III. *Magnetical Experiments and Obſervations.* *By Mr.* Tiberius Cavallo, *F. R. S.*

The Lecture founded by the late Henry Baker, Eſq. F.R.S.

Read Nov. 16, 1786.

THE Bakerian Lecture, which laſt year I had the honour to deliver to the Royal Society, contained the account of ſome magnetical experiments, particularly concerning the magnetiſm of braſs, from which it appeared, that moſt braſs becomes magnetic, ſo far as to attract the magnetic needle, by being hammered, and loſes its magnetiſm by annealing or ſoftening in the fire; but that there is ſome braſs, which poſſeſſes no magnetiſm naturally, nor acquires any by hammering.

Several experiments, made ſince the reading of that paper, having ſhewn a few particulars, which tend to correct what was advanced in the ſaid Paper, I ſhall in the preſent lecture mention, firſt, thoſe particulars, and ſhall then proceed to relate other experiments and obſervations relating to other branches of the ſame ſubject of magnetiſm.

In performing the experiments on the magnetiſm of braſs, I generally uſed a magnetic needle ſuſpended in a peculiar manner, as it is deſcribed in my laſt lecture; *viz.* a common ſewing needle, or a piece of ſteel wire rendered magnetic, and ſuſpended fuſpended by means of a chain of hair; which ſort of ſuſpenſion I find not only from the experiments then made, but alſo by ſeveral ſubſequent trials, to be the nimbleſt hitherto contrived; becauſe ſome ſubſtances which ſeem to be quite deſtitute of magnetiſm, by not attracting any of the magnetic needles otherwiſe ſuſpended, will ſenſibly affect this. However, notwithſtanding the nicety of this method for diſcovering a very low degree of magnetic attraction, it was found ſtill inferior to that of exploring ſubſtances floating on the ſurface of quickſilver, as uſed by M. BRUGMAN *. It ſeemed, therefore, neceſſary to repeat ſome of thoſe experiments on braſs, and alſo upon platina, by examining their magnetiſm by this means, *viz.* by putting the pieces of braſs or grains of platina upon the ſurface of quickſilver, and then preſenting a ſtrong magnet near them. The reſult of thoſe experiments was, that very ſeldom a piece of braſs, or grain of platina, occurred, which was not affected by the magnet; and even when they were not affected by it, their indifference, as may be expreſſed, was not very clear and deciſive; and indeed there are very few ſubſtances in nature which, when examined by this means, are not in ſome degree attracted by the magnet, ſo general is the diſperſion of iron, or ſuch is the tendency which moſt bodies have towards the magnet.

Such braſs which in the former experiments appeared to have no magnetiſm naturally, nor to acquire any by hammering, was now found to be moſtly magnetic, though in ſo very ſmall a degree as to be diſcoverable only when floating upon quickſilver. The ſame was the caſe with the grains of platina before they were hammered; but after hammering

* See his Magnetiſmus, ſeu de Affinitatibus Magneticis, printed at Lyons in 1778.

their attraction towards the magnet became more evident; whereas those pieces of brafs, which naturally had not any degree of magnetifm fufficient to affect the needle, nor acquired any by hammering, but yet fhewed fome tendency towards the magnet when floating upon quickfilver, never, or very feldom, had that tendency increafed by hammering.

As in the courfe of thofe experiments it naturally occurred to obferve feveral particulars, which may be of ufe to thofe perfons who wifh to repeat thefe experiments, I fhall now fubjoin the principal of them.

It is neceffary firft of all to obferve, that the veffel wherein the quickfilver is put for the purpofe of examining the magnetifm of divers bodies, muft be at leaft fix inches in diameter; otherwife the fubftances that are fet to float upon the mercury, will be continually running towards the fides of the veffel, on account of the curvature of the furface of that metal, which in narrow veffels begins from a greater diftance from the edge, than in veffels of a larger diameter.

