NATURE OF POISON OF YELLOW FEVER, AND ITS PREVENTION.*

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YELLOW FEVER is a disease, running a regular course, and accordingly depending, like small-pox, scarlet fever, measles, and other kindred diseases, upon a specific poison of animal origin, being a product of the diseased human organism itself. This assertion is proved by the characteristic feature it presents in common with all other specific diseases, consisting in the immunity from a second attack, imparted to the individual once affected; and this immunity from a second attack, which most strikingly distinguishes specific diseases from all others, must be constantly kept in view, in studying the nature of the peculiar poison producing yellow fever. In connection with this prominent feature of the disease, I may be permitted to mention that, from what I have learned, my friend Professor J. Dickson Bruns has, in his lectures to medical classes, always laid particular stress upon this feature of yellow fever, for the purpose of proving its contagious nature.

The precise nature of the particular pathological changes which impart to the organism this immunity from the effects of the poison afterward, we are at present unable to determine. Perhaps the most popular view is that the poison alters the constitution of the blood in such a manner, and so permanently, as to be unable to make any further impression upon it afterward. Equally difficult is it to say whether the changes are wrought upon the colored blood-corpuscles in particular, or upon the plasmatic portion, the liquor sanguinis; furnishing the nutriment to the organs and tissues of the body. Judging from the unusually rapid disturbance in the nutrition of the organism, manifested by the phenomenon of fatty infiltration and degeneration in a number of organs, and also by nervous disorders, the liquid portion of the blood would appear to be first affected; while, on the other hand, the facility with which the blood corpuscles seem to part with their hæmoglobin, as my observations have shown, indicates that these bodies must likewise be affected. For a number of years I have, in relation to the difference existing in the impression of the poison upon the blood in miasmatic and in yellow fever, been in the habit of illustrating it in a somewhat homely manner by assuming that in the former the impression of the poison is only functional in character, and in consequence evanescent; while in the latter it is organic in its nature, and therefore permanent, and insuring immunity from a second attack. But, though it appears to me very reasonable that the immunity should depend upon certain permanent changes, wrought by the poison upon the blood, I cannot forbear to presume that similar changes may at the same time take place in the nervous tissues, rendering them in the same manner unimpressible for another attack.

Besides the immunity from a second attack, characterizing yellow fever as a disease produced by a specific poison, the clinical symptoms, and, moreover, the anatomical changes observed after death, also speak for its peculiar and specific character. But, as a specific disease cannot be produced in the human body by a general cause, it is obvious that the specific poison producing it must be derived from another human body affected by the same poison, and that this poison must necessarily be a product of the organism of the affected individual. We may, therefore, presume that a general cause cannot produce a specific disease.

To illustrate this proposition, we may take, for example, a number of persons of different age, sex, and constitution, exposed to a sudden and severe change of temperature by leaving, insufficiently clothed, a warm and overcrowded room, and entering the cold night air of the street. It will be found, in this case, that the effects produced by a general cause—the cold air—upon these persons, may differ in each individual; for, while in the one they may manifest themselves in the form of a simple coryza, they may result in the others in bronchitis, pneumonia, rheumatism, diarrhoea, or other affections, implicating the one or the other organ of the body. Each of these different affections, at first local in its nature, may, if extending, involve the nervous system sufficiently to give rise to the phenomenon of fever. Here we see how one and the same cause produces different effects, according to the peculiar diathesis or constitution of the affected individual. But, if now, on the other hand, a number of these persons, some time after they have recovered from their various affections, are introduced into a room, the air of which is tainted with the exhalations of a number of small-pox patients, while the rest of them are taken to a room infected with the exhalations of yellow patients, it will be found that the first party will surely be affected with small-pox, and the other with yellow fever, unless they had been affected with these diseases at a previous time. Each person will, to the exclusion of every other disease, become affected with that particular disease of which he has inhaled the specific poison, emanating from the affected individuals. In every contagious disease, therefore, the infectious poison first proceeds from the diseased organism itself, and, in the same manner as we have seen in putrefactive septicæmia, increases in intensity with each individual through whom it passes.

