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The same original volume of gas in *a d* will last for a long time, as we keep replenishing it as often as the fiftieth division is reached.

The experimenter cannot help remarking, that on suddenly exposing the sentient tube to a bright light, *the liquid for an instant rises* on the scale, and on dropping the cap *in an instant falls.* This important phænomenon, which is strikingly seen under the action of an electric spark, I shall consider hereafter.

In conclusion, as to comparing the tithonometric indication at different times, if the gases have the same constitution, the observations will compare; and if they have not the value can from time to time be ascertained by exposure to a lamp of constant intensity. To this method I commonly resort.

From the space occupied in this description the reader might be disposed to infer that the tithonometer is a very complicated instrument and difficult to use. He would form, however, an erroneous opinion. The preliminary adjustment can be made in five minutes, and with it an extensive series of measures obtained. These long details have been entered into that the theory of the instrument may be known, and optical artists construct it without difficulty. Though surprisingly sensitive to the action of the indigo ray, it is as manageable by a careful experimenter as a common differential thermometer.

University of New York, Sept. 26, 1843.

L. *On the Spectral Images of* M. Moser; *a Reply to his Animadversions, &c. By* Robert Hunt, *Secretary to the Royal Cornwall Polytechnic Society* *.

To R. Taylor, Esq.

Dear Sir,

I CANNOT but regret that any remarks which I may have made on the very interesting discoveries of Professor Moser, should have so far disturbed the philosophic quiet of his mind, which it is so important to maintain, when engaged in the investigation of truth, as my paper on Thermography† appears to have done. I am, however, called upon to reply to M. Moser's remarks, which appear in your Journal for November, in a way that is very unpleasing to me. However much men may differ in the interpretations they give to obscure phænomena, I do not fancy they will approach any nearer the truth, or facilitate the progress of inquiry, by indulging in personal attacks. I have ever pursued my inquiries with, I hope, but one object in view. The investigation of curious phænomena has ever been a pleasure to me, and an occasional discovery has been its own exceeding great reward. I never

* Communicated by the Author. † Phil. Mag., Dec. 1842.

expected to be charged with *repeating the experiments of others and giving them out as my own discovery.* The mind of that man, who thinks to elevate himself by any paltry piracy of this kind, is of a low order, and the attempt to defraud the public by any such means is certain, sooner or later, to have its full amount of punishment in the contempt of the many and the pity of the few. But I feel myself put upon my defence. The note you have placed at the foot of page 356 partly relieves me from the charge *, but not entirely; I must therefore presume upon your kindness, and as briefly as possible explain the matter as it stands.

Immediately after the meeting of the British Association at Manchester, I heard, for I was not present at that meeting, of the announcement of M. Moser's discovery, that "when two bodies are sufficiently near, they impress their images upon each other." I immediately tried some experiments, and was much interested in the results. Now, it will be remembered, this announcement at Manchester was unaccompanied by any statement of experiments. I had already made a great number of experiments when I received the *Comptes Rendus* for the 18th of July and the 29th of August, 1842, containing communications, "Sur la formation des images Daguerriennes," which I have distinctly referred to in the very first sentences of my paper on Thermography. These communications gave me M. Moser's views, but not the experimental evidence by which he arrived at these views; and it was not until the publication of the Eleventh Part of the Scientific Memoirs, in February 1843, that I gained any further information on this subject. M. Moser's memoirs appear to have been published in Poggendorff's *Annalen* about June or July 1842, but it is unfortunate for me, that the thoughts and labours of the thinking German nation are sealed books until they appear in my own language, owing to my entire ignorance of theirs. The valuable *Annalen* I have never yet by any chance seen. On the 8th of November I read my paper before the Royal Cornwall Polytechnic Society, the President, Sir Charles Lemon, in the Chair; and this communication, which was immediately printed, and which appeared in many of the leading scientific journals for December, was, I believe, the first series of experiments on this subject published in England.

* Our conviction, upon a comparison of dates, that the charge was groundless, did not deter us from publishing a translation of the paper containing it. On the contrary, we thought it more just towards those included in M. Moser's attack, that they should not remain unaware of misrepresentations which were in circulation abroad, and thus be enabled to meet them.—Edit.