It is neceffary likewife to keep the quickfilver very clean, and alfo very pure; the want of which precautions will render the event of the experiments precarious. I have obferved a very remarkable phænomenon, with refpect to the furface of the mercury that is ufed for this purpofe. It is, that though fubftances will float upon it with wonderful nimblenefs, when the mercury is firft poured out of the bottle into the open veffel, yet a fhort time after, *viz.* after having remained for an hour or two, and fometimes for a fhorter time, expofed to the atmofphere, a piece of brafs or other fubftance will by no means float upon it with equal facility; fo that fome pieces of brafs, or grains of platina, which, after firft pouring the quickfilver into the open veffel, were evidently attracted by the magnet, about

about an hour after were not in the leaſt attracted by it. The method which, when the ſurface of the quickſilver is rendered thus ſluggiſh, will effectually purify it, is to paſs the quickſilver through a funnel of paper, *viz.* a piece of clean writing paper rolled up conically, and having at its apex an aperture of about a fiftieth part of an inch in diameter; which operation is ſometimes neceſſary even on firſt pouring out the quickſilver, and which I have often been obliged to repeat three or four times in the courſe of an hour. There ſeems to be formed a kind of cruſt upon the ſurface of the mercury when expoſed, which, though inviſible by mere inſpection, may be perceived by moving the floating body; for if it be tried immediately after having paſſed the quickſilver through the paper funnel, the floating ſubſtance will ſeem to proceed by itſelf; whereas, ſome time after, the ſame body, when moved, ſeems to communicate that motion to the adjacent quickſilver, and to drag it along with itſelf, ſomewhat like when one moves a body, which floats upon the ſurface of a liquor that begins to coagulate.

The formation of this cruſt ſeems to be moſtly owing to the imperfect metals, which in various quantities are uſually amalgamated with the common ſort of quickſilver; becauſe that amalgamation tends to dephlogiſticate thoſe metals, and the ſemi-calcined part floats at the top, and it is not unlikely that the dephlogiſtication goes on much quicker in the open air. The reality of this ſuppoſition is corroborated by obſerving, that the purer the quickſilver is, the ſmaller is the cruſt formed, or oppoſition made to the floating bodies. However, I have obſerved it in ſome meaſure even in the pureſt quickſilver that can be procured; and am inclined to think, that it muſt be

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partly owing to moiſture, or to very fine duſt that adheres to the quickſilver when expoſed to the atmoſphere.

In performing ſuch experiments care ſhould be had to keep the air and the quickſilver as much undiſturbed as poſſible, and alſo to preſent the magnet in a proper manner, *viz.* ſo as not to touch the ſurface of the mercury, nor to ſtrike againſt the floating body, eſpecially when that is in motion; for after the impulſe, though that may be very ſlight, the floating body will be impelled backwards, which may be often miſtaken for magnetic repulſion. The leaſt exceptionable method is the following: firſt, if the floating body be in motion let it ſtop, then hold a ſtrong artificial magnet nearly in a perpendicular direction, and with one pole juſt over one ſide of the floating body, or rather ſo that the perpendicular, let fall from the pole of the magnet to the ſurface of the quickſilver, may be about one-tenth of an inch diſtant from the body to be tried. The height of the magnet above the quickſilver ſhould be juſt ſufficient to let the floating body paſs under it without touching it. In this ſituation the magnet muſt be held ſteady; and if the floating body has any magnetiſm, it will be ſoon drawn directly under the magnet.

In theſe experiments it will be generally found, that one part of the floating body is more magnetic than the reſt, which appears from that particular part being conſtantly drawn directly under the pole of the magnet; whereas, when the magnetiſm is diffuſed equably, the center of gravity of the body (provided its ſhape be not very irregular) becomes ſtationary juſt under the pole of the magnet.

It is not every magnet that will diſcover the very weak magnetiſm of certain ſubſtances; for ſometimes a powerful mag-

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net will evidently attract what a weaker one will not move in the leaſt.

I ſhall laſtly obſerve, with reſpect to the experiments of laſt year's lecture, that though then I thought to have fuſed and incorporated together braſs and iron, yet ſome ſubſequent trials gave reaſon to believe, that the iron is concealed in ſome part or other of the melted braſs, rather than equably diffuſed through the ſubſtance of the latter; and the principal reaſon for this ſuſpicion is, that when thoſe pieces of mixed metal are tried upon the quickſilver, ſome points in their ſurfaces are generally attracted by the magnet in preference to others.

Experiments and obſervations relating to the attraction between ferrugineous ſubſtances and the magnet, in their different ſtates of exiſtence; to which are added, ſome thoughts concerning the cauſe of the variation of the needle.

