

A series of experiments relating to phosphori and the prismatic colours they are found to exhibit in the dark / by B. Wilson ... ; Together with a translation of two memoirs, from the Bologna Acts [1744 & 1747], upon the same subject, by J.B. Beccari.

Contributors

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Beccari, Jacopo Bartolomeo, 1682-1766. De quamplurimis phosphoris nunc primum detectis.

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Wilson
on
Phosphorus

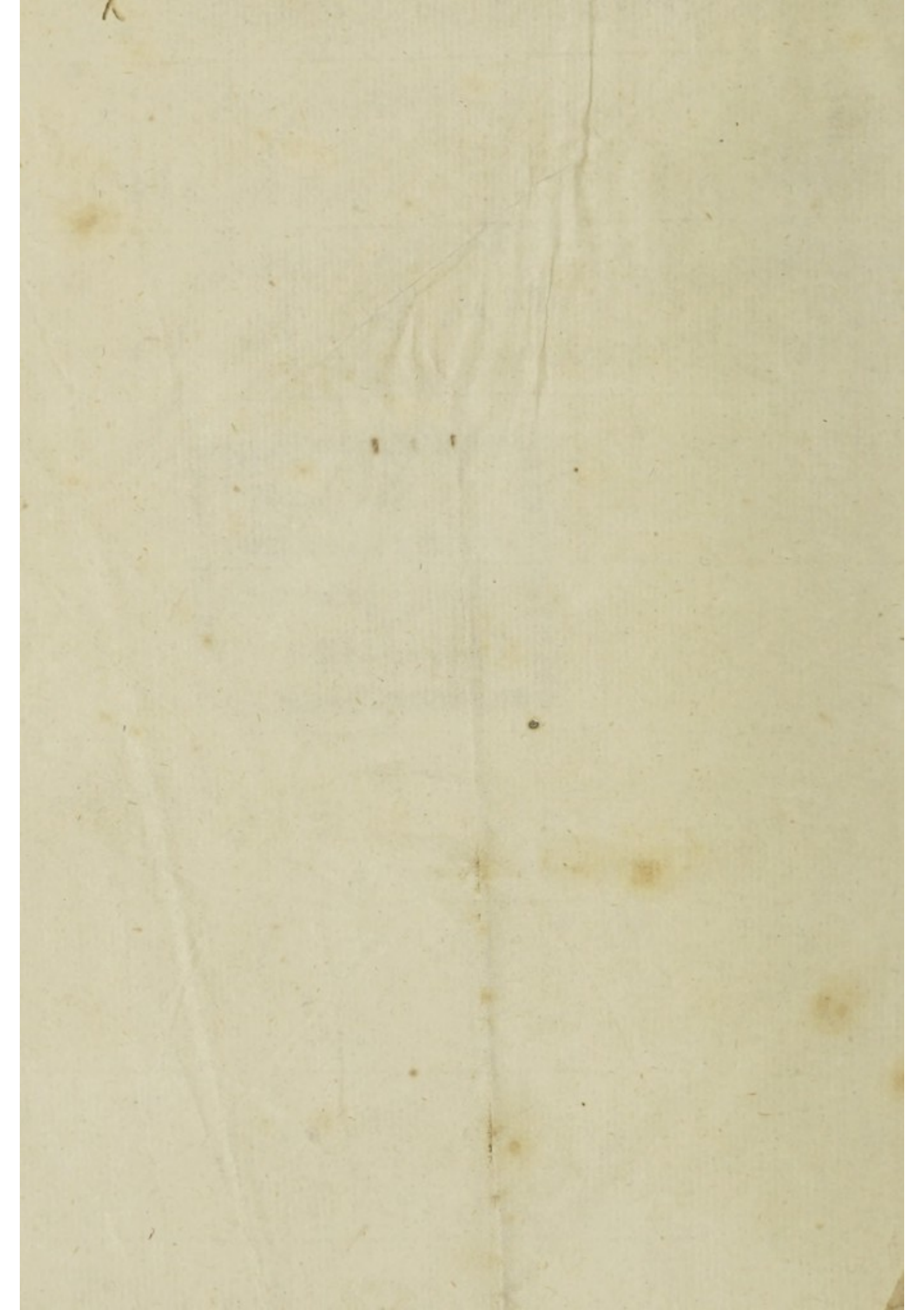
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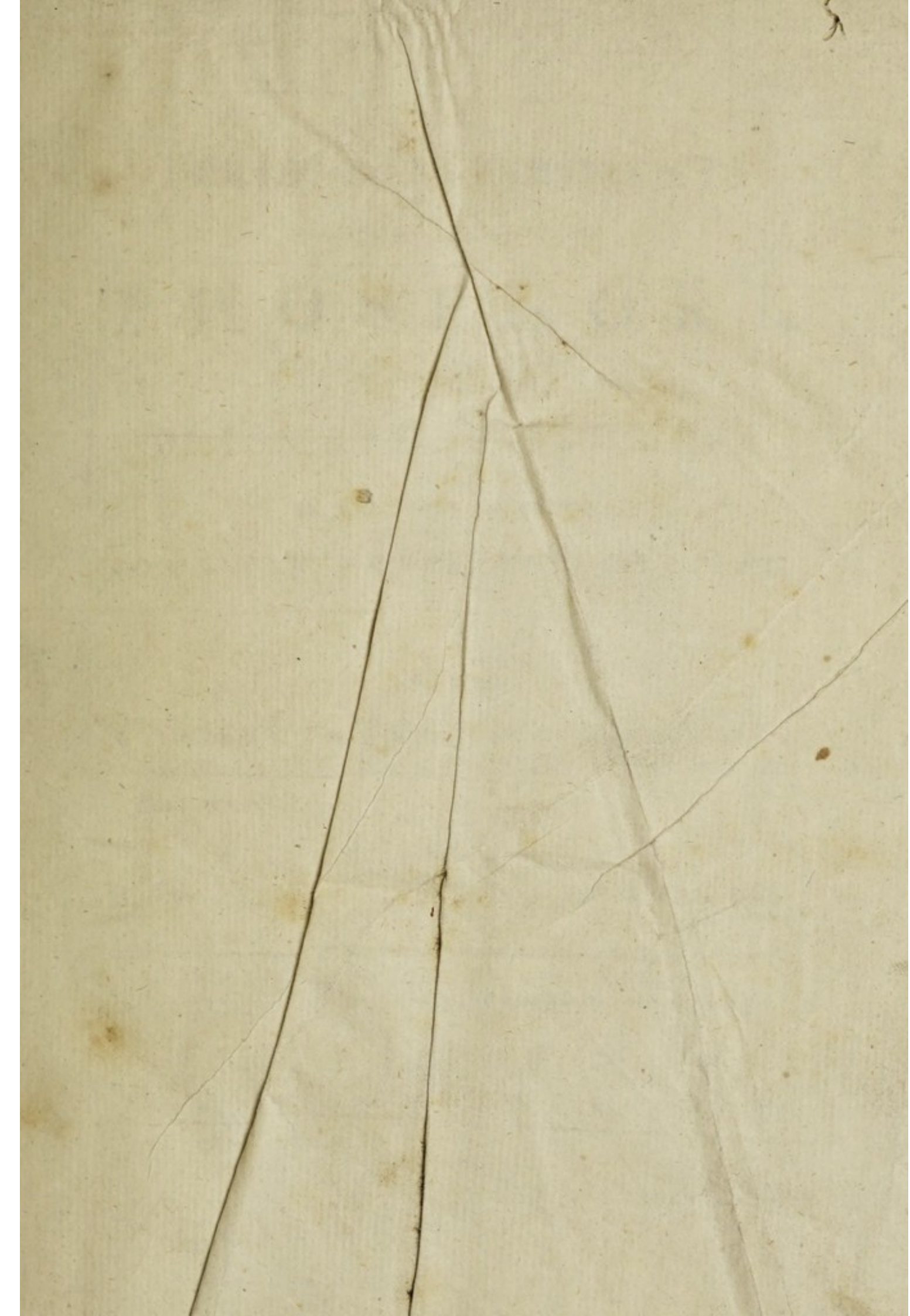
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A
SERIES OF EXPERIMENTS
ON THE SUBJECT OF
P H O S P H O R I,
AND THEIR
PRISMATIC COLOURS:

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.

THE SECONDEDITION, WITH ADDITIONS.

L O N D O N :

PRINTED FOR JOHN NOURSE, IN THE STRAND,
BOOKSELLER TO HIS MAJESTY.

MDCCLXXVI.

P R E F A C E.