From this explanation, it will be seen that yellow fever, when it first appears, never attacks a number of persons, isolated from each other, at one and the same time, but always, like every other contagious disease, starts from single centers, represented by the infected individuals. And the only manner in which it spreads is either directly from individual to neighboring individual, or indirectly, when the poison, emanating from the affected individual and adhering to clothes and other objects, is carried to a distant place and inhaled by another person. A number of centers may arise in different localities of a city, which, increasing in dimensions, may at last meet. In this manner alone the infectious poison is able to extend over a large city, like New Orleans; and the surest proof of this assertion is offered by the slow march in which the disease walked over our city during the last epidemic. An infectious poison, on the contrary, being contained and distributed throughout the air of a city or locality, either in the form of living organisms, or in the form of vegetable or animal effluvia arising from decomposing matter, will necessarily affect every person susceptible to infection nearly at the same time, and, in consequence, rapidly extend over a city or district. For this reason, the theory of a contagious virus, or of the miasmatic or effluvial origin of yellow fever, is entirely inconsistent with the contagious character of the disease. But, notwithstanding these existing circumstances, there are still a number of physicians who, while they recognize the contagiousness of yellow fever, entertain at the same time the incongruous idea that the cause of the disease is represented by minute living organisms, and, for the sole reason, as they say, that this is

the only theory upon which they can understand the multiplication of the poison, as nothing could multiply unless it were endowed with life.

I shall try to explain this subject satisfactorily by a suitable comparison. In every organized body, whether plant or animal, we meet with an albuminous substance which is regarded as the basis of life, and is generally known by the name of "protoplasm." This protoplasm, as we all know, forms the basis of all tissues performing a vital function, whether consisting of cells or fibers; and, though it is not an organized body, it nevertheless represents living matter and is endowed with the power of appropriating other matter unlike itself, and of imparting to it its own properties; or, in other words, of converting this matter into protoplasm. Now, it is upon this principle that all organs and tissues of the body not only rejuvenate themselves but also increase in size. Thus, the protoplasm of a muscular fiber appropriates new matter and converts it into muscular fiber; the protoplasm of a ganglion cell imparts its properties to the same new matter furnished by the plasma of the blood and converts it into nervous tissue, etc. The new matter is in all cases the same, but it is the protoplasm of each particular tissue which represents the converting agency.

The specific poison, having entered the circulation, and being diffused throughout the blood, imparts, like the protoplasm, its own properties to this fluid, giving rise to all the pathological phenomena characterizing the disease. Still, it must be remembered that the degree of intensity of the disease depends entirely upon the quantity of the poison taken up by the blood. If the quantity is sufficiently large to affect all the blood circulating through the organism, the disturbance of nutrition would be so great as to interfere at once with all the vital functions, and the case would prove fatal very rapidly. We may, therefore, presume that, with some rare exceptions, in all other cases of yellow fever the poison affects only a smaller or larger portion of the blood, leaving to this fluid sufficient integrity to nourish the organs and tissues at least for some days, thus affording an opportunity to the organism to rid itself of the poison. This, however, can be accomplished in no other way than by means of the action of secreting, or, as we may say in this case, excreting cells, the same organs through which the organism rids itself of all other foreign or wasted matter. Thus the poison, already increased in quantity by that portion of blood to which it imparted its own properties, is absorbed from this fluid by the cells of different glandular organs during its passage through the capillaries surrounding the individual minute glands. And, in passing through these cells, it again increases in quantity by imparting its noxious properties to their natural secretion, together with which it is finally eliminated from the organism. In yellow fever, therefore, as in all other contagious diseases, it is in the secretions, especially those of the glandular cells of the skin and of the respiratory passages, that the noxious poison is contained, and with which it is removed from the system.