It is a propoſition well eſtabliſhed in magnetics, that ſoft iron, or ſoft ſteel, acquires magnetiſm very eaſily, and loſes it with equal facility; but that hard ſteel acquires that power with difficulty, and afterwards retains it obſtinately. From the conſideration of thoſe properties I was led to imagine, that if a piece of ſteel, whilſt red-hot, were placed between magnetic bars, and whilſt ſtanding in that ſituation cold water were to be ſuddenly poured upon it, ſo as to harden it, there might, perhaps, be obtained an artificial magnet, much more powerful than what can be produced by the ordinary way; becauſe the magnetic bars, employed for ſuch purpoſe, would communicate a great degree of magnetic power to the ſteel, when red hot and conſequently ſoft, which power would be fixed upon the ſteel by the hardening.

In order to put this project to the trial, fix magnetic bars were fo difpofed in an oblong earthen veffel, as that the north poles of three of them might be oppofite the fouth poles of the three others, forming two parcels of bars, lying in the fame direction, and about three inches afunder, which nearly was the length of the fteel bar which was intended to be rendered magnetic. Things being thus difpofed, the fteel bar was made quite red hot, and in that ftate was placed between the magnetic bars; after which cold water was immediately poured upon it, which rendered it fo hard as not to admit being filed: its magnetifm was found to be confiderably ftrong, but by no means extraordinary. From repeated trials with fteel bars of different fizes, and by ufing a greater or lefs number of magnetic bars, I found, that fhort fteel bars acquire a proportionably greater degree of magnetifm by this method, than thofe which are longer; that the magnetifm in the longer bars is not proportionably as ftrong, principally becaufe the artificial magnets being placed at their extremities have very little power on thofe parts of the pieces of fteel which are nearer its center; and, laftly, that when, in order to remedy the juft mentioned impediment, more magnets are placed nearer the middle of the fteel bar, then this piece of fteel generally acquires many fucceffive magnetic poles.

Upon the whole it feems, that though this method alone be not fufficient to communicate to fteel bars an extraordinary degree of magnetifm, yet it may be of great ufe in conftructing large artificial magnets; for if thofe bars, inftead of being hardened in the ufual way by plunging them when red-hot in water, be hardened whilft ftanding between powerful magnets, they will thereby acquire a confiderable degree of magnetic power without any additional trouble to the work-

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men. They may then be poliſhed, after which they may be rendered more ſtrongly magnetic by the uſual method of touching them with other magnetic bars; whereas it is a very laborious operation to render magnetic large bars of hardened ſteel from the very beginning, *viz.* when they have none of that power.

In the courſe of performing thoſe trials, I frequently obſerved, that the pieces of ſteel, whilſt they were red hot, ſeemed not to be attracted by the magnets; ſo that the leaſt ſhock, and even the pouring of the water, could remove them from the proper ſituation, which rather ſurpriſed me; becauſe it has been aſſerted, by ſome authors, that the magnet attracts red-hot iron as well as cold. KIRCHER eſpecially ſays, that he tried the experiment *, and found that a piece of iron heated ſo as to be hardly diſcerned from a burning coal, was attracted by the magnet as eaſily as when cold; and he even aſſigns a reaſon why the power of a magnet is deſtroyed by a great degree of heat; whereas the red heating of the iron will not prevent its being attracted by the magnet. The reaſon he gives is, that the fire corrupts and calcines the magnet, but purifies the iron. The following experiments were made in order to aſcertain this matter.

I kept a piece of ſteel in the fire till it was quite red hot, and in that ſtate preſented the magnet to it, ſo as to touch it repeatedly in various places; but no ſign of attraction could be perceived before the redneſs diſappeared. I mean, however, ſuch redneſs as may be evidently ſeen in the clear daylight; for, as was ſhewn by other experiments, when the magnet begins to attract the heated iron, the redneſs of the latter can ſtill be ſeen in the dark.

* De Magnete, lib. I. p. II. theorem xxxi.

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Having repeated the experiment with different pieces of iron and of ſteel, the reſult was conſtantly the ſame, *viz.* whilſt the iron or ſteel remained quite red hot or white hot, the magnet did not attract it; but the attraction began when that degree of redneſs, which is clearly perceivable in the daylight, began to diſappear; and it was as ſtrong as ever when the iron was cooled a little more than when the redneſs quite diſappeared in the dark. In regard to this limit or maximum of attraction, I think I have obſerved, as well as the nature of the experiments would permit, a difference between ſteel and iron, which is, that in the ſteel the maximum of attraction follows the diſappearance of the red heat ſooner than in iron.