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 observa-

tion 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.*

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 published, (in which *condition* it still remains) 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 *uncorrected*: 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 philosopher 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
asserts

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 op-
portunities 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 *distinc-*
tion, had it fallen to his lot: neverthe-
less, as a *plain man*, and a *plain philoso-*
pher, he has, in the best manner he was
able, industriously applied himself to *one*
capital object, namely, a search after phi-
losophical truths, in an unwearied attempt
to make what discoveries he could, for

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 *in-*
decisive ;

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

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S E R I E S
O F
E X P E R I M E N T S
O N T H E S U B J E C T O F
P H O S P H O R I,
A N D T H E I R
P R I S M A T I C C O L O U R S.

TH E 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
A 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 ^{latter,} were about six feet ^{long} by five and a half ^{wide}; and the height 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 ^{adjoining} 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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iron, heated
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the Box, is
luminous.*

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,

fect, 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 solicitous 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.

*Page 6.
Linen
as the Pa
is brighter
ger.*

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. *Loaf sugar*

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

lour 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

Sugar. Candy.

*Gum copal
Arabic*

Cotton.

Glue

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,

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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 phosphoric: but some of those shone better than others.

Two rubies, a sapphire, 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*.

MARBLE S, 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. *Portland Stone shines much.*

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
C they

Green Spar
Sweden
is 6 Minutes
 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.

*Borax
calcined
in contact
with chalk,
among the
most brilliant
Phosphore
in Page 33*

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 alkali. 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
general

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. *Rainbow Colours.*

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 greenish hue, seemed to lye in greater abundance next the red than in any other part. In making my observations, some parts of the oyster-shell that gave the phosphoric red, crumbled away by the slightest touch: for these observations were made on the sixth day after the colours appeared; and probably this would not have been the case on the first day, as the whole mass was then firmer and more compact.

Some of these colours continued visible six days at least, but not near so distinct as on the 1st and 2d days; for the yellow, green, and blue, were considerably fainter than the red. On the 8th day they were all vanished, and a white, yellowish light occupied their places; and this light appeared rather brilliant than otherwise. I have been very circumstantial in describing this experiment, because I imagined something material would depend upon it. On the loss of this curious specimen I was very industrious to replace it by repeating the experiment. But further trials soon taught me that it was not so easy a matter as I at first apprehended.

However, in my second attempt, I prepared an equal quantity of the same solution, and poured it upon an equal quantity of oyster-shells, which were taken from the former parcel. The whole mass, after being pressed down in the crucible as before, was placed in the fire for an equal time, and then exposed to the light; after having broke a little off from the bottom of it, as in the other experiment;

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 began 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 ob-

servation 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 vi-
triol.

A fo-

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.

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. Sp. nitre.

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.

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. Oil of vitriol.

An equal quantity of spirit of salt, treated in the same manner with the shells, shewed much the Spirit of salt.

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 began 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 alkaline 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.

*given
a light
very intense
& which lasted
1/2 an hour.*

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.

*see
Page
19.*

E

shells,

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 distance

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

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

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

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 Iron. 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.

Round two large shells I wound two yards of 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.

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

Canton's
phosph.

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-fil-
ings.

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

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.

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

Steel and
Iron.

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

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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.

*2 oysters
shells make
a vivid
& beautiful
order of
colours.*

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 to examine the effects of that substance upon the shells.

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. Prussian blue.

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

Upon considering the nature of gold, which so obstinately resists the most violent attacks of fire, without suffering the least diminution of its 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 Gold.

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 laminae, 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 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.

Silver grains a light of 2 hours
 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. Nevertheless, 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: Tin. the red and yellow were but faint, compared with the green and blue that the same shells exhibited.

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. Zinc. I had no sooner gone through the process with it, but I examined the shells, even before they were quite cold,
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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.

*Time
= $\frac{1}{2}$ 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 observed 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
my

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

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. Traga-
canth.

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.

otherwise. Two of the shells however, shewed only a fine blue light. These were exposed again to the fire for ten minutes more (but in the crucible) in consequence of this, they exhibited all the colours; but the blue was weakened considerably. From the effects produced by this second application of heat, I suspect that the shells in some of the preceding experiments, which only exhibited green or blue, were not continued long enough in the fire.

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

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

Sugar. A lump of sugar, the size of a hazle nut, exposed, with the shells in a crucible, to a fire for twenty minutes, produced some exceeding fine specimens of colours. In three of the shells there were great quantities of fine red and green, a little yellow, and an intense blue, but less purple. In one of the shells the colours lay regularly, and in their order.