Now, with regard to the experiments themselves, surely Professor Moser will not claim as his own the experiments, of placing a coin on a glass or polished metal plate, or of writing on glass with a piece of steatite, and bringing out the images by breathing on them. Dr. Draper in 1840 published this*; and when a schoolboy, twenty years ago, I tried these experiments without ever suspecting their scientific value, which M. Moser was the first to call attention to. M. Moser, in his memoir 'On the Action of Light on Bodies,' states, "Silver and other metallic plates were *made warm*, and *cold bodies*, variously cut stones, figures of horn, pasteboard, cork, coins, &c. allowed to remain on them for some time." It must be distinctly understood, that at the meeting of the British Association it was stated, that the images could be brought out by the vapours of water, mercury, &c.; but without being, at the period of making my experiments, October 1842, aware of the above, which I did not see until February 1843, my first simple experiments convinced me that some connexion existed between the conducting powers of bodies, as it regards heat, and the strength of the impressions made by them. With this in view I tried good and bad conductors of heat, from copper plates and coins to platina ones, glass and charcoal. These constitute the experiments given in paragraphs from 2 to 7 of my paper. Now if M. Moser used all these materials, and I do not doubt but he may have done so, he certainly did not make his experiments with the same object in view, or he would not have neglected to observe the fact, which I was the first to announce, that "*bodies which are bad conductors of heat placed on good conductors make decidedly the strongest impressions.*" I am quite ready to give up any claim to the experiments, but I reserve to myself the interpretation they afford.

M. Moser says, "I cannot name a single experiment, &c. &c. which I had not previously described." Will M. Moser oblige by directing me to any of his memoirs, where may be found the experiments named in paragraph 8, which show the power of electrical discharges in evoking again these mysterious images after they have been effaced? Or that in paragraph 15, where a copper plate is described to have been so changed in its molecular constitution, by being warmed in contact with a piece of paper, that it readily amalgamated with mercury over the parts which the paper covered, but not so over the other portions of the plate?

Nearly all the other paragraphs of my paper, to the 22nd, are details of experiments with coloured glasses and transparent bodies, placed upon plates of unprepared copper and

* In Phil. Mag., S. 3, vol. xvii. p. 217.—Edit.

silver. I find that M. Moser has also used coloured glasses, but principally upon iodized silver plates. It is to be lamented that he has made so great a number of very careful experiments in this way; for, by regarding the colour of the glass, and the colour of the ray which permeates it, as the same, he has been led to some very incorrect conclusions, as the slightest acquaintance with the valuable labours of Sir John Herschel would have shown him. My use of coloured glasses in *these experiments* was confined to the *heating powers of the different colours*, and these were contrasted with smoked glasses, and the like, the results showing, whether the experiments were made in sunshine or at night, that those glasses, the red and blackened ones, which admitted the permeation, or absorbed the largest quantity of *heat*, made the most decided impressions on metal plates. I cannot see how M. Moser makes out his claim to these experiments, except it is upon the principle that Professors Faraday and Daniell are guilty of scientific piracy in publishing, in their valuable memoirs, results obtained with zinc and copper plates, Volta having used the same kind of plates before them.

I have only to deal with one more of Professor Moser's charges. He says, "He has not devised a single new experiment, for even those which appear to him sufficiently important to be adopted as the running head of his paper, 'The art of copying engravings, or any printed characters from paper on metal plates,' will be found nearly word for word in the *Annalen*, vol. lvii. p. 570." The latest memoir of M. Moser with which I am acquainted, is the 18th article in the 3rd vol. of the Scientific Memoirs, which is stated to be "from Poggendorff's *Annalen*, Band lvii., 1842, No. 9. p. 1." I presume M. Moser alludes to a more recent publication. He, however, relieves me from a difficulty by saying, "*It is that experiment in which I caused a* SEAL *to depict itself on mercury with which a pure or silvered copper-plate had been coated, and afterwards produced the image in the iodine vapours.*" Now we will examine the similitude between this and my published experiment. A copper plate is amalgamated by nitrate of mercury, and a *line or mezzotinto engraving, a wood-cut or lithographed print,* ON PAPER, is placed upon it for a few hours. With certain precautions the plate is exposed to the *vapour of mercury*; this vapour attacks those parts of the plate which correspond with the white parts of the paper, and a faint image is formed; the plate is now placed in the *iodine box* for a little time, and its vapour attacking and blackening those parts of the plate, which correspond with the dark portions of the paper, brings out a very decided and beautiful copy of the print. I am quite satisfied to leave it to yourselves and your readers to say if this "sufficiently important" experiment is

