THE

REPERTORY

OF

ARTS, MANUFACTURES,

AND

AGRICULTURE.

NUMBER XVIII. SECOND SERIES. Nov. 1, 1803.

Specification of the Patent granted to LAWRENCE HOLLISTER, *of Norfolk-street, in the Parish of St. Mary-le-bone, in the County of Middlesex, Artist; for certain Machinery for improving Roads.*

Dated May 5, 1802.

With a Plate.

TO all to whom these presents shall come, &c. NOW KNOW YE, that I the said Lawrence Hollister, in compliance with the proviso in the said letters patent granted unto me for certain machinery, for the purpose of cleansing and repairing roads, do hereby declare the meaning and intention of my invention in manner and form following; that is to say: My said invention consists of two or more wheels, double tyred, on an iron axletree or axletrees, on which I place springs, supporting a large board, which I call a bed or bottom, to which I fasten

I fasten my machinery, which has an angular piece of iron, see L, Fig. 2, Plate XVII. (the lower part of which represents the edge of a knife,) with three uprights in the same form, working through the bed in spring sockets, by means of a rack, which rack hath holes to put pins in the socket, as also three other sockets in the same manner, to which I attach two curved or semi-circular irons, which I call rutlers, the bottoms of which are formed as the angular one, into which, by the motion of the machine, the dirt, gravel, or earth, is collected from the sides of each rut or furrow made by the wheels of carriages, and returned through an open part in each rutler into the ruts or furrows, so continuing the gravel on the road. These irons I secure by a cross bar, and each or all are raised higher or let lower as found necessary, by means of pinions in cocks F, in Fig. 1, fastened to the bed by screws, and by the use of the racks aforesaid. Behind those rutlers, is a board, which I call a tail-board, secured to the bed, to which a harrow is fixed, to level new-made gravel, and may be used or disused at pleasure. From the outside of each rutler (they taking the ruts or furrows made by the wheels of carriages) as also from the front point of the angular iron to take the middle of the road, I fasten chains or ropes, holes being made for that purpose, and hook to the front of the bed, or to the shafts, or other parts of the machine, as preventors, when the roads are so heavy as to endanger the works. At the extreme ends of the tail-board, or to the bed, or other parts of the machine, is fastened a roller or rollers, to take the road so levelled as before described by the rutlers, &c. to press and roll it, occasioning the wet to rise and run off it, thereby making it more firm without scraping it, consequently

will

will require much less gravel than by the present method of scraping roads. See the drawings and explanation hereunto annexed, whereby the same are more particularly described. Other modes I have adopted to produce the same effect; as, for instance, the fixing the bed or bottom so as to be drawn on three or four wheels, and an iron stay may be fixed from the tail-board or hind end of the bed or bottom to act as a pressure on the harrows, should an extra weight be required to any large quantity of gravel.

And farther, in addition to the aforesaid semi-circular rutlers may be added two scrapers, of a like form as scrapers, which may be raised up or let down at discretion, and which will gather up all the dirt, gravel, &c. that may escape the rutlers. Over the roller or rollers is suspended a scraper K, to keep them from dirt, &c. which may adhere thereto. To secure the works from injury by weather, a box, the size of a bed, is put on the top thereof, divided into partitions and folding covers; in the front part will be put stones or other weight, when required, the better to balance the machine.

In witness whereof, &c.

Explanation of Plate XVII.

Fig. 1, A, the wheels. B, the shafts. C, the bed or bottom. D, the cocks which receive the pin or bolt through the rack that prevents the rutlers from dropping down after being raised to the height required. E, the sockets through which the uprights pass. F, the pinions which, by means of the handle, as *per* Fig. 6, raises the rutlers either separate or together. G, the tail-board, about three inches thick, one foot and half deep, fastened to the bed or bottom edgewise close to the back of the

rutlers.

rutlers. H. B. The tail-board the same breadth as the bottom. [illegible], the harrow, fastened to the tail-board with joints, and may be raised up and secured by a chain, or lowered down to level the gravel at discretion. I, the roller fastened to the extreme ends of the tail-board, which serves to level and press the road. K, the scraper, to scrape off the dirt that may adhere thereto.

Fig. 2, L, the angle or foot rutler, which follows the horse-track, and gathers up the dirt, and conveys it into the semi-circular rutlers through which it passes into the wheel-ruts or furrows. M, the semi-circular rutlers which follow the wheels, and gather up the dirt raised up by them, which it forces through the open part into the ruts or furrows. N and O, the uprights and racks which enter the sockets. P, the chains in the rutlers to fasten to the front of the bed or bottom, or any other part most convenient.

Fig. 3, the angle or foot-rutler. A, the holes that receive the stirts and screws belonging to the springs.

Fig. 4, the right and left rutlers, which join to the angle or foot-rutler by means of springs, Fig. 5, with stirts set at the back, and one or more screws, with nuts on the points of the screws to secure them. B, the channel through which the stirts and screws pass into the holes.

Specification

Specification of the Patent granted to THOMAS BROWN, *of Alnwick, in the County of Northumberland, Whitesmith; for a Machine for cutting or shreding Tallow, cutting Turnips, Carrots, Potatoes, or other Fodder for feeding Cattle, for cutting Tobacco, kneading Dough, bruising Fruits, or any other Matter requiring the same.* Dated July 2, 1803.

With a Plate.

TO all to whom these presents shall come, &c. NOW KNOW YE, that in compliance with the said proviso, I the said Thomas Brown do hereby declare that the nature of my said invention of a new method of cutting turnips, potatoes, carrots, or any other fodder for feeding cattle, for cutting or shreding butchers fat, for the easier melting it into tallow, and by which means a great saving is made, as well in collecting a greater quantity of tallow (from a given quantity of fat), as in time, labour, and fuel, for the cutting tobacco, kneading dough, bruising fruits, or any other matter requiring the same, is particularly described and ascertained by the drawings hereunto annexed, in the following manner; that is to say:

A, Fig. 1, (Plate XVIII.) is a crank, to which is attached the fly-wheel B, and the working rods C, C, C, C, to the joints D, D; in these rods are attached parallel joints E, E, E, E. These prevent friction and noise, otherwise the machine may be made without them. To the lower part of the working rods C, is fixed the knife or bruiser F, which, by the motion of the crank, is raised or driven downwards with the necessary force required. G, is the trough or box (made to slide between the

the frame-work) into which the matter to be cut or bruised is to be put. H, is a bevelled cog-wheel, fixed on the crank A: this works I, another wheel, fixed horizontally on the upright-shaft or axle K, working on a bearer. At the lower end of this shaft is fixed another horizontal wheel L, working into the wheel M. These wheels are all bevelled, and of one size and number. This last-mentioned wheel is fixed on a horizontal shaft or axle, Fig. 3, working in the frame-work below the box. This shaft has a tooth N, Figs. 3 and 4. On the bottom of the box is fixed a rack O, Fig. 2, into which the tooth N works, and thus moves the box until one end thereof comes close to the knife, when it will go no farther, the tooth N not acting on the smooth pieces at each end of the rack O. On reversing the motion of the crank, it will be driven back again until the matter is sufficiently cut or bruised. The motion of the box may be accelerated or retarded at pleasure, by mechanical methods already well known. The machine may be driven by the hand, horse, steam, water, or any other power. If a circular box is preferred, it may be worked with circular racks, and on a pivot or centre.

In witness whereof, &c.

Observations by the Patentee.

First. It must be obvious that the cutting of turnips, carrots, cabbages, and such like food, will have many advantages in the feeding of cattle, and this method of doing it will be so easily performed, and at so small an expense, when a boy, or an old man, can cut as much food in an hour as will serve a good number of cattle a whole day; and the machine is so portable, that it may be be used with equal ease and advantage in the field, the farm-yard, the hovel, or the barn.

Second. It will prevent cattle being choaked, as is often the case with whole turnips, &c.

Third. It will cause them to eat their food clean up, and prevent much waste, (even the tops or shaws may be cut with the roots,) whereas it often happens that when turnips, &c. are broke into by cattle in wet or frosty weather, they are thus left to rot upon the ground, without the cattle ever again touching them, and thereby much loss is sustained.

Fourth. In frosty weather turnips, &c. are so hard, that scarcely any beast can penetrate them, but particularly young cattle, when casting their teeth, so that they are nearly starved with hunger, and lose their flesh considerably; and the summer is often far advanced before they regain the weight and condition they lost in the winter and spring.

Fifth. In the cutting or shreding of tallow in the easy, expeditious, and regular manner, in which the machine does it, it is also obvious there must be a great saving of labour, time, tallow, and fuel. In cutting tallow by hand, which is laborious, slow, and imperfect, there are often lumps uncut, which, when put into the boiler, are so slow in melting, and sometimes never melt at all, that a great loss is sustained by the unmelted tallow, and also in the evaporation of the melted tallow, in waiting for the lumps melting; whereas, by being effectually cut, it melts regularly, expeditiously, in better condition, and is more productive. These facts have been clearly ascertained by all the tallow-chandlers who have tried the machine, to whom references might be given if required. A man may cut with ease ten stone in five minutes.

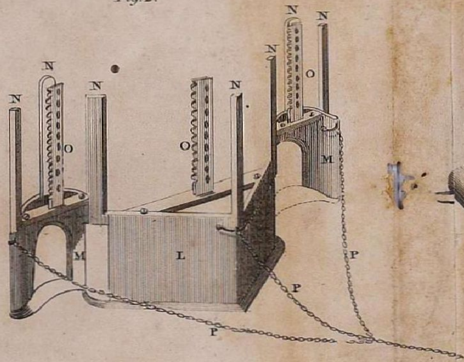
Sixth.

Sixth. With respect to tobacco, it will do its work with equal facility; and it is at a much lower price than many of those cumbrous machines which are in use for that purpose. Its utility in kneading dough, bruising fruit, &c. is equally obvious at first sight.

Specification of the Patent granted to THOMAS SAINT, *of the City of Bristol, Engineer; for a Method of increasing the Effect of Steam-Engines, and saving Fuel in the working thereof.* Dated December 21, 1802.

TO all to whom these presents shall come, &c. Now KNOW YE, that in compliance with the said proviso, I the said Thomas Saint do hereby declare that the nature of my said invention, and the manner in which the same is to be performed, are hereinafter particularly described and ascertained; that is to say: I make an opening of considerable magnitude in the bottom of the boiler, nearly as large as the flue, which would be suitable to the boiler made use of; in which opening I fix a pipe or tube, through which pipe or tube a communication takes place between that part of my furnace or fire-place, in which the flame or heated air rises or circulates, and the interior space of my boiler, in which steam is produced or maintained, for the supply of the engine, which pipe or tube may remain open. But I prefer to close the said opening by a valve or cock, or safety tube, so adjusted, counterpoised, or constructed, according to the well-known methods of engineers, chemists, and other operators, as that no part of the steam or heated air contained in the said interior space of the boiler shall be permitted

Fig. 2.



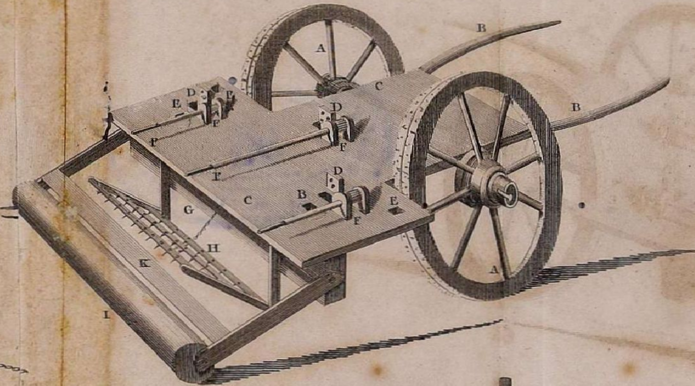


Fig. 4.

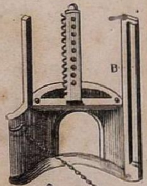


Fig. 3.

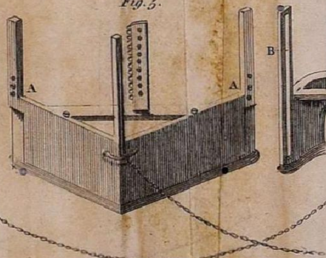


Fig. 4.

