

et de Chimie. His investigations enabled him to discover a very simple method of determining the temperature of the interior organs of man and the lower animals. Of this method he made numerous physiological applications, discovering that whenever a muscle is contracted a certain amount of heat is evolved. He was likewise one of the creators of electro-chemistry. In 1828 he availed himself of this new science in the production of mineral substances, and in treating by the humid process the ores of silver, lead, and copper. In consequence of these valuable researches he was elected a corresponding member of the Royal Society of London, and in 1829 he was made a member of the Académie des Sciences. In 1837 the former society awarded him the Copley medal for his numerous researches in electrical science. In the same year he was appointed Professor of Physics in the Museum d'Histoire Naturelle, and was also promoted to the rank of Commander of the Legion of Honor. Among the list of new substances which Becquerel obtained by the action of electricity may be mentioned aluminum, silicon, glucium, crystals of sulphur and of iodine, and numerous metallic sulphides, such as dodecahedral pyrites, galena, sulphide of silver, iodides and double iodides, carbonates, malachite, calspar, etc. He also discovered a process of electric coloring on gold, silver,



ANTOINE CESAR BECQUEREL.

and copper, which has been extensively applied in practice. In his electro-chemical investigations, Becquerel's object was to discover the relations existing between the electric forces and the so-called chemical affinities, and to excite the latter into action by the former. All kinds of plating with gold or silver by the humid process are only so many applications of electro-chemistry. Many of his researches relate to the electric conductivity of metals, to galvanometers, to the electric properties of tourmaline, to atmospheric electricity, the effects produced by vegetation, the electromagnetic balance, to the use of marine salt in agriculture. M. Becquerel was a voluminous writer on science, the most important of his works being "Traité de l'Électricité et du Magnétisme" (7 vols., 1834-40), "Traité d'Electro-Chimie," and "Traité de Physique Appliquée à la Chimie et aux Sciences Naturelles." Besides these he published, in conjunction with his son, M. Edmund Becquerel, several works on meteorology, on agricultural chemistry, etc., etc., and on several divisions of electrical science, to which the father and son had devoted the larger part of their lives. M. Becquerel died on the 19th of January, 1878, at the advanced age of ninety years.

ON THE ETIOLOGY OF THE CARBUNCULAR DISEASE.*

By L. PASTEUR, Assisted by Messrs. CHAMBERLAND and ROUX.

ONE of the diseases which cause the greatest destruction of cattle is the *carbuncular disease* or *anthrax*. Almost all portions of this country suffer from it; some in a slight degree and others very heavily. The pecuniary loss from this disease is very serious in some localities, as, for instance, in the department of Eure et Loire. Among the many herds raised there, there is hardly one which is not afflicted by it every year. Any farmer there considers himself fortunate, and even pays no attention to the disease, if his loss is not greater than from two to three per cent. of the number of animals in his flock. This disease is known in all countries. In Russia it is particularly disastrous, and it is called the *Siberian plague*.

For a long time the belief has prevailed that the carbuncular disease is due to various incidental causes, such as the nature of the ground, of the water, of the fodder; the methods of breeding and of feeding. Every cause has been invoked to explain its spontaneous existence. Lately, however, the researches of Messrs. Davaine and Delafond, in France, of Pollender and Branell, in Germany, have called attention to the existence of a microscopical parasite in the blood of animals who have died of this disease. Moreover, rigorous researches have disputed the doctrine of the spontaneous generation of microscopical beings, and the effects of fermentations have been attributed to specific microscopical germs. From these causes, the idea has arisen that possibly animals suffering from carbuncular disease may have acquired its germs, which are the germs of the parasite, from the exterior world, and that there is not, properly speaking, any spontaneous origin to this disease. This opinion became still more definite when, in 1876, Dr. Koch, of Breslau, published that the bacteridia, in its vibronary or bacillary shape, may be resolved into germ corpuscles or spores.