In some contagious diseases, the poison, before leaving the body, gives rise to certain pathological processes in the epidermic cells of the skin, manifested by divers exanthematous eruptions which, in some instances, as in small-pox or the cow pox, proceeds to the formation of vesicles and pustules containing the poison in liquid form. Nevertheless, even in these affections, a portion of the poison leaves the body in a gaseous form, possessing the same virulent properties as the liquid contained in the vesicles and pustules. In yellow fever the poison emanates from the body of the affected individual only in a gaseous form, in which it may be communicated to another individual, or may adhere to surrounding objects, as clothes, bedding, furniture, etc. In the latter case, however, it may be supposed that, while adhering to these objects, it undergoes a certain condensation, in which state it is transported to distant places. But, as soon as acted upon by a certain degree of moisture and heat, it again assumes the gaseous form, in which it may then be communicated to other persons. But, as the intensity of the attack of the disease is perhaps in most cases proportionate to the amount of poison taken up by the affected person, it is not necessary that the first case to which the poison is conveyed by the medium of these objects shall terminate fatally. On the contrary, a number of persons may successively become affected without a fatal termination. But, as the yellow fever poison, like the purulent poison of septicæmia, increases in intensity with each individual through whom it passes, death will eventually be the result. This fact will explain the phenomenon of the considerable interval of time which has frequently been observed to intervene between the first fatal cases of yellow fever; cases appearing in different localities of a city and between which no connection can be traced, for the reason that it is only the fatal cases which in the beginning of an epidemic attract attention.

Much has been said and written about certain meteorological conditions required for the activity and propagation of the yellow fever poison, such as a certain amount of moisture in the air, together with a continued tropical heat, a deficiency of ozone, etc. Though nothing positive concerning these conditions has thus far been ascertained, I entertain no doubt that these suppositions are to a certain extent founded upon truth; for, the fact that yellow fever has assumed the dimensions of extensive epidemics only at shorter or longer periods, while, on the other hand, it has frequently existed in the form of so-called minor epidemics, or been even represented only by a few sporadic cases, shows that these atmospheric conditions are not always present. Neither does the noxious poison, after its absorption into the blood, manifest its activity in the same degree in different persons, a phenomenon to be explained by the different degree of susceptibility which these persons possess to the effects of the poison, depending probably upon the particular state of their constitutions at the time when it is brought in contact with the latter. Thus we frequently meet with persons who have been exposed in many different ways to the action of the poison during several severe epidemics, without ever being affected, but who, after the lapse of many years, are finally attacked by the disease. Nothing is known of the nature of this singular immunity, whether it is owing to a peculiar constitution of the blood or of the nervous system.

Although the customary assertion that native-born or long acclimated persons enjoy immunity from an attack of yellow fever has now been sufficiently proved as unfounded, it cannot be denied that, in most instances, acclimatization greatly ameliorates the severity of the attack. This is most strikingly observed in unacclimated persons, especially in those who have lived in northern latitudes, beyond the yellow fever zone, who were generally the first victims of the disease. And it has become a general rule, corroborated by all writers, that the nearer a person has been born and lived to the North Pole, the more liable he is to be attacked by the

disease, and the smaller his chances are for recovery. This fact evidently shows not only that there exists a difference in the constitution of the blood of these different persons, but, moreover, that the air of the yellow fever zone must in some essential points differ from that of northern climes, enabling it to exert a certain unknown influence upon the constitution of the blood of those persons who breathe it, adapting it, so to speak, to the yellow fever poison. And to this circumstance it is mainly due that the rate of mortality during the last epidemic has been proportionately much larger in Memphis, Vicksburg, Holly Springs, and numerous other places, than in New Orleans, or even in other parts of southern Louisiana.