This experiment is ſubject to two ſources of miſtake, which perhaps miſled Father KIRCHER, and which are neceſſary to be mentioned for the ſake of others who wiſh to repeat this experiment. The firſt is, that when a piece of iron of no great extent is red hot, or even white hot, in one place, and below a red heat in other parts, the magnet will frequently attract it, though the red-hot ſide be preſented to it. The ſecond cauſe of miſtake is, that when a ſmall piece of iron or ſteel, as a common ſewing needle, is made red-hot, and is then preſented to the magnet, if the magnet touch it, that contact cools it inſtantly below the neceſſary degree of heat, and of courſe the attraction takes place. It is owing to this laſt cauſe that I have not yet been able to aſcertain, whether the attraction between the magnet and the iron be quite annihilated, or only diminiſhed to a great degree, by rendering the iron red or white hot; ſo that I can only ſay with certainty, that a magnet will not attract a certain piece of iron red hot or white hot; whereas it will attract another piece of iron at leaſt fifty times bigger, if it be cold or below a red heat.

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To try this property in a different and more convincing way, I heated a large iron nail till it was white hot, and in that ſtate placed it upon an earthen ſupport near one pole of the magnetic needle, ſo as to lie not in the ſame direction, but on one ſide of it. Then, looking attentively on the graduated circle of the compaſs, I obſerved, that the needle was not in the leaſt moved from its natural ſituation, whilſt the nail remained red-hot; but, as ſoon as the redneſs began to diſappear, the needle advanced towards the nail, and a few ſeconds after the needle pointed directly towards it.

I tried whether in this experiment any difference was occaſioned by the magnets being natural or artificial; but, as it might be expected, there was none.

In purſuance of thoſe magnetic experiments wherein heat is concerned, I tried the effects which took place when the magnet was heated; but as the diminution of its power by heating, and an increaſe of it by cooling, were obſerved and deſcribed by the late Mr. CANTON, I ſhall only add a circumſtance, which may perhaps be new. It is that an artificial magnet, after having had its power diminiſhed by heating, does not recover it intirely again by cooling; having conſtantly found, that the magnets which had been heated, after cooling would never hold as great a weight of iron as they did before. The heat to which thoſe magnets were expoſed never exceeded that of boiling water. This was rendered more evident by the following experiment.

A magnetic bar was placed in an earthen veſſel at ſome diſtance from the ſouth pole of the needle of a very good compaſs; by the action of which magnet that end of the needle was drawn ſeveral degrees from the magnetic meridian, or the direction in which it ſtood before. In this ſituation of the

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apparatus boiling water was poured into the veffel wherein the magnet ftood, in confequence of which the needle went back two degrees and a half. Some time after, when the water was quite cold, the needle was found nearer to the magnet, but not fo near as it ftood before the hot water was poured into the veffel.

Next to the effects of heat, I was defirous of trying what could be effected by decompofing the iron; and for this purpofe an earthen veffel, containing about two ounces of iron filings, was placed near the fouth end of the needle of the compafs, by which the needle was drawn a little out of its natural direction. Having marked where the needle now ftood, fome water firft, and then fome vitriolic acid, were poured upon the filings, which occafioned a brifk effervefcence, and a copious production of inflammable air; but foon after the beginning of the effervefcence, I was furprifed to obferve, that the needle came nearer to the veffel, fhewing that the attraction between the needle and the filings had been increafed by the action of the vitriolic acid upon the latter, which is contrary to what could have been expected; for if we confider that the power of a magnet is diminifhed by heat, and that red-hot iron has either no attraction at all, or an exceedingly fmall degree of it, towards the magnet, we might have concluded, that the action of the vitriolic acid upon iron would immediately diminifh its attraction, befides the other ftrong reafon arifing from the dephlogiftication of the iron occafioned by the effervefcence; and in fact fome time after, when the violence of the effervefcence, and of courfe the production of inflammable air, begins to abate, the attraction between the needle and the filings begins likewife to diminifh; and at laft, when the effervefcence is hardly perceptible, the needle is found to ftand ſtand farther from the veſſel containing the filings, &c. than it ſtood before the vitriolic acid was added, which diminution of attraction is certainly owing to the loſs of phlogiſton; it being well known, that iron is leſs and leſs attracted by the magnet in proportion as it approaches nearer to the calcined ſtate. Here follows the particular account of the above-mentioned experiment.