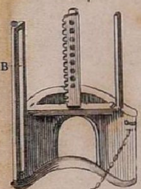
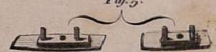


Fig. 6.

Fig. 5.



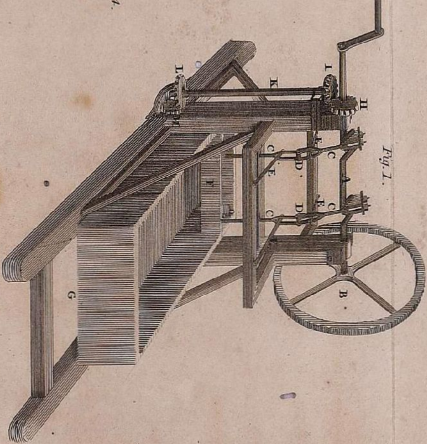


Fig. 1.

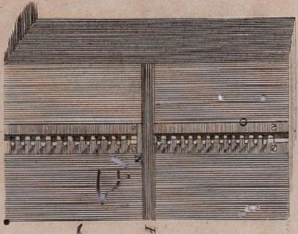


Fig. 2.

Fig. 3.

Fig. 4.

permitted to escape through the said opening, but that the flame or heated air from the furnace or fire-place shall or may enter, and be admitted through the said opening whenever and so often as the elastic force or re-action of the steam shall be less than any determinate or particular degree at which the adjustment, counterpoise, or construction of the said valve or cock, or safety tube, shall open, and allow a clear passage for the flame or heated air. And I do also construct and dispose the parts of my furnace or fire-place so as may best serve to cause the said flame or heated air to pass in due quantity through the said opening when unclosed, instead of being totally, or for the most part, driven up the chimnies, or in other less useful directions; that is to say, I place the lower entrance of the flue of my chimneys at a moderate distance higher than the grate which supports the fuel, and I fix a moveable shut or register therein; and I am particularly careful that the space above the fire, into which the flame or heated air do descend, shall be well closed on all sides, so that the side flame or heated air shall not be allowed to escape in any manner or direction except through the said opening, or through the flue of the chimney. And I do declare that the power and effect of engines worked by the steam from boilers, having a communication with the fire, according to my said invention, is greatly increased, so that a greater quantity of work may be performed with the same expense of fuel than could be performed by a like engine not supplied according to my said invention. Or in case the quantity of work required to be done by engines supplied with steam, or heated air, according to my said invention, be the same as is ordinarily done by like engines not supplied according to my said invention, then the quantity of fuel necessary to do the work according to my said invention, would be much less than would otherwise have been required to produce the same effect. And in order that the practical process, or means of carrying my said invention into full effect, may be perfectly known and understood, I proceed to describe the same as follows: first, I do (in preference) make my opening through the bottom of the boiler immediately over the fire-place, and I fix a tube or pipe round the said opening, and passing upwards through the body of water and water-wheel, need not be deeper on the crown of the boiler than six inches, so as to have its upper orifice in the clear space above the water at a sufficient height to prevent any part of the water from entering or passing through the same. Secondly, I fix a valve, opening upwards at the top of the said pipe or tube as large as the bore will permit. Thirdly, as the weight of the valve would prevent its rising, even when the strength of the steam was such as to suffer the flame or heated air to enter through the opening into the boiler, I fix a rod to the said valve, and pass the same through the top of the boiler, where I attach the same to a lever, with a counterpoise, which may be set at different distances on the lever, according to the force of descent which it may be found desirable to allow the valve to retain. Fourthly, in such cases or constructions in which there may be any reason to fear that the upper part of the pipe or tube, or circumference of the opening, should become so hot as to suffer injury, I conduct the feeding water of the boiler directly upon the crown of the valve, or into a small bordering cavity or trough round the inner edge of the pipe or tube or opening, by which means the temperature of the heat cannot exceed that of boiling water. And, in general, I declare that the flame or heated air can and may be introduced into the boiler by various other

other methods or modifications. But what I claim as my invention is, that the furnace or fire-place in which the flame or heated air rises or circulates shall have an open or regulated communication with the interior space of the boiler, or other apparatus wherein steam is produced, so that a combination of the flame or heated air, and the steam generated, may pass from the boiler to the cylinders of fire-engines.

In witness whereof, &c.

Specification of the Patent granted to RICHARD HARE, *of Limehouse, in the County of Middlesex, Brewer; for an Apparatus whereby the essential Oil of Hops (which he is informed is the most efficacious and preservative Part of that Vegetable, and which, till this Invention, was lost and dissipated in the Air during the Operation of boiling Worts for Beer) is preserved and applied to use; and that, by Means of the said Apparatus, his Water for brewing is heating to such a Degree of Heat as he judges necessary, and as he believes is customary, and that his said Water is heated in less than the usual Time for boiling Worts for Beer, and that without any Application of Fire to the Vessel containing the said Water.* Dated September 12, 1791 *.

With a Plate.

TO all to whom these presents shall come, &c. NOW KNOW YE, that I the said Richard Hare, in compliance with the said proviso, do hereby describe and ascertain the nature of my said invention, and the man-

* See an account of a trial concerning the validity of this patent, page 232 of this volume, in which the same principles were held to be exemplified in the following specification of Mr. Wood.

 ner

ner in which the same is to be performed, as follows; that is to say:

A, Fig. 1, (Plate XIX.) the trunk, which incloses the valves. B, the rising valve and frame. C, the sinking valve (see Fig. 3.) D, the lever, with its weights to shift occasionally. E, the valve plate, Figs. 1 and 3, on which the valve frames are fixed; the valves also are made in it. F, the bottom of liquor-back. G, a large cylinder connected and communicated with the main copper. H, H, two doubled curved branch pipes connected with the cylinder G, and opening into the jack-back near its bottom by the valves at *d*, *d*, occasionally. I, the straight branch pipe communicating with the cylinder G, with a curve returned at *b*, from the under side, in which is inserted the three pipes *c*, *d*, *e*, with their cocks and screws, (see Fig. 3). K, jack-back or boiling-back. L, the boiler and man-hole. The upright pipe or cylinder G is open into the copper, into which pipe the steam rises when the copper begins to boil. It is stopt at the top by the valves B and C; it has free communication into the pipes H, H, where it may pass into the liquor in the jack-back, or not, as thought proper. It has also a free communication into the straight branch pipe I, with its three branches Fig. 2. These three branches have cap screws at their ends, and cocks so fixed as to be opened when they are wanted. The valves B and C are reverse, one opens downward and one opens upward, with adjusting weights and levers, (Fig. 1 and Fig. 3). The one that rises upwards prevents any overstrain happening to the boiler, and entirely secures it from inward strains. The valve that opens downwards admits the atmosphere to pass into the boiler in case it is emptied or cooled too hastily, and perfectly secures the boiler from any accident happening that way. In witness whereof, &c.

Specification

Specification of the Patent granted to SUTTON THOMAS WOOD, *of the City of Oxford, Brewer; for certain new Discoveries in the Application of Steam, and also certain Methods of using the Water produced from condensed Steam, and for applying the Water from the Coppers or Boilers of Steam-Engines to other Purposes than that of working the Steam-Engine; and also various Methods of heating and applying Water for the several Purposes of the Breweries and Distilleries, and for forwarding the Process of Brewing; and also certain Methods of constructing and adapting Coppers, Boilers, Tubes, and other hollow Bodies, for the more effectual Means of heating Water and Worts, and of rendering such Coppers, Boilers, Tubes, and other hollow Bodies as are employed in the Breweries and Distilleries Steam and Air tight.*

Dated November 17, 1784.—Term expired.

TO all to whom these presents shall come, &c. Now KNOW YE, that in compliance with the said proviso, I the said Sutton Thomas Wood do hereby declare that the nature of my said invention, and the manner in which the same is to be performed, is hereinafter particularly described and ascertained; that is to say:

My first application of steam is, the application of that steam which arises from the water, liquor, worts, or other solutions which are or may be heated or boiled in all or any of such coppers, pans, kettles, or boilers, as are used and employed in breweries, distilleries, the various manufactures of sugar, soap, salt, and allum, and all other trades and manufactures whatever, to those engines commonly called or known by the name of steam or fire engines, so that the same steam which arises either during the process of the trade or manufacture, or from water or liquor heated or boiled for the purposes of the same,

same, may be rendered capable of working the steam-engine; and this I do by covering over the top of the copper, pan, or boiler, with copper, iron, brass, or any other materials, and by means of pipes, tubes, funnels, or other hollow bodies, or by any other means, forming a communication with the steam-engine; to which copper, pan, or boiler, I apply safety valves and doors, either upon the same principle as those which are generally applied to the boilers of steam-engines, or upon any other principle. But I have various ways of adapting and connecting coppers or boilers used in trades and manufactures to the steam or fire engine, in order that the steam arising therefrom may be applied upon the above-mentioned principle, and rendered capable of working the same. Many of which methods I have particularly specified and described in a specification, duly inrolled agreeably to and by virtue of certain letters patent, granted to me the said Sutton Thomas Wood, by his present Majesty king George the Third, bearing date the twentieth day of August, in the year of our Lord one thousand seven hundred and eighty-four, "for certain new improvements on the steam-engine, and other things therein contained *," as in and by the said specification (relation being thereunto had) may more fully and at large appear. But let it be here observed, that it is not of any consequence in what shape, form, or method, the coppers, pans, or boilers may be constructed or adapted, provided they are constructed or adapted upon the above-mentioned principle; namely, that the same steam which arises from the water, liquor, worts, or solutions, heated or boiled therein, may be rendered capable of working the steam-engine, nor is it of any signification from what copper, pan, or boiler, the steam may arise,

* This specification is deposited in the Rolls Chapel. It is extremely voluminous, and contains little that would be interesting at this day.

which

which is applied to the working of the steam-engine, provided it arises from any copper, pan, or boiler, which is either employed in the carrying on, or in any wise applied to the use of any business, trade, or manufacture whatever. Nothing, however, above mentioned is meant to extend to those coppers or boilers which are solely appropriated to, and applied to no other purpose than the working of the steam or fire engine.

My second application of steam is, the application of steam to the making and preparing of sugar, salt, and allum, which I do by placing one pan or vessel over another, and by fastening and connecting them together either by means of doors or shutters, or by means of pipes, tubes, or funnels, or by any other means so as to cause the steam which arises from the lower pan or vessel to heat the sugar, salt, or allum, in the upper pan or vessel, and make or prepare the same. But, in order to hinder any particles of the condensed steam from falling back into the lower pan or vessel, and checking the evaporation, I sometimes make the top of the lower vessel, which may likewise serve for the bottom of the upper vessel, somewhat convex, spherical, or inclined, so as to cause the drops as they generate to roll down the same into grooves, furrows, or channels, which are made to surround the sides of the lower vessel, and to be conveyed away by means of holes or apertures, or cocks or valves placed therein for that purpose. But I have various ways of constructing pans and vessels upon the above principle, which, together with the nature of the grooves, furrows, or channels, and the valves and doors that may be applied upon this occasion, I have more particularly mentioned in the specification above referred to, wherein I describe my method of adapting and connecting those coppers, pans, or boilers, used in trades or manufactures, to the steam or fire engine, where the

the nature of the trade or manufacture requires a quick evaporation.

My third application of steam is, the application of steam to the heating of stoves, either such stoves as are used for the drying of sugar or salt, or any other stoves, or to the heating of all or any such places and things as are in general heated by fire, or by any other means, and to heat which steam may be used or applied, which I do by means of tubes, pipes, funnels, or other hollow bodies, made in such forms and shapes, and thrown in such directions, as may be most suitable to the particular places and purposes for which they may be required.

My fourth application of steam is, the application of steam to the heating of water, or any liquor for the purposes of trades or manufactures; and this I do either by causing water, liquors, worts, or other solutions, to be placed in vessels or other hollow bodies, which are made to surround or to be in contact with the copper, boiler, or vessel, in which water or liquor are heated or boiled so as that the steam arising from such copper or boiler may heat the same, or by conveying the steam by means of pipes, tubes, or other hollow bodies, to the sides, top, bottom, or any other part of the vessel or hollow body in which the water or liquor is or may be contained, or by conveying the steam through the water or liquor by means of pipes or tubes as aforesaid, or by any other means, so as that the water or liquor required to be heated may be heated by steam.

