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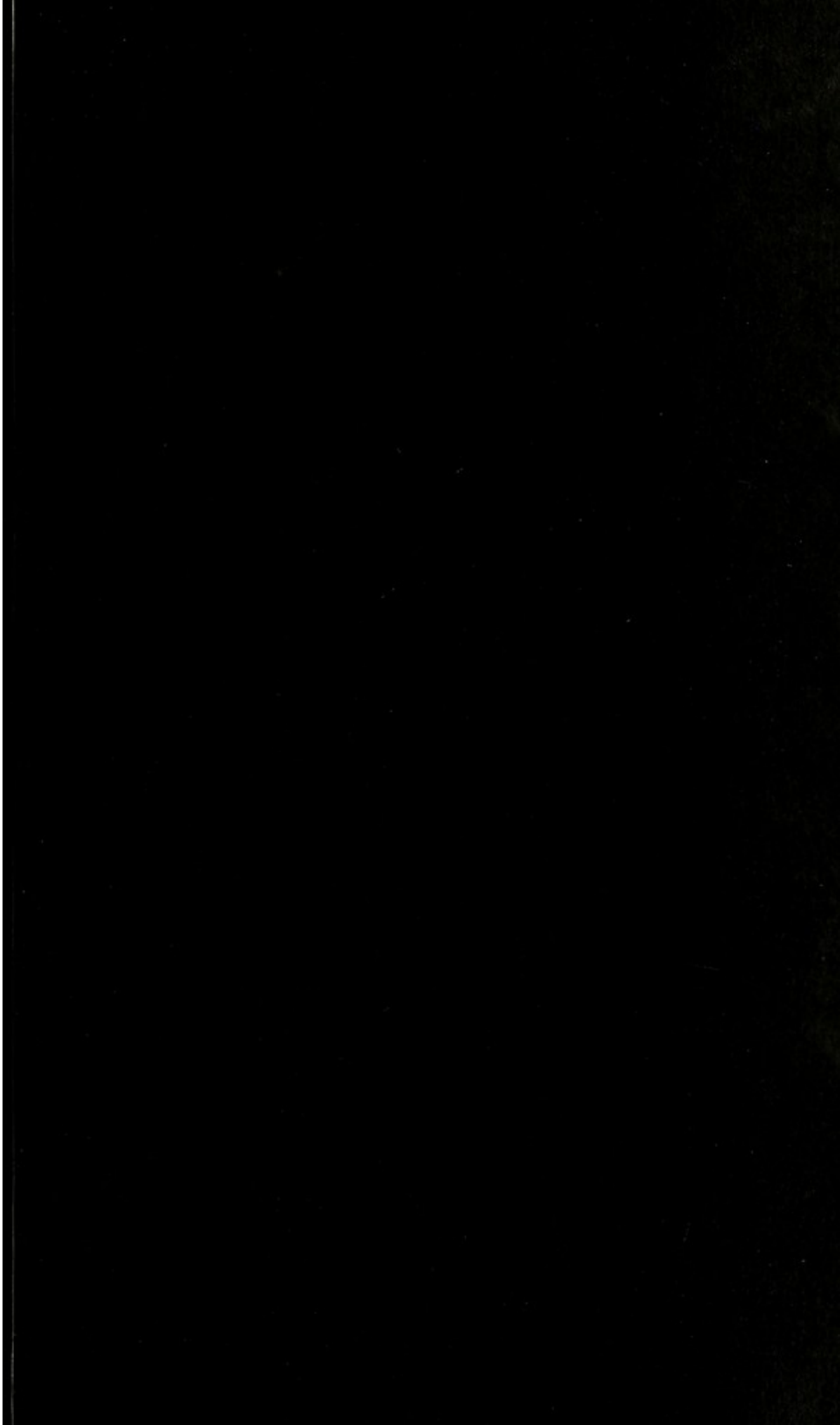
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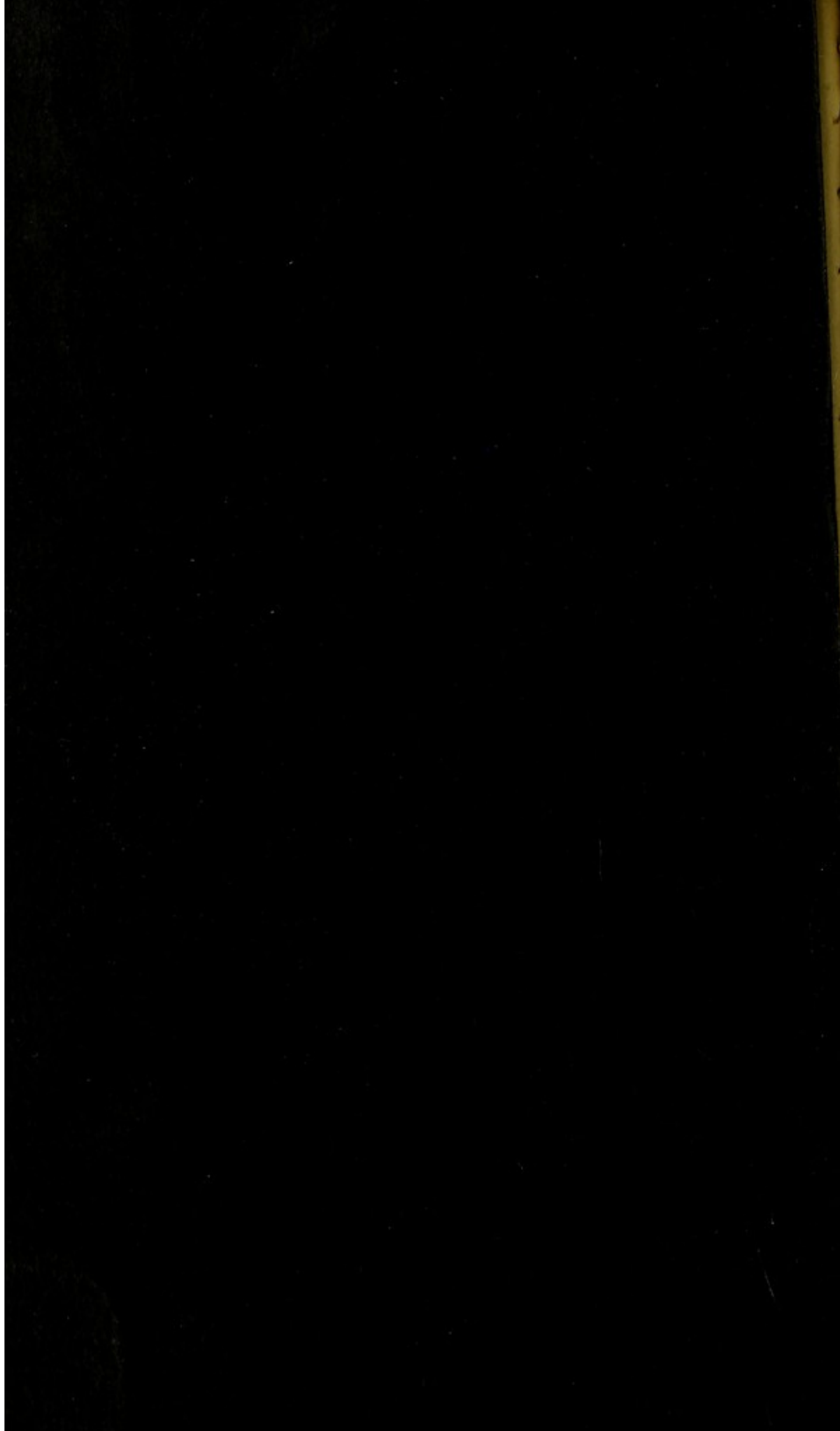
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# THOUGHTS ON URÆMIA,