M. Moser's, or otherwise. I have seen paragraphs stating that M. Moser has succeeded in copying engravings from paper, but I do not, even now, know his experiments. I shall not dispute with M. Moser the point of priority, but I trust he will do me the justice of acknowledging, that he has judged hastily in accusing me of having appropriated his experiments without acknowledgement. In the paper in question I thought I had sufficiently acknowledged the high merits of Professor Moser; I again do so. He has opened a new and important path of physical inquiry, which promises to lead to some great truths connected with the constitution of matter, and the operations of the imponderable elements; at the same time, however, that I admit the importance of his discoveries, I must be allowed, for the present, to dissent from his conclusions.

It is not my intention to offer any further remarks in explanation of that portion of M. Moser's paper which particularly applies to myself, but I must be allowed this opportunity of reviewing some of the opinions he has put forth, and of explaining my reasons for differing from him.

M. Moser states *that every body must be considered as self-luminous*, and he appears to view the accelerating power exerted by heat, as stated in his own experiments, as the influence of caloric increasing the intensity of the invisible radiations, whilst as "their temperature becomes higher their refrangibility decreases." It becomes necessary, in the first place, to ascertain upon what evidence this *self-luminosity* of bodies is asserted. It has been long known, that light acting upon ioduret of silver, alters its condition, and renders it capable of condensing the vapours of mercury in a remarkable manner;—this constitutes the Daguerreotype. It has been shown that if we breathe over portions of a metallic plate, the other parts being covered, and then the vapour is allowed to dry off, the plate is in a condition to receive vapours over definite spaces, in the same manner as if it had been exposed to the light. Again, any solid body being placed for a short time on a polished plate of metal or of glass, either of them become susceptible of receiving vapory deposits, which will mark distinctly the spaces occupied by the bodies in contact. "By these experiments, I think," says Moser, "*I have proved that contact, condensation of vapours, and light produce the same effect on all bodies*;" and hence he rushes to the conclusion that all bodies are self-luminous, and has even speculated on the colour of the latent light of vapours, in a way, which betrays his fears lest the hypothesis he has framed should be destroyed by his own results. Light, or rather, as I am inclined to think, some element intimately connected with light, and

having its origin in the sun, but broadly distinguished from it by its producing no influence on the organs of sight, certainly "so modifies the surfaces of bodies that they condense vapours otherwise than usual;" and like modifications are produced by condensing vapours on parts of the surface, or by placing other bodies in contact with it. Whatever method we may adopt to disturb the surface of any body, be it metallic or vitreous, we have an unequal condensation of vapour. If without touching the surface of a metal plate, we subject it to the disturbance produced by a blow or two on the back of the plate, we shall find an irregular deposit of vapour if we breathe on it. The molecular change which bodies undergo, under the most trifling circumstances, is certainly one of the most curious matters with which photography has brought us acquainted. By light, by heat, by electricity we can dispose plates to receive vapours over definite spaces; by lowering the temperature of parts of any body, the same effect is produced, and by any mechanical force we do the same. If we place a copper plate so that one half of it shall *rest on* a cold body, and apply the heat of a spirit lamp for a few seconds to the other half, carefully avoiding touching the polished surface, and then expose the plate, when *quite cold*, to mercurial vapour, it will be found that a larger quantity of vapour is deposited over the half that was warmed than over the other half.

If a piece of wood is placed upon a polished plate, and one or two gentle blows is given to it, the plate will exhibit, when submitted to vapour, not merely the shape of the wood, but a perfect picture of its fibres. Again, if we give a metal plate a few gentle blows upon the *back*, the *surface* will distinctly show, when exposed to vapour, the spaces corresponding with those on the back on which the hammer fell. If we subject portions of a metal plate to any chemical action, even though it may be inappreciable to the sight, it will exhibit the spaces to which the action was confined, the moment it is exposed to the influence of vapour.

