, do eaſily receive the external light, and it as eaſily paſſes through them; they cannot therefore exhibit in the dark that light which they do not retain. But when the corpuſcles, by the infuſion of the ſalt, are driven together in *thicker maſſes*, and are diſturbed by their mutual contact; in this caſe the light does not find ſo free a paſſage through them, but is obliged to ſtop. In the mean time, whilſt the light continues in the bodies, it remains viſible, provided there be no obſtruction. There are ſeveral circumſtances that may obſtruct, and amongſt theſe are colours and metallic mixtures; for if, inſtead of oil of tartar, you throw in *ſal ſaturni*, you will find that the water is become turbid and milky, but not phoſphoric.

55. What has been ſaid concerning the light, namely, that it appears, or does not appear, according as it is reflected or refracted, has indeed a great appearance of truth: but we are prevented from pronouncing the certainty of it with any great confidence, becauſe it depends but upon a few obſervations; and there are many that ſeem to oppoſe it. Gems, cryſtals, and glaſs, always remain dark, whether they continue whole, or be ground into the fineſt powder. Therefore, theſe

bodies do not derive any phoſphoric quality, either from their tranſparency, or from their extreme whiteneſs: by the firſt of which properties, the light paſſes through them with the greateſt eaſe, and by the other they partly retain the light, and partly reflect it in all directions. In like manner ſome of the cleareſt diamonds are ſplendid phoſphori, whilſt others of them are entirely dark. Laſtly, a great number of opaque bodies, either rough, or poliſhed, or ground to powder, are phoſphori, or the contrary. Which makes it manifeſt, that the ſtructure of bodies, both external and internal, does ſometimes aſſiſt their phoſphoric qualities, and ſometimes the contrary: but it has no certain and conſtant connection with the cauſe of their phoſphoriſm. And if ever we might hope to diſcover this cauſe, the preſent ſeems to be the favourable opportunity. For ſince, by the induſtry of myſelf, and many others, it has been found out, that almoſt all bodies poſſeſs that power, which was heretofore believed to be peculiar to one only, and afterwards to a few: it may eaſily happen, that in ſo great a number, and in ſuch a variety of ſpecies, ſome one may diſcover that ſecret which has been kept ſo faithfully by a ſmaller number. And if we ſhould ever come to a ſolution of that queſtion which has been often debated in our Academy, namely, By what means luminous bodies communicate their ſplendor to phoſphori, I do not ſee what bounds can be ſet to the ſpeculations of philoſophers concerning *light*; for if we ſhould determine that the light paſſes from the luminous body into the phoſphori, which is the opinion of ſome, and which agrees with many forms of expreſſion uſed in this book, we ſhall, in this caſe, have ſomething new and wonderful with reſpect to the light. It is an old opinion, that light conſiſts of exceeding ſmall and active corpuſcles, which are thrown off continually, every way, with a vibratory motion from the luminous body, and with the greateſt velocity: but to imagine, that theſe corpuſcles are not preſently diſperſed and diſſolved by the agitation of their own motion: to imagine, that they do not immediately vaniſh: and further, that they exiſt when out of the ſources from whence they ſprung, and that for a conſiderable time: and moreover, that they adhere to bodies, which they meet with in the manner of heat and odors: and laſtly, that they repoſe themſelves in theſe bodies, in a manner inconſiſtent with their ſuppoſed activity: all theſe are new ideas; at leaſt, they are not perfectly underſtood, and certainly have not as yet been clearly explained. If we ſuppoſe, therefore, that the corpuſcles of light are not diſſolved immediately upon their leaving the luminous body, but that they continue for a time, and that they are found in the greateſt numbers round about their original ſource: what more can be required in order to conſtitute that atmoſphere about every luminous body, which has been aſcribed to the

greateſt

greateſt luminary by aſtronomers and philoſophers? And if theſe atmoſpheres do in a great meaſure conſiſt of *light*, we will not then have any further diſpute about them: for the diſputes in the laſt age, with reſpect to the ſun's light, namely, what was the cauſe of it, were very idle. For this would be quite as uſeleſs as to enquire what makes light ſhine. Thus much with reſpect to the light which proceeds from the luminous bodies. But if we turn our thoughts to the bodies that imbibe it, is it not ſtrange, that this phoſphoric quality ſhould be ſo common as to be reckoned amongſt their general properties? for who could imagine or conceive what, and how far, and in what manner, thoſe moſt ſubtile and active corpuſcles act upon the denſer fabric of thoſe bodies which they penetrate, into which they inſinuate themſelves, and wherein they are retained? But how long are they retained? if we conſult our ſenſes, but a very little time: but, if our ſight could diſcern the very ſmalleſt particle of light, which perhaps ſome animals that ſee in the dark are able to do; in that caſe I believe we ſhould be of a different opinion. For as it is a common notion, and a very probable one, that there is no body truly cold; ſo it may be ſaid with equal probability, that no body is entirely dark. But our obſervations for this purpoſe muſt be made in a place that never ſaw the light; and conſequently on this earth, which alternately enjoys light and ſhade, no ſuch dark

place

place can be found. — But if phoſphori do not ſhine by a borrowed light, but by their own; and that their own light is produced only when put in motion, and lighted up by the rays of a luminous body, (which opinion is likewiſe confirmed by ſome experiments,) we have then new doctrines never before thought of, and a great fund added to natural philoſophy. There will then be a certain hidden and ſecret principle in bodies inflammable by this moſt ſubtile fire. We ſhall then diſcover in nature a perpetual fire; or at leaſt that bodies are very frequently taking fire upon being expoſed to the light. This motion will deſerve a conſiderable rank, amongſt thoſe hitherto imagined by the philoſophers, to be ſerviceable in exciting and reſtoring the forces of natural cauſes. There will likewiſe be a perpetual diſſipation of the matter which is thus ſet on fire; and ſince this happens without any ſenſible detriment to the bodies that are burnt, who can comprehend either the ſubtility of this matter, or the means of reſtoring it, which are adapted to ſupport ſo continual a waſte?. But this is not a time to purſue this enquiry, or any of it's conſequences, even in the ſlighteſt manner; for they require more diſquiſition and thought than at preſent can be found. It is enough for me if my obſervations, or ſpeculations, have opened the way to others who may purſue theſe great points.