BY

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NOTHING, perhaps, more clearly indicates the progress of mind in our profession than the recent valuable researches in reference to the healthy and morbid states of the urine. The deductions drawn from these researches by observers, if not always sound, afford at least a new field for inquiry, and invite the co-operation of all who are zealously engaged in the pursuit of truth. Until within comparatively a short period, authors were silent on the subject of the poisonous properties contained in the urinary secretion—or, at least, they did not attach to it that specific interest which late discussions have excited; and hence the term *Toxæmia*, or blood poisoning, was not employed, as it now is, to denote a very peculiar and important condition of the economy. Whilst *Toxæmia* is the generic term, there are various species or grades of blood poisoning; and I propose, for the present, to limit myself to the consideration of that peculiar form denominated *Uræmia*. The existence of *Uræmia* has been differently explained by authors;—for example, some contend that it is due to the presence of albumen in the urine; others that it is caused by urea in the blood; whilst again, both of these opinions have been rejected, and a new one advanced,



viz : that uræmia results from the transformation of urea into the carbonate of ammonia. It will not be denied that these are interesting questions, and involve much that is important in the treatment of disease. They, therefore, merit a careful and minute examination, which I propose to institute under the following heads : 1st, What is Uræmia ? 2d, Is albuminuria always followed by uræmia ? 3d, What is the cause of albuminuria ? 4th, Is urea a poison ? 5th, What is the true explanation of uræmia ? 6th, What is the treatment of uræmia ?

#### WHAT IS URÆMIA ?

*Uræmia* consists in disturbed action of the two nervous centres—the brain and spinal cord ; producing either coma, partial or complete convulsive paroxysms—these disturbances being directly traceable to the action of the peculiar poison constituting uræmia on these nervous centres. They may be affected separately, or together ; and hence, according to Carpenter, there may be three forms of uræmic poisoning : 1st, A state of stupor supervenes rather suddenly ; from which the patient is with difficulty aroused, soon followed by complete coma, with stertorous breathing, &c., as in ordinary narcotic poisoning ; 2d, Convulsions of an epileptic character, often affecting the entire muscular system, suddenly occur, but without loss of consciousness. 3d, Coma and convulsions may be combined. The term uræmia is employed to denote a peculiar kind of poisoning, which it is supposed results from an accumulation of urea in the blood.