Wheat
flour. Flour of wheat (about three tea-spoonfuls) put into a crucible with the shells, produced the colours; but then they were rather faint, excepting the

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 solu-
tion 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 conse-
quence 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 there-
of. This paste was put into a crucible, and the
shells were then forced into the paste. The cru-
cible 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 appearance, Scuttle-fish.

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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

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generally

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; after 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.

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

Solid
phos.

*Shells
continued
luminous
above
 $\frac{1}{2}$ an hour.*

Chang-
ing of co-
lours.

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 *Experiment* might be thought too tedious.

One of the shells, which gave no other phos- Light.
phoric 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 four-
teen 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.

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 ex-
posed to the collected rays, there issued from it a
va-

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.

On

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
contact,

contact, 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 contact 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 respectively 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

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
of

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 surpris'd, 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

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 continually 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.

After trying a few experiments upon some small pieces of a shell, known by the name of mother-
K
Mother-
of-pearl.
of-pearl,

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.

Nevertheless, 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
 9 nature,

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'écaillés d'huitre faite dans l'esprit
 “ de nitre, précipitée par l'esprit de vitriol, & cal-
 “ cinée avec les charbons, donnera une lumière
 “ 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 confit 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.

*ca Lumin
ferreâ—
of Iron-
Plates—
not, boxes
of Brass.*

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 conclusion, by the same author, (for I have not seen the original) is, " that phosphori shine by their own *native light*, after they " have been *kindled* by foreign light."

I am inclined to think that the learned Zanotti is considerably nearer the truth than Dr. Priestly apprehends: for it seems highly probable, that (if he be now living) he will have no reason to *retract* his conclusion, after reading the following experiments and observations; or at least till they are contradicted by other experiments.

1. My first attempt, in repeating M. Beccaria's experiment, was with cylindrical glasses of different colours, that were open at one end, and covered, occasionally, with three pieces of black leather. The depth of each glass was about six inches, and the width about two inches. Into these glasses I put, at different times, the same calcined shell; which had been previously prepared to exhibit, upon occasion, a fine white light that would continue for a considerable time. I then tied the leathers over the aperture of the glass with which the experiment was made; and, as each glass had a little bending border round the top, which projected outwards, it was the more convenient to exclude the light, by tying the leathers close below this border. The advantage gained by having glasses, instead of metal boxes, was, that the shell within was more exposed to the sun than in M. Beccaria's method. And, that the least time might be lost 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 distance

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

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.

B Y Profeffor 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^o. 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 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, 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.

A D D I T I O N S.

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

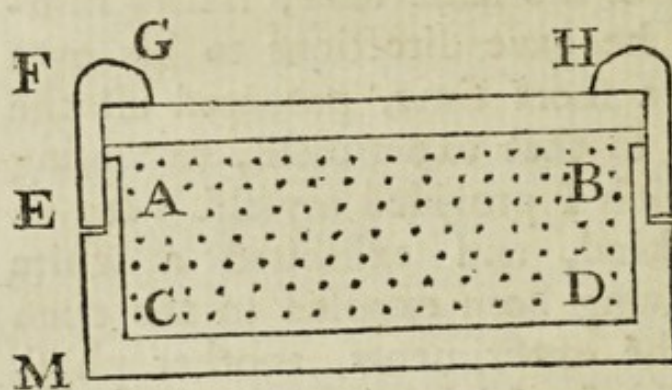
to the subject; and then describe the experiment as I myself repeated, and varied it, with all those additional circumstances.

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 elettif-
 “ fimo; nella stanza buia metto tali scattolette
 “ 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.



I “ Ho sperimentato lo stesso
 “ con diamanti
 K “ fosforici. AB
 “ C D fosforo.
 “ A M B L scat-
 L “ toletta di ot-
 “ tona. G F E,
 “ H I K coperchio di ottona. G H vetro colorito
 “ incastrato nel coperchio.

“ GIAMBATISTA BECCARIA, d.S.P.

“ Mondori, 1 Settembre, 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

“ 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 cop-
 “ iously 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
 N small

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

E X P E R I M E N T 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 glass, in the place of the red ; the yellow refracted rays appeared a little more full, intense, 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 glass ; the green refracted rays appeared a little more full, intense, and lively than the yellow ; the yellow more than the blue ; and the blue more than the violet, or red refracted rays.