A FURTHER ACCOUNT OF A GREAT NUMBER OF PHOSPHORI,

LATELY DISCOVERED BY

J. B. BECCARI, Profeſſor of Philoſophy.

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1. IT uſually happens, that in the inveſtigation of new ſubjects, many things at firſt paſs unobſerved, which, when we proceed further, we are ſurprized eſcaped our notice. I am ſenſible of the truth of this obſervation, with reſpect to my own enquiries, concerning the phoſphoric qualities of bodies. For although I uſed as great diligence as I thought was poſſible, yet I was obliged to leave many things which my obſervations did not reach, or which I had not time

to perfect. And yet these matters required no laborious process, nor any singular degree of industry, but only an attention to some few things, and those apparently of no great moment. Moreover, these circumstances did not occur to me until I had published the former part of this work. They came into my mind spontaneously, at least without any labour: insomuch that I wondered, and took it hard, and indeed was almost angry with myself, when I reflected, that I had not before turned my attention, (which is generally ready enough upon those subjects,) to these resources, which now appeared to be quite at hand, and obvious.

2. Being prepared, therefore, by a more accurate method of observing, I once more applied myself to my former studies, which I have continued for several months past, and to this day. For it has always been my opinion, that philosophical enquiries can seldom be neglected without a considerable loss to the student, and never without some detriment to natural knowledge. For, as on the one hand, it is much easier to pursue a work already begun, than to take it up again when intermitted: so on the other, it may be said, that if the first undertaker of any study does not bring it to perfection, it will be long before any other will take it up; which perhaps may never happen. I hope therefore to be excused for returning to my phosphori, after having laid them aside for more than two years: and the rather, because this intermission has been compensated by the addition of an incredible number

of new phoſphori to thoſe mentioned in the former work. For I can now affirm, without arrogance, that, by this induſtry of mine, it appears, that the dominion of light is extended to all known bodies, with a very few exceptions. In my former *memoir*, I declared this new and ſingular property of light, or of the nature of bodies in general, with a great degree of doubt and caution; ſuch as was proper, conſidering that I then could propoſe no more than a conjecture or an opinion. Which conjecture, though ſupported by a very great number of bodies that were phoſphori, was yet made doubtful by a good number of bodies being otherwiſe. But there is now no room for doubt, or fear, ſince we find, by repeated and clear obſervations, that theſe very bodies are in fact phoſphori. I ſhall now deliver theſe obſervations in proper order, and rather fully; ſo as to deſerve the confidence of my readers, and to open to others a ſafe and eaſy way to the ſame obſervations.

3. Since therefore this diſcourſe relates only to thoſe phoſphori, which become ſuch by being expoſed to the light, the ſame rules that were laid down for this purpoſe, in the former work, muſt be obſerved now. For whatever was ſaid there, related only to theſe kind of phoſphori, and the whole apparatus for obſervation had reſpect to them alone.

4. Having diſcovered the ſource of this miſtake, I found it neceſſary to alter my apparatus. Therefore, I fixed, to the upper margin of the window,

two curtains of a thick black cloth: one on the inſide, the other on the out: ſo broad, as to exceed the ſize of the window, at leaſt ſix fingers breadth. Hanging therefore downwards in a looſe manner, and ſticking pretty cloſe to the edges of the window, they effectually excluded the light. In the mean time, the obſerver, lifting up the lower edges of the curtains a ſmall matter, was enabled, upon taking any ſubſtance in his hand, to hold it out in the light at arms length, and taking it in again, to examine it. But in doing this, he muſt ſhut his eyes to prevent their being affected by the light. Drawing back his hand, and upon the curtains falling down, (which two operations would take up but a moment of time,) and being ſecured from the light by the window being cloſed, he might immediately caſt his eyes upon the body, and judge of it's ſplendor, or obſcurity.

5. And in this manner I provided againſt the principal inconveniencies that occured in my former enquiries. But I found by experience, that there were ſome other particulars to be obſerved, the neglect of which would render any judgment concerning theſe phoſphori fallacious, if not impoſſible. And in the firſt place, it muſt be obſerved, that the bodies ſhould be placed in the *cleareſt light*. Now perhaps ſome people may think this a very needleſs caution. For who will not imagine, that the bodies exhibit a light, bright in proportion to the brightneſs of the light

imbibed?

imbibed? And yet ſome will be of a different opinion; eſpecially thoſe who imagine, that ſome phoſphori ſhine brighter in a cloudy day than in a clear one, or even than when expoſed to the rays of the ſun. Nor is this notion deſtitute of probable reaſons. Upon which account I think it proper to declare, that I do not find that obſervation to hold true with reſpect to my phoſphori, although it may be true with regard to the Bologna ſtone, which is the principal ſubject to which that obſervation is applied. And if we admit, that theſe bodies differ in their phoſphoric quality from that ſtone, ſuch difference may be properly aſcribed to their being of a different nature. Our phoſphori are naturally ſuch; whereas the Bologna ſtone, and other ſubſtances of a like nature, derive that quality from art, and the application of violent heat. As to theſe *natural phoſphori*, I could never find that the *light of the ſun* was at all prejudicial to them. So far from it, that many of them are of a nature ſo difficult, as to perceive the leaſt differences in the light of the ſun; and will not ſhine, unleſs they be expoſed to a light quite clear from every degree of *vapour*. There are likewiſe ſeveral that will not ſhine even by the brighteſt ſun-ſhine; but continue dark until they are expoſed to the rays of the ſun collected by a lens.