Two years ago, I had the honor of submitting to the Minister of Agriculture and to the President of the Council General of Eure et Loire, a project of research on the etiology of the carbuncular disease, which was accepted with

alacrity. I also had the good fortune to find in M. Manoury, Mayor of the Village of St. Germain, near Chartres, an enlightened agriculturist, who had the kindness to allow me to establish on his farm a small flock of sheep, under the same general conditions that are usual in the Beauce for sheep penned in the open air. Moreover, the Superintendent of Agriculture very obligingly placed at my disposal two shepherd pupils of the School of Rambouillet, to watch and feed the sheep.

The experiments began in the first days of August, 1878. These consisted in feeding certain lots of sheep with lucern, watered with artificial cultivations of the bacteridia of carbuncular disease, containing the parasite and its germs. Without entering into details in this place, I will give the following results of our experiments:

Notwithstanding the immense number of spores of bacteridia swallowed by all the sheep of one lot, many of them escape death, often after being visibly ill; others, in smaller numbers, die with all the symptoms of spontaneous carbuncular disease, after an incubation, which may extend to eight or even ten days, although toward the last the disease assumes those suddenly violent characteristics which have led some observers to think that the period of its incubation is very short.

The mortality may be increased in a marked degree by mixing with the food soiled by the germs of the parasite, bodies with sharp points, such as the pointed ends of thistle leaves, and the barbs of oats cut up into fragments about a centimeter in length.

It was important to ascertain if the *post mortem* examination of animals dying in these conditions would show lesions similar to those which are observed in animals who die spontaneously in sheepfolds or in open air pens. It was found that the lesions in all cases were identical, and the nature of these lesions authorize the belief that the disease begins in the mouth and in the back part of the throat. The first observations of this kind were made in *post mortem* examinations, conducted under our own eyes, by M. Boutet and by M. Vinot, a young veterinary surgeon, and a graduate of the School of Alfort. Both of them have helped us with great zeal during all our experiments in St. Germain.

The idea that sheep which die spontaneously from the carbuncular disease in the department of Eure et Loire are infected by the spores of the bacteridia of this disease mixed with their food, acquired more consistency in our mind from these examinations. But whence come the germs of these bacteridia? If we reject every theory of the spontaneous generation of this parasite, we must direct our attention to the animals buried under ground.

We must here explain what is done when an animal dies spontaneously from carbuncular disease. If there is an establishment in the neighborhood for skinning animals, the body is taken there. If no such establishment is in the neighborhood, or if the hide is of little value, as is the case with sheep, a grave is dug from 0.50 to one meter deep and the body is thrown in and covered over with earth. This grave is dug wherever the animal has died, or in some neighboring field, if he dies in a stable. We may ask: What happens in this grave, and is there in it any cause for disseminating the disease? Many persons will answer in the negative, for Dr. Davaine has ascertained by accurate experiments that an animal who has died of anthrax cannot, after putrefaction, communicate the disease. Very recently numerous experiments have been made by one of the eminent professors of Alfort, a great partisan of the spontaneity of all diseases. He has reached this conclusion: "That waters charged with the blood of animals who have died of carbuncular disease; that composts made by stratifying earth, sand, and stable manure with remains of bodies of dead sheep brought from Chartres, have never (by inoculation) caused the least symptom of carbuncular disease." (Colin, *Bulletin de l'Académie de Médecine*, 1879.) But here we must take into account the difficulties of this research, difficulties of which M. Colin was entirely unaware. To take specimens of earth from the fields of the Beauce, and show in them corpuscles from one to two thousandths of a millimeter in diameter, capable of infecting animals with the carbuncular disease, this is in itself a difficult problem. However, by proper washings, and by making use of the susceptibility of Guinea pigs and rabbits to contract the carbuncular disease, something could be done if the parasites of this disease were the only ones in the earth. But the earth must contain an infinite multitude of microscopical germs of various species, and in the cultivation of these on a living animal, or artificially in vessels, they interfere with one another.* During the last twenty years I have often called the attention of this Academy to the struggle for existence between microscopical beings. I may add that to isolate the carbuncular bacteridia from a portion of earth in which it may exist as germs, recourse must be had to special methods, whose application requires the most delicate attention. The action of air, of vacuum, changes in the nature of the media of cultivation, influence of variations of temperature; these are the means which must be used to prevent one germ from hiding the action of another. Any method of research which is not characterized by the most careful attention is powerless, and negative results only prove that, with the conditions in which the observations were made, the bacteridia did not show itself. The main argument presented by the eminent professor of Alfort is that the bacteridia disappears from the body of an animal as soon as it putrefies. This is an accurate statement, and the fact was known by those who flay and cut up the bodies of dead animals long before it was confirmed by Dr. Davaine. I have often heard these men, when handling the dead bodies of horses who had died of anthrax, when I put them on their guard against the danger they were running, say that there is no danger when the body is in an *advanced* state; the danger only exists when it is still warm. Although this fact is not strictly accurate, it agrees very well with what is true. In a previous investigation, published by M. Jaubert and myself, may be found the true explanation of the phenomenon. As soon as the bacteridia in its filiform state is deprived of air, if it is placed, for instance, in vacuo or in carbonic acid, it resolves itself into granulations of great tenuity which are dead and innocuous. Putrefaction places the bacteridia precisely in these conditions of disaggregation. The germ corpuscles or spores do not go through the