My fifth application of steam is, that of condensing such steam as arises from the coppers, pans, or boilers, used in trades and manufactures, which I do not only with, but also without, the power of the steam or fire engine, either by means of cold water or other liquor, or by causing the steam to flow through cold pipes, funnels, or hollow bodies, or to come in contact with cold bodies,

or

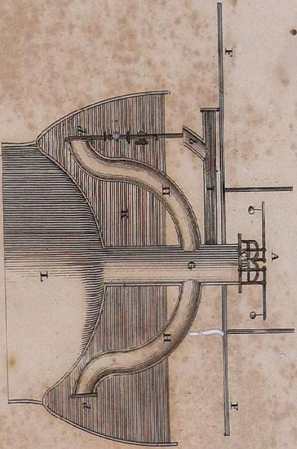


Fig. 1.

Fig. 2.

Fig. 3.

or by any other means; and of applying the hot water produced by such condensed steam to the various purposes of trades and manufactures. Let it be remembered also, that I apply the hot water which is produced by such steam as is or may be condensed by the power of the steam-engine to the like purposes of trades and manufactures.

My sixth application of steam is, the application of that steam which arises from sugars, unfermented worts, and all other unfermented solutions, which I do not only apply to any of the purposes above mentioned, and condense the same, and use the liquor produced by such condensed steam, but I also cause such liquor as is or may be produced by the condensed steam of sugar, unfermented worts, and other unfermented solutions, to be put into a state of fermentation, and to pass through the process of distillation. And the liquor produced thereby I use either for the purposes of trades or manufactures, or for any other purpose to which it may be applied.

My first improvement in the constructing and adapting of coppers or boilers consists of the following principle, namely; in causing that heat which is generally absorbed and lost in the sides, back, and other parts of such fires or furnaces as are used in trades and manufactures, to be applied to the heating or boiling of water or liquor in other coppers or vessels than those which are generally used for the purposes of trades or manufactures; so that a greater quantity of water or liquor may be heated and boiled either for the purposes of the trade or manufacture, or for any other purpose; and the process of the trade or manufacture may be thereby greatly forwarded without any extra consumption of fuel. And this I do by adapting, connecting, or applying, coppers, boilers, or vessels, near to, or in contact with, such fires or furnaces, either on the sides or back, or any part of the

the same, so as that the water or liquor contained in such coppers or vessels may be heated or boiled thereby. Which method of constructing and adapting coppers or boilers I have more particularly described in the specification above mentioned and referred to; wherein I have explained my method of working the steam or fire engine by means of such separate and detached coppers or boilers as are constructed and adapted upon the above-mentioned principle.

My second improvement is the adapting and connecting coppers, boilers, or vessels, constructed upon the above-mentioned principle to the steam or fire engine; so that the same fire necessarily used for the purposes of trades and manufactures may at the same time be applied to the use of the steam or fire engine.

My third improvement is in causing the heat of the fire to be conveyed through the water or liquor, either by means of tubes, flues, or other hollow bodies, or by causing the fire or furnace to be in the middle, or in any other part within the body of the liquor, either in the form of a cone or pyramid, or in any other form, and the water or liquor to surround the same; nor is it of any consequence in what form or shape the copper or boiler is made or constructed, or in what direction the pipes or tubes are placed, provided it is constructed upon the above principle of causing the fire or heat to be conveyed through the body of the water or liquor contained therein instead of conveying the heat or fire entirely through the water or liquor in the copper, boiler, or vessel. I sometimes cause the bottom of the copper or boiler to be made in the shape of a cone or pyramid, so as to cause a greater surface to be exposed to the heat of the fire, and the heating or boiling of the liquor contained therein to be forwarded thereby.

In witness whereof, &c.

Observations

Observations on the Quantity of horizontal Refraction; with a Method of measuring the Dip at Sea.

By William Hyde Wollaston, *M. D. F. R. S.*

From the Philosophical Transactions of the Royal Society.

In a paper which I some time since presented to this Society, (printed in the Phil. Trans. for 1800,) I endeavoured to ascertain the causes, and to explain the various cases, of horizontal refraction, which I had either observed myself, or had seen described by others.

At the time of writing that essay, I had not met with the "Mémoires sur l'Egypte," published but a short time before; and I was not aware that an account had been given by M. Monge, of the phenomenon known to the French by the name of *mirage*, which their army had daily opportunities of seeing, in their march through the deserts of Egypt.

In the perusal of this memoir, I could not fail to derive instruction from the information it contained; but, as the facts related by him accord entirely with the theory that I had advanced, I was by no means induced to adopt the explanation that he has proposed, in preference to my own.

The definite reflecting surface which he supposes to take place between two strata of air of different density, is by no means consistent with that continued ascent of rarefied air, which he himself admits; and the explanation founded on this hypothesis will not apply to other cases, which may all be satisfactorily accounted for, upon the supposition of a gradual change of density, and successive curvature of the rays of light by refraction.

 I have

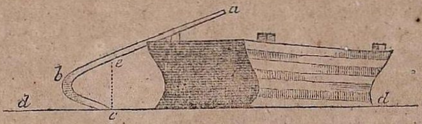
I have since learned that the same subject had also been ably treated by Mr. Woltman, in Gilbert's "Annalen der Physik;" but I have to regret that this dissertation, as well as that of Gruber, in the same Annals, were written in a language that was unknown to me, and that I could not avail myself of the assistance that I might otherwise have received from their researches.

When I formerly engaged in this inquiry, being impressed with the advantage to be derived from it to nautical astronomy, on account of the variations in the dip of the apparent horizon, from which all observations of altitude at sea must necessarily be taken, I suggested the expediency of a series of observations, to be made by a person attentive to those changes of temperature or moisture of the atmosphere, on which he might find the depression of his horizon principally to depend. I had at that time no expectation that I could myself pursue this subject farther to any useful purpose, having little prospect of residing for a sufficient length of time in view of the sea, and seeing no other method by which the same end might be accomplished. I have, however, since that time, found means to satisfy myself, by observations over the surface of the Thames, that although the quantity of refraction varies in general with any change of the thermometer or hygrometer, yet the law of these variations is not altogether so simple as I had hoped it might be found.

I shall, on the present occasion, first relate the facts on which this opinion is founded, and which are in themselves sufficiently remarkable, on account of the unexpected quantity of refraction observable over a short extent of water; I shall, in the next place, shew that the exact determination of the concurrent changes of the atmosphere are of less value, and their irregularities of less

less consequence, than I had conceived, as there is a very easy method whereby the quantity of dip at sea may be at any time correctly measured; and therefore the end which I sought by indirect means, may be at once directly attained.

The first instance that occurred to me, of observable refraction over the surface of the Thames, was wholly accidental. I was sitting in a boat near Chelsea, in such a position that my eye was elevated about half a yard from the surface of the water, and had a view over its surface, that probably somewhat exceeded a mile in length, when I remarked that the oars of several barges at a distance, that were then coming up with the tide, appeared bent in various degrees, according to their distance from me. The most distant appeared nearly in the



form here represented; *d d*, being my visible horizon by apparent curvature of the water; *a b*, the oar itself in its inclined position; and *b c* an inverted image of the portion *b e*. By a little attention to other boats, and to buildings on shore, I could discern that the appearance of all distant objects seen near the surface of the water was affected in a similar manner, but that scarcely any of them afforded images so perfectly distinct as the oblique line of an oar dipped in the water.

A person present at the time (as well as some others to whom I have since related the circumstance) was inclined to attribute the appearance to reflection from the surface of the water; but, by a moderate share of attention, a very

very evident difference may be discovered between the inversion occasioned by reflection, and that which is caused by atmospherical refraction. In cases of reflection, the angles between the object and image are sharp, the line of contact between them straight and well defined, but the lower part of the image indefinite and confused, by means of any slight undulation of the water. But, when the images are caused by refraction, the confines of the object and its inverted image are rounded and indistinct, and the lower edge of the image is terminated by a straight line at the surface of the water. In addition to these marks of difference, there is another circumstance which, if attended to, must at once remove all doubt; for, by bringing the line of sight near to the surface of the water, boats and other small objects are found to be completely hidden by an apparent horizon, which, in so short a distance, cannot be owing to any real curvature of the water, and can arise solely from the bending of the rays by refraction.

When I reflected upon the causes which were probably instrumental in the production of these phenomena, they appeared referrible to difference of temperature alone. After a succession of weather so hot that the thermometer, during one month preceding, had been twelve times above 80°, and on an average of the month at 68°, the evening of that day (August 22, 1800) was unusually cold, the thermometer being 55°. The water might be supposed to retain the temperature it had acquired during a few weeks preceding, and, by warming the stratum of air immediately contiguous to it, might cause a diminution of its refractive density, sufficient to effect this inverted curvature of the rays of light, in the manner formerly explained. As I was at that time unprovided with instruments of any kind, I had it not in my power to esti-

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mate the quantity of refraction, or temperatures; and can only say that, to my hand, the water felt in an uncommon degree warmer than the air.

Being thus furnished with an unexpected field for observation, I from that time took such opportunities as similar changes of the weather afforded me, of examining and measuring the quantities of refraction that might be discovered by the same means over another part of the river, that I found most suited to my convenience.

The situation from which the greater part of my observations were made was at the SE corner of Somerset-house. The view from this spot extends under Blackfriars-bridge, towards London-bridge, upwards of a mile in length, and in the opposite direction through Westminster-bridge, which is three quarters of a mile distant.

Such distances are however by no means necessary; and indeed the air over the river, in cold weather, is generally, or at least very frequently, not sufficiently clear for seeing distinctly to so great distances. For, since the winds which are most likely to effect a sufficient change of temperature, on account of their coldness, are usually from the E, or NE, the principal smoke of the town is then brought in that direction, and hovers, like a dense fog over the course of the river. This circumstance deprived me of many opportunities which the changes of the thermometer indicated to be favourable for my purpose, and obliged me often to make use of shorter distances than I should otherwise have chosen, by bringing the line of sight as near as I could to the surface of the water.

For this purpose, I had a plane reflector fitted to the object-end of a small pocket telescope, at an angle of 45°, so that, when the telescope was held vertically, it gave a horizontal view at any level that was found most eligible.

When

When the water has been calm, I have observed that the greatest refraction was visible within an inch or two of its surface, and I have then seen a refraction of six or seven minutes in the space of 300 or 400 yards: at other times, I have found it greatest at the height of a foot or two; but, in this case, a far more extensive view becomes necessary.

The first measures that I took were on the 23d of September, 1800. The water was $2\frac{1}{2}°$ warmer than the air, and I found a refraction of about 4′.

Oct. 17. The difference of temperature was 3°, and the refraction 3′.

Oct. 22. The water was $11\frac{1}{2}°$ warmer than the air, yet the quantity of refraction did not exceed 3′.

The smallness of the quantity of refraction upon this occasion I attributed to the dryness of the atmosphere, conjecturing that a rapid evaporation might in great measure counteract that warmth which the water would otherwise have communicated to the air.

From that time, therefore, I have noted not only the heights of the thermometer in the water and in the air, but have added also the degrees of cold produced by keeping the bulb of it moistened for a sufficient time to render it stationary. In confirmation of my conjecture respecting the dryness of October 22, I have also, in the following table, which comprises the whole of my observations, inserted a column from the Register kept at the apartments of the Royal Society, containing the heights of the hygrometer, on those mornings when my observations were made.

TABLE.

TABLE.

At 8, A. M.	Air.	Water.	Difference.	Refraction.	Cold by Evaporation.	Hygrometer.
1800. Sept. 23	57	60½°	3½°	4′	- -	72°
Oct. 17	46½	49½	3	3	- -	72
22	38	49½	11½	3	- -	67
Nov. 1	41	45½	4½	8	½°	76
4	43¾	46¼	3	3—	1¾	72
5	37	45	8	8 +	1	69
12	44½	48	4	1 +	3½	73
13	40	44½	4½	5	½	76
1801. June 13	50	63	13	9 +	5	65
22	55	61	6	6 +	6	65
23	55	62	7	6	4½	65
24	55	61	6	5	3	67
Sept. 8	60	64	4	7	2	78
9	64	64¾	¾	5	3	74
10	58	64	6	7	2	70
12 o'clock, 10	63	64	1	2		

From a review of the preceding table it will be found, upon the whole, that when the water is warmer than the air, some increase of depression of the horizon may be expected: but that its quantity will be greatly influenced and in general diminished, by dryness of the atmosphere.