6. It is material not only to conſider in what light the bodies muſt be placed, but likewiſe in what ſituation. For there are ſome that ſhine

better

better in the ſame light, when placed in one poſi-tion, than when in another. But it is difficult to lay down any certain and general rule in this mat-ter; becauſe the ſurface and the figure of the body, and the different nature of the component parts, and perhaps many other circumſtances, may make conſiderable differences. It is difficult to foreſee theſe conſequences ſeparately; but to provide againſt all of them together, is quite impoſſible. In general, however, that poſition is the beſt, wherein the greateſt quantity of light is tranſ-mitted from the luminous body to the phoſphorus. More light will fall upon a rough ſurface than upon a poliſhed one; for, according to the laws of optics, the latter will reflect a great part of the approaching light before it touches the body. Likewiſe, if the ſurface be uneven by hollows and riſings, care muſt be taken, as far as poſſible, that every part be equally enlightened. For it often happens that the riſings project a ſhade upon the lower parts, ſo as great part of the body will remain dark, even when expoſed to the cleareſt light. For which reaſon, in theſe circumſtances, the whole body will be in appearance dark, eſpecially if it be a weak phoſphorus. For a faint light, if it be broken and diſperſed, will ſcarcely ſtrike the obſerver's eye, and will rather exhibit an appearance of darkneſs than of light. —It likewiſe often happens, that experiments are made upon ſubſtances, whoſe parts differ very much from each other in their nature and qualities: in in which caſe it is evident, that the moſt phoſphoric parts ſhould be turned to the light; that is, the white rather than the dark, the dry rather than the wet, and the opaque rather than the tranſparent: but becauſe it ſometimes happens, that theſe differences are not known at the time of making the experiment, it will be proper to keep changing the body, during the experiment, into different ſituations, in order to diſcover the moſt phoſphoric part of it. This rule is frequently uſeful with reſpect even to bodies that ſeem to be of an uniform nature: I experienced this in a drinking glaſs of a conical figure, which was expoſed to the light, in ſuch a manner, as to have it's axis parallel to the horizon, and in that ſituation exhibited a tract of light, which was likewiſe parallel to the horizon; though, in every other poſition, the glaſs remained quite dark.

7. It is proper likewiſe, that the obſerver ſhould be very exact in turning the enlightened parts to his eye, and not the dark ones. It may ſeem very eaſy to be exact in this point, but in experience it will be found much otherwiſe: for the hands, which in our uſual motions are directed by the ſight, when they are deprived of that aſſiſtance in this great darkneſs, are very often deceived.

8. But there are other accidents, whereby the ſituation of the body is ſo far changed, that the enlightened parts are not expoſed to the obſerver's eye. This happens with reſpect to bodies that

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are broken into ſmall pieces, or powdered. Theſe cannot be brought into the place of obſervation, eſpecially if a quick motion is uſed, without being ſhaken. In which caſe it happens, that the faces of many particles are turned round, and there is a confuſion amongſt them : ſo that the enlightened particles loſe all their ſplendor, being covered by the dark ones. For the ſame reaſon it is very difficult to make experiments upon liquids. Theſe muſt be confined, ſo as to prevent them from flowing, as much as poſſible. This may be done by putting them in bottles of the cleareſt glaſs, which being quite filled, will keep the liquor from being ſhaken or agitated. The light, which paſſes through the glaſs, will be ſufficient to excite the phoſphoric quality in the liquid, provided the glaſs be clear and thin : and when the light is once imbibed by the liquor, enough of it will remain for obſervation, as there will be no motion of the parts. Powders muſt be placed in very open veſſels, and then compreſſed in ſuch a manner as to make the particles cohere. By this means they will preſerve that poſition wherein they received the light, and will exhibit the ſame light entire to the obſerver.

9. Laſtly, it muſt be obſerved, that it is better to make experiments upon large bodies than upon ſmall ones : and if the light be faint, it is abſolutely neceſſary that the body ſhould be large. For it is plain, that a faint light, when diffuſed over a large ſurface, may ſtrike the eye, but not ſo when

when the ſurface is ſmall. Therefore where there can be a choice, it is always beſt to prefer large maſſes. And in ſeveral experiments where I have not been able to find any light at all upon ſmall pieces of bodies, I have yet found that larger pieces were ſtrong phoſphori.

10. But we have not always an opportunity of trying large bodies, either becauſe there are no large ones of this kind, as in the caſe of gems, or becauſe large ones are not to be procured. We muſt, in that caſe, procure that magnitude by artificial means which nature or opportunity have denied: either by faſtening together many minute corpuſcles of the ſame body, and diſpoſing them in as large a ſurface as poſſible, upon a ſtand of black wax; or, if we cannot procure a number, we muſt powder, ſuch as we have, very fine, and ſtrew the duſt upon a like ſtand. By this means I have obſerved the phoſphoric power of ſome exceeding ſmall gems, which, when viewed ſeparately, appeared dark: and I have no doubt, but that thoſe diamonds, in which no light has yet been diſcovered, would become phoſphori, if treated in that manner.