It will be one of the objects of the present paper to examine the propriety of this term.

#### IS ALBUMINURIA ALWAYS FOLLOWED BY URÆMIA ?

That the presence of albumen in the urine is not necessarily followed by uræmia is abundantly proved by obser-



vation--and it appears to me important that this fact should be well understood, for the reason that much error has arisen from the opinion entertained by certain writers, that there is a direct connection between uræmia and albuminuria. This error is not so much owing to any inherent difficulty of the subject, as it is to that loose appreciation of facts, or, more properly speaking, to that want of healthy digestion of well settled principles, which, unfortunately, too often characterises the writings of professional authors. I might cite a long list of observers, to show that albumen very frequently exists in the urine without any development of uræmic intoxication; but I apprehend this would be unnecessary. I shall, therefore, limit myself to two or three undoubted references. Franz Simon, for example, says he has frequently detected albuminuria in persons apparently in the enjoyment of good health; also, others have observed it in articular rheumatism, in inflammation of the thoracic organs, intermittent and typhus fevers, in measles, cholera, chronic affections of the liver, &c. In transitory renal catarrh, such, for instance, as occurs in erysipelas, nearly as often as in scarlatina, albumen, together with the well known epithelial cylinders of Bellini's ducts is found as constantly in the urine as in inflammatory affections of the kidneys, where it exists in connection with the fibrinous plugs from the same ducts, as in true Brights' disease. (1)

Edouard Robin says, the urine becomes albuminous in croup, in ascites, and in cases of capillary bronchitis, with emphysema accompanied by dyspnœa; in pulmonary pthisis, in gestation when sufficiently advanced to occasion an habitual congestion of the kidneys; in cyanosis, diabetes, &c., &c. (2.)

In order to prove that albumen may exist in the urine

(1.) Physiological chemistry by Lehmann, T. 1, p. 345.

(2.) Ed. Robin, London Lancet, Jan. 24th, 1852, p. 96.



independently of any disease of the kidney, and without any of those nervous disturbances characteristic of uræmic intoxication, Dr. M. F. Tegart mentions the following interesting and conclusive experiment upon himself, and also confirmed in the person of one of his friends: He made for some time a portion of his ordinary nourishment to consist of half-a-dozen eggs, and albumen as a consequence was soon detected in his urine. (1.)

There are but few practitioners of careful observation, who will not endorse these statements. Indeed, I conceive the principle to be so well established that the existence of albuminuria is not necessarily connected with the presence of urea in the blood, that further citations can scarcely be necessary to demonstrate the fact. (2)

We now proceed to the examination of the third question, which is one not only of great interest to the medical man, but one which has called forth numerous and conflicting opinions, viz.:

#### WHAT IS THE CAUSE OF ALBUMINURIA?

According to Ed. Robin, the passage of albumen into the urine is the result of imperfect combustion. He maintains that urea is produced by the oxygenation of the albumen in the blood, and that if this oxygenation do not take place, the result will be albuminuria. This hypothe-

(1.) Thèse sur la maladie de Bright, Paris 1845, Gazette Medicale, Paris 1846, p. 39.

(2) Recently much has been written, and questions proposed by learned academies, respecting the connection between albuminuria and puerperal convulsions; and the writers are almost unanimous in the opinion that albuminuria is the cause of these convulsions. Now, we contend that puerperal convulsions are nothing more than uræmic phenomena, as is proved by their causes, symptoms, diagnosis, pathology, &c., &c.

If, then, puerperal convulsions be the result of uræmic intoxication, they are not necessarily produced by albuminuria. There is often a co-existence of puerperal convulsions, albuminuria and oedema, general or local; but each one of these conditions may, and has existed irrespectively of the other.



sis has one attribute—that of ingenuity—but can its truth be demonstrated? We think not, and for this obvious reason, that when albumen passes into the urine, the quantity of urea as a necessary consequence should not be increased in the blood. It is, I believe, conceded that although albumen does occasionally exist in the urine without a diminution in that fluid of urea, yet the converse of this is very often observed, viz., an increase of urea in the blood co-existing with albumen in the urine. This is in direct conflict with the explanation as given by Robin.

Dr. Williams says that "*per se*, albuminuria indicates nothing more than congested kidney." Now, we shall attempt to prove that other causes than simple congestion of the kidney will produce albuminuria; and, in doing this, it will follow that Dr. William's opinion is far too sweeping. It is quite certain that the presence of albuminuria is not traceable to any one cause—for we find it under a great variety of circumstances; and I shall endeavor to show that the passage of albumen into the urine is due to one of the following conditions: 1st, a change in the composition of the blood; 2d, a change in the kidney, either structural or dynamic; 3d, pressure on the renal veins.