These experiments afford us a sufficient amount of evidence to conclude, *that any cause producing a change upon solid surfaces disposes them to condense vapours unequally.* They also prove the correctness of all the statements made by M. Fizeau, Professor Grove, Mr. Prater* and others; and at the same time as they do this, they convincingly show us, that these observers have only been dealing with a few curious facts, which cannot be allowed to explain these remarkable phænomena, to which, in particular, M. Moser wishes to direct attention, *viz.* the power which the solar rays have of produ-

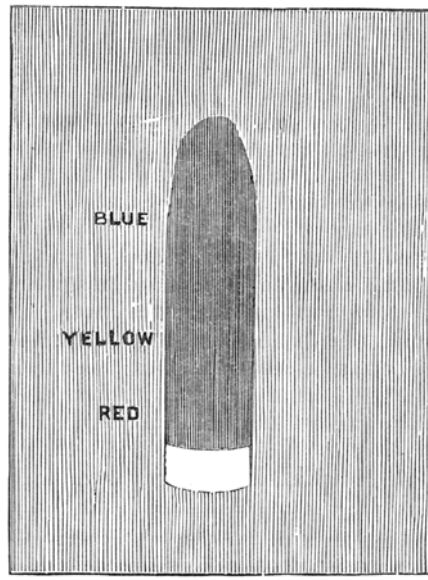
* See p. 225 of the present volume.—Edit.

cing definite changes in the condition of the surface of solid bodies, and the remarkable property of two substances in juxtaposition, inducing upon each other changes which may be rendered evident to the senses; and these changes, it must be remembered, are not confined to the surface merely, but they penetrate to a considerable depth into the solid structure of the mass, as I have already proved in several of my published experiments.

These results, in addition to many which I have previously given, also show the impropriety of considering these spectral images as the effects of "*invisible light*," "*light radiated in absolute darkness*," as they have been by M. Moser. I must in this place express my conviction, that any term involving an idea contrary to our received ideas, is calculated to produce much confusion. Light is that element which affects the organ of sight, enabling us to distinguish objects; that which does not do this is no longer light. The prismatic spectrum has been proved to consist of one element giving *light* and *colour*, of another element affording *heat*, and of a third element, which is active in producing chemical change. Now M. Moser informs us that it is neither of these, but a class of rays which are still more refrangible than those which have been called the "invisible chemical rays." "According to my experiments," he says, "the invisible rays of light pass very readily through aqueous solutions of various kinds, and through different oils, but they decidedly do not pass through the thinnest plates of glass, mica, or rock-salt, &c." Now, as M. Moser has not given us his experiments, it is very difficult to deal with them. In the first place, he has not, as it appears to me, as yet afforded any evidence of the existence of such a class of rays as those he speaks of; and if the "thinnest plates of glass, &c." are not permeated by these rays, upon what principle does he, by the use of *coloured glasses*, prove "that light and mercurial vapour are identical in their effects?" The prism must be useless in this inquiry on M. Moser's own showing; I am therefore quite at a loss to know by what means he has succeeded in establishing, with so much accuracy, the refrangibility of these "invisible rays of light." I am very far from denying that the phænomena in question have been produced by *invisible solar emanations*. I am not at all prepared to deny the absorption of such emanations by solid bodies, or their existence in a latent state, or their radiation in darkness. I contend only that LIGHT has nothing whatever to do with the phænomena. In the present state of the inquiry it does appear to me that HEAT is a very active agent in producing the effects in question; indeed M. Moser himself says "if the temperature be raised, they (the invisible

rays of light) pass very readily plates of glass and mica." I shall have to name an experiment or two presently which will show, in a striking manner, the influence of the "calorific rays" on metallic plates. I regard those emanations, which are the acting ones in all our photographic operations, as possessing powers of the most energetic and extraordinary kinds; powers which are in constant activity, decomposing and recomposing, maintaining the conditions of growth and decay, of vitality and corruption; indeed, effecting that mighty system of change, which is the order of the visible creation, and these *may be* the radiations by which all the spectral images of Moser are produced. Sir John Herschel* was inclined to attribute these phænomena to a class of rays ranging above the red rays of the spectrum, and to which he gave the name of "parathermic rays." The evidence, however, of the existence of these rays, as a distinct class, is not sufficiently clear even to satisfy the mind of this talented and most indefatigable observer, to whom we are indebted, more than to any other person, for our knowledge of the conditions of the solar spectrum.

At the Cork meeting of the British Association I communicated the results of some experiments made with the prismatic spectrum itself, which is the only way in which, as far as I am aware, we can arrive at any satisfactory determination on the point in question.