* I am led to believe that in this infinite quantity of germs is to be found the true solution of the nitrification which Messrs. Schloesing and Nitz have shown to depend on fermentation. One day, if I remember rightly, in July, 1878, I received a visit from these excellent observers. They brought me little pellets from their nitrifying tubes, on which they had not been able to detect microscopic organisms. These, however, were full of germs. I do not believe that any special ferment, any body in the process of development (which would then have a contrary effect) causes nitrification; it is rather a physical effect of absorption and transportation of oxygen on the elements of ammonia by the innumerable germs in the earth analogous to the influence of *Mycoderma* cells on alcoholic liquids.

same process, as was ascertained by Dr. Koch. At any rate as the animals at the time of their death only contain the filiform parasite, putrefaction must destroy it entirely. If this opinion was accepted as explaining the facts that take place in nature, we would only have an imperfect idea of the truth.

When a horse, a cow, or a sheep, which has died of the carbuncular disease, is buried in the ground, we may imagine that in most cases some blood finds its way out of the body, even if the animal has not been wounded. A habitual characteristic of this disease is that at the time of death blood runs out through the nostrils, through the mouth, and even in the urine, which becomes red with blood. Besides, several days must elapse before the bacteridia are resolved into innocuous granulations by the gases free from oxygen which are produced by putrefaction. Meanwhile the excessive swelling of the dead body causes the liquids to run out through the natural openings, and through such ruptures as may exist of the skin and other tissues. The blood and other matters thus mixed with the surrounding portions of aerated earth are no longer in the same conditions as those of putrefaction, but rather in the conditions of artificial cultivation, suitable for the formation of the germs of the bacteridia. Does experiment confirm these preconceived ideas?

We have mixed blood from animals who had died of this disease with earth watered with yeast extract or with urine at the ordinary temperature of summer, and at such temperatures as are maintained by the putrefaction of dead bodies. In less than twenty-four hours, multiplication and production of germ corpuscles of bacteridia have taken place from the bacteridia in the blood. These germ corpuscles may afterward be found in a condition of latent life, ready to develop, ready to propagate the disease not only after weeks of stay in the earth, but even after years.

But these are only laboratory experiments. We must ascertain what happens in fields exposed to the open air, and to all the alternations of dryness and moisture. In the month of August of 1878, we buried in a garden of the farm of M. Manoury, a sheep of his flock which had died of the carbuncular disease, as verified by a *post mortem* examination. Ten months after this, and also fourteen months afterward, we took up earth from the grave, and we easily ascertained in this earth the presence of the germ corpuscles of the bacteridia, and, by inoculation on guinea pigs, we caused their death by the carbuncular disease. Moreover, and this is a circumstance worthy of note, the same investigation was carried on with earth from the surface of the grave, and the germ corpuscles were found to exist, although the earth of the grave had not been disturbed in the interval. Finally, similar experiments were made with earth from graves in the Jura of the depth of two meters, in which had been buried the bodies of cows that had died of this disease in the month of July, 1878. Two years afterward, which was quite recently, we have collected earth from the surface, and we have obtained deposits from it, which gave rise to the carbuncular disease. Three different times in this interval of two years we have obtained carbuncular disease from this same surface earth. We have finally ascertained that the germs on the surface of graves, in which animals are buried who have died of this disease, may be found after the operations of cultivation and after the gathering of crops. These last experiments were made in several places on the farm of M. Manoury. When these experiments were repeated on earth situated at a considerable distance from the graves, no carbuncular germs have been obtained.