Although the facts already mentioned suffice to prove the animal origin of the poison producing yellow fever, and the contagiousness of the disease, I shall, in corroboration of this assertion, adduce some additional evidence. In discussing in the preceding pages the application of the germ theory to infectious diseases, I have already shown its futility, unless the actual presence of the bacteria in the blood of the diseased individual is proved in every case occurring; for even a few exceptions will show that the presence of these organisms is not essential to the disease. Now, in yellow fever no bacteria, or any other living organisms, are found in the blood of the patients during any stage of the disease. As I have stated in my paper on the pathology of yellow fever, published in the February number of the New York Medical Journal, 1879, I examined at the Charity Hospital, during the last epidemic, very carefully, the blood of fifteen living patients in the different stages of the disease, without detecting even a single bacterium, or any other foreign body in this fluid. Neither did I observe any of these organisms in the blood when examined shortly after death; it is only when decomposition commences that they make their appearance. But not only is the blood, but the other tissues also are found free from anything living, provided they are guarded from decomposition. These facts require no further comment, as they show most forcibly the impossibility of applying the germ theory in any way to yellow fever. There is, however, another fact corroborating my assertion that the noxious poison of this disease represents a vitiated secretion of the diseased body itself. It relates to the peculiar odor associated with certain contagious diseases, such as small-pox, etc., emanating especially from the secretions and excretions of the skin. In small-pox this odor is sufficiently strong and characteristic to enable the physician, in some cases, to diagnose the disease by the olfactory sense alone, and before the eruption has appeared. In yellow fever, also, there is a peculiar odor associated with the exhalations of the patient. This odor is so peculiar as to have always been observed, and also described by a number of writers; some of these even regard it as a pathognomonic symptom of the disease. Emanating from the body of the patient, the odor may be regarded as a manifestation of the contagious principle in its gaseous state. And judging from its strength, as it emanates from only a single patient in a badly-ventilated sick room, it is easy to form an idea of the quantity of the poisonous principle to which it is due; and, furthermore, of the facility with which it may adhere to the clothes of visitors to be carried to distant places, or how easily it may communicate itself through the open windows to neighboring houses and people.

The peculiar odor associated with the exhalations of yellow fever patients, I noticed very particularly while I was engaged in examining the blood of living patients during the last epidemic, and when, in order to obtain the specimen of blood, I was obliged to approach and remain for some time in very close contact with the patient. And it was during these examinations that I became affected with the disease myself, though I had escaped it in former epidemics—an attack, however, from which I soon recovered. But it is not only the living body from which this odor is perceived to emanate; on the contrary, the cadaver also, as long as it is warm, is characterized by a peculiar odor, slightly differing from the former. Here, however, it is especially perceived when the cavities of the body are opened, or when the muscles are deeply cut into or laid bare.

The question, whether yellow fever may spontaneously arise among us, has always been agitated among medical men, and, owing to the existing diversity of opinion regarding the nature of the disease itself, has never been satisfactorily answered; and unless physicians will base their opinions only upon solid, observed facts, instead of unobserved fancy, it never will be correctly answered. As far as I am able to judge, yellow fever is almost always communicated from person to person in the manner already explained; and reasoning from this fact, it is obvious that, in order to arise anew after having been once extinguished, it must be freshly imported. But, besides this, it is also possible that some of the poison, adhering to clothes and other objects during the winter, in a state of inactivity, may resume its activity as soon as the particular conditions required are present, and thus give rise to new cases of the disease. The possibility of such an origin is proved by a number of authenticated cases of scarlatina, variola, and other contagious diseases, which arose in this manner. But, in considering that the disease must have arisen spontaneously at some previous period, however remote, it is only reasonable to admit also the possibility of a spontaneous origin at the present time. However, if the disease ever arises spontaneously in this manner, it can only be confined within the limits of a small locality, and in the presence of all the elements, necessary to a spontaneous origin of the disorder in the human organism, from which eventually the specific poison emanates. Typhus fever, though in most cases communicated from person to person, has spontaneously arisen in this manner, when a large number of persons, insufficiently clothed and nourished, have been crowded into a small room without ventilation. In such a case the exhalations emanating from the bodies of the various individuals, already vitiated by the broken-down constitutions, are inhaled again, and, together with other causes arising from the sufferings and wants of these individuals, such as a continued exposure to cold, etc., give rise to that peculiar condition of the blood characterizing this fatal disease. Yellow fever may sometimes arise under similar circumstances, though, unlike typhus, only in the presence of a high temperature, combined with moisture and other atmospheric conditions. Such an origin, however, appears to me not very probable, though its possibility cannot be disputed.