A condensed prismatic spectrum was, by means of a good heliostat, maintained in one place upon a very highly polished copper plate for three hours. At the expiration of that time, the plate was exposed to the action of mercurial vapour. This vapour was condensed over the whole plate, but in very different proportions beyond and within the limits of the spectrum. The space occupied by the luminous image was evidently protected from that influence which disposed the other parts of the plate to condense the

* Philosophical Magazine, July 1843, (pres. vol.) p. 510.

vapour, but the space occupied by the extra spectral red rays, had undergone that change, which renders metals most susceptible to the action of vapours, and a thick white line of vapour very distinctly marked this calorific region, giving a spectral image similar to the accompanying figure. It does not appear that so long an exposure to solar influence was necessary, for a similarly condensed spectrum was allowed to *traverse over* a polished copper plate for a few hours. On submitting this plate to vaporization, a line of thickly-deposited mercurial vapour distinctly marked the path of the extra-spectral red rays. I should explain that I mean the extreme red ray of the spectrum, which is seen when it is looked at through a cobalt-blue glass, and which has been made the subject of much attention by Sir John Herschel, and some rays below this extreme red ray. These rays cover a space which correspond as nearly as possible with the large heat-spot in Sir John Herschel's thermographic spectrum. I have been exceedingly anxious to repeat these experiments on other metals, but the unfavourable state of the weather, and the advanced season has compelled me to abandon this examination for the present.

M. Moser has stated that this "invisible light" will not permeate glass. In my first communication on "Thermography" (Phil. Mag. S. 3., vol. xxi. Dec. 1842, p. 464), paragraph 9, I have shown that some influence is exerted through red glasses which is not exerted through blue glasses. It may be said that this was an influence radiated from the red glass, in greater quantity than from the blue. I think, however, that I have distinct proof of the permeation of the rays which are active in producing these spectral images.

Three very large flat white glass bottles were provided, and filled with coloured fluids, blue, yellow and red. The light had to pass through about $1\frac{1}{4}$ inch of fluid in each case, this was very carefully examined with the prism, and the depth of colour adjusted until they represented, very fairly, the three great divisions of the spectrum. Figures were cut in paper and placed upon highly polished copper plates, being pressed down by these bottles of coloured fluid. This arrangement being left in the dark during a night, the plates were submitted to vapour, and they exhibited, each of them, impressions of the paper figures, in which little or no difference could be detected. The same arrangement was exposed for different periods, varying from half an hour to two hours, to the influence of the sun's rays. Under the bottle containing the red fluid, a most perfect image was formed by mercurial vapour, even after the shortest exposure. Prolonged exposure gave a

faint image on the plate under the yellow fluid, but no trace of an impression could be detected, by the influence of the same exposure, under the blue fluid. Surely these results prove, that the calorific rays are the most active in these phænomena; and instead of inventing the purely conjectural notion of "invisible light," is it not much more rational, when we have distinct evidence of the powerful action of heat, to look for an explanation of these phænomena in the calorific radiations, which are equally active in light or darkness? Instead of assuming that bodies are all *self-luminous* for the purpose of explaining these curious facts, which "self-luminosity" we are not in a condition to prove, is it not more consistent with the spirit of inductive philosophy, to seek for the cause in the *invisible radiations of* HEAT, which we know take place under all circumstances? And it is admitted, even by Moser himself, that heat is a powerful accelerating agent.

M. Moser has not told us how he has determined that his *dark light* passes readily through aqueous solutions and oils. I have tried some experiments which are instructive, and I therefore record them. Having put an edge of wax around a polished copper plate, to the depth of one-eighth of an inch, I covered the plate with water, and upon two small pieces of glass I supported, by the edges, a silver medal, so that it just touched the upper surface of the water; in twelve hours the plate was much tarnished over every part, except that directly under the medal, which remained as bright as at first. Under one of the pieces of glass a very decided change of colour was produced, and from its whiteness I was at first inclined to think, that silver had been removed from the medal, and deposited on the plate; I have, however, since proved that it was an oxidation of the copper plate merely. The same arrangement was made with very fine olive oil, and the result was similar; but upon leaving the medal and plate undisturbed for some days, the whole surface of the copper was oxidized, the oil became a very fine green colour, and the under surface of the silver was covered with a film of copper. Here we have analogous phænomena to those we have been considering, but in these cases it is very evident the cause was neither light nor heat, but *voltaic electricity*. The plates, after the water and oil were removed, were thoroughly cleaned and exposed to the vapour of mercury, which gave images of the medal in outline, and of the glasses.