I would not be surprised if, while I am speaking, doubts should rise in your minds concerning the accuracy of these observations. For how can the earth, which acts as a filter so thoroughly, allow microscopical germs to rise to the surface? Such doubts could easily find a justification in the experiments which M. Joubert and I have published. We have announced the fact that water of springs which rise from the earth, even from a moderate depth, are so entirely free from germs that they cannot produce a change in those liquids which are the most easily affected. The waters of springs, nevertheless, rise below portions of the ground through which rain waters are constantly passing, even during centuries, and their tendency is to carry downward the finest particles of the earth situated above these springs. There is certainly a great difference between such results and those to which I have called your attention, in which microscopical germs rise from below, even from great depths, in a contrary direction to the flow of rain water. Here is certainly an enigma. The members of this Academy will certainly be surprised to hear the explanation of it. You may even be astonished that the theory of germs, but lately born from experimental research, has in store such unexpected revelations. Earth worms are the carriers of germs, and it is to them that we owe it that the terrible parasite of carbuncular disease is brought to the surface from the depths of the earth, for it is in the little cylindrical agglomerations, and in the finer pellets voided by these worms, and deposited on the surface after heavy dews and after rains, that we find the germs of the carbuncular disease, together with many other germs. We may, by direct experiment, ascertain that it is to this agency that is due the transfer of the germs to the surface. If, in a volume of earth in which spores of the bacteridia have been mixed throughout the mass, we leave a number of earth worms for several days, we will, on opening their bodies, so as to carefully extract the earthy cylinders which fill their intestinal canal, find in these a great number of spores of the bacteridia.

If the loose earth at the surface of graves of animals who have died of the carbuncular disease contains the germs of the bacteridia, often in great quantities, they must originate from the disintegration by rain water, of the cylindrical excrements of earth worms. The dust from this disaggregated earth is thrown on plants growing at the level of the ground, and, in this way, animals in the open air find in some pastures the germs of the carbuncular disease, and become infected exactly in the same way as those in our experiments who fed on lucern, soiled by artificial cultivation of the bacteridia. These results lead us to meditate on the possible influence of the soil on the etiology of other disease, on the danger of cemeteries, and the usefulness of cremation!

Do not earth worms carry to the surface of the ground other germs which may, to the worms themselves, be as harmless as those of the carbuncular disease, but which may be the cause of disease to man and to domestic animals? They are, indeed, constantly filled with germs of all kinds, and in all cases the germs of the carbuncular disease are found associated with those of putrefaction and septicemia.

As to the prevention of the carbuncular disease, it seems easy of accomplishment. Animals must never be buried in fields in which fodder is raised or in which cattle are penned. Whenever such soils can be found, preference

* Translated from *Comptes Rendus de l'Académie des Sciences*, of July 12, 1880, p. 86, by P. Casamajor.—*Chemical News*.

should be given, for burying dead animals, to sandy or calcareous soils, in dry situations, as such soils are not favorable to the life of earth worms. The eminent Chief of Agriculture, M. Tisserand, lately told me that the carbuncular disease is unknown in the region of Savarts, in Champagne. The absence of this disease may be attributed to the fact that in the poor soils of this kind, as in the case of the Camp at Chalons, the thickness of arable land is not greater than 0.15 to 0.20 of a meter, and the subsoil is a bed of chalk in which earth worms cannot exist. In a soil of this kind, the burying of a carbuncular animal may give rise to many germs, which from the absence of earth worms will remain at a depth in the ground where they are harmless.