11. And theſe are the chief points which muſt be attended to by ſuch as chooſe to repeat theſe experiments; in relating of which, though I may have been ſomewhat long, yet I have ſaid nothing ſuperfluous, unleſs an accurate deſcription of the proper means for conducting philoſophical en-

quiries ſhould be called ſuperfluous. For ignorance of the particulars juſt now related has concealed, even to this time, theſe extraordinary phoſphoric appearances, not only from very ſagacious obſervers, but almoſt from myſelf, who not only ſuſpected the truth, but was labouring this particular point with the cloſeſt attention. And in truth a vaſt number of bodies have been pronounced by me to be dark, which by this more accurate proceeding I have found to be phoſphoric. And now I ſhall proceed to enumerate theſe bodies: in which undertaking I hope not to be again accuſed of ſuperfluity, as if this particular enumeration could give no light to the general aſſertion in the beginning of this paper: namely, that *all bodies*, except a very few, were phoſphori. For although this quality be common to all bodies, yet each kind of body has ſome ſingularity in this reſpect, which is worthy of notice. And the obſervation of theſe ſingularities gives a probability to the general aſſertion.

12. In this new catalogue we ſhall follow the ſame order that was obſerved in the former work, and ſhall divide our new phoſphori into three claſſes, of *foſſils*, *vegetables*, and *animals*. In that work we gave the firſt rank amongſt the foſſils to *earths*, of which we ſaid, that a great many were phoſphoric, but that ſome were dark. It was obſerved too that though no certain criterion of their ſplendor, or their darkneſs, could be derived from their qualities, eſpecially from their colour,

yet

yet I added, that of all colours, white, and the colours approaching to it, were the moſt phoſphoric. But now, ſince my late obſervations, I affirm that all earths are phoſphoric, without excepting even the black or the red. For I have ſeen undoubted phoſphoric appearances, though faint ones, in thoſe black earths uſed by our painters: eſpecially at the *edges and angles*, which laſt is a circumſtance common to many bodies. The *vicetina fullonia*, which was mentioned in the former work, afforded a brighter appearance, though it's light was far from being proportionable to it's whiteneſs: ſo that it is not ſtrange that in giving inſtances of white bodies that were not phoſphori, I ſhould in the former work mention this ſubſtance. For, even at this time, I was very near pronouncing the yellow earth, which our painters procure from the Campania Romana, to be no phoſphorus: ſo obſtinate was it in refuſing even the ſun's light. But at laſt it exhibited phoſphoric appearances, upon receiving the ſun's rays through a lens.

13. With reſpect to ſands, which I placed next to the earths, I have ſcarce any thing to add to the former obſervations. For I aſcribed to moſt of them theſe qualities, provided the colour did not obſtruct. I aſſerted in the former work, that the ocre which adheres to the yellow ſands, that are found in the hills near this city, hinder their phoſphoric appearances: but I now find that theſe ſands are phoſphori, even though the ocre be not

washed off. I was likewise mistaken in asserting that those spangles of a golden colour, and which partake of the nature of talcs, and which are mixed with the same sand, were dark: for I find them to be phosphori.

14. Amongst the larger stones, marbles deserve the principal place, since no stones are larger: and besides, marbles exceed all, in their polish and brightness. They likewise excel as phosphori. The softer and the whiter marbles shine the most. But the hard and the dark ones are not entirely dark, though I formerly doubted upon this point. I find that porphyry, ophites, and granate, become phosphori by the sun's light; but more so by the application of a lens: though in the former work I mentioned these as uncertain. Besides the basalties, a kind of Æthiopian marble, which in hardness and colour resembles iron, may be made to shine by means of a lens. To these may be added a kind of Ægyptian green marble, and likewise the antique green marble, together with the oriental green marble, called della stella: though, when I wrote the former part, I thought these marbles entirely dark, or at least that they gave a very doubtful light, and that at the *edges only*.

15. In the former part we distributed the stones of small bulk into their genera, and described one class of them as being not so hard as marble, and the other as being harder than any marble. We then subdivided the first of these classes into three new divisions. I have nothing to add to the first

division,

division, namely, that which comprehends the ſtones which are altogether deſtitute of any regular form. In the ſecond diviſion we place thoſe ſtones which contained a certain regular internal formation with an irregular outſide: and gave for inſtances the amiantus, ſeveral ſpecies of talc, and a certain ætites, from Nani: all theſe bodies were there called obſcure. But later obſervations have diſcovered a manifeſt light, not only in theſe but in others of the ſame claſs: eſpecially in a certain very hard oriental ætites, and even in the paper made from the amiantus. The laſt claſs, conſiſting of ſmall and ſoft ſtones, having regular formations, both internal and external, I, at that time, conſidered as abounding with phoſphori. But having then ſpoken of certain ſpeckled and ſtarlike (ſtones) rather doubtfully, I take this opportunity of declaring them to be very manifeſt phoſphori.