The ſouth end of the needle coincided with the 285° 15′ on the divided circle. When the pot with the iron filings was placed on one ſide of it, the needle pointed to 286° 15′, being drawn juſt one degree nearer. Having added the diluted vitriolic acid to the filings, the needle came nearer, and ſtood at 286° 45′. Ten minutes after the beginning of the efferveſcence it ſtood at 286° 35′, having receded a little; and a few minutes after this obſervation it ſtood at 286° 30′. An additional quantity of diluted vitriolic acid was now added, which increaſed the efferveſcence conſiderably; and on obſerving it a ſhort time after, the needle was found at the ſame point at which it ſtood before, from which time it began to go back very gradually; ſo that about three hours after it ſtood at 285° 50′, *viz.* farther from the efferveſcing mixture than it ſtood before any vitriolic acid was poured upon the iron filings.

As a ſingle experiment ought not to be depended upon when an error may be occaſioned by many concurring circumſtances, I repeated this experiment with great precaution, taking care that nothing could ſhake the needle, or the reſt of the apparatus; but the reſult was nearly the ſame, the attraction between the iron filings and the needle being increaſed by the action of the vitriolic acid.

In order to aſcertain that this effect was not owing to the heat generated by the efferveſcence, the pot, with ſome iron filings, was placed near the magnetic needle, as before; then ſome boiling water was poured upon the filings, which heated them much more than the diluted vitriolic acid could have done; but the magnetic needle was not moved in the leaſt from its original ſituation.

The ſuſpicion which occurred next was, whether the effervеſcence might not agitate the iron filings ſo as to bring a greater number of them to that ſide of the veſſel, which ſtands contiguous to the magnetic needle. In order to obviate this objection, the experiment was repeated with a ſingle piece of iron inſtead of the filings; but as this piece of iron preſented a very ſmall ſurface to the diluted acid, the effervеſcence was very weak, and of courſe the magnetic needle did not move when the acid was poured into the pot. At laſt, in order to remedy this ſecond inconvenience, ariſing from the want of ſurface, I uſed a long piece of ſmall ſteel wire, which was twiſted in various directions, ſo as to be admitted into the pot; in which caſe the metal preſented a large ſurface to the acid, and it was not ſubject to be moved by the effervеſcence. The reſult was ſimilar to that of the firſt experiment, *viz.* the attraction was increaſed by the action of the acid on the wire; and here follows the particular account of the experiment.

About ſix yards of clean ſteel wire, ſomewhat leſs than one-fiftieth of an inch in diameter, being twiſted in various directions, was put into an earthen veſſel, which was placed near the ſouth end of the magnetic needle, which in conſequence of that was drawn from its natural ſituation, *viz.* from 281° to 280°. After adding the diluted vitriolic acid, a ſtrong effervеſcence commenced, and the needle came to 279° 47′. About five minutes after, it ſtood at 279° 35′. Five minutes after this, it ſtood at 279° 30′. And a little after this obſervation, it ap-

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peared to be even ſomewhat nearer to the pot than the above-mentioned point. The experiment was then diſcontinued, and on removing the pot, the needle went back to its original ſituation, *viz.* 281°; which ſhewed, that its alteration during the proceſs was occaſioned by the action of the acid on the iron, and not by any extraneous cauſe.

On examination the wire was found only blackened on its ſurface, but not nearly conſumed; I had therefore the curioſity of trying the ſame wire again, and accordingly it was placed in the ſame veſſel near the magnetic needle, which attracted the latter from its original ſituation 281° to 280°. After adding the acid, the needle came nearer, as in the preceding experiment; and a ſhort time after it ſtood at 279° 30′, at which time the pot was removed, there being no occaſion to continue the experiment any longer.

On pouring the liquor out of the pot, the wire did not appear to be much waſted. The pot was then replaced near the needle, ſo as to attract it a little nearer as before; but on pouring boiling water upon the wire, a pretty briſk effervefcence took place, and the needle was in conſequence attracted ſtill nearer. This experiment ſhewed, that though the diluted acid had been poured out, yet there remained a quantity of it adhering to the wire, which was ſufficient to renew the effervefcence, when aſſiſted by the heat of the boiling water.

Upon the whole, it appears, that the action of vitriolic acid upon iron or ſteel increaſes their attraction towards the magnet; that this increaſe of attraction has a limit, after which it begins to decreaſe; and that this limit ſeems to come ſooner when iron than when ſteel is uſed; but, however, in reſpect to this laſt particular I am not yet quite certain, ſince, in the experiments hitherto made, the variety in the ſhape or bulk of the iron or ſteel may have occaſioned a conſiderable difference.