It would be very desirable to have careful statistics stating, in given localities, whether the carbuncular disease is prevalent or not, and also stating the nature of the soil, whether favorable or not to the presence of earth worms. M. Magne, member of the Academy of Medicine, has informed me that in the Aveyron, in localities in which the carbuncular disease is found, the soil is argillo-calcareous, while in those in which the disease is unknown the soil is schistose and granitic. I have always understood that in these latter soils earth worms do not abound.

I will take upon myself to close this communication with the assurance that, if agriculturists desire it, the carbuncular disease will soon be a thing of the past, because this disease is never spontaneous, and can only be found where it has been deposited, and where its germs have been disseminated by the innocent complicity of earth worms; and, finally, that, in any locality, it will soon disappear unless the causes of its propagation are maintained.

On the proposition of M. Thenard, the Academy decides that the paper of M. Pasteur will be sent to the Minister of Agriculture and Commerce.

REPORT ON YELLOW FEVER IN U. S. STEAMER PLYMOUTH.

Extracted from the Report of the Surgeon-General of the Navy, 1880.

In the Hygienic and Medical Reports of the Navy Department for 1879, Surgeon Theodor Woolverton gave an account of yellow fever on board the steamship Plymouth, which not only redounded to the reporter's credit, but to that of naval surgeons as a class. It is to be regretted that the excellent opportunities enjoyed and sound sense so often displayed by our naval surgeons have been imperfectly appreciated by students of yellow fever. Case after case has been recorded and made the subject of special inquiries, with the most frequent result of demonstrating dangerous forms of naval decay and rottenness which, in tropical cruises, have combined with the essential marine factor for the development of this plague. In his statement, Surgeon Woolverton said: "It is my conviction, from the course of the disease, that the yellow fever infection is confined to the hull of the ship, and especially in the rotten wood about the berth deck." It will be remembered that the Plymouth was frozen out, but much of the badly decayed wood was left in her, notwithstanding some considerable repairs at the Boston Navy Yard, prior to sailing for the Windward Islands, last year. No sooner was the vessel subjected to tropical heat than yellow fever recurred, and the cruise had to be abandoned. Surgeon Woolverton declared that it was "not shown that cold will not destroy the yellow fever infection, but only that cold did not sufficiently or entirely penetrate the spongy wood to reach it."

With commendable solicitude, the Secretary of the Navy has pursued this case to the end, and Surgeon-General Wales has done wisely to exhaust all means of research within his reach. The monograph before us is full of interesting and suggestive matter. It shows how gradual, and yet not slow, was the deterioration in the Plymouth's sanitary condition. Launched at midsummer, 1868, and extensively repaired in 1873, she was peculiar for having an excess of white oak over live oak in her hull, and in 1877 "she began to show herself to be a decidedly unhealthy ship." In the fall of 1869 she suffered from yellow fever, notwithstanding stringent precautions against the introduction of the disease. In a report made at Hampton Roads, November 17, 1878, Surgeon Woolverton gave it as his opinion "that the fever had a purely local origin in the ship."

It was the second outbreak, on the brief cruise of 1879, after a winter's frost, that staggered naval officers and the public at large. The board over which Medical Inspector R. C. Dean presided has dispelled the mystery, while fully sustaining the mature opinion of the steamer's own surgeon. Surgeon-General Wales ordered a thorough inspection, by exposing all parts of the ship to view, and confined spaces, whence openings gave exit to a very offensive odor, were discovered. The ceiling and hanging knees, clamps, etc., were extensively decayed all along the berth deck. Fungus abounded. Behind the locker on the port side was found a large quantity of partially decomposed refuse. Fætor was unlocked, especially above "the port and bunker, and here four cases of yellow fever, all occurring during the first outbreak, were billeted. One case, occurring during the second outbreak, was billeted on the starboard side, exactly opposite, and one near the galley—localities where decayed wood was found to be particularly abundant."

Omitting various data of a like description, it is interesting to read, that "on the starboard side one of these spaces was found to be completely filled with a soft mass, of intolerably offensive odor, which seemed to consist chiefly of beans in all stages of decay; the other space was equally full of sponge, clothing, chips, and other refuse, more or less decomposed. The stench from these deposits was such as to drive the workmen on deck."