It appears, however, that no observable regularity is deducible from the measures above given; but that the quantity, on some occasions, is far different from what the states of the thermometer and hygrometer would indicate. On the 9th of September, for instance, the difference of temperature is only ¾°, and the evaporation, to counteract this slight excess of warmth, produced as much as 3° of cold; nevertheless, the refraction visible was full 5′. In this observation I think that I could not be mistaken, as the water was at the time perfectly calm,

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the air uncommonly clear, and I had leisure to pay particular attention to so unforeseen an occurrence.

This one instance appears conformable to the opinion entertained by Mr. Huddart, and by M. Monge, that, under some circumstances, the solution of water in the atmosphere causes a decrease in its refractive power; but on no other occasion have I been induced to draw a similar inference.

The object that I have at all times chosen, as shewing best the quantity of refraction, has been either an oar dipped in the water at the greatest discernible distance, or some other line equally inclined; and the angle measured has been, from the point where the inverted image is terminated by the water, to that part of the oar itself which appears to be directly above it. (The apparent magnitude of *e c*, Fig. p. 421.)

The eight first angles were taken with a mother-of-pearl micrometer in the principal focus of my telescope, and are not so much to be depended upon for accuracy as the succeeding eight. These last were measured with a divided eye-glass micrometer, and consequently are not liable to any error from unsteadiness of the instrument or object.

From the foregoing observations we learn, that the quantity of refraction over the surface of water may be very considerable, where the land is near enough to influence the temperature of the air. At sea, however, so great differences of temperature cannot be expected; and and the increase of dip caused by this variation of horizontal, refraction, it is to be presumed, is not so great as in the confined course of a river; but, if we consider that it may also be subject to an equal diminution from an opposite cause, and that the horizon may even become apparently elevated, there can be no question that the

the error in nautical observations, arising from a supposition that it is invariably according to the height of the observer, stands in need of correction.

The remedy employed by Mr. Huddart *, of taking two angles of the sun from opposite points of the horizon at the same time, and considering the excess of their sum above 180° as double the dip, must without doubt be effectual; but, from causes which he assigns, it is practicable only within certain limits of zenith distance; for, where the zenith distance is small, and the changes of azimuth rapid, there is required considerable dexterity and steadiness of a single observer who attempts to turn in due time, from one observation to another; and when it exceeds 30°, the greater angle cannot be measured with a sextant, and consequently his method is, with that instrument, of use only in low latitudes.

On account of the difficulty attending some of the adjustments for the back observation, he rejects that method for taking angles in general, with much reason; but he has thereby overlooked a means of determining the dip, which I am inclined to think might be employed with advantage in all latitudes, without any occasion to hurry the most inexperienced or cautious observer.

By the back observation, the whole vertical angle between any two opposite points of the horizon may be measured at once, either before or after taking an altitude. Half the excess of this angle above 180° should of course be the dip required.

But if it be doubtful whether the instrument is duly adjusted, a second observation becomes necessary. The instrument must be reversed, and, if the apparent deficiency of the opposite angle from 180° be not equal to

* Phil. Trans. for 1797, p. 40.

the excess before obtained, the index error may then be corrected accordingly; and, since the want of adjustment, either of the glasses at right angles to the plane of the instrument, or of the line of sight parallel to it, will affect both the larger and smaller angles very nearly in an equal degree, the $\frac{1}{4}$ part of their difference will be extremely near the truth, and the errors arising from want of those adjustments may with safety be neglected.

This method of correcting the index error for the back observation at sea was many years since recommended by Mr. Ludlam *; yet I do not find that it has been noticed by subsequent writers on that subject, or suggested by any one for determining the dip; but I can discover no reason for which it could be rejected as fallacious, and I should hope that in practice it would be found convenient, since in theory it appears to be effectual.

The most obvious objection to this, as well as to Mr. Huddart's method, is the possibility that the refraction may be in some measure different in opposite points of the horizon at the same time. When land is at no great distance, such an inequality may be found to occur; but, upon the surface of the ocean in general, any partial variations of temperature can rarely be supposed to exist; and it is probable, that under any circumstances, the difference will not bear any considerable proportion to the whole refraction; nor can it be thought a sufficient reason for rejecting one correction proposed, that there may yet remain other smaller errors, to which all methods are equally liable, but which it is not the object of the present dissertation to rectify.

* Directions for the use of Hadley's quadrant, 1771, § 82, p. 56.

On

On a rich and cheap Compost. By A. Hunter, *M. D.*

From Hunter's Georgical Essays.

In the Essay on the Nourishment of Vegetables *, I endeavoured to show that oil, made miscible with water, constitutes the chief nourishment of vegetables. A greater number of proofs might have been produced in support of that doctrine; but I flatter myself that those already advanced will be thought sufficient.

Having reason to believe that my theory was founded upon facts and experiments, I was desirous of converting it to public utility. And as I apprehended that a compost might be discovered, upon the principles advanced, which would come cheap to the farmer, and be of easy carriage, I diligently employed myself in prosecuting the inquiry.

In the course of investigation I took care to reason upon proper data; carefully avoiding every degree of partiality to my system.—In philosophy nothing is so delusive as prejudice.

After making various trials, I at last discovered what I so ardently sought after; but as I have not the vanity to think my experiments sufficiently conclusive, I embrace this opportunity to request the assistance of the practical farmer, in order that the merits of the invention may be fully determined.

Should my theory concerning the food of plants be found erroneous, the compost of course will be disregarded. But, on the contrary, should it be agreed to, that oil, under certain modifications, made miscible with water, constitutes the chief nourishment of vegetables,

* See the last Number of this work, page 349.

then

then the invention will probably become a subject for future experiment.

Though theory may direct our inquiries, yet experience must at last determine our opinions, for which reason I propose to enlarge my experiments; and as I have no other view but the investigation of truth, I shall lay them faithfully before the public, whether they prove successful or not.

We know that a number of experiments, made by different persons, and in different places, are essentially necessary towards establishing the truth of any received opinion in agriculture. How much more necessary is it to request the assistance of the practical farmer, in determining the merits of a new invention? for such I esteem the compost I here communicate.

Virgil, indeed, has recommended the lees of oil as a manure, and the ingenious Dr. Home has mentioned olive oil; but neither of them reflected upon the absolute necessity of rendering the oil miscible with water, by means of an alkaline salt.

I judge it unnecessary to repeat what I have already advanced upon the food of plants. I shall therefore refer the reader to the first Essay, as it contains the greatest part of the reasoning upon which the following compost is founded.

To make Oil-Compost.

	l.	*s.*	*d.*
Take North-American potash 12 lb.	0	4	0
Break the salt into small pieces, and put it into a convenient vessel with four gallons of water. Let the mixture stand forty-eight hours, then add course train-oil, 14 gallons	0	14	0
	£0	18	0

In

In a few days the salt will be dissolved, and the mixture, upon stirring, will become nearly uniform.

Take fourteen bushels of sand, or twenty of dry mould. Upon these pour the above liquid ingredients. Turn this composition frequently over, after adding to it as much fresh horse-dung as will bring on heat and fermentation; in six months it will be fit for use.

I apprehend that the above quantity will be found sufficient for an acre: my trials, however, do not give me sufficient authority to determine upon this point.

For the convenience of carriage, I have directed no more earth to be used than will effectually take up the liquid ingredients. But if the farmer chooses to mix up the compost with the mould of his field, I would advise him to use a larger portion of earth, as he will thereby be enabled to distribute it with more regularity upon the surface. I have not yet had any extensive trial of its efficacy upon pasture and meadow grounds: but I presume that whatever will nourish corn, will also feed the roots of grass. When used upon such lands, it should be put on during a rainy season, as all top-dressings are injured by the solar heat.

All kinds of cattle must be kept off the lands for some time, as they will bite the grass too close in quest of the salt contained in the compost, which I have found to be the case in small trials.

I shall here observe, that the oil-compost is only intended to supply the place of rape-dust, soot, woollen rags, and other expensive hand-dressings. It is in all respects inferior to rotten dung: where that can be obtained, every kind of manure must give place to it.

At the same time that dung affords nourishment, it opens the pores of the earth. Hand-dressings, on the contrary, give food to plants, but contribute little towards

wards loosening the soil. This is an useful and practical distinction, and may be applied through all the variety of manures made use of by the farmer.

I presume that the oil-compost resembles the natural food of plants; but I submit that, as well as every thing else, to experience, our unerring guide.

It may be objected, that it has not sufficiently under-done the *putrid ferment*, to attenuate the oily particles. The use of rape-dust, soot, horn-shavings, and woollen rags, take off that objection, and at the same time confirm the theory upon which the above compost is founded.

I do not take upon me to direct the experienced farmer in the manner of using this new compost. I would have every person apply it in the way most agreeable to himself. Many things will occur to the practical husbandman, that no reasoning of the philosopher could foresee. By attending to the different ways of using it, we may reap considerable advantages. Improvements may be collected even from the highest degree of mismanagement.

Facts must ever be the foundation of our reasoning. Without them the philosopher is a kind of *Ignis fatuus*. Instead of unfolding nature, he covers her with a cloud, and endeavours, as it were, to bring old Chaos back again into the world.

Should I presume to instruct the farmer in the management of the compost, I would recommend it to be sown immediately after the grain, and both harrowed in together.

The following experiment, though trifling in its own nature, gave me the first encouragement to prosecute the subject upon a larger scale.—I took four pots, Nos. 1, 2, 3, 4.

No. 1.

No. 1 contained 12 lb. of barren sand with 1 oz. of the sand oil-compost.

No. 2—12 lb. of sand, without any mixture.

No. 3—12 lb. of sand, with ½ oz. of slaked lime.

No. 4—12 lb. of sand, with 4 oz. of the sand oil-compost.

In the month of March I put six grains of wheat into each pot, and during the summer I occasionally watered the plants with filtered water. All the time that the plants were consuming the farina, I could observe but little difference in their appearance. But after one month's growth I remarked that No. 1 was the best. No. 2, the next. No. 3, the next. No. 4, much the worst.

In August I made the following observations:

No. 1 had five small ears, which contained a few poor grains.

No. 2 had three small ears, which scarce deserved the name of ears, containing a few grains, much inferior in goodness to the former.

No. 3 had no ears. Only I observed two very small ones within their respective sheaths, which, for want of vegetable strength, never made their appearance.

No. 4 had no ears; the stalks appearing stunted in their growth.

I removed the plants from their pots, and took a view of the roots of each.

No. 1. The roots tolerable large, and well spread.

No. 2. The roots not so large.

No. 3. The roots very short and small.

No. 4. The roots much the shortest, with the appearance of being ricketty.

Upon this experiment I remark:

1. That the oil-compost may be considered as a vegetable food; but that, when used too liberally, the alkaline

salt will burn up the roots of the plant, and hinder vegetation. For which reason I would recommend the compost to be exposed to the influence of the air for some months before it is laid on.

2. That lime contains no vegetable food, and is, in its own nature, an enemy to vegetation. It is, however, of excellent use in assisting vegetation, in the manner described in the Essay on the Nourishment of Vegetables.

My experiments teach me, that all kinds of soils may be benefited by this manure. The limestone, gravelly, sandy, and chalky soils seem to require it most. The rich loams and good clays have nourishment within themselves, and stand more in need of the plough than the dunghill.

It is observed by farmers, that rape-dust seldom succeeds with spring-corn unless plentiful rains fall within a few weeks after sowing. I have more than once made the same observation upon the oil-compost, which induces me to recommend it for winter crops only. From the unctuousness of its nature, it is more than probable, that it should lie exposed for a long time to the influence of the weather, which benefit it is deprived of when used for barley, and such crops as are sown late in the spring. I am confirmed in this idea from repeated experiments made with the compost upon turnips, which generally proved unsuccessful. But at the same time I invariably found that those parts of the field on which the compost had been spread produced the best crops of grain the following year. From this slow manner of giving its virtues, it seems to be an improper dressing for all plants that have a quick vegetation.