16. Next follow ſuch ſtones as are harder than marble: I found few of theſe that, upon the firſt trial, would ſhine in the dark: certainly much fewer than their brilliancy, when expoſed to the day-light, ſeems to promiſe. But upon further trial I found that they were all phoſphori; and would ſhine if properly aſſiſted. The aſſiſtance of a burning-glaſs was found the moſt certain and efficacious; but ſeveral, eſpecially ſuch as conſiſted of minute corpuſcles, required to have many fragments of the ſame body joined together: or to have their powder, or ſmall duſt,

ſpread

ſpread upon black wax, ſo as to make a *large ſurface* in the manner before-mentioned. In the former work I divided this kind of ſtones into the opaque, the tranſparent, and thoſe of a middle nature: and going through all theſe diviſions, I, at that time, admitted only the lapis lazuli into the number of phoſphori, excluding all the reſt, and particularly the whole family of jaſpers, and the malachites. But I now find, that all theſe ſtones are phoſphori. For the malachites, and likewiſe the lapis nephriticus, are manifeſt phoſphori: and that jaſpers, whether they be red or of a dark green, or be mixed with many different colours, ſhine when expoſed to the lens; and many of them by the light only of the ſun, eſpecially ſuch of them as are ſtreaked with white veins.

17. In the other claſs of precious ſtones, namely, that between the opaque and the tranſparent ones, upon examining one hundred agates, I found only one which could not be made to ſhine by the lens. This ſtone was of many dark colours, ſo that I aſcribed it's obſcurity rather to the colour, than to the nature of the ſtone. Moreover, I examined in this claſs, opals, onyxes, and carncoli, which I had omitted in the former trials, and found them all to ſhine by the ſimple light of the ſun.

18. The laſt claſs containing cryſtals, and all the tranſparent gems, was much more favourable to theſe experiments than I expected. They had formerly

formerly withſtood many attempts of Du Fay, and of my own, though I have been generally pretty lucky upon ſuch occaſions. But I accompliſhed in theſe laſt experiments what I could not in the firſt. Many of the cryſtals ſhone by the ſun's rays only, and ſome of the more difficult ſhone by means of a lens: but all of them were made to ſhine by means of the day-light alone, after they had undergone an eaſy proceſs. I rubbed off from them, ſome very fine, and white powder, by ſtriking them one againſt another. This powder was then rubbed (by the cryſtals) between each other, and eſpecially in thoſe places of the cryſtals that were rough and unpoliſhed: in conſequence of which, the cryſtals became phoſphori in the places ſo rubbed. The ſame proceſs ſucceeded likewiſe with glaſs, by which means this ſubſtance, which before was thought obſcure, became phoſphoric. With reſpect to gems, they all ſhine when expoſed to the lens, except a very few. The eaſieſt of all are the emeralds, many of which ſhine by the ſimple light of the ſun. After the emeralds come the ſapphires, eſpecially thoſe of the whiter kind: the cryſolites are almoſt as good as theſe: then follow the amethiſts and topazes; next the rubies, eſpecially the paler ones; and laſtly the granates and hyacinths. The laſt mentioned would ſcarce ſhine at all when examined ſeparately, but were much eaſier when taken in numbers; but in all ſituations they are the moſt difficult of gems; inſomuch, that having taken ſome granates of a fine colour, high polish, and great size, together with a certain gem of the hardness and colour of a ruby, and likewise two beautiful hyacinths; I could not make any of these bodies shine when examined separately. But I ascribe this, and I think justly, to the nature of the colour; for I have found, by a good many observations, that red, amongst other colours, is very opposite to all phosphoric appearances.

19. After the gems, I considered metals, following that division which *Woodward* had formerly made amongst the fossils. Du Fay formerly tried many experiments upon metals; and I have made not a few. Indeed I have never desisted from trying every means I could devise to make them phosphoric, but never with success. It is surely a hard and stubborn kind of body, since it rejects light and electricity, and even dew. These metals constitute that small number of bodies which appear to me to be the only ones in nature that are not phosphori. For the darkness of all others may be properly ascribed to some accidental and foreign cause, and not to their nature. But in metals there is no such cause yet discovered, to which we can attribute their darkness.

20. Whatever has any affinity to metals, I formerly thought, partook of the same obscurity: and I find this observation true, with respect to cinnabar, zink, and mundick; but it does not hold with respect to the lapis calaminaris, which I now find to be phosphori, as are likewise some other fossils

fossils of the same kind. For instance, I find that some loadstones exhibit a very faint light, which is diffused through their whole substance: and likewise, that a certain mineral iron exhibits a scattered light. Magnetia is a phosphorus: so is the smiris, the bloodstone, and the armenus; especially with a lens: I had another stone of the colour and weight of iron, which appeared phosphoric by the light of the sun only, at the sides, which were uneven. Therefore these bodies, and a good many others, which are not named here, though they have a mixture of metal in them, and though their colour is black or dark, yet they are found to exhibit a light, which, though very weak, may yet be perceived by a very nice observer.

21. Following Woodward's method, I assigned to earthy juices the last place in the class of fossils: and, still following the same author, I divided them into salt juices and fat ones; and admitted almost all salts into the number of phosphori: excluding those only, which had some mixture of metal in them: to which I was induced principally by an experiment made upon vitriol. For I found, that every kind of vitriol, however transparent, was always dark; but afterwards, upon repeating the experiment more accurately, I discovered some traces of a faint light upon a crystalline lump of this sort. This light was more diffused, but still as faint, in some vitriol, that was powdered and pressed down upon

a plain ſurface: but the light was pretty conſiderable in that white calx into which vitriol is reduced, when expoſed to a hot ſun. By the application of a lens, the light, which appeared in the powdered vitriol weak, and almoſt uncertain, was made quite manifeſt, and clear of doubt; and, by means of the lens, the light in the vitriolous calx was increaſed three-fold.