"Between other frames, on both sides, were masses of bacterial growth similar to that found in the forward shell room; and on the port side, in the *culs de sac* corresponding to those containing the beans and refuse on the starboard side, were found a quantity of unrecognizable decomposing organic matter. It was directly above this hold that three cases of yellow fever occurred during the first outbreak."

"Under the flooring of the magazines was a great accumulation of decomposing chips and auger dust, emitting a foul odor, and evidently left there when the ship was built. Under the lead-lining the sheathing was entirely rotten and blackened, as if the wood had been charred, and in this rotten wood dead rats and their nests were found. The thick strakes and ceiling were badly decayed. On cutting through the bread-room floors, confined air of offensive odor escaped. Living and active flies crawled out of confined spaces between the floors."

"We are of opinion," says the board, "that the various deposits of decomposing organic matter, and the quantity of decayed wood above described, are closely connected

with the development of yellow fever on board of the Plymouth."

The narrative as published is most instructive, and we regret that we cannot follow the reporters in their speculations as to disinfection and the germ theory. The germ is still undiscovered, if there be any; its discovery cannot, in our opinion, upset the truth that low temperature is destructive of the yellow fever poison. That is the one incontrovertible fact in the history of the disease; but no sane man would pretend permanently to purify a foul ship like the Plymouth, without first removing all fetid accumulations and rotten wood.—*Med. and Surg. Reporter.*

FUCHSIN IN BRIGHT'S DISEASE.

In Virchow's *Archiv*, Bd. 80, Prof. De Renzi, of Genoa, gives his experiments on Bright's disease. He arrived at the following conclusions: 1. When chronic Bright's disease is left entirely without treatment, in general no improvement is shown; hence this must be excluded from the category of those diseases which in many cases end in spontaneous recovery. In the first days after entering the hospital, or when the treatment is purposely left off, the patients show a considerable quantity of albumen; this rule has, however, some exceptions which have not up to the present time been well explained. 2. Fuchsin, which has lately been recommended in the treatment of Bright's disease, produces a remarkable diminution in the quantity of albumen. It was employed in two forms: dissolved in water, or, mixed with a suitable extract, in pills of 2½ centigrammes each. As, however, the strong coloration of the infusion of fuchsin in water is somewhat repulsive, Dr. De Renzi found it preferable to administer it in the pill form, and to this he has adhered in his latest prescriptions. 3. The daily dose of fuchsin may be much greater than that in which it has hitherto been recommended in the treatment of Bright's disease. Dr. De Renzi generally began with a very small dose of 5 centigrammes (0.75 grain), raising it to 25 centigrammes (3.8 grains), to be taken in twenty-four hours. A considerable physiological action on the principal functions of the body was observed. According to the dose of fuchsin, the urine began sooner or later to show a red color, which maintained itself throughout the treatment. 4. The urine in Bright's disease often exhibits a great deal of mucus. Fuchsin is very useful against this complication, as, in a short time, it causes the complete disappearance of the mucus from the urine. 5. The mucous membrane of the digestive tract is deeply colored by fuchsin, and the blood-plasma undergoes considerable coloration. In two cases, the quantity of hæmoglobin and the chromometric state of the blood were examined with Bizzozero's instrument, with the following results: *Maria Molinari*: Cytometric state, 160; Hæmoglobin, 68.7; Chromometric state, 175. *Theresa Gabella*: Cytometric state, 115; Hæmoglobin, 29.7; Chromometric state, 112. It is evident that the increased intensity of color is not to be ascribed to the increase of hæmoglobin, but rather to the increase of the fuchsin in the blood. 6. When fuchsin does not pass into the urine, this is an indication of an organic disturbance, which must not be neglected. In these cases, it is of no use in the treatment of albuminuria. 7. Rest of the patient in bed is a very important means of diminishing the albuminuria in Bright's disease. In a patient, *Vittoria Rossi*, complete rest in bed, together with milk diet, brought about the greatest diminution of the albumen in the urine. Dr. De Renzi was several times able to ascertain that unusually active movement of the person had a dangerous effect in Bright's disease. 8. Apomorphia was in general well tolerated; and Dr. De Renzi has ordered it in larger doses than usual (viz., 5 to 6 centigrammes daily) without causing the least disturbance. In one case, this medicine considerably improved the condition of the patient.