Regarding the decline and final disappearance of an epidemic of yellow fever, it is very generally believed by physicians, as well as by the people, that the noxious poison is destroyed by frost. The apparent truth of this assertion, however, is rendered very doubtful, not only by the consideration of the fact that at New Orleans the epidemic has almost always nearly disappeared when the first frost occurred, but, furthermore, by the new cases arising frequently long after the appearance of this meteorological phenom-

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non. Thus, in the epidemic of 1867, I paid the first visit to my last case of yellow fever on the tenth day of December, a case which, though not proving fatal, was nevertheless a severe one, accompanied with jaundice and extensive hemorrhages from the lungs, the patient being an unacclimated foreigner. The same circumstances obtained, as far as I remember, during the last epidemic at Memphis and other places situated more northerly than New Orleans, where the disease continued to prevail for some time after the appearance of the frost. Thus the epidemic always reaches its maximum height a considerable time before frost appears; and there is no doubt that its decline is mainly owing to the decrease of the number of those persons susceptible to the action of the yellow fever poison, and also to the gradual disappearance of those unknown meteorological conditions of our atmosphere which, when present, favor the evaporation of the poison from the bodies of the affected individuals, and its inhalation by other persons; while, at the same time, the systems of those persons who thus far escaped the disease are reinvigorated with the approach of cooler weather in the month of October. These are, I suppose, the main causes to which the decline of the disease may be attributed, though the germ-theorists firmly believe that the frost kills the germs, representing, as they suppose, the cause of yellow fever.

In regarding the nature of the cause of yellow fever, such as has been expressed and demonstrated in the preceding pages, there will be no difficulty met with in determining the proper means of prevention, especially if an agent could be found possessing the property of effectually destroying the activity of the yellow fever poison, adhering to the clothes and other effects of passengers, or to merchandise brought in ships to our shores from distant ports infected with the disease, but without damage to these objects themselves. Without such an agent it is impossible to establish a quarantine both efficient and, at the same time, not interfering seriously with the interests of commerce. It is, therefore, of the greatest importance that to this subject particularly the attention of scientific men should be directed. In recent times very numerous investigations and experiments have been made by the scientific men of Europe for the purpose of discovering a disinfecting agent possessing the properties above mentioned; and, accordingly, many substances have been tested, the most efficient of which, however, are impracticable for the purposes of a quarantine. As it is impossible to give an account of the details of these experiments in this place without enlarging upon the subject, I will only mention that the process of oxidation is generally looked upon as the surest means of disinfection, and that, in consequence, free ventilation is in all cases the first law to be observed. Thus the exposure of infected articles to the fresh air alone may be sufficient to cause the decomposition of the adhering noxious poison. More efficient, however, will be the artificial application of pure oxygen, especially in its electrical state, that of ozone. Dry heat, also, is very effectual, and has been used for a number of years in European hospitals for the purpose of disinfecting the clothes of patients affected with contagious diseases. In fine, the poison must either be decomposed by oxidation, or be rendered harmless by diffusion in the higher strata of the atmosphere, where it will undergo a still more certain decomposition by the influence of the ozone upon it. Sunlight, also, is a most efficient agent for the destruction of animal poison.

But if, notwithstanding an efficient, well-regulated quarantine, yellow fever should again make its appearance in our city, the most important sanitary measure would certainly consist in a perfect solution of the first cases from the surrounding community, a policy which in every European country is practiced with the greatest care and severity, and which is the only sure way to be pursued for preventing a contagious disease from assuming the form of an epidemic. If thus the poisonous principle is confined to a limited locality it must eventually become destroyed by the proper means, or even by the action of the air alone.

In concluding this article, I cannot forbear to point once more to the absolute necessity of first studying more systematically the pathology of yellow fever before we can hope to keep this disease successfully in check—that is, confined to limited localities; and, furthermore, to renounce all empty speculations regarding the nature of its cause; but, on the contrary, to look the enemy in the face, in regarding it as a contagious disease of animal origin, such as I have demonstrated in this essay. For if, on the other hand, the cause producing yellow fever really consisted of minute living organisms, existing in large numbers in the air we breathe, and capable of multiplying indefinitely, as has been asserted by some writers, it must be obvious that any quarantine, however strict and well regulated it might be, must prove a failure. And, moreover, as these organisms would be inhaled by every person alike, and indefinitely multiply within the blood, there would not remain the slightest hope for any person ever to recover from an attack of yellow fever.