22. In the former work I was very much miſtaken in regard to the fat juices; for I did not admit one of them to be phoſphori: but at preſent I ſhall do them more juſtice. I have found, that at leaſt amber, and likewiſe ſulphur, as well native as fuſed, are phoſphori. But jet and pit-coal are not. But theſe bodies are ſo black as to caſt a darkneſs upon the brighteſt body: and therefore it is not to be wondered at, that in the greateſt light they have no luminous appearance.

23. Thus far of foſſils. We ſhall now ſpeak of vegetables. In my firſt experiments I imagined that plants were the leaſt phoſphoric of bodies: not that they were without this property, but that it wanted the proper force and vigour: inaſmuch as it was oppreſſed and overcome by being connected with ſome principle that was oppoſite to phoſphoriſm. And it was eaſy for me to frame a conjecture as to this principle, from the experiments which I made upon the wood and fibres, whereof all plants conſiſt; as well as upon artificial phoſphori, which are in a great meaſure made out

of

of vegetables. For wood and fibres being of a drier compoſition than other parts of plants, are therefore more phoſphoric: the whole preparation of ſuch phoſphori conſiſting only in drying them properly: from whence it was eaſy to conceive that plants have an innate phoſphoric quality, which is oppreſſed by the redundancy of moiſture wherewith they abound; and which quality would exert itſelf upon the moiſture being removed. But I am not fond of conjectures, though experiments almoſt oblige me to give way to them: and, I muſt ſay, that I can find nothing in plants which may not be made phoſphoric, provided all the humidity be exhaled. In moſt plants nothing more is required than to be dried in the wind: others will not ſhine unleſs they be firſt heated. In this number are ſome plants of the bull-ruſh kind, and amongſt theſe the tipha. But they all have their ſplendor improved by the application of fire, ſo that it be very moderate, and not to mark them. I could find but one plant which would not ſhine, even when fire was applied, and that is the red beet.

24. But this quality is not ſo entirely ſubdued by humidity as not to exert itſelf in ſome meaſure, even in plants that are in full verdure, and full of juice. Moſt of them will ſhine by a very clear ſun-ſhine: but the obſerver muſt be prepared in the manner mentioned in the former part of this diſcourſe. In that ſituation he will find that almoſt all roots ſhine, provided the colour does not ob-

ſtruct. But the principal ſplendor will be in the ſkin; for the middle will appear dark if it conſiſts of a ſoftiſh pulp. But it will ſhine if the juice be ſqueezed out. Stalks and barks follow partly the nature of the wood, and partly the nature of the boughs, according as their texture inclines to one more than the other. But leaves for the moſt part become ſplendid by the ſun-ſhine. And of all leaves, ſuch as are greyiſh, hairy, and of a dryiſh nature, are the moſt phoſphoric: and ſo is that ſide of the leaf that is next the ground. If theſe bodies do not ſhine by the ſun's rays, they always will by a lens: which is likewiſe of great ſervice in making flowers ſhine: for many of theſe, although their colour be not very dark, are yet more hard to make ſhine even than leaves. Fruits that are even ſoft and juicy are not very difficult; but dried ones are much eaſier.

25. The kernels of fruit, though not dried, and much leſs when roaſted, are good phoſphori. Theſe methods do indeed improve their light, but they will ſhine without them. In this particular I have changed my opinion very much: for I formerly thought that not only fruit and kernels, but that meal and the whiteſt ſtarch, and almoſt all ſeeds, were not naturally phoſphori; but would become ſuch artificially, that is, by moderate roaſting. But as I had not then my preſent accuracy of obſervation, and did not know the effect of a humid air in deſtroying theſe powers, I hope this miſtake will appear pardonable. Things appeared

peared to me quite different, when I returned to theſe obſervations this laſt time, which was in hot weather, a clear ſky, and a dry air: all which circumſtances were entirely different from thoſe that attended the firſt experiments. Therefore I now ſay, that nuts of all kinds, grains of wheat, pulſes, and in particular *peaſe* and *tares*, ſhine very brightly without any preparation; the two laſt mentioned are brighter than moſt other phoſphori.

26. I found that moſt vegetable juices were pretty ſtrong phoſphori: and that all ſhone a little. I formerly reckoned ſugar as a principal ſalt. Manna and honey are juſt as much inferior to ſugar in brightneſs as they are in drineſs. Honey ſcarce ſhines at all, unleſs it be made hard, in ſome meaſure by cold, and ſo as to exhibit the appearance of grains. Gums, in which the whole genus of vegetable juices, (that are not already mentioned,) conſiſt, are very good phoſphori. Gum arabic, and likewiſe ſome drops of plum-tree gum, appeared very ſplendid: ſo will the tragacanth, provided it's texture be not looſened by humidity; and, if it is, it muſt be dried.

27. The laſt genus, which is that of the fat juices, is phoſphoric, though not in ſo great a degree as the other two. All reſinous diſtillations ſhine more or leſs: thoſe that are clear, white, and dry, are the beſt phoſphori; and the worſt are thoſe of an oppoſite nature. The reſin called benzoe could hardly be moved to ſhine by the lens: the ſame was obſerved of the turpentine reſin.

This

This laſt would not ſhine whilſt in a liquid form; but ſhone a very little when hardened by cold.

28. We muſt reckon oils amongſt the fat juices. And if experiments made upon two ſorts of oil will warrant us to conjecture as to other ſorts, we may ſay that oils, whether they be preſſed from fruits, kernels, or ſeeds, or be diſtilled by a chymical fire, do all of them exhibit phoſphoric appearances. One of theſe oils, was oil preſſed from olives: the other was diſtilled from anniſeed. Both of them ſhone by the ſun's rays only, but not till they were froze. From whence it might eaſily be ſuſpected that other oils would likewiſe ſhine in the ſame circumſtances.