Agreeably to the theory advanced in the first Essay, I presume that all lands, which have been exhausted by frequent crops, are robbed of their oily particles, and consequently

consequently have become barren. The oil-compost, as it plentifully restores particles similar to those that are carried off, has a fair appearance of proving an excellent restorative. To lands under such circumstances lime alone is the worst manure that can be applied.

This last observation naturally leads me to wish for a general history of manures, upon sound and rational principles. I cannot help regarding that necessary part of husbandry as a subject but imperfectly understood. Whoever succeeds in that difficult task will prove himself a real friend to mankind. Without it agriculture must remain a vague and uncertain study.

Observations on the Process of Tanning.

By Mr. Davy.

From the Journals of the Royal Institution of Great Britain.

I. *On the Preparation of Skin for Tanning.*

IN all the processes for forming leather, the skins are depilated, and freed from flesh and extraneous matter before they are submitted to the action of the tanning lixivium. In some cases, when large skins are employed, a slight degree of putrefaction is induced, for the purpose of enabling the hair to be readily separated; but in general this effect is produced by a mixture of lime and water.

The process by putrefaction is so simple as to require no comment: the epidermis is loosened by it, and the cellular substance that constitutes the bulb of the hair softened in such a manner that it may be easily separated from the cutis or true skin.

When lime is employed, it has been generally supposed that it acts by destroying the epidermis, so as to render it soluble in water. This, however, does not appear to be the case: I exposed to two ounces of lime-water four grains of epidermis, separated from cow-skin, and which had been freed from loose moisture by blotting-paper; but, after five days, it appeared rather of larger volume than before; and instead of having lost any weight, I found that it had gained very nearly half a grain.

The epidermis has been supposed to consist of coagulated albumen. In comparing its properties with those of the coagulated white of the egg, there was a striking analogy perceived between them: both were soluble in the caustic alkalies by long exposure, and were acted upon by the acids.

In examining the circumstance of the action of lime-water, and of milk of lime, upon skin, I have always observed that the cuticle is rendered extremely loose and friable after the action: from which it is probably that it combines with the lime, so as to form an insoluble compound. This may be observed indeed in washing the hands with lime-water: the cuticle becomes extremely rough and dry; whereas, after the action of weak alkaline solutions, which form soluble compounds with it, it is found smooth.

Not only the epidermis, but likewise the soft matter at the extremity of the hair, is acted upon by lime; and this effect must tend considerably to facilitate the process of depilation. Likewise the fat and oily matter adhering to the skin, from saponaceous compounds with the earth, and these compounds are removed with other extraneous matter, before the skins are submitted to any new chemical agents.

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It has been proposed to use the residuum of the tanning lixivium or the exhausted ooze, for the purposes of depilation; but this liquor seems to contain no substances capable of acting upon the epidermis, or of loosening the hair; and when skin is depilated by being exposed to it, the effect must really be owing to incipient putrefaction.

Skins, after being depilated and cleansed, are in this country generally subjected to other processes of preparation before they are impregnated with the tanning principle.

The large and thick hides which have undergone incipient putrefaction, are introduced for a short time into a strong infusion of bark, when they are said by manufacturers to be *coloured*; and after this they are acted upon by water impregnated with a little sulphuric acid, or acetous acid formed by the fermentation of barley or rye. In this case they become harder and denser than before. and fitted, after being tanned, for the purpose of forming the stouter kinds of sole leather. The acids are capable of combining both with skin and with tannin; and it would appear that, in this process, a triple combination must be effected on the surface of the skin, though from theory one should be disposed to conclude, that the interior part could be little modified in consequence of the colouring, and the action of the acids.

The light skins of cows, the skins of calves, and all smaller skins, are treated in a very different way, being submitted for some days to the action of a lixivium, called the grainer, made by the infusion of pigeons-dung in water. After this operation they are found thinner and softer than before, and more proper for producing flexible leather. When the infusion of pigeons-dung is examined, after being freshly made, it is found to contain a little

little carbonate of ammonia, but in a short time it undergoes fermentation, when carbonic acid and hydrocarbonate are given out by it, and a small quantity of acetous acid formed. The alkali in the grainer may probably have some action upon the skin; it may be supposed to free it from any oils or calcareous soap that remained adhering to it: but the great effect probably depends upon the complicated process of fermentation, during which the skin loses its elasticity, and becomes soft; and it is found by tanners, that dung which has undergone fermentation is wholly unfit for their use.

I have tried several experiments on different substances, as substitutes for the pigeons-dung used in the grainer, but without gaining successful results. Very weak solutions of carbonate of potash and carbonate of ammonia seemed to soften considerably small pieces of skin that had been depilated by lime; but when they were tried by Mr. Purkis, in the processes of manufacture, the effects were less distinct. In the western counties of England the excrement of dogs is employed instead of pigeons dung, and culver, or the dung of fowls is in common use. The dung of graminivorous quadrupeds enters only slowly into fermentation, and it is not found efficacious in the process.

II. *On the Impregnation of Skin with the Tanning Principle.*

The tanning lixivium or ooze is generally made in this country, by infusing bruised or coarsely powdered oak-bark in water.

Skins are tanned by being successively immersed in lixiviums, saturated in different degrees with the astringent principles of the bark. The lixiviums first employed are usually

usually weak; but for the completion of the process they are made as strong as possible.

In the process of tanning, the skin gains new chemical properties; it increases in weight, and becomes insoluble in boiling water.

The infusions of oak-bark, when chemically examined, are found to contain two principal substances; one is precipitable by solution of gelatine, made from glue or isinglass; and gives a dense black with solution of common sulphate of iron. The other is not thrown down by solution of gelatine; but it precipitates the salts of iron of a brownish black, and the salts of tin of a fawn colour.

The substance precipitable by solution of gelatine is the tanning principle, or the tannin of Seguin. It is essential to the conversion of skin into leather, and in the process of tanning it enters into chemical union with the matter of skin, so as to form with it an insoluble compound. The other substance, the substance not precipitable by gelatine, is the colouring or extractive matter; it is capable of entering into union with skin, and it gives to it a brown colour; but it does not render it insoluble in boiling water.

It has been usually supposed that the infusion of oak-bark contains a peculiar acid, called gallic acid; but some late experiments render this opinion doubtful: and this principle, if it exists in oak-bark, is in intimate combination with the extractive or colouring matter.

In the common process of tanning, the skin, which is chiefly composed of gelatine, slowly combines in its organized form with the tannin and extractive matter of the infusions of bark; the greater proportion of its increase of weight is however owing to tannin, and it is from this substance the leather derives its characteristic properties;

but

but its colour, and the degree of its flexibility, appear to be influenced by the quantity of colouring matter that it contains.

When skin, in large quantity, is suffered to exert its full action upon a small portion of infusion of bark, containing tannin and extractive matter, the fluid is found colourless. It gives no precipitate to solution of glue, and produces very little effect upon the salts of iron, or of tin.

The tanning principle of oak-bark is more soluble in water than the extractive matter. And the relative proportion of tannin to extractive matter is much greater in strong infusions of oak-bark, than in weak ones; and when strong infusions are used for tanning, a larger proportion of tannin is combined with the matter of skin.

For calf-skins, and light cow-skins, which are usually prepared in the grainer, weak lixiviums are used in the first part of the process; but thick ox-hides, for the purpose of stout sole-leather, are generally kept in a strong ooze, preserved constantly in a state approaching to saturation, by means of strata of bark.

Calf-skins, and light cow-skins, in the usual process, require for their full impregnation with tannin from two to four months; but thick ox-hides demand from ten to eighteen months.

In any case the state of the skin with regard to impregnation with tannin may be easily judged of, if it be cut transversely with a sharp knife: in this case the tanned part appears of a nutmeg colour; but the unimpregnated skin retains its whiteness.

The tanned hides designed for sole-leather are, while drying, generally smoothed with a stout steel pin, and beat with a mallet. By this process they are rendered

denser,

denser, firmer, and less permeable to water: calf-skins are not subjected to the operation of beating; and they are treated in different ways by the currier, according as they are needed for different purposes.

III. *General Remarks relating to the Processes of Tanning.*

A very great number of vegetable productions, besides oak-bark, contain the principle essential to the conversion of skin into leather: galls, sumach, the bark of the Spanish chesnut, of the elm, of the common willow, and of the Leicester willow, the branches of the myrtle, tormentil, and heath, have all been used in the processes of tanning.

Different methods have been proposed for estimating the quantity of tannin in different vegetable productions. Tannin by being dissolved in water increases its specific gravity, and the hydrometer has been used for estimating the strength of the tanning ooze. The results given by this instrument are, however, often fallacious in comparative experiments, in consequence of the presence of extractive matter, and of saline substances; and the action of the solution of gelatine affords the best indication of the quantity of the tanning principle.

The solution of gelatine most proper for the general purposes of experiments, is made by dissolving an ounce of glue, or of isinglass, in three pints of boiling water.

The substance to be examined as to its tanning power, may be used in the quantity of two ounces; it should be in a state of coarse powder, or in small fragments. A quart of boiling water will be sufficient to dissolve its astringent principles.

The solution of glue, or gelatine, must be poured into the astringent infusion, till the effect of precipitation is at an end.

The turbid liquor must then be passed through a piece of blotting-paper, which has been before weighed.

When the precipitate has been collected, and the paper dried, the increase of its weight is determined; and about two-fifths of this increase of weight may be taken as the quantity of tannin in the ounce of the substance examined.

When solution of gelatine cannot be obtained, a solution of albumen may be used. It is made by agitating the white of an egg in a pint of cold water. It does not putrefy nearly so readily as the solution of glue, and it may be employed with equal advantage in experiments of comparison; but the composition of the precipitates it forms with tannin, has not as yet been ascertained.

The tanning principle in different vegetables is possessed of the same general characters; but it often exists in them in states of combination with other substances.

In galls it is in union with the gallic acid. In sumach it is mixed with saline matter, particularly sulphate of lime; and in the greater number of barks it is in combination with mucilage and different extractive and colouring matters.

Leather tanned by means of different astringent infusions differs considerably in its composition; but it seldom contains more than one-third of its weight of vegetable matter.

Gallic acid, and the saline matters in general, in cases when they are combined with tannin, are not absorbed with it by skin; but they remain in their primitive forms.

The leather made from infusions of Aleppo galls, and of sumach, is composed probably of pure tannin and the matter of skin. Its colour is very pale, and the increase of weight is greater than in most other cases.

Extractive,

Extractive, or colouring matters, in cases when they exist in astringent infusions, as in the instance of oak-bark already mentioned, are wholly or partly absorbed with the tannin by the skin. The leather from barks in general is coloured, and contains different proportions of extractive matter.

Of all the substances that have been examined as to their tanning properties, catechu or terra japonica, is that which is richest in the tanning principle. This substance is the extract of the wood of a species of the mimosa, which grows abundantly in India; and calculating on its price, and on the quantities in which it may be procured, there is great reason to believe that it may be made a valuable article of commerce.

In a paper published in the Philosophical Transactions for 1803, a statement is given of the comparative value of different astringent substances, oak-bark being considered as the standard.

The attraction of tannin for water is much stronger than that of any other of the principles usually found in astringent vegetables; and the saturated infusions obtained from substances containing very different proportions of astringent matters, are usually possessed of the same degree of strength with regard to their tanning powers.

When saturated solutions of the tanning principle are used in the process of manufacture, the leather is tanned in a much shorter time than in the common operation with weaker infusions. The rapid method of tanning has been recommended by M. Seguin; and is ably described in a pamphlet published by Mr. Desmond.

It has however been generally observed, that leather too quickly tanned is more rigid, and more liable to crack than leather slowly tanned. And there is every reason to

 believe

believe that its texture must be less equable, as the exterior strata of skin would be perfectly combined with tannin before the interior strata were materially acted upon; and the want of colouring or extractive matter in the strongest lixivium in many cases must affect the nature of the leather.

The substances used for tanning should, in all cases, be preserved in as dry a state as possible before they are used. When they are exposed to moisture and air, the tanning principle by degrees is destroyed in them, and for the most part converted into insoluble matter.