THE ELECTRIC LIGHT IN LARGE CITIES.

At the recent session of the British Parliamentary Committee, Mr. William Henry Preece, assistant engineer and electrician to the Post-office, was a witness. In reply to the chairman, he said he had devoted much attention to the progress of electric lighting. The application of this light to the Post-office was one of serious importance—first, on economical grounds, and secondly, from a hygienic point of view. The Post-office paid about £7,000 a-year for gas, so that any means of reducing the expense was a matter of importance to them; and again, in their large telegraphic galleries they had at times 1,000 people all through the night, during which a great part of the work was done, owing to the press messages. Five million cubic feet of air were vitiated, of which one million were by the operators. The other four millions were vitiated by the gas, and it was of importance to be able to remove those four millions. He had examined the various systems of electric light in London and Paris, and seen everything that was possible in connection with the subject.

The Chairman: What are the conditions you think should be attained for illuminating the great telegraph rooms?

Mr. Preece replied that three conditions were necessary: (1) Any electric light should be of the brilliancy of at least 1,000 candles, as the structure of their rooms was such that such a light would enable them to be illuminated as efficiently as they now were. (2) The light should be absolutely steady, for in carrying out the work—reading dots and dashes on thin strips of paper—it was essential that the eyes should not be wearied by an unsteady light. (3) The light must last all night long without requiring attention. Their lights in winter sometimes were burning for eighteen hours, and therefore any electric light must at least last that time.

He had not yet found any lamp that had combined these three conditions. The obstacles to this did not, however, consist in the currents, for in the dynamo machine perfection was almost reached; and the Siemens machine reproduced in the form of electric currents 90 per cent of the capital energy thrown into it. The difficulty was in the lamps. The Serrin lamp gave the requisite brilliancy, but not the durability; the Wallace-Farmer would burn for a week, but did not give a steady light; the Werdermann was perhaps the steadiest lamp, but it did not meet the said essentials. He had made experiments as to the subdivision of currents, and he found that if a series of lamps were joined upon one current the intensity of each lamp diminished with the number inserted. As to the cost, he thought each horsepower economically applied ought to give a light equivalent equal to 1,250 candles. The lights on the Embankment were produced by a Gramme alternating machine, which was practically four machines using 90-horse power. Each of these machines ought to give 6,250 candles of light, but they were joined up in series, and in each wire there was a series of five candles. The square of these was 25, and if 6,250 were divided by 25 the result was 250, which ought to be the light of candles not obscured by globes. The globes diminish the light by 40 per cent., so that the light on the Embankment ought to be equal to 150, and photometrical experiments agreed with that figure. The electrical light was only economical when one machine was used to produce a single light, otherwise there was waste.

The Chairman: What is your opinion as to the appliance of the electric light for general street lighting?

Mr. Preece said he did not think it was applicable, because if a long street had an electric light at one end sufficiently powerful to illuminate the extreme end of the street, then at the nearest end there must be an enormous light, for as the light diminishes with the square of distance the loss at one end must be largely in excess of the light at the other. From that point of view he had written a paper in which he brought out a result so extraordinary that he almost hesitated to accept it—that a street of 1,000 ft. in length with 40 gas-lamps of 15-candle power would be better lighted than with one electric light of 6,000-candle power. But in symmetrical spaces, such as squares, then one single light would have the same effect as a largely distributed light, and would also be economical.

The Chairman: The point upon which I am specially anxious to examine you upon is whether there is any danger to be apprehended from the general use of wires to convey electric light with regard to the telegraph wires?

Mr. Preece: That is a point to which we have paid a great deal of attention; and as soon as we found so many corporations applying for power to lay down pipe wires, I investigated this question to see how far such wires would interfere with our working of wires. In the first place it was necessary to ascertain the relative power of these different currents, and I found that the strength of the light currents was 3,000 times that of the telegraph currents. I placed telegraph wires alongside light wires, and watched their effect with a telephone. The wires were 6 ft. apart, and we found a very powerful effect; and the inference from my experiments was, that we could not place our wires within 6 ft. of the lighting wires, so that any wires conveying illuminating powers ought to be at least that distance apart, unless they are crossing each other or are only of short length. As Mr. Cooke said, a return wire would prevent induction, but that would necessitate doubling our system.