1st. *Change in the composition of the blood.*—It was a favorite doctrine of the old-schoolmen, that the blood contained certain deleterious elements, which could not continue in the system without generating disease. This, too, was the opinion of Sydenham, Pitcairn, Cullen, &c.; and the master-minds of the present day, with all their supposed progress, are compelled to admit that there is something more than mere conjecture in what was formerly termed the "peccant humours." The particular organs through which these humours or poisons pass from the economy are called glands; and each gland has its specific office assigned to it—that is, one of these glands furnishes an outlet for one character of material in the blood,



and another gland for a different substance. Thus, whilst the Liver is engaged in the secretion of bile, &c., and the Kidney water, urea, &c., we find the intestines the media through which certain effete materials are thrown off.—These different offices are performed through what is called secretion, the true nature of which is still involved in mystery. We understand certain general principles respecting the secreting processes, but we must acknowledge that we are unable to explain many of the phenomena connected with this fundamental part of the physical mechanism. Although, therefore, we are ignorant, if I may so speak, of many of the processes connected with glandular elaboration in a state of health, yet it does not follow that we cannot explain some of the causes which, interfering with healthy secretion, result in morbid action. Now, then, in order to apply this reasoning to the question before us, we will suppose—what will not be controverted—that in most of the diseases which we have enumerated as being occasionally accompanied with albuminuria, such, for example, as cholera, scarlatina, diabetes, &c., the constituents of the blood become changed by the introduction either of a poison, or some other unusual substance. If this occur, it is quite manifest that the blood is no longer normal—and because of its altered condition, its elaboration in the kidney will also be modified. In other words, in lieu of the ordinary elements contained in the urine, we shall sometimes find albumen, an absence of urea, &c.—May this not be satisfactorily explained on the principle that the product of endosmosis will be modified in proportion to the changes in the Fluid on which it acts? Again, the blood is changed in pregnancy, various circumstances tending to this modification, viz: the formation of Kiesteine, the secretion of Milk, the quantity of blood-materials passing through the circulation of the Foetus, and the diseases of the embryo itself, not to speak of its excretions, some of which we know enter the blood of the mother.—



These, then, being so many influences capable of altering the constituents of the blood, will they not account, in some instances, for the occasional presence of albuminuria in the pregnant female? And when albumen is discovered in the urine after the completion of gestation, may it not be owing to the changes in the blood produced by the milk fever, lochial discharge, &c?

But, in my judgment, there is another circumstance, which is calculated to modify in a very special manner the blood of the pregnant woman. The general rule is that, during gestation, the menstrual fluid is suppressed. Now, I am not so confident that Pliny and many of the writers among the Arabians did not at least approach the truth when they advanced the opinion that the catamenial discharge contained certain noxious elements.—Many of their comparisons, it must be admitted, were fanciful, and some of their illustrations supremely ridiculous; but laying these exuberances aside, I believe there is much truth in the aggregate of opinion they entertained on this subject. Most modern writers, however, are disposed to smile with something less than contempt at what they are pleased to term “the crude notions” of the early Fathers respecting the properties of the menstrual blood. The smile might be pardoned if those who indulge in it, had given us something positive and well-defined touching this question, so interesting both in its physiological and pathological relations. It will be found that there exists much discrepancy of opinion on this subject by the writers of the present day. One, for example, Donné says “the menstrual blood, when examined by the microscope, contains like ordinary blood both fibrin and red corpuscles.” Carpenter tells us that “the catamenial discharge appears usually to consist of blood *deprived* of its fibrin, &c.” Simon says “there can be little doubt that there is fibrin in the menstrual secretion, &c.” Vogel failed to detect fibrin in his analysis of the menstrual fluid. Dr. Letheby \* in his

\* See London Lancet, Aug. 2d, 1845.



examination of this fluid, which had been retained in the uterus because of an imperforate hymen, found it to be entirely free from fibrin.

In this way, I might proceed to show the numerous conflicting statements made by recent authors, as to the real nature or properties of this secretion. I have no experiments to offer with the view of demonstrating that the menstrual blood positively contains noxious materials—but I argue the affirmative of this question from certain pathological states, which we observe to follow an abnormal condition of the catamenial function. For instance, in one hundred unmarried women, who may labor under suppression of the menses from the operation of any of the influences known to produce this result, such as cold, mental emotion, &c., we will discover that in at least ninety-five the suppression will be followed by more or less disturbance of the nervous system. In some, it is true, the symptoms will be light and evanescent, but in others, they will assume a more marked character, sometimes even producing mania—and at others, coma, epilepsy, catalepsy, chorea, &c. May not these phenomena be due to a species of toxæmia traceable to the poison of the menstrual blood upon the nervous centres? This opinion seems to be confirmed by the important fact that all the nervous disturbances cease with the return of the function. I have enjoyed full opportunities for observing the effects on the system of the various forms of menstrual aberration; and I have also noticed an extremely interesting and significant circumstance—a circumstance, which certainly tends to corroborate the hypothesis that the derangements of the nervous system under unnatural suppression, &c., of the menstrual function are owing to a species of blood-poisoning; the circumstance to which I allude is this—when the catamenial discharge becomes suddenly, or otherwise abnormally arrested, the urinary secretion is usually diminished in proportion to the intensity of the nervous symp-



toms—and what is still more significant is, that the nervous disturbance will yield in proportion to the effects of diuretic and sudorific remedies. There is no error as to the fact—its truth is readily susceptible of demonstration.