PHOTOGRAPHY OF THE INVISIBLE.

AMID the many modifications and improvements that have taken place in the gelatine processes, we are apt to forget that, at all events in certain work, collodion is not quite put on the shelf. Among the applications of the latter is one which for the time has been lost sight of, and one which, as an aid to scientific research, no doubt, will have much to say—we allude to the process of photography by means of the invisible rays of the spectrum, which are usually called heat rays. It is now some nine months since Captain Abney gave his Bakerian lecture at the Royal Society on this subject, and, pending its publication in the Transactions of that body, the public have heard but little regarding it, though, as we can testify, the discoverer of the process has not been idle in adapting it to various uses. Among other work, we find that Captain Abney has been employing it to find out the color of colorless liquids. For instance, in the ordinary way of thinking, we class water, alcohol, ether, benzine, etc., among colorless bodies; but photographs of the spectrum, taken through a foot-thick of these bodies, show that in some of these, certain of the invisible heat rays are entirely absent; that is, if the eye were sensitive to these dark rays, ether, for example, would have as definite a color as a solution of bichromate of potash or any other visibly colored liquid. We hope we are not anticipating the publication of the paper when we give, as another example of the utility of the research, that which relates to water, which is shown with a thickness of four feet to cut off nearly every ray which could warm animal structure. Thus, at that depth the only warmth which a fish can get is that due to the absolute warmth of the water, and not to any sunshine which might pass through the body of the water. We have merely given these as examples of what a useful application this method of photographing with these dark rays may be put to. From what we have already learnt of this subject from papers communicated to the Photographic Society and elsewhere, it appears that Captain Abney employs a species of silver bromide which, when viewed by transmitted light, is of a peculiar blue tint, a tint, by the way, which seems to be different to that which is so common in the gelatine processes; and it appears from what we have learnt from him that gelatine is not a hopeful vehicle in which to obtain this peculiar modification of the silver bromide. In the first place, it seems that it is necessary to employ an excess of silver in its preparation, and also a large excess of acid, both of which would appear to be fatal to its formation in gelatine. Again, another remarkable point is that the slightest pressure is able to reduce this blue bromide of silver to a condition of insensitiveness, the pressure of dried gelatine being sufficient to do so. In one experiment Captain Abney transferred the bromide from collodion to gelatine, and prepared plates with it, expecting to find the sensitiveness intact; but the fact of the transfer rendered it slow, and totally insensitive to any rays except those ordinarily useful for photographers. The peculiarity of this

especial preparation is that, instead of having one part of maximum sensitiveness to the spectrum, it has two—one in the blue, as is the case with the ordinary preparation of the bromide, and the other just outside and below the red, and it is this part, of course, which enables photographs of the invisible radiations to be impressed. When Dr. Vogel showed that what is called non-actinic but visible light could be rendered actinic by means of the addition of certain dyes, he made a great step forward in the direction to which we have been referring, and, no doubt, as Captain Abney was good enough to point out to us, if a dye could be found which absolutely absorbed the invisible heat rays, any ordinary emulsion could be used for the same end merely by dyeing the film; but, unfortunately, all the dyes which are impressionable by light—or, as Dr. Vogel calls them, which are optical sensitizers—transmit nearly all of the heat rays, and are not, therefore, affected by them. Be that as it may, the whole of the rays of light, visible and invisible, have been proved capable of impressing a photographic image, and it now remains for photographers to utilize them in the most effective way they can. It is something even now to know that a kettle of boiling mercury can be photographed in an absolutely dark room by the radiations which it emits, and it seems in the bounds of probability that the radiations from a human body may suffice to give an impression on a sensitive plate under similar circumstances.