The process of drying bark by heat, when carefully conducted, must, as there is great reason to believe, on the whole be advantageous. The tanning principle is not decomposed at a temperature below 400°. And in fresh vegetable substances, tannin appears to be sometimes developed or formed by the long application of a low heat: this fact I observed with my friend Mr. Poole in September 1802, with regard to acorns; and I have since made the same remark upon the horse-chesnut.

Comparison of the French definitive Metre with an English Standard, brought from London by M. A. PICTET, *one of the Editors of the Bibliothéque Britannique.*

From the BIBLIOTHEQUE BRITANNIQUE.

THE measurement of the earth, and the investigation of its figure, were the subjects, at various times in the course of the eighteenth century, of the labours of a number of philosophers of the first eminence in different countries. Some Swedish Astronomers are now employed in a second measurement of the same degree which

which was measured sixty years ago by the French Academicians in Lapland, under the polar circle. In France, when the idea of seeking in the dimensions of the globe itself the unit to which all measures and weights might be referred, had once been conceived and adopted, it was necessary to make an effort proportional to the importance of an undertaking which was thus become national. In the midst of a long and sanguinary war, together with difficulties of every other kind, a chain of triangles has been formed between Dunkirk and Barcelona, comprehending the tenth part of the arc of the meridian which extends from the Equator to the pole, and which is equal to one-fourth of the circumference of the globe; and the ten millionth part of this arc, thus determined, has been adopted for the unit of the metrical system: it has been fixed by the construction of standards made of substances proper to resist the attacks of time; and by a careful examination of the precise relation of the length of the metre to that of the pendulum vibrating seconds, on the level of the sea, in a given latitude, the determination of this unit has been rendered independent of any accident that might destroy or impair the standards representing it; while in the formation of these standards all the precautions have been employed that could be suggested by the present improved state of natural philosophy, and of the arts.

In England, on the other hand, operations have been carried on for these five and twenty years, which are to be the foundations of an exact map of Great Britain. These labours, begun by the late General Roy, have been conducted with much sagacity and precision; and the results are likely to procure very interesting information respecting the figure of the earth. Sir George Shuckburgh, an eminent member of the Royal Society of London, has successfully employed himself in private, in researches intended to fix the precise length of the standards, which have served as bases for the measurements made in Great Britain.

It was therefore to be regretted, that operations so similar, conducted in two neighbouring countries, and capable of acquiring a new interest by comparison, should remain unconnected, for want of an actual standard of the measures of the one country, which might be transported into the other, after the definitive determination of the French measure. This regret we had deeply felt at various times when these objects were laid before our readers; and we may say with truth, that if the hope of procuring this medium of comparison was not the only motive of the journey to England that one of us has made, it at least greatly contributed to induce him to undertake it.

Our colleague took some steps in his passage through Paris, to obtain an authentic metre, in order to be submitted to the examination of the Royal Society, to which he has the honour of belonging, but he did not remain long enough in Paris to be able to succeed in this attempt. He took advantage of his longer stay in England, in procuring from the hands of Mr. Troughton, an artist celebrated for his accuracy in the construction and division of geometrical and astronomical instruments, a standard rigorously conformable to that which he had made for Sir George Shuckburgh, and with which this philosopher had compared the principal English standards. Our colleague procured also from the same artist the comparative apparatus of Sir George Shuckburgh, composed of two excellent microscopes, the one bearing a micrometer which divides the English inches into ten thousand equal parts. Upon his return to Paris he made haste to exhibit

exhibit these instruments to the minister of the interior, and to the National Institute. This learned body, nominated three of its members in order to proceed to the regular comparison of the definitive metre with the English standard. The undertaking, by no means so easy as it at first appeared, occupied the committee in five different meetings, of nearly four hours each; and it was performed with all the care and precaution that the nature of the subject required. M. Prony, who, as the translator of General Roy's memoir on the first trigonometrical operations in England, was particularly interested in these researches, acted as secretary to the committee, and it was at his house, and with the assistance of a comparative apparatus belonging to him, that the principle experiments were made. He has been so obliging as to furnish us with an authentic copy of the report made to the Institute, which was deemed of sufficient consequence to be read at the public sitting of the last quarter.

National Institute of Sciences and Arts. 6 *Nivose, Year* 10, (27*th December*, 1801.)

A member read, in the name of a committee, the following report on the comparison of the standard metre of the Institute with the English foot.

M. Pictet, Professor of Natural Philosophy at Geneva, submitted to the inspection of the class, in the month of Vendémiaire, an interesting collection of objects relative to the sciences and arts, which he collected in his journey to England.

Among them was a standard of the English linear measure, engraved on a scale of brass, of 49 inches in length, divided by very fine and clear lines into tenths of an inch.

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It was made for M. Pictet by Troughton, an artist in London, who has deservedly the reputation of dividing instruments with singular accuracy; it was compared with another standard made by the same person for Sir George Shuckburgh, and it was found that the difference between the two was not greater than the difference between the divisions of each; that is, it was a quantity absolutely insensible. This standard may therefore be considered as identical with the standard described by Sir George Shuckburgh, in the Philosophical Transactions for 1798.

M. Pictet also exhibited to the Institute a comparer, or an instrument for ascertaining minute differences between measures, constructed also by Mr. Troughton. It consists of two microscopes with cross wires, placed in a vertical situation, the surface of the scale being horizontal, and fixed at proper distances upon a metallic rod. One of them remains stationary at one end of the scale, the other is occasionally fixed near to the other end; and its cross wires are moveable by means of a screw, describing in its revolution $\frac{1}{100}$ of an inch, and furnished with a circular index, dividing each turn into 100 parts; so that having two lengths which differ only one-tenth of an inch from each other, we may determine their difference in ten thousandths of an inch. The wires are placed obliquely with respect to the scale, so that the line of division must bisect the acute angle that they form, in order to coincide with their intersection. General Roy has described, in the 75th volume of the Philosophical Transactions, a similar instrument made by Ramsden, for measuring the expansion of metals.

M. Pictet offered to the class the use of the standard, with the micrometer described, for the determination of the comparative length of the metre, and the English

foot;

pound of good wood-ashes sifted, and well beat up the matter.

The vat must then be covered for eighteen hours; after which half uncover it, and when the temperature encreases uncover it entirely after six hours more; if attention were not paid to this particular, a great part of the spirit would be volatilised. If a white froth appear upon the surface it is a sign that the fermentation proceeds as it ought. Forty-eight hours after the mixture of the paste, the pulp begins to rise; this disposition is promoted by gently stirring the matter, and covering the vat a second time. In seventy two to ninety hours, the whole becomes clear, and the fermentation is finished.

The vats must not be larger than to supply one or at most two distillations. The distilled liquor is rectified every day to half proof, and only to whole proof when there is sufficient to fill the still.

For distilling in summer, take a sixth part less of corn to the quantity of water prescribed, that the mixture may be thinner; and, after the addition of the cold water, the degree of temperature should not at the highest exceed 8° or 9° of Reaumur. It is of very great advantage, in those situations where it can be done, to surround the distillery with water, to the height of a foot, in stormy and in very hot weather. This water absorbs the heat, which would otherwise strike to the matter, hasten the fermentation too much, turn the matter sour, and occasion a considerable loss of spirit.

 Descrip-

Description of a Pyrometer of Platina; invented by M. GUYTON.

From the ANNALES DE CHIMIE.

AT the meeting of the National Institute, on 26 Floreal (May 16), M. Guyton exhibited an instrument for measuring the highest degree of heat produced in furnaces.

It consists of a rod or bar of platina, placed edgewise in a groove, made in a bed of refractory clay. One end of this bar rests upon the brick-work that surrounds the groove; the other end bears upon a lever with two arms, the largest of which forms an index or needle upon an arch of a graduated circle; so that the movement of this index shews the encrease in length of the bar of metal by the heat.

The bed of clay having been burned in the most intense degree of heat, no apprehension need be entertained of its shrinking; and the dilatation it might experience during the time of incandescence, would affect only the very small distance of the needle's centre of motion from the point of contact with the bar, that is, so as to diminish the effect rather than augment it. The whole of this instrument being of platina, it is liable neither to fusion nor oxydation.

As to its dimensions, the author thinks they ought to be reduced as much as possible, only leaving them of the magnitude necessary for distinctly marking the variations, in order to render its use convenient and certain; *convenient*, for the facility of placing it beneath a muffle, or under a crucible reversed; *certain*, by reason of the dimunition of accidents arising from inequality of heat, which it is impossible to avoid to a certain extent, even amidst a large mass of fire.

The

The variations will be sufficiently perceptible if the operator is able not only to calculate but to judge exactly of the prolongation of the bar, $\frac{1}{200}$ of a millimetre (about $\frac{1}{451}$ of a line), and this the author effects by the proportions he has adopted.

The bar is 45 millimetres in length, 5 broad, and 2 thick. The arm of the lever, which rests upon the extremity of this bar, is 2,5 millimetres in length, and the other, which acts as a needle upon the arch of the graduated circle, is 50 millimetres, or twenty times as long as the former. Thus the distance, which the shorter arm is displaced by the dilatation of the bar, will be multiplied in the proportion of one to twenty.

As the larger arm has a nonius, which shews on the same graduated circle the tenth parts of a degree, we thus have distinctly $\frac{1}{200}$ of one of these degrees.

Lastly, as the decimal division of an arch of a circle, whose radius is 50 millimetres, gives only 7,8538 decimillimetres for one of these degrees, it is evident that we may measure a prolongation of ,078538 decimillimetres or $\frac{1}{3730}$ of the length of the bar.

As it might happen that in drawing the instrument from the furnace, the motion might change the position which the dilatation had given to the needle, a spring of platina has been contrived at its extremity to keep it in its situation.

The author has begun a series of experiments to prove the accuracy of this pyrometer, to compare it with Wedgwood's, and thus determine what degree of reliance can be placed on those instruments, the manner of using them, and the cases in which they may be advantageously employed in scientific researches and in the arts.

Manner of making Vinegar with the Refuse of Beehives after the Honey is extracted.

By M. LOMBARD.

From the BIBLIOTHEQUE PHYSICO-ECONOMIQUE.

WHEN the honey is extracted from the combs by means of pressure, take the whole mass, break and separate it, and into each tub or vessel put one part of combs and two of water; place them in the sun, if his rays possess sufficient power, or in a warm place, and cover them with cloths. Fermentation takes place in a few days, and continues eight to twelve days, according to the higher or lower temperature of the situation in which the operation is performed. During the fermentation, stir the matter from time to time, and press it down with the hands, that it may be perfectly soaked. When the fermentation is over, put the matter to drain upon sieves or strainers. At the bottom of the vessels will be found a yellow liquor, which must be thrown away because it would soon contract a disagreeable smell, which it would communicate to the vinegar. Then wash the tubs, put into them the water separated from the other matter; it immediately begins to turn sour; when the tubs must be again covered with cloths, and kept moderately warm. A pellicle or skin is formed on their surface, beneath which the vinegar acquires strength; in a month's time it begins to be sharp; it must be left standing a little longer, and then put into a cask, of which the bung-hole is left open, and it may then be used like any other vinegar.

Account of Experiments shewing that violent Conflagrations may be extinguished by very small Quantities of Water by Means of a portable Hand-Engine.

By M. Van Marum.

From the Annales de Chimie.

A SWEDE, named Van Aken, nine years ago, publicly shewed at Stockholm, Copenhagen, and Berlin, that he could very expeditiously extinguish fires by a small quantity of a liquor, denominated anti-incendiary, and which he for some time kept secret. Having seen in the journals that M. Van Aken had repeated his experiments with great success at Berlin, in the presence of some of the members of the Academy of Sciences, I wrote to the celebrated M. Klaproth, requesting him to communicate to me the composition of M. Van Aken's anti-incendiary liquor if he was acquainted with it, with the intention of ascertaining the merit of the invention by an experiment on a large scale. For this purpose, as soon as M. Klaproth had communicated to me the method of preparing it, I caused a quantity to be made under my own inspection. It consists of a solution of 40 lbs. of sulphate of iron and 30 lbs. of sulphate of alumine, mixed with 20 lbs. of red oxyd of iron (colcothar) and 200 lbs. of clay. I then began to make comparative experiments by setting fire to two masses of combustibles, equal in every respect, and by extinguishing one of them with Van Aken's liquid and the other with common water. I was surprised to see, in several trials, that by using the two liquids in the same manner, the fire was always more expeditiously extinguished by water than by the anti-incendiary liquor; but I observed, at the same time, that

that a very small quantity of water, when properly directed, extinguished an extremely violent conflagration. My first experiments on this subject led me to make others on a larger scale; I shall take notice only of the latter.