2d. *A change in the Kidney, either structural or dynamic.* Every structural change in the kidney may result in albuminuria, but we do not yet comprehend in what essentially these various changes consist. For example, though it may be true that the presence of albumen in Bright's disease, in scarlatina, &c., may be due to a desquamation of Bellini's tubes, yet this cannot be said of many other diseases of the kidney, in which albuminuria exists, but in which no desquamation takes place. Several interesting experiments have been made to prove that the urinary secretion is not absolutely dependent upon the nervous system by Segalas, (1) and some of a more decisive character by Dr. Brown—Sequard; (2) whilst, on the other hand, it has been satisfactorily shown that the nervous system may, under certain circumstances, exercise a marked influence over this secretion, as is demonstrated by the researches of Brachet, J. Muller, (3) and Marchand. The latter has pointed out a very important fact connected with this subject: he produced in a dog not only all the symptoms of uræmia, after placing a ligature on the renal nerves, but also discovered urea in the blood, and in the matter vomited by the dog. Kramer states that he has detected albumen in the urine of animals, after dividing the sympathetic nerve in the neck. This, however, seems to need confirmation, as the same result has not followed the experiments of others. Dr. Sequard, after repeated trials, has failed in establishing the fact mentioned by

(1.) Bulletin des Séances de l'Acad. de Med. de Paris. (Séances des 27 Avut et 23 Septembre, 1844.

(2.) Experimental researches applied to Physiol. and Pathol., Philad., 1852, '53, p. 13.

(3) Manuel de Physiol, trad. française de Jourdan, Edité par E. Littré, Paris, 1851; t. i., p. 391.



Kramer. Budge has found albuminuria after a puncture of the cerebellum; and Cl. Bernard (1) has occasionally obtained the same result after puncturing the medulla oblongata. In addition, however, to these demonstrations, we have numerous instances occurring constantly in practice, illustrating the influence of the nervous centres, when laboring under disease or traumatic injury, over the urinary secretion; and it is quite possible that the irritation of the uterine nerves, during pregnancy, and in many of the diseases both organic and functional of the uterus may, by reflex action on the medulla spinalis, constituting the eccentric nervous disturbance of Marshall Hall, produce various morbid changes in the urine. Again, it does appear to me that if it can be proved that sudden emotions, shocks, &c., have an influence on the peculiar processes by which the blood is continually ridding itself of its deleterious materials, we shall, in this way, have opened to us a new field in our investigation of disease, we shall be enabled to elucidate many morbid phenomena which have heretofore been obscure, and, as a necessary consequence, deduce rational therapeutic principles. It is only a few days since I was called to Brooklyn to see a lady under the following circumstances: she was twenty-eight years of age, had been married two years, no children, nor had she ever been pregnant. Her health had always been good from early childhood; no menstrual irregularities, &c. Five weeks before I visited this patient, whilst giving some directions to her servant, who was arranging her library, a large book fell on her head; this was soon followed by vomiting, but in two or three days the effects of the concussion on the stomach passed over, and the lady thought nothing more of the matter. Within the last two weeks, however, she has noticed a gradual swelling of her lower limbs, with an increased tumefaction of her abdomen. It

(1) *Comptes Rendus de l'Acad. des Sciences de Paris*; t. xxviii., p. 393.



was on this latter account that I was requested to see her. There was evident œdema of her limbs, with peritoneal dropsy. Here, then, was not only an interesting, but a remarkable state of things. Without any manifest cause, a patient previously in the enjoyment of good health is attacked with dropsical effusion. The only circumstance which had occurred was the blow on the head by the fall of the book, followed by vomiting. But what connection is there, it may be asked, between this transitory concussion and the dropsy? None, certainly of a direct nature. I was, at first, utterly at a loss to account for the disease with which my patient was affected; and I requested that some of the urine should be put aside, in order that it might be examined. Accordingly, on the following day the urine was tested, and albumen detected with no diminution of urea. It was now not very difficult to account for the dropsy—certainly not as difficult as it was satisfactorily to explain the existence of albuminuria. The serous effusion was undoubtedly the result of the albuminuria; but what caused the latter? In my judgment, the starting point of the dropsy was the concussion of the brain—the blow upon this nervous centre modified the urinary secretion, which previously had been perfectly normal, and the result of this change was an abstraction of albumen from the blood, and its presence in the urine. This, at least, is the view I have taken of the case, and my treatment has been based upon this hypothesis. It has been essentially restorative, with the object of supplying to the blood its lost albumen. If I can succeed in accomplishing this purpose, the presumption is, if my opinion of its pathology be correct, that the case will terminate favorably. Every physician much engaged in midwifery practice has occasionally observed cases in which the foetus has died *in utero* from the effects of sudden mental emotion experienced by the mother. I think this result is susceptible of explanation as follows: the shock upon the nervous system