29. At length we ſhall turn our diſcourſe to animals. It was ſaid in the former part, that animals were as good phoſphori as foſſils or vegetables: but that this quality was confined to thoſe parts of animals which conſiſted entirely of an earthy principle, hardened into ſolidity. And that in other parts it was, by the mixture of fat, extinguiſhed; or at leaſt greatly weakened. But I find by my preſent experiments, that although this power be weakened by the oleaginous principle, yet it is not entirely extinguiſhed. For which reaſon thoſe parts of bodies, which I have formerly called dark, are to be reckoned only as leſs ſplendid, and not dark. By which means, whatever diſagreement may appear between the former obſervations and the preſent, will be eaſily reconciled. All parts of animals, therefore, do admit the light. Some with eaſe, others

others with difficulty. Of the former ſort are ſuch parts as ſeem to grow out of bodies: as horns, hoofs, and hairs of quadrupeds; the feathers, beaks, and talons of birds: but the ſcales of fiſh, are faint phoſphori: though the ſhells of certain ſhell-fiſh are bright ones, as was obſerved before. I formerly imagined that the ſolenes, and the fins of fiſh, and the unguis odoratus, did not ſhine: but they are phoſphori, though faint ones. The hook of an elk will ſhine, if the rays paſs through a lens, and fall upon the brighteſt parts of it.

30. Upon taking off the hairs and feathers of land animals, and birds, the epidermis is obſerved to be phoſphoric. The hides of quadrupeds, as well raw as dreſt, and even though they be of deep colours, are phoſphori. So is the ſkin of birds, eſpecially in thoſe places that have leaſt fat, or are lifted up by the bones: for in thoſe places the ſkin is moſt lean. For it is a general rule, that all membranous and nervous parts, or rather whatever approaches to that glutinous nature, wherewith theſe animals, when living, abound, is very favourable to phoſphoric appearances. Inſtances may be given in teeth, bones, and horns, that are boiled up and reduced into the form of ice: likewiſe in cakes of portable meat ſoup: alſo in the concretion formed by the cerum of the blood, upon evaporating the aqueous part: and, laſtly, in that tough and yellowiſh coat that grows upon the blood; which is taken from patients in high inflammatory fe-

vers: for all theſe bodies, without the application of fire, are pretty ſtrong phoſphori. Though it is true, that this quality is improved by fire, yet it exiſts without it. If I had known this when I recommended the roaſting of theſe ſubſtances, I ſhould certainly have mentioned it, leſt it might have been imagined, that becauſe I propoſed artificial means, that therefore theſe bodies were not naturally phoſphori.

31. The fleſh of animals ſeems to be the leaſt phoſphoric of all their parts. For which reaſon, in my firſt paper, I did not reckon any of them phoſphori, except white meats and thoſe roaſted. For I doubted ſo much with reſpect to brown meats, which colour I knew to be oppoſite to phoſphoriſm, that I paid no attention to them. And if I had obſerved them, as my apparatus was then ſo imperfect, I believe I could have made nothing of them: ſo languid and tranſient is even that light which they receive from the ſtrongeſt ſun-beams. It is otherwiſe as to bones. But in this reſpect I have nothing to add to what was before written. It is now ſhewn, that all the ſolid parts of animals are phoſphori. And if any of them ſhine with difficulty, this may be leſſened by drying them properly, as was before obſerved of vegetables.

32. With reſpect to animal juices, I ſhould imagine that they are all phoſphoric, ſince I have at length diſcovered that milk and fat are ſo, though I had formerly been of opinion, that milk

was

was obſcure, at leaſt ſo long as it continued fluid: and that fat extinguiſhed the phoſphoric qualities of bodies, or at leaſt weakened them. Whoever chooſes to try experiments upon milk, muſt make uſe of thoſe cautions before laid down, with reſpect to experiments upon liquids in general: and with reſpect to animal fat, I ſhall content myſelf with the obſervations juſt now made upon vegetable oils; that is to ſay, they muſt be hardened by the cold: for if they receive the leaſt moiſture, their ſplendor will be entirely loſt.

33. Having found, by the foregoing obſervations, and many others, (which, to avoid prolixity, I have not mentioned) that this phoſphoric quality was ſo widely diffuſed amongſt animals, I began to conſider, whether living animals were endued with that power which I had ſeen in the different parts of dead ones. For I could ſee no reaſon why Nature ſhould have denied this quality to living bodies, ſeeing that ſhe had beſtowed it upon ſuch as wanted ſenſe and life. But this imagination of mine, though it was ſupported with probability and analogy, yet it had not any clear and certain experiment to authorize it. Moreover, no ſuch obſervation could be made without changing my firſt method of obſerving, and making myſelf the object of my own experiment. I had ſubſtituted the double curtain in the place of my cylindrical tympanum, as before mentioned. By means of this contrivance, it was in my power to thruſt out my naked hand, or

arm, and to bring it back into the place of obſervation at pleaſure. It will be eaſily underſtood, that, in the common courſe of my experiments, I muſt have ſeen any ſplendor that might happen to remain upon my hand.