It is to be hoped that the publication of the Transactions of the Royal Society will not be long delayed, so that every one interested in this kind of research may have the full benefit of what has already been done.—*Photographic News.*

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TABLE OF CONTENTS.

| | PAGE |
|--|------|
| I. ENGINEERING AND MECHANICS.—Fraser's Water Meter, 3 figures.—Vertical section, horizontal section, and plan. | 4119 |
| Transmission of Power to a Distance.—Wire ropes.—Compressed air.—Water pressure.—Electricity. | 4120 |
| The Livadia at sea. | 4120 |
| The Herreshoff Launch and Ignition. | 4121 |
| New Steering Gear, 2 figures.—Steam steering gear for Herreshoff launch. | 4121 |
| II. TECHNOLOGY AND CHEMISTRY.—Glucose. | 4126 |
| American Manufacture of Corn Glucose. | 4126 |
| The Conversions.—Starch.—Dextrine.—Complete glucose. | 4126 |
| Depreciation of a Glucose Factory. | 4126 |
| The Fire Risks of Glucose Factories and Manufactures. | 4126 |
| Glucose Factory Fires and Ignitions. | 4127 |
| The Hirsch Process. By ADOLF H. HIRSCH.—Improvement in the manufacture of sugar from Corn. | 4127 |
| Time in the Formation of Salts. By M. BERTHELOT. | 4127 |
| An Old Can of Preserved Meat. By G. W. WIGNER. | 4127 |
| Chemistry for Amateurs. 6 figures.—Reaction between nitric acid and iron.—Experiment with Pharaoh's serpents.—Formation of crystals of iodide of cyanogen.—Experiment with ammoniacal amalgam.—Pyrophorus burning in contact with the air.—Gold leaf suspended over mercury. | 4128 |
| Carbonic Acid in the Atmosphere. 2 figures. | 4129 |
| On Potash Filling Soaps. By W. J. MENZIES. | 4129 |
| Photography of the Invisible. | 4134 |
| III. ELECTRICITY, LIGHT, HEAT, ETC.—Exhibition of Gas and Electric Light Apparatus, Glasgow. | 4135 |
| Electric Light in the German Navy. 1 illustration.—Armored Frigates Friedrich Karl and Sachsen.—Dispatch Boat Grille, and Torpedo Boat illuminated by Electric Light. | 4130 |
| Interesting Facts about Gas and Electricity.—Gas as Fuel.—Gas for Fire Grates. | 4130 |
| A New Electric Motor and its Applications. 6 figures. Trouve's New Electric Motor. | 4131 |
| On Heat and Light. By ROBERT WARD. | 4131 |
| Photophonic Experiments of Prof. Bell and Mr. Tainter. By A. BREGUET. | 4132 |
| Distribution of Light in the Solar Spectrum. By J. MACK and W. NICATI. | 4132 |
| Mounting Microscopic Objects. | 4132 |
| New Sun Dial. By M. GHOOTYX. 1 figure. | 4132 |
| Antoine Cesar Becquerel, with portrait. | 4132 |
| IV. HYGIENE AND MEDICINE.—On the Etiology of the Carbuncular Disease. By L. PASTEUR, assisted by HAMBRELAND and ROUX. An extremely valuable investigation of the nature, causes, and conditions of animal plagues. | 4133 |
| Report on Yellow Fever in the U. S. Steamer Plymouth. By the Surgeon-General in U. S. Navy. | 4134 |
| Fuchsin in Bright's Disease. | 4134 |
| V. ART, ARCHITECTURE, ETC.—Artists' Homes, No. 7. Sir Frederick Leighton's House and Studio. 10 figures. Perspective, plan, elevation, details, etc. | 4121 |
| Initials by Eisenlohr and Weigle in Stuttgart. Full page. | 4123 |
| Suggestions in Decorative Art. 1 figure. Reserved part of a Great Saloon. By H. PENON, Paris. | 4124 |
| Great Saloon (text). | 4124 |
| Cologne Cathedral. The Historical Procession. | 4124 |
| Suggestions in Decorative Art. 1 figure. Mantelpiece in Walnut. By E. CARPENTIER. | 4125 |

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