In reply to Lord Lindsay, Mr. Preece admitted the condition that an electric light should be equivalent to 1,000 candles within a small limit; but he had placed it thus low because lamps capable of that power existed, though they did not meet his other conditions. But there had been a great advance in the direction of steadiness, and there were indications of ultimate success in that direction. According to theory, if there were two lamps at different points in a large hall, one-fourth of the central light would be obtained, but practically the result was less than one-fourth. He did not think the telephone would be an instrument of the future and be largely adopted by the public; for although it had been largely adopted in America, we had not the same necessity for it, for we had a superabundance of messengers for all purposes, which the Americans had not. His calculation that forty 15-candle lights would be better than one electric light of 6,000, was so startling that he was inclined to think there must be some mistake.

Lord Lindsay: I am rather inclined to agree with you in that calculation.

Mr. Preece said further that the Post-office never used return wires, and he could not tell what they would cost, but their adoption would mean the doubling of their present system, which had been laid down at a cost of £10,000.

By Sir U. K. Shuttleworth: One great disadvantage of the electric light would be that the shadows would be all in one direction.

By Mr. Moore: There would be no objection to the light and the telegraph wire being more than 6 ft. apart.

By Sir D. Wedderburn: He laid stress on the hygienic advantages of the electric light as against gas, because of the vitiation of the air by the latter. Where one person would vitiate 3,000 cubic feet of air one gas burner would vitiate 9,400 ft. The persons employed in the Post-office galleries did not suffer, because every possible precaution was taken, and they had a splendid system of ventilation.

By Mr. Henry: The light used at the Albert Hall and the British Museum would not serve for the Post-office, for it would not burn long enough nor sufficiently steady. The same wire could not be used for lighting and for telegraphing, because of the difference in strength, the strength of the lighting current being infinitely greater than the telegraphing current.

By Sir D. Wedderburn: At first the telegraph wires were placed within 6 in. of each other, but as the apparatus improved, and the work increased, the distance was enlarged, till now they were 13 in. apart, and even more than that for long circuits, as from London to Holyhead. As to why the Post-office did not put their wires underground, as the Germans did, that would involve three or four times the cost of the present system, and would diminish the transmitting power of the wires to some extent.

By Lord Lindsay: Last year the Post-office transmitted messages from London to Ireland by the Wheatstone automatic instrument at the rate of sixty-one words a minute, but now the rate was 150 a minute, and next year it would be 200; but if light wires were placed near these wires, the speed would be reduced probably to sixty-one.

By Mr. Stanhope: The Post-office had not adopted the magnetic machine; they used only batteries for producing currents.

EDISON'S ELECTRIC CANDLES.

In a recent English patent, by T. A. Edison, the special feature of the invention relates to the candle for the diffusion of the electric light. Mr. Edison's candle is in the form of a slightly tapering hollow cylinder, divided vertically, except at the upper end. By this arrangement he claims that uniformity and complete incandescence are secured as the electric current passed up one side and down the other. At the base of the cylinder, which is enlarged for the purpose, the electric conductors are connected. A thermal circuit regulator, which has been described in an earlier patent recently published, is attached at the bottom of the candle, and is so arranged that, if the current becomes excessive, the regulator becomes heated, and, consequently, expands so as to bring a movable spring into contact with the adjusting screw of the illuminating apparatus, and thereby diverts the current and lessens its action in the light.

Mr. Edison specifies the materials of composition and method of manufacturing the light-giving substance, the essential condition of which is necessarily its power to resist fusion by the heat developed in the passage of the current. He says:

"Metals or oxides of metals are made use of which produce in a comparatively large candle sufficient resistance to render the whole incandescent. Finely divided metal or particles of metal having a high melting point are caused to adhere by earthy materials, such as magnesium, or zircon oxides, or magnetic oxide of iron, or other substances that are with difficulty fused. The oxides of metals may be obtained by chemical precipitation or otherwise, and the candle is moulded either in a dry or moist condition by pressure. The fine particles of metal may be platinum, iridium, rhuthium, or other metal."