I took two barrels which had contained pitch, and the inside of which was still covered with that inflammable substance. I took out the top and bottom of each, and, to give increased power to the flames, I altered them to a conical figure, twenty inches in diameter at the upper end, and sixteen inches at the other. This I placed on an iron frame, about three inches from the ground, that a free current of air, rising through the barrel, might render the flame as fierce as possible. I put a fresh covering of pitch over the inside of each barrel, and by means of shavings set fire to them one after the other. I began to extinguish the fire when most violent. For this purpose I employed an iron ladle, containing two ounces of water, and provided with a very long handle, as the heat of the fire kept me at the distance of four or five feet. I carefully poured the water out of the ladle in very small streams over the inside of the barrel, applying it to the edge, and moving it along the edge, according as the flames ceased. In this manner the first ladlefull put out nearly half of the fire; and what remained was extinguished by the second, applied in the same way.

The uncommon success of this experiment induced me to repeat it in the presence of several persons; and, by practice in the economical employment of water, I have more than once been able to extinguish a pitched barrel in a state of the most complete conflagration by a single ladlefull, consisting of two ounces of water.

It must at first appear surprising, that so small a quantity of water can extinguish such a violent fire. But the

the reason will easily be conceived upon reflecting that the flame of any burning substance must cease, according to well-known principles and experiments, as soon as any cause prevents the atmospheric air from touching its surface: thus, when a small quantity of water is thrown upon a body in a state of violent conflagration, this water is at first partly reduced to vapour, which, rising from the surface of the burning substance, repels the atmospheric air, and consequently represses the flame, which, for the same reason, cannot again appear whilst the production of the vapour continues.

From these experiments it appears that the art of extinguishing a violent conflagration with very little water consists in throwing it where the fire is most powerful, so that the production of vapour from the water, by which the flames are smothered, may be as abundant as possible; and in proceeding to throw the water on the nearest inflamed part, as soon as the fire ceases in that where you began, till you have gone over all the burning parts as expeditiously as possible. In thus regularly following the flames with the water, they may be every where extinguished before the part where you began has entirely lost, by evaporation, the water with which it was wetted, which is frequently necessary, to prevent the parts from taking fire again: after the flames of a burning body are extinguished, it cannot again take fire, for the above-mentioned reason, till all the water thrown upon it be evaporated.

Being convinced by these experiments, that very little water may suffice for extinguishing ordinary conflagrations, particularly at their commencement, I have endeavoured to convince many of my fellow citizens of it by repeating the experiments just described; and I have advised the procuring of small portable engines to be be used in cases of necessity. Many followed my advice immediately, and after their good effects had been seen, in some cases their number increased more and more in many of the towns of Holland, especially after the experiment which I made here in May, 1797, to shew upon a larger scale the advantages that may be derived from a judicious application of water to extinguish even the most furious conflagrations by means of portable engines, with a very small quantity of water. The experiment was the following:

I constructed a shed of dry wood, forming a room twenty-four feet long, twenty wide, and fourteen high, having two doors on one side, and two windows on the other. This shed was provided with the wood-work of a roof, but was not covered, and stood about six inches from the ground, that there might be a thorough current of air to increase the fierceness of the flames when the building should be set on fire. The inside of it was completely covered with pitch, and lined with straw, which was likewise pitched. To this straw lining I fastened wood-shavings, and cotton dipped in oil of turpentine, to set fire to the whole inside of the shed at once. Soon after the fire was applied, the flames being increased by the wind, were every where so violent that all the spectators thought they could not possibly be extinguished. I however succeeded in about four minutes, by the method already described, with five buckets of water, part of which was wasted through the fault of those who assisted me, as the following experiment proved.

I invited but very few to be present at this first experiment on the 8th of May, but on the 11th I repeated it in the presence of a very numerous company, after repairing and restoring the shed to its original state. The fire was not less violent than in the preceding experiment.

ment. I then directed the water myself, without any assistance and effectually extinguished the fire in three minutes, having used only three buckets of water, each containing about four gallons and a half.

Being at Gotha, in July, 1801, the Duke and Dutchess of Gotha pressed me, at their expence, to repeat the experiment, of which they had seen the details in the German journals, that it might be made more generally known in that part of Germany, where, as in other countries, great injury is sometimes sustained from conflagrations, because the people know not how to employ judiciously the small quantity of water they have at hand. The obliging manner in which their highnesses requested me to repeat the experiment, and my wish to make it of more general utility induced me to undertake it. The celebrated astronomer Von Zach was likewise present, and drew up the account inserted in a German periodical publication, intituled, "Reichs Anzeiger," of 6th August, 1798.

M. Lalande arrived at Gotha four days after the experiment, and was informed of its result. He mentioned it, as he lately informed me, soon after his return to Paris, to the National Institute, but he at the same time told me, that doubts were entertained of the truth of his narrative. To remove all doubts on this head, I shall annex the following account of the experiment, drawn up by the celebrated astronomer of Gotha, and inserted by him in the above-mentioned periodical publication.

"Doctor Van Marum having made some stay at Gotha in the course of a literary tour in Germany, in 1798, the Duke of Gotha, known as an amateur of the mathematical and physical sciences, expressed a wish that he would exhibit, on a large scale, an experiment of his method of extinguishing fire, the effect of which M. Van Marum

Marum had shewn by extinguishing, by means of a ladle-full of water, a pitched barrel, which he had set on fire. A shed of old, and perfectly dry, wood was in consequence erected, under the direction of M. Van Marum, in front of the dutchess's garden. Its dimensions were in every respect equal to that which served for the same experiment at Harlem, being twenty-four feet long, twenty wide, and fourteen in height. There were two doors on the north-east side, and two large apertures, in the form of windows, on the north-west side. The top was quite open, to give the flames a free passage.

"The inside of this shed was covered with pitch, and afterwards with straw mats, plentifully besmeared with melted pitch. To the bottom of these straw mats were fastened cotton wicks, dipped in spirits of turpentine, that the place might take fire in every part at once. In consequence, the fire being considerably increased by the wind, was at first so powerful, and the flames enveloped in thick clouds of smoke, rose with such violence, to the height of several feet above the opening of the roof, that the nearest spectators were obliged to retire precipitately, and many of them declared that it would be impossible to extinguish the conflagration, and that the shed would be entirely reduced to ashes. When the straw mats were completely consumed, the wood of the shed was soon in flames in every part. The circumstances under which this experiment was made were highly unfavourable; for the wind drove the flame exactly out at the doors on the north-east side, at which the water for extinguishing it was to be introduced. But notwithstanding this, M. Van Marum placed a small portable engine before the door, nearest the south-east side, without regard to the fears and opposition of his assistants, and ordered it to be worked there, stationing himself as near

near as the heat of the fire would permit him; he first directed the water to the south-east side, as near the door as possible, and as soon as the flame was extinguished in one part he guided the water to another. He then directed it along the north-east side, so that in a few minutes the flames were completely extinguished on those two sides. The engine was then placed before one of the apertures made in the form of windows, on the north-west side. He in a very short time extinguished the south-east side, and then coming to the middle of the shed, which was still on fire in several places in the crevices of the planks and the holes made by the nails, he completely extinguished the fire, which from time to time broke out again in small flames, and this terrible conflagration was entirely got under. According to the calculation of several of the spectators, the fire was extinguished in three minutes, at most, after the engine began to work. It is true the flames broke out again in several places, but they were of so little consequence that they were extinguished by means of wet rags fastened to a stick. Before the engine began to work, the reservoir was filled at two different times with two buckets of water. But in the removal of the engine to the first aperture or window of the shed, and afterwards to the middle of it, a considerable quantity of water, that may be estimated at nearly a pailfull, was spilt; so that it may with truth be asserted, that this violent conflagration was extinguished by three buckets of water, exclusive of what was afterwards used to extinguish those parts of the shed that remained red. When the fire was out, every one could see that it was not only the matted straw which had been burned, but that the wood, of which the building was constructed, had been so completely on fire that the space of an inch could not be found that had not

 been

been burned to a greater or less depth. The north-east side in particular, against which the wind had driven the flames with the greatest violence, was entirely charred. The experiment made at Gotha differs materially from that at Harlem in the following particular, that the flames and thick smoke that issued from the doors, rendered the approach to the shed with the engine extremely difficult at the former place, so that it was only by persuasion, and the courageous example he himself set, by placing himself always in front with the engine-pipe, that M. Van Marum could induce his assistants to approach the danger they so much dreaded."

From what has been already stated, it results, that in the application of this method of extinguishing fire, the whole art consists in attending to what follows: that to stop the most violent flame it is necessary only to wet the surface of the burning substance where the flame appears, and for this purpose only a small quantity of water is required, if it be applied with judgment to the burning part. Thus the point to be attended to in extinguishing a fire is to direct the water so that the whole surface of the burning part may be wetted and extinguished, and that in such a manner that no extinguished spot may be left between two others that are on fire: for if attention be not paid to this particular, the heat of the flame burning here and there, rapidly changes into vapour the water with which the extinguished wood has been wetted, and it again takes fire. Therefore, to extinguish fire of every kind, and in whatever manner it may have happened, nothing more is necessary than to apply to the burning part a sufficient quantity of water to wet its surface.

On

On some Experiments made with a Magnetic Needle, in order to distinguish immediately a Bar of Iron from a Bar of Steel. By P. Torelli di Narci.

From the Journal des Mines.

I MADE two small square magnetic bars, four inches long, and two, or two and a half, lines on each side; and in order to try their attractive and repulsive force, I held them to a magnetic needle, about three inches in length; I judged, by the distance at which they acted, of their greater or less degree of magnetic power.

I had at hand some bars of steel, of different forms, sizes, and lengths, which had never been polished, tempered, or rubbed with the loadstone. I held one of these bars to the magnetic needle, to see at what distance it would act, but I found it inconsiderable in comparison to the magnetic bars which I had made.

At this moment the idea occurred to me to repeat the experiment of the polarity of a common bar of iron, to see at what distance that would act.

This experiment consisted, as every one knows, in holding alternately to the two extremities of a magnetic needle, fixed on its pivot, the ends of a bar of iron, eighteen or twenty-four inches in length, more or less, which is held in a vertical position, and which, by this alone, is instantly rendered magnetic, and thus acquires polarity (if that expression may be used to signify that it acquires two poles); so that if the lower extremity of the bar of iron be held to the end of the needle marked N, which turns towards the north, it is repelled, which shews that this lower extremity is the north pole of the bar when placed vertically. If the iron bar be lowered, still keeping it in a vertical position, and if without derang-

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ing the needle, which must be placed on the edge of a table, the other extremity or upper end of the bar of iron be held towards it, attraction then takes place, and this extremity is found to be the south pole of the bar.

If the bar be reversed, the poles instantly change by this alteration; which may be ascertained by presenting it in this new position to the magnetic needle.

This experiment, which is well known, proves that by the vertical situation a bar of iron becomes an artificial magnet.

I resolved to repeat it with the bars of unwrought steel which I had in my possession, but I obtained results so different and so various that I thought it would be interesting to begin these experiments again in some order, for the purpose of comparing them with each other. One of their results is, that they have furnished me with a very simple method of immediately distinguishing a bar of iron from a bar of steel. A bar of iron acquires polarity by its vertical position alone; but with steel the case is otherwise, as the following experiments evince.

Care must be taken that the bar of steel employed shew no sign of magnetism, which may easily be discovered by plunging it into iron filings, or by alternately presenting each extremity of the bar of steel to the two ends of the magnetic needle.

It frequently happens that steel acquires the magnetic property by a simple shock, or a slight friction; but the experiments which I am about to describe will serve to shew whether the bar of steel that is intended to be tried has obtained the smallest degree of magnetic power or not.