of the parent may so modify the character of her blood as to render it destructive to the child—producing, in a word, a state of toxæmia. Now, if it be asked, why this poisoned state of the blood does not also affect the mother, my answer is that there are not only different grades of toxæmia, but there are also differences in the susceptibility to its influence. Do we not see, for instance, one woman thrown into violent convulsions from uræmia, and another, with the same amount of poison in her blood, exhibit no disturbed action of her nervous system? Surely, then, if there exist this difference in the constitutions of the adult, there can be no difficulty in appreciating far greater differences in the mother, and the foetus she carries in her womb.

3d. *Pressure on the renal veins.* Whatever may be the other causes which operate in the production of albuminuria, there is a mass of irresistible testimony to demonstrate the positive influence of an obstructed renal circulation. G. Robinson, (1) Meyer, (2) and Frerichs, have abundantly proved that a ligature tied more or less completely around the renal veins, will cause albumen to pass from the blood into the urinary secretion; and again, when the renal veins have become obliterated, in every instance in which the urine was examined, albuminuria was detected. Cases of this nature have been observed by Dance, Rayer, Duges, Velpeau, R. Lee, Cruveilhier, Stokes, Blot, Leudet, &c. In gestation, and especially in primiparæ, albuminuria is often caused by the pressure of the impregnated uterus on the renal vessels. Dr. Rose Cormack, I think, was the first to call attention to this subject. In 106 multiparæ, Blot has found 11 women whose urine contained albumen, whilst in 99 primiparæ 30 exhibited albuminuria. The proportions, therefore, for the former are as 1 to 10—the latter as 1 to 3. This is a remark-

(1) Medico. Chirurg. Transact. of the Royal Med. Chirurg Soc., of Lond., 1843, V. VIII, p. 51.

(2) Gaz. Med., de Paris, 1844, p. 419.



able difference, and must be due to some special cause, which I hold to be explained as follows: Women in their first pregnancy have a very different condition of abdominal walls from those who have already borne children. In the former, these walls are firm and resisting; in the latter, they are relaxed, and have lost much of their original tension. For this reason, in primiparæ, the impregnated uterus is more perfectly in the line of the axis of the superior strait of the pelvis. In multiparæ, on the contrary, this organ is disposed to fall forward constituting ante-version of the fundus. Precisely in proportion, therefore, to the departure of the uterus forward from the direct line of ascent will be the probability of diminished pressure on the renal circulation. I believe, also, there is another reason why albuminuria is observed less frequently in multiparæ than in primiparæ. It is a well known fact, that women are much more disposed to miscarry in a first than they are in subsequent pregnancies; and, *cæteris paribus*, this is no doubt owing in a measure to the greater irritation of the uterine nerves consequent upon a first gestation, together with the increased difficulty encountered by nature to distend the uterus for the accommodation of the developing germ. May not, therefore, this excess of irritation, by modifying the urinary secretion, be occasionally a cause of the more frequent presence of albuminuria? I think so; and again, when, under these circumstances, the passage of albumen into the urine is followed by the presence of urea in the blood—as is often the case—even admitting that full uræmia does not take place, may not the nervous system become so much excited by the existence of urea as to induce premature action of the uterus, and consequently miscarriage? If there be any force in this reasoning, the preventive treatment of miscarriage in this condition of system may prove far more successful than it has heretofore been.

It is quite evident from the facts I have mentioned, that



albuminuria is of frequent occurrence in pregnancy; and Cahen (1) has endeavored to show that it is caused by disease of the kidney. It cannot be denied that disease of the kidney may co-exist with gestation, and in such case the albuminuria may be traced to the disease of this gland; but to say that albuminuria cannot exist in pregnancy other than as a result of disease of the kidney, is in direct opposition to well established observation. Blot (2) demonstrates the fact as follows: 1st. The rapidity with which albuminuria disappears after delivery in almost every case, very often in two or three hours, sometimes in one, after the expulsion of the child; 2d. absence of the symptoms of diseased kidney; 3d, certain characters of the urine entirely different from those of Bright's disease, as, for instance, increase in the density of the urine, and the presence of more salts, and particularly urates; 4th. In seven women, who died, and in whom albuminuria had been detected, only three had slight pathological alterations in the kidney.

The opinion is now well settled, and concurred in by a great majority of writers that albuminuria is, in many cases, simply the result of an active or passive congestion of the kidney. Any thing, therefore, capable of obstructing the renal circulation, whether it be an enlarged uterus from pregnancy or disease, an ovarian tumor, or enlargement of the abdomen of any kind, may be enumerated among the causes of albuminuria.