It may not be out of place here to say a few words relative to the explanation given by M. Fizeau of the production of these images. There is no doubt "if different parts of a polished surface are unequally soiled by extraneous bodies, even

in an exceedingly minute quantity," that it will condense vapours irregularly. But in the communication which I made to the British Association at Cork, I stated several experiments, which appear to me to prove that the images are produced quite independently of any layer of organic matter. Copper plates were polished with water only and boiled, and all the things placed on these plates were boiled also, yet very perfect images were produced. I have since tried the effect of exposing the plates, &c. separately to a very strong heat, and when cold placing them in contact; there has been no apparent difference between the images formed under these circumstances, and those formed upon plates, and with medals, &c. which have been purposely covered with very slight films of organic matter. I have also repeated these experiments many times, in the best vacuum which could be maintained with a good air-pump, to avoid the action of any vapours on the metal or glass plates, and in every case the images have been well defined. I cannot imagine any volatile film of the kind described, exerting an influence to so great a depth into the solid metals as I find to be the case. Often I have, by polishing, removed layers of metal, and yet the images have been reproduced by exposing the plate to vapour; and in paragraph 8 of my paper on Thermography, above alluded to, I have stated the result of an experiment in which electricity reproduced a succession of images which had been obliterated from the copper plate.

It has been suggested that electricity may be engaged in the production of these spectral figures. I have just tried an experiment which appears to show the probability of this element's being involved in some way in these very complicated phænomena. I arranged four electro-positive metals, nickel, bismuth, cadmium and silver, and two electro-negative ones, arsenic and antimony, on a copper plate, and they were allowed to rest upon it for three hours. Being removed, the plate was submitted to the vapour of mercury. The space covered by the nickel was marked, by being left free of vapour; that on which the cadmium lay was still more decidedly marked in this way; where the bismuth was placed the image was exceedingly faint, but still it was observable by a deficiency of vapour; and the silver was more decidedly outlined with vapour, but none on the spot it covered. On the contrary, the space occupied by the antimony was *covered in a most remarkable manner with vapour, presenting a perfectly white spot*, which in all positions distinguished it from the other parts of the plate, whilst the arsenic left no trace behind.

I think I have now shown, that many different causes may

produce similar effects, and certainly I have established the necessity of examining all the phænomena with great care and attention, before we attempt to establish a theory which shall embrace the whole class. I have given the name Thermography to these images, under a conviction that heat was most importantly engaged in producing them, and I see no reason for altering the name. At the same time I beg to be very distinctly understood, that I am not wedded to this opinion; I feel conscious that we are dealing with some of the most subtile agents in nature, and that we cannot too jealously guard against the deceptions of the senses. That which heat appears to produce, may be the creation of some other element, which is excited only by calorific influence. But although I hold my judgement under suspense, particularly when I find light, heat, electricity, chemical action and mechanical force, all producing the same effects, I cannot at present entertain the idea of "invisible light," although M. Moser states, that in his memoir on Vision, he has demonstrated its existence. In conclusion, I must hope that I have been successful in proving that I have not in this instance, or in any other, endeavoured to appropriate the experiments of another. I have ever studiously endeavoured to give the merits of even the slightest suggestion to its author, and if in any one instance, in the case of Professor Moser, I have not done so, I have erred through ignorance, and not by design. I cannot, however, detect any grounds for M. Moser's attack. I commenced my first paper with a statement of his results, and I concluded it by giving my opinion on the importance of the discoveries he had made. I cannot, however, allow myself to be led away from that which appears to me to be the legitimate path of inquiry by any unkind feeling. I shall pursue the investigation, and the moment I can convince myself that light is engaged in these phænomena in darkness, I will acknowledge the correctness of Professor Moser's views with heartfelt pleasure.

Falmouth, November 4, 1843. ROBERT HUNT.

LI. *On Theine and its Preparation.* *By* JOHN STENHOUSE, *Esq., Ph.D.**

THE process which I have found most suitable for preparing theine, is both easy and productive, and is simply as follows:—A decoction of tea is first treated with a slight excess of acetate of lead, which throws down the tannin, and almost

* Communicated by the Chemical Society, having been read March 21 and May 2, 1843.