After the reſult of thoſe experiments, it was natural to examine the effect which other acids might have on iron and ſteel; therefore the above-mentioned experiment of the ſteel wire was repeated with nitrous inſtead of vitriolic acid; the reſult of which was that the attraction between the magnetic needle and the wire was increaſed, but not ſo much as when vitriolic acid had been uſed. Here follows the particular account of the experiment.

About ſix yards of clean ſteel wire, near one-fiftieth of an inch in diameter, being twiſted in various directions, was placed in the uſual veſſel near the ſouth end of the needle; in conſequence of which the needle was attracted from its natural ſituation, *viz.* from 283° 32′ to 282° 50′. About two ounces of water were then poured over the wire, and immediately after, near one ounce of nitrous acid was added, which produced an effervеſcence: the magnetic needle, however, hardly moved from its former ſituation; but in about a minute's time, the effervеſcence being increaſed very much, the needle came to 282° 42′. About a quarter of an hour after, the violence of the effervеſcence abated a little, and the needle went back again to 282° 50′. A ſhort time after, it ſtood at 283° 2′. At laſt, when about half an hour had elapſed ſince the beginning of the operation, the effervеſcence was hardly perceivable, the liquor was become red like the colour of red ochre, and the needle ſtood at 283° 15′, viz. farther from the veſſel than it did before the acid was added to the wire.

It appears, therefore, that the effervеſcence occaſioned by the nitrous acid produced a ſimilar effect, though not in ſo great a degree as the vitriolic. The maximum of the attraction ſeems

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to come ſooner when nitrous than when vitriolic acid is uſed, after which limit the attraction decreaſes much faſter in the former than in the latter caſe, which is evidently owing to the metal being more eaſily dephlogiſticated and diſſolved by the nitrous than by the vitriolic acid.

The marine acid was tried next; but, notwithſtanding all the precautions I could take, it always occaſioned a very weak effervеſcence, and the needle was not in the leaſt affected by it.

A ſtrong effervеſcence ſeems to be neceſſary to increaſe the attraction between the magnet and iron or ſteel; for when I tried the experiment by putting a ſmall quantity of acid into the veſſel, the effervеſcence was exceedingly weak; the magnetic needle was not at firſt affected by it, but ſeveral hours after it ſtood a little farther from the pot, which was evidently owing to the dephlogiſtication of the iron.

After obſerving the action of acids on iron, I next tried to decompoſe that metal by means of fire, to obſerve what effect would take place with reſpect to its magnetiſm. For this purpoſe two ounces of iron filings were mixed with an ounce of flowers of ſulphur and an ounce of nitre. This mixture was put into a ſmall and ſhallow earthen veſſel, and was placed near the ſouth extremity of the magnetic needle, which attracted the needle nearer than its natural ſituation by about one degree and a half. A pane of glaſs was interpoſed between the magnetic needle and the veſſel. Things being thus diſpoſed, the mixture was fired, and it burned rather ſlowly, the fire ſometimes going out, ſo as to require being fired again; till at laſt it would burn no longer. During this combuſtion the needle was once attracted ſomewhat nearer; but its motion that way was ſo little, that I could not be quite certain of it. This happened not long after the fire was firſt communicated to the mixture,

after

after which the needle generally vibrated backwards and forwards, but upon the whole it gradually receded.

On repeating the experiment with a larger quantity of the mixture, and alſo with different proportions of ingredients, I could not obſerve any particular attraction. The needle vibrated rather more than before, but gradually receded; ſo that at laſt it ſtood farther from the veſſel than it did before the mixture was ſet on fire, though not quite in its natural direction.

The vibration of the needle in this experiment, or its waving motion, was probably owing to the irregular burning of the mixture, and perhaps to the heating of one part of it whilſt the other was burning. The gradual receding of the needle was certainly owing to the dephlogiſtication of the iron.

After having thus related the reſult of experiments, I ſhall now beg leave to add a few thoughts concerning the application of thoſe obſervations towards accounting for the variation of the magnetic needle.