Christison, Rayer, and others maintain that the diminution of urea in the urine, and consequently its accumulation in the blood, is in proportion to the quantity of albumen—but this does not appear to be invariably the case; for Bence Jones has recorded an instance of *mollities ossium*, in which he presents an analysis of the urine,

(1) *De la nephrite albumineuse chez les femmes enceintes.* These, Paris 1847.

(2.) *De l' albuminurie chez les femmes enceintes.* These, Paris, 1849.



showing that albuminous matter may exist in great quantity, whilst the amount of urea remains perfectly natural.

#### IS UREA A POISON ?

Urea was, I believe, first discovered in 1771 by Rouelle, who detected it in the urine. It owes its present name, however, to Fourcroy and Vauquelin; it was obtained pure for the first time by Dr. Prout in 1817. There is one interesting circumstance connected with this production—it is the first instance known of an organic compound being artificially produced, which was effected by Wohler from cyanic acid and ammonia. The true nature of urea is variously described by authors, the general opinion being that it is a poison. Todd, (1) Williams, (2) Cormack (3) Simon, (4) and others regard it in this light, and contend that its presence in the blood will occasion coma, convulsions, and other nervous phenomena. Indeed, it may be said that this has been the prevailing opinion; it is proper, therefore, that its merits should be examined.—Opinions in our Science, unless fully confirmed by well-directed observation, are not abiding—they do not present that fixedness, if I may so speak, which will alone entitle them to be classed among accepted truths. If urea be a poison, capable of producing convulsions, &c., the numerous experiments made on living animals in no way establish the fact. Among others, Prevost and Dumas, (5) Segalas, Tiedemann and Gmelin, Mitscherlich, Cl. Bernard and Barreswill, Stannius, (6) and Frerichs, have extirpated the kidneys, and have never known convulsions to en-

(1) Lumlian lectures in London Med. Gaz., 1849 and 1850.

(2) Principles of General Pathology.

(3) London Journal of Medicine, 1849, pp. 690 and 699.

(4) Lectures on General Pathology, Amer. edit. p. 151.

(5) Annales de Chimie et de Physique, V. YXIII, p. 90.

(6) Gaz. Med. de Paris, 1841, p. 168.



sue. This, it may be argued, is only negative proof. Negative, however, as it is, it must be admitted that it is testimony not without value; and to it may be added the interesting experiments of Bichat, Courten, Gaspard, Vauquelin, Segalas, Stannius, Frerichs (1) and others who, after injecting into the veins urea and urine, never in a single instance observed a case of convulsions. Again, Bright, Christison, Rees, and Frerichs have cited cases in which a large quantity of urea existed in the blood of man unaccompanied by any of the symptoms of uræmia; and Frerichs says, in one instance in which he detected the greatest amount he had ever observed, there was no approach to uræmic disturbance. Vauquelin and Segalas, so far from regarding urea as a poison, have proposed to administer it as a diuretic. The conclusions, therefore, from these facts appear irresistible that urea, to say the least, is not a virulent poison, its excess in the blood will not *per se* produce uræmic intoxication, nor will it explain the numerous phenomena which are so frequently found to accompany its presence in the circulation. It was in view of all these circumstances, that Frerichs attempted to demonstrate that uræmia depended neither upon a diminished quantity of urea in the urine, nor upon an excess of this substance in the blood, nor upon albuminuria—but was traceable solely to the existence of carbonate of ammonia in the system which, he says, is formed through the agency of a ferment, from the urea itself. In other words, Frerichs' doctrine is, that uræmia is exclusively due to the transformation of urea into the carbonate of ammonia.—The *modus in quo* as to this transformation is not clear; there is no proof as to the manner in which it is accomplished; but the major point, viz: the dependence of uræmia on the presence of the carbonate of ammonia seems to rest on strong and cumulative testimony.

Many years ago, Orfila produced convulsions in an animal

(1) Die Bright'sche Nierenkrankheit, 1851. Analyzed in Braithwaite's Retrospect, 1852, part, xxv. p. 135.