With blue glass ; the blue refracted rays appeared a little more full, intense, 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 glass, as I could not procure any that was good enough for the purpose.

The degrees of different-coloured light transmitted by each of the different-coloured glasses mentioned in those experiments, are particularized, merely to give some idea of the fulness, intenseness, and liveliness, of the several refracted rays as they appeared to my eye upon comparing them together. Although, from more accurate observations, those differences may possibly turn out to be a little more in some of the colours, or a little less in others, than are here set down. Had the ascer-

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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

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and

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
O necessary

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.

E X P E R I M E N T VII.

N O R 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.

I N 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 suffered 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

[III]

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
6 yellow

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*, (induced, 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
P charged,

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.

and although the light was not so great as might
perhaps be expected by other methods, yet the
shells exhibited the prismatic colours; but then
these colours were not so lively as the co-
lours caused by electric light.

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

The experiment was repeated several times, and
the result was the same.

The experiment was repeated several times, and
the result was the same.

The experiment was repeated several times, and
the result was the same.

The experiment was repeated several times, and
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AN
ACCOUNT
OF A GREAT NUMBER OF
PHOSPHORI,

DISCOVERED BY
J. B. BECCARI,
PROFESSOR OF PHILOSOPHY IN *Bologna*.

PUBLISHED IN THE BOLOGNA ACTS, 1744.

I. 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*,
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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
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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 *Homburg*, 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

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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 Zagoni) 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 light 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

posed 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 talcs, or felenites, 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.

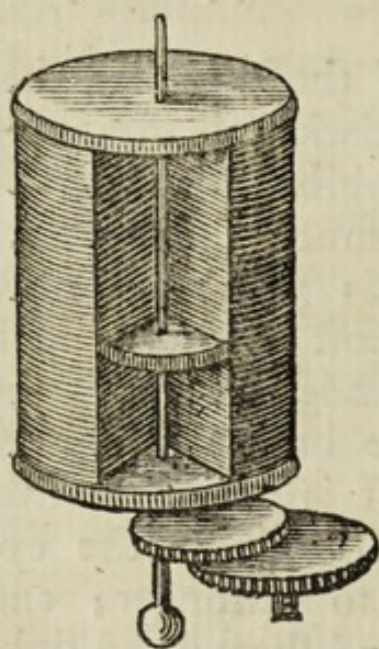
13. This new investigation brought with it
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more labour than profit, with respect to the principal point; but afforded a great deal of matter which was little expected from it. For being obliged in the course of these experiments to adopt a very nice 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.

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 its axis,

which was perpendicular. To the borders of the window were fixed two wings, and to the upper and lower borders were fixed two lifts: 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

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 *Vicentina 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 Argentinian 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*. Porphyry, 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 respect

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 limestones: 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 amiantus, 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 stalactites 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

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astroitæ, with respect to their form, may be reckoned of this kind; but, 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 dentritæ, 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 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

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 incruited 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 marcasites, 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 sulphurous 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 sapphire, 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 foliaceous 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 its 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:
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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 *pinnae*: 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*. Its 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

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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
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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 *Homborg*, 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
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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 its 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
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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.

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same sort
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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 daylight, 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 its 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 its first origin, and is, therefore, a great proof of the strength of it.

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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

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 application 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

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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 endeavour

your 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 the 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
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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,
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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 its 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 quicklime. 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
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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

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 subtilty, 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
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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. Cer-
 tainly, 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. There-
 fore, 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

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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

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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
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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 its transparency. To this class we must refer that ore 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:

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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 its 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

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

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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
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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 subtilty 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

A FURTHER
A C C O U N T
OF A GREAT NUMBER OF
P H O S P H O R I,
LATELY DISCOVERED BY

J. B. BECCARI, Professor of Philosophy.

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1. **I**T 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

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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
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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 occurred 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
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better in the same light, when placed in one position, than when in another. But it is difficult to lay down any certain and general rule in this matter; 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 transmitted 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:

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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
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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,
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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 carneli, 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 ha
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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 amethysts 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

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polish,

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.

red
not
phosphoric

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.

Not
Metals.

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
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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-

fruct. 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 ap-
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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.

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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 fevers :

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

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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

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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
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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 sebaceous 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 served

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 persons

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. Nevertheless, 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.

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