I took a square bar of steel, about twelve or fourteen inches long, by two, or two and a half lines in thickness, which

which gave no indication of magnetism; I held it vertically, and presented the lower end to the north pole of the needle; attraction took place, which proves that this lower extremity of the bar of steel had not become the north pole by the vertical situation. I reversed the bar of steel, and obtained the same result, that is, there was an attraction; I lowered it vertically, for the purpose of bringing its upper extremity near the north pole of the needle, fixed on its pivot, and placed on the edge of the table; I still found that there was attraction. I then presented the same extremity of the steel bar, still holding it vertically to the south pole of the needle; it was again attracted; which proves that this bar had not acquired any polarity, and likewise that it afforded no sign of magnetism, nor consequently of polarity; for if a bar of steel obtain, in any manner whatever the smallest degree of magnetic power, the poles immediately shew themselves.

To prove that it is the vertical situation alone which instantly gives poles to a bar of iron, and makes it a kind of artificial magnet, I shall mention an experiment that I have made, and which I have no where seen described. The idea of making it arose from a desire to discover whether a bar of steel, which had afforded signs of attraction and repulsion, when held vertically, actually had poles.

After having proved the polarity of a bar of iron held vertically, and which I had reversed several times, to change its poles, I wished to ascertain whether it retained any sign of magnetism; for this purpose I held my bar of iron horizontally in the same manner, and at the same height, as my magnetic needle; I kept the bar nearly east and west, that is, perpendicular to the north and south direction of the needle; I advanced it by degrees

till

till I was near enough to act upon it, and attraction took place. I varied the experiment by reversing my bar, and presenting it alternately to the two extremities of the magnetic needle; it still produced attraction, which is an evident proof that this bar possessed no magnetic virtue, and that its magnetism and polarity were owing to its vertical position alone.

Another experiment, which follows, confirms this assertion. I presented the bar of iron, holding it horizontally to the north extremity of the magnetic needle; I slowly brought it nearer, till I perceived a commencement of attraction, when, without deranging the extremity of my bar of iron, next to the needle, I raised up the other end, which was farthest from it, so as to make it describe the arch of a circle, whose centre was at the extremity of the bar next to the needle. I then observed, that in proportion as I raised the extremity of my bar of iron, and it traversed the arch of 90 degrees, to pass from the horizontal to the vertical line, the attraction which existed between the extremity of the bar and the north pole of the needle was converted by degrees into repulsion; and it was sufficient for me to describe an arch of 20 degrees with my bar before I began to perceive the repulsion. I cannot positively say whe- the maximum of repulsion is obtained by the vertical situation; it appeared to me to increase till about 70 or 80 degrees, where it stopped. These are delicate experiments, and, to appreciate their effects, it requires instruments appropriated to the purpose, and skilful persons to manage them.

By reversing this experiment, that is, placing the whole apparatus in the same manner, but lowering the extremity of the bar of iron farthest from the magnetic needle, instead of raising it, and describing with it the

fourth

fourth part of a circle, below, quite contrary effects are produced, that is the attraction is more and more augmented.

If the experiment be repeated with the south end of the needle, you obtain the same results reversed.

The following are the experiments I have made with bars of steel, which afforded some signs of magnetism.

The first bar of steel which I employed for the purpose of repeating the experiments, which prove that iron instantly acquires polarity by its vertical position alone, and to ascertain whether the same were the case with steel, convinced me that it did not possess the same property.

I was desirous of repeating this experiment with other bars of steel, of different forms, sizes, and lengths. I took some round pieces of steel, three, four, and up to six lines in diameter, and from fifteen to eighteen inches long. I likewise took bars, twelve or fifteen inches in length, and three or four lines on each side; in a word, any that I happened to have in my laboratory, form and magnitude being objects of perfect indifference.

I found some of these bars, which, upon the first experiment, that is, upon being held vertically, and bringing the lower end near the north pole of the needle, shewed an evident repulsion. This at first disconcerted me. I turned the bar the contrary way, and attraction took place; which convinced me that the bar of steel had begun to acquire a magnetic power, which had given it poles. I continued my experiments, presenting (as in those made with the bar of iron, and described above) the extremities of the bar of steel, held in a horizontal position, to the north end of the needle. I then, by alternately changing the end, obtained attraction or repulsion, according as the same or the opposite poles of the bar

bar of steel and the magnetic needle were approached to each other.

Among the bars of steel, of the forms, sizes, and lengths above mentioned, which I tried, I found only three which gave no indication of the commencement of magnetism, and with which the experiments just related perfectly succeeded. With regard to those which indicated the commencement of magnetism, and consequently of polarity, from the experiments in which they were employed, it may be concluded that they were of steel, since they had acquired and preserved a sufficient degree of magnetism to discover their poles; a property not possessed by iron, which neither preserves its polarity nor magnetism in any other than a vertical position, and in which that property is so unsteady, that if you reverse a bar of iron it is sufficient to change the poles, and, by placing it horizontally, it loses all its polarity and magnetic power.

From the experiments just described, I think it may be concluded, that a bar of iron may easily be distinguished from one of steel by presenting it to the magnetic needle.

If the bar which is tried instantly acquires polarity by being placed vertically, which is seen by the attractions and repulsions observed in the course of the experiment, and if it lose that polarity when held horizontally, which is easily discovered, because in this latter position whatever end is presented, and whichever pole of the magnetic needle is approached, the phenomenon of attraction will always be observed; if, finally, upon turning it, the pole be changed, and that as often as the bar is reversed, it may confidently be pronounced a bar of iron.

If,

If, on the contrary, the bar submitted to the same experiments shews no sign of polarity, and attract indifferently the two extremities of the needle, in whatever manner and situation it be presented, then it may be concluded that the bar which is tried is of steel.

If this same bar shew signs of polarity, and in whatever situation it be presented, the phenomena of the attraction of the opposite poles of the needle and the bar, and the repulsion of the same poles, invariably and uniformly take place, it may thence likewise be concluded that the bar is of steel, for steel alone possesses the property of acquiring and preserving, for a very long period, even when not tempered, the smallest degrees of magnetism.

Intelligence relating to Arts, Manufactures, &c.

(Authentic Communications for this Department of our Work will be thankfully received.)

New Metallic Alloy called Palladium.

IN a paper lately read before the Royal Society, by Mr. Chenevix, concerning the nature of a metallic substance lately sold in London, as a new metal, under the title of Palladium, Mr. Chenevix describes the properties of an alloy of platina and mercury, which was described by the person who first made it as a new noble metal, and exposed by him for sale at Mr. Forster's, in Gerrard-street.

Palladium, or the new alloy, resisted all the attempts that were made to decompose it, and Mr. Chenevix was led to the discovery of its nature only by synthetical experiments. It is produced by different methods, but all

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of them are in some measure capricious. One of the most simple is by the action of a solution of green sulphate of iron upon a mixture of a solution of mercury and a solution of platina. In the process that succeeded best, heat was applied.

The palladium sold by Mr. Forster, which is perhaps the most perfect combination of mercury and platina, is of a specific gravity about 11; but different solid alloys, containing mercury and platina, of different specific gravities, between 11 and 15, were obtained by Mr. Chenevix.

The palladium and the alloys that Mr. Chenevix obtained most resembling it were of a dull grey colour, approaching to that of platina; when palladium was acted upon by fire, its tint changed like that of steel. At an intense white heat it entered into fusion, and burned when in contact with oxygene. It combined with sulphur at a moderate heat.

It was acted upon by all the mineral acids; a solution of it may be made in the nitric acid, which is of a bright red colour.

It was precipitated from its solutions in an oxydated state by the alkalies and earths, and generally of an orange tint.

The solutions of the alloy gave a deep orange or brown precipitate to muriate of tin, an olive coloured one to prussiate of potash, and to sulphurated hydrogene gas a dark brown one.

In no way could the mercury and platina, after they had once entered into union, be separated from each other; but, from his most accurate experiments of composition, Mr. Chenevix concludes, that the two metals are to each other nearly as one to two.

After

After having completed the account of his researches upon palladium, Mr. Chenevix gives the history of some experiments on the affinity between the metals, which he thinks prove,

1. That gold has an attraction for mercury, for antimony, and for arsenic.
2. That platina has an affinity for silver, for mercury, and for antimony.
3. That silver has an affinity for mercury. And,
4. That mercury has an affinity for copper, for lead, and for arsenic.

In the course of some observations that he made upon the salts of platina, he mentions that the muriate of tin is the most delicate test for this metallic body. It acts in the smallest quantity upon its muriatic solution, and renders it of a bright red colour.

Mr. Chenevix anticipates the objections that might be brought forward against his opinions, in consequence of the small specific gravity of palladium, which is much less than that of either of the metals supposed to compose it, and on this subject he mentions some instances in which very great changes of specific gravity arise from combination; and remarks, that we have no right to disbelieve a fact because no parallel instance can be adduced, as such a conduct would be raising an insurmountable barrier against the progress of science; it would be setting up our own feelings in the place of nature.

Gunpowder.

In another paper, read at the Royal Society, from Mr. Roebuck, of Madras, communicated by Mr. Watt, Mr. Roebuck states, that nitre loses something by being washed, which renders it less proper for forming gunpowder.

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He likewise states, that gunpowder much stronger than that usually made may be formed by adding nitric acid to the nitre employed. The proportion is one ounce of strong nitric acid to ten pounds of nitre. A solution is made, and the nitre is recrystallized. In Madras two kinds of charcoal are made use of for gunpowder, the one from the dolichos saja, the other from the euphorbia tirucalli.

The proportions are,

Nitre	45
Charcoal of the first kind	8
Sulphur	7

Or,

Nitre	45
Charcoal of the second kind	10
Sulphur	5

The two kinds of gunpowder are, according to Mr. Roebuck, of the same strength, and superior to the powder in use in the king's navy.

Ship's Log.

M. Seguin has invented a new log, by means of which he hopes to obtain the distance a ship goes with greater accuracy than by the common log. The latter, it is well known, consists of a triangular frame, to which is fastened a line with knots, a certain number of fathoms distant from each other. The pilot throws this frame into the sea, lets the line run out for half a minute, and then counts the knots. If three knots run out in that time, the vessel proceeds at the rate of a league an hour, and so in proportion.

proportion. The new log is furnished with a wheel, which is set in motion by the water. An index, which goes through the divisions, points out the distance which the ship has gone *.

The public is already indebted to M. Seguin for another instrument, invented in 1790, and approved by the commissioners of longitude, of Amsterdam. The latter is a compass with four legs, which serves to reduce, to the true distance, the apparent distance of the centre of the moon from the centre of the sun or a star, and consequently to simplify the calculations for obtaining the longitude.

New Method of Tiling Buildings.

M. Cathala, a French architect, has invented a new method of employing tiles for the roofs of houses; by which one half of the quantity usually required for that purpose is sufficient. The tiles are to be made of a square instead of an oblong form. The hook by which they are fastened is at one of the angles, so that when fastened to the laths, they hang down diagonally, and every tile is covered one-fifth part, on two sides, by the superior row †.

* Several patents have been published in this work for improvements in ships' logs, and seemingly upon the same principle as that described by M. Seguin.

† A gentleman in this country (Mr. Chiffney) some little time since obtained a patent for laying slates in a similar manner.

List of Patents for Inventions, &c.

(Continued from Page 400.)

CHESTER GOULD, of Red Lion-street, Clerkenwell, Middlesex, Gentleman; for an hydrometer on a new principle, for the purpose of ascertaining the strength of spirits, and determining the specific gravity of fluids. Dated September 3, 1803.

JOHN ISAAC HAWKINS, late of Bordenton, in the United States of America, now residing in King-street, Clerkenwell *, Middlesex, Merchant; for machinery and methods for writing, painting, drawing, ruling lines, and other things; and for applying part of the aforesaid machinery to other purposes. Dated September 24, 1803.

ROBERT RANSOME, of Ipswich, Suffolk, Iron-founder, being one of the people called Quakers; for a method of making and tempering cast-iron plough-shares, and other articles of cast-iron for agricultural uses. Dated September 24, 1803.

* We are requested to state that Mr. Hawkins has since removed to No. 21, Pall Mall, where he exhibits the above inventions.

END OF THE THIRD VOLUME, SECOND SERIES.

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