The patentee then details the mode of manufacture:

"In cases where these oxides or earthy materials are mixed with the fine metallic particles, such particles are thoroughly and uniformly mixed into a mass before it is moulded, and hence such particles are kept separate by the earthy material, and cannot fuse or run together, and the metallic oxides themselves are electric conductors to a greater or less extent, and hence may in some instances be used without the particles of metal. In all cases the substances are moulded and pressed into shape under powerful pressure, and if the particles do not adhere together sufficiently, sugar, tar, silica, or similar substances may be used in moulding the candle into shape, and these substances may be volatilized by the heat. The metallic particles are rendered highly incandescent, and the earthy substances and oxides are also rendered luminous by the passage of the electric current."

Finally, the patentee declares that his invention "is not limited to any particular form in which the candle may be moulded or otherwise shaped, but it relates to the materials employed in such candles, whereby the mass is adapted to the passage of the electric current, and it is rendered highly luminous or incandescent by such current."

In the statement of claims appended to the specification Mr. Edison sets forth as follows:

"What is claimed as the invention secured by these letters patent is that a candle or light-giving body for electric lights, in which particles of metal or metallic oxides are moulded into a mass, is adapted to the passage of the electric current, and made luminous by the same, substantially as set forth."

THERMIC AND GALVANOMETRIC LAWS OF THE ELECTRIC SPARK PRODUCED IN GASES.

By E. VILLARI.

THE heat developed in gases by an electric spark is proportional to the quantity of electricity which produces it. The galvanometric deviations produced by the discharge of Leyden jars are proportional to the quantities of electricity condensed. The galvanometric deviations produced by one and the same charge of the condensers are constant and independent of the length of the spark produced at any given point of the circuit. If one and the same quantity of electricity amassed in any condenser is discharged through a metallic circuit, interrupted so as to give rise to a spark, the quantity of electricity brought into play in the discharge is constant and independent of the length of the spark. The quantity of heat developed in any gas by the electric spark increases in proportion to its length, whence the temperature of the spark at its different points is independent of its length, and the electric resistance of the gases is proportional to the thickness of the gaseous layer traversed by the discharge. When the charge producing the spark remains the same the quantity of heat developed by this spark is independent of the surface of the condenser. In fine, the thermic and galvanometric deviations produced, the former by the spark and the latter by the discharge of a condenser, are proportional to the quantity of electricity which produces them, and at the same time to the length of their active circuits (this name being applied either to the length of the spark or to the length of the galvanometric wire).

THE CURIOUS ASTRONOMICAL PHENOMENON.

To the Editor of the Scientific American:

Seeing that you have honored me with a reprint of my letter to the New York Tribune, dated April 13, in which I gave a superficial description of a comet-like object moving through space from northwest to northeast, and being daily questioned by astronomers as to correct and more details of data, I take pleasure, now that the matter seems to become one of general interest, in giving a more minute statement, and at the same time vindicate myself—being accused by some of the "learned professional astronomers" for not having published my discovery through the proper scientific channel and in the proper manner. I am as yet in the infancy of astronomical researches and totally inexperienced in the discoveries of "wonders," such as "volcanoes in action on the moon's surface," "second companion to Polaris," "satellite to Venus," or "a third satellite to Mars," and a thousand other wonderful discoveries that are yearly reported to the Smithsonian Institute or the Naval Observatory, and thrown into the waste basket. I have never made any discoveries, never intend to. I have an utter detestation for people who speculate in scientific matters, who stir up the world with dispatches of their discoveries on one day, and the next become the laughing stock of an intelligent community.

I did not think that the above phenomenon was anything but of a meteoric nature, and coupled with this belief, which I at present adhere to, it would have been ridiculous for me or any one else to have made a great outcry. The publication of my letter to the Tribune, and the message sent by me