34. And in truth, when I firſt began to uſe my new apparatus, I ſaw, to my apprehenſion, ſome faint traces of light upon my fingers, eſpecially at the ſides and ends. I was pleaſed with the novelty of the appearance, but not ſo entirely poſſeſſed with it as to give immediate credit to my ſenſes. For which reaſon I carefully repeated the experiment, by expoſing ſometimes one part of my hand, and ſometimes another, to the light, by obſerving it in the dark with the utmoſt care, in order to diſcover whether the light would follow the various bendings of my fingers, or the ſhaking of the whole hand, by a conſtant adheſion to the ſame parts: in ſhort, by comparing together, with the utmoſt exactneſs, the effect of all my obſervations; this diligence was very ſucceſsful, and I found my firſt conjecture confirmed.

35. But although the exiſtence of this light was quite certain, yet I found myſelf involved in a new queſtion; namely, whether the light reſided in the ſkin, or in ſome ſubſtance upon the ſkin; for I well knew that bodies, which were of themſelves dark, will become phoſphori, upon being rubbed with a very ſmall quantity of duſt; and will return to their original darkneſs, upon the

the duſt being wiped off. And I well remember, that I had been very near determining, that ſome metallic maſſes were phoſphori, when I was miſled by ſuch an appearance. I knew too that the hands very eaſily contract dirt, by handling bodies. And although the ſkin be not made foul by foreign ſubſtances, yet it has in it's own texture, even in the niceſt perſon, a principle, by which it acquires filth. For it naturally becomes greaſy, by the perpetual effuſion of certain fat matter, which ſometimes becomes dry, and aſſumes the form of ſcurf; which laſt ſubſtance I had before found to be phoſphoric. This made me ſuſpect, that this fat ſubſtance might ſhine when it lay upon the ſkin in a ſebatious form, although it was imperceptible to the ſight; and conſequently, that, if the human ſkin does ſhine, it is not by it's own light, but by that of another ſubſtance. This ſuſpicion was ſtrengthened, by the entire extinction of the light, as often as the fingers, or the whole hand, were dipped in warm water; and, on the other ſide, this ſuſpicion was leſſened, by conſidering the exquiſite cleanneſs of the hand before ſuch waſhing; and conſidering further, that this ſplendor was ſo vivid, and ſo widely diffuſed, that it ſeemed impoſſible to aſcribe it to ſo minute a quantity of dirt as was imperceptible to the ſight.

36. When I was in this uncertainty, and in great doubt, occaſioned by the arguments I have juſt now mentioned, it happened, about the mid-

dle of December laſt year, that my hand was obſerved to ſhine with an unuſual ſplendor; although, in the judgment of another obſerver, who was preſent, it appeared to be all over as clean as poſſible. I waſhed it ſeveral times; I wiped it, and expoſed it again to the ſun; and found it not at all, or, a very little impaired in it's brightneſs, which it retained even when wet. Both wet and dry it was often expoſed to the lens; on the back; on the hollow, which laſt had generally appeared dark; on the ball of the thumb; and, laſtly, on the tops of the fingers. All thoſe places ſhone very much, eſpecially the tops of the fingers, and of the thumb in particular: and it was obſerved that the tops of the fingers, and ſome other parts of the hand, ſhone brighter for being preſſed cloſe. From which it was inferred, that the differences of the light, mentioned before, follow the different ſtates of the ſkin, and do not depend upon the accretion of any foreign ſubſtance: and certainly ſometimes the whole light was extinguiſhed by the ſimple immerſion of the hand in warm water; which was ſo ſlight, that it could only ſoften the hand, but not waſh off any dirt that might be upon it; and upon many occaſions it has been obſerved, that ſoftneſs is oppoſite to the phoſphoric quality, and that drineſs is favourable to it.

37. But at length a favourable ſeaſon cleared up this doubt, which my greateſt induſtry had

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ſerved only to increaſe and confirm. For in the beginning of laſt January I returned to theſe experiments, and obſerved, that my whole hand, and the ſurface of my arm, were brighter than they had ever been before. For the places, that had ſhone before, were now brighter; and ſuch places as had been dark, now exhibited a conſiderable light. No external cauſe could be diſcovered. The hand and arm were perfectly clean. The cauſe therefore muſt be in the ſkin. But, upon the moſt diligent conſideration, there could be nothing unuſual that could affect the ſkin, except what aroſe from the extraordinary change which had happened in the weather: for, at the time of theſe laſt experiments, there was a moſt ſevere froſt; and every body knows, that the ſkin becomes rough, hard, pallid, and lean, in froſty weather; and it is as well known, that bodies with theſe qualities are diſpoſed to be phoſphoric. And thus this doubt was cleared, and the human ſkin proved upon good grounds to be phoſphoric.

38. And now we have gone through the ſeveral kingdoms of animals, vegetables, and foſſils of the viſible earth: and having, by this means, ſhewn the general prevalence of phoſphoric appearances in all bodies, there ſeems to be a natural end of what was at firſt propoſed. For I never propoſed more in theſe enquiries than to ſearch, as far as I was able, into theſe wonderful natural appearances; leaving their hidden cauſes to be diſcovered by per-

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ſons of better abilities. This laſt is the province of parts and ingenuity, in which reſpects I am ſenſible of my own weakneſs. Though I pretend to ſuch merit as may ariſe from induſtry and diligence. Neverthelesſs, in order to be as uſeful to philoſophers as may be, I intend to contract the obſervations, hitherto made, into a few general heads; to point out their connections, and their conſequences; to propoſe ſome conjectures, if I find room for them; to move ſome queſtions, with a view of inducing others to ſpeculate upon theſe points: and laſtly, I ſhall inſert ſome new obſervations in order to ſtrengthen the foregoing, or to open the way to new ideas. And having gone through theſe points with all the diligence in our power, we ſhall put an end to this whole enquiry.

F I N I S.

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