This wonderful phænomenon has, ſince it was firſt diſcovered, employed the thoughts of very able philoſophers; many hypotheſes having been offered, not only for its explanation, but even to foretel the future variations in various parts of the world. I need not detain this learned Society with a particular hiſtory of thoſe hypotheſes, but ſhall only obſerve in general, that neither their predictions have anſwered, nor any of them was founded upon evident principles. The ſuppoſition of a large magnet being incloſed within the body of the earth, and of its relatively moving with reſpect to the outward ſhell or cruſt; the ſuppoſition of there being four moveable magnetic poles within the earth; the hypotheſis of a magnetic power, partly within and partly without the ſurface of the earth; together with ſeveral other hypotheſes on the ſame ſubject, are not

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only unwarranted by actual experiments, but do neither ſeem analogous to the other operations of nature. The late ingenious Mr. CANTON, Member of this Society, was the firſt, who endeavoured to account for the daily variation of the magnetic needle by the heating and cooling of the magnetic bodies in different parts of the earth's ſurface; which was in conſequence of his having firſt obſerved, that the action of the magnet on the needle was diminiſhed by heating, and increaſed by cooling *.

Following Mr. CANTON's judicious method of deriving the explanation of natural appearances from properties actually proved by experiments, I think, that the increaſe and diminution of magnetic attraction by heating and cooling of the magnet, as obſerved by Mr. CANTON, together with the reſult of the experiments recited in this Paper, ſeem fully ſufficient to account for the general variation of the needle.

If we collect under one point of view all the cauſes hitherto aſcertained, which can increaſe or diminiſh the attraction between magnetic bodies, we ſhall find, that the attraction between the magnet and iron, or between magnet and magnet, is increaſed by cooling, by a regeneration of iron or phlogiſtication of its calx, and within certain limitations by the action of acids upon the iron; that this attraction is diminiſhed by heating, and by the decompoſition of iron; and, laſtly, that it is probably annihilated by a very great degree of heat.

Theſe truths being premiſed, it muſt be conſidered, firſt, that, according to innumerable obſervations and daily experience, the body of the earth contains almoſt every where ferruginous bodies in various ſtates and bulks; ſecondly, that the magnetic needle muſt be attracted by all thoſe bodies, and its

* Philoſophical Tranſactions, vol. LI. p. 398.

ſituation

ſituation or direction muſt be determined by all thoſe attractions conſidered together, *viz.* from their common center of action; thirdly, that by removing or altering the degrees of attraction of ſome of thoſe bodies which are ſituate on one ſide of the magnetic meridian, more than of thoſe ſituated on the other ſide, the above-mentioned common center of attractions, and, of courſe, the direction of the magnetic needle muſt be altered, which in fact is the variation of the needle; and, laſtly, that this alteration in the attractions of ſome of the ferruginous bodies in the earth muſt undoubtedly take place, it being occaſioned by the parts of the earth being irregularly heated and cooled, by the action of volcanoes which decompoſe or otherwiſe alter large maſſes of ferruginous ſubſtance, by earthquakes which remove ferruginous bodies from their original places, and we may add alſo by the *aurora borealis*; for though we are as yet ignorant of the cauſe of that ſurpriſing phænomenon, it is however certain, that the magnetic needle has been frequently diſturbed when the *aurora borealis* appeared very ſtrong.

The magnetic needle, therefore, being neceſſarily affected by thoſe cauſes, it ſeems unneceſſary to have recourſe to other hypothetical cauſes which are not eſtabliſhed on actual experience.

In order to exemplify this explanation of the variation in a familiar manner, I made the following experiment, with the account of which I ſhall conclude this Paper. Four earthen veſſels were diſpoſed round the magnetic needle, two near its ſouth, and the other two near its north pole, but not at equal diſtances. In one of thoſe veſſels there was placed a natural magnet; the ſecond contained ſeveral ſmall bits of magnetic ſteel mixed with earth; and in each of the other two there were

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put about four ounces of iron filings. Things being thus diſpoſed, and left undiſturbed for about half an hour, the needle remained unaltered. Then the pieces of magnetic ſteel and earth were ſtirred with a ſtick, in conſequence of which the needle was agitated. After this, ſome diluted vitriolic acid was poured upon the filings in one of the veſſels, the action of which attracted the needle that way; but whilſt the needle remained in that ſituation, ſome diluted vitriolic acid was poured upon the iron filings in the other veſſel, which ſtood on the other ſide, in conſequence of which the needle went back again towards its former direction. Whilſt the effervescences were going on in the two veſſels, the magnet in the firſt veſſel was heated by means of boiling water, which occaſioned another alteration in the direction of the magnetic needle; and thus, by altering the ſtate of the ferruginous ſubſtances in the veſſels, the needle's direction was altered, in evident imitation of the natural variation.

T. CAVALLO.