by giving it internally the carbonate of ammonia; the animal, after becoming convulsed, died. This fact seems to have escaped the observation of G. Zimmerman, who, in objecting to the theory of Frerichs, says "carbonate of ammonia given internally in large doses produces neither convulsions nor coma." Cl. Bernard and Barreswill have detected carbonate of ammonia in the stomach and intestines of animals after the removal of the kidneys; and Lehmann has also observed it in the matter vomited by patients affected with cholera. Christison, Jakchs, &c. have recognised, under certain circumstances, an ammoniacal odor in the blood. Until, however, the exposition of the peculiar views entertained by Frerichs as to the true cause of uræmic intoxication, no significant value was attached by authors to the presence of the carbonate of ammonia in the exhalations. Frerichs states that he has ascertained, by chemical analysis, the existence of carbonate of ammonia in the blood in all cases in which the symptoms of uræmia are developed; but the quantity of the salt varies considerably. He further remarks that the two following propositions he has demonstrated beyond a doubt: 1st, That in every case of uræmic intoxication, a change of urea into the carbonate of ammonia takes place. 2d, That the symptoms, which characterise uræmia can all be produced by the injection of carbonate of ammonia into the blood. After citing many experiments to fortify his opinion, he says he has frequently detected the alkaline salt in the expired air of animals deprived of their kidneys, and into the veins of which he had injected urea; these animals remained quiet and awake as long as the expired air was not impregnated with the ammonia; but the moment the ammonia was observed, the various disorders of the nervous system characteristic of uræmic poisoning developed themselves. If future observation should confirm these views of Frerichs, it will undoubtedly tend, not entirely but in part, to settle a vexed question, which has called forth



the ingenuity of both the physiologist and chemist. It may, however, be that the future will reveal the existence of other poisonous materials in the blood, which to the present time has eluded observation; and, in their recognition, we may find additional causes for the production of toxæmia. It has, indeed, been suggested that in Bright's disease, the accumulation of oxalic acid in the blood will develop the symptoms of uræmic intoxication.

It is a well-known fact that profuse losses of blood, and starvation, are frequently followed by intense headache, and sometimes even by mania. Whether the blood be deprived of its albumen by hemorrhage, or artificial depletion or by the passage of this substance into the urine, the general constitutional effects of this anæmic condition of the system are more or less the same, and will develop more or less nervous disturbance; so that, even without uræmia, the mere deprivation of the blood of its albumen, may satisfactorily account for a variety of phenomena, resulting directly or indirectly from the exercise of a morbid influence on the brain. Coma does not, I imagine, exclusively depend upon an undue afflux of blood to the brain; it sometimes ensues from a deficiency of nutritious blood, and hence its occasional development in prostration of the vital energies from disease, or excessive depletion. Nothing is more marked than the effects of exhaustion in early infants. Thousands of children die from coma, the immediate result of excessive depletion—and when the symptoms of stupor supervene, they are too often regarded as an additional motive for the continuance of the anti-phlogistic treatment. Marshall Hall, Abercrombie, and Gooch, have rendered a solid service to science by directing attention to this practical fact; but humanity has not received a tithe of the benefit, simply because the fact has not been sufficiently heeded in the sick-room.

Physiology and chemistry are fast revealing a new basis for the treatment of disease—thought is now in the right



direction, and a bright future is at hand. In less than ten years, therapeutics will have assumed a new character—the practice of medicine will be more certain, because its principles, through the investigations of the chemist and physiologist, will have become consecrated as so many unerring developments of truth !

#### TREATMENT OF URÆMIA.

If there be any force in the above observations, it follows as a necessary consequence, I think, that the treatment of uræmia involves two objects : 1st, The immediate restoration of the principal eliminators of the system, such as those of the kidney, skin, and bowels, with a view of diminishing, through these outlets, the quantity of urea and noxious substances which may exist in the blood. 2d, The protection of the nervous centres, as far as may be, against the injurious effects of the carbonate of ammonia.

Dr. Maclagan, of Edinburgh, has recently drawn attention to the value of the *Colchicum Autumnale*, in uræmic poisoning. The excellence of this remedy consists in its power of increasing the amount of urea in the urine. This fact, I believe, was first discovered by Chelius of Heidelberg. Prof. Krahmer, (1) of Halle, has made some very interesting experiments on the subject of diuretic medicines. According to him, the average of urea secreted during the day in healthy urine is 19-64 grammes, while the tables of Becquerel give 16 grammes. Krahmer has shown that under the influence of *Colchicum* the urea is increased to 22-34 grammes ; and under the administration of *guaiacum* to 22-74 grammes. From the experiments of Krahmer, therefore, it appears that *Colchicum* and *guaiacum* produce a greater secretion of urea than any other known remedies.

(1) Brit. and For. Med. Chir. Review, July, 1848, p. 250.



## CONCLUSIONS.

1st. Uræmia is a nervous disturbance arising from a peculiar blood-poisoning.

2d. Albuminuria is often connected with uræmia, but is not the cause of it.

3d. Diseases of the kidney will often produce albuminuria; but in a great many cases albuminuria exists without true disease of the gland, and as a consequence of an active, or a merely passive congestion; and it will also result from a variety of nervous disturbances.

4th. If urea be a poison, the quantity of it which accumulates in the blood in cases of extirpation of the kidneys in animals, or in suppression of urine in man, is not sufficient to produce any manifest deleterious effect.

5th. According to Frerichs, uræmia is merely a poisoning by the carbonate of ammonia, which is a product from the decomposition of urea.

6th. The treatment of uræmia must consist in the free use of diuretics, sudorifics, and purgatives—the most suitable diuretics for this purpose being colchicum and giuacum.

New York, July 1st, 1853.



