

Observations on the Pathology of the Human System, when laboring under the disease denominated DIABETES MELLITUS—By
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DOCTOR CULLEN, in his system of Nosology, has placed diabetes as a genus of diseases under his order spasmi. Under the genus diabetes he has designated two species—diabetes mellitus with “urine of the odor, color and taste of honey”—and 2d, diabetes “with limpid urine, not sweet.” The following observations embrace the first species, or diabetes mellitus.

Dr. Cullen devotes but three pages in his “first lines” to the consideration of this disease. He says that, though he believes the disease has been cured, yet he observes—“In all the instances of this disease which I myself have seen, and in several others of which I have been informed, no cure of it has been made in Scotland, though many instances of it have occurred, and in most of them the remedies recommended by authors have been diligently employed.” Such is the statement which the justly celebrated Dr. Cullen has given, with respect to diabetes mellitus, as that disease appeared in Scotland in his day. Dr. Cullen therefore concludes, that as the proximate cause of diabetes was so little known, he could not propose any rational mode of cure in the disease. Many efforts have been made by physicians, since Doctor Cullen advanced the foregoing sentiment, to develop the pathology of the human system when laboring under this intricate and formidable disease; and we may yet, perhaps, with great propriety, in the language of Dr. Cullen, say that the proximate cause of diabetes mellitus “is so little known,” and is, therefore, wrapt in considerable obscurity. Our design is to examine a few of these pathological theories which have been given to the medical world. A correct pathological knowledge of a disease must tend to the adoption of a more successful plan of treatment for its removal. Dr. Rollo considered the stomach to be the original and primary seat of the morbid action of the disease denominated diabetes mellitus. “On the whole, therefore,” Dr. Rollo observes, “we repeat, that the proximate cause of diabetes mellitus, seems to us to consist of a morbidly increased action of the stomach, with consequent secre-

tion and vitiation of the gastric fluid, marked by an eagerness of appetite and acidity. The direct effects of which are, the formation or evolution of saccharine matter, with a certain defect of assimilation, preventing the healthy combinations, and exciting the immediate separation of the imperfectly formed chyle, by the kidneys. The more remote consequences will be, such changes in the natural structure of the parts, as may prevent the entire restoration of health. That the altered appearances which have been found in the dissections of those who have died of the diabetes mellitus, are merely the effects of the disease, and probably only in certain constitutions, is evident from the cases of Dr. Cawley and Dr. Pearson, but more remarkably from that of Mr. Thomas." Such is the pathological foundation which Dr. Rollo has laid to support the plan of treatment which he adopted for the removal of the disease. The doctor confined his patients to a rigid use of animal food, for the purpose of preventing the formation of saccharine matter in the stomach. Hepatized ammonia was the medicine that the doctor principally relied on, to effect the removal of the "morbidly increased action of the stomach," which he considered a characteristic symptom of diabetes mellitus. It appears that the disease was removed in the case of Capt. Meredith. Dr. Rollo's plan of treating the disease, however, has been strictly enforced in many cases, under the direction of different physicians without such success as inspired a confidence in the remedy. Even if we admit that Dr. Rollo is correct in his pathological doctrine, that the "immediate cause of diabetes mellitus is a morbid condition of the stomach, forming or evolving from vegetable substances saccharine matter, which is quickly separated as a foreign body, by the kidneys," we cannot perceive how the pathology of the disease is to be explained from this source. The foreign body which Dr. Rollo says is produced by a "*morbid*" action of the stomach, in this disease, must enter and become intimately mixed with the circulating mass of blood. It is evident, therefore, that a comparatively small portion of this foreign body will be conveyed to the kidneys, through the emulgent arteries. But in a minute and extensive post mortem examination of the body of a patient, who had died of this disease, Dr. Baillie ascertained that the stomach exhibited a perfectly healthful condition of organic structure. This examination of Dr. Baillie, in my opinion, cuts

up the pathological theory of Dr. Rollo by the roots. Dr. Lubbock, admitting that the saccharine quality of the urine is characteristic of the disease under consideration, undertakes to account for it by the retention, in the patient's system, of carbonic acid, in consequence of a suppressed perspiration. Now, we cannot admit the doctor's theory as a satisfactory explanation of the saccharine quality of the fluid discharged, *per urethram*, by a diabetic patient. For, in the first place, we alledge that carbonic acid is not retained in the system of a diabetic patient in consequence of a suppressed perspiration; inasmuch as the carbonic acid which the experiments of Cruikshank and Abernethy ascertained to be discharged from the system daily, with the perspirable matter, is formed upon the *surface of the body*, by the action of atmospheric oxygen upon the carbon of the blood, contained in the cutaneous capillary blood-vessels. If the suppressed perspiration, in a diabetic patient, *could* prevent the discharge of carbonic acid, the consequence would be a retention of *carbon* in the blood of the patient. But *carbon alone* cannot produce *saccharine* matter. But, in the second place, admitting that carbonic acid is retained in the system of a diabetic patient, in consequence of a suppressed perspiration, according to the theory of Dr. Lubbock, it would not account for the saccharine quality of the urinary discharge; because a very diminutive portion of this supposed acid would be conveyed to the kidneys, where the saccharine matter is formed in the opinion of some pathologists. We are, therefore, induced to conclude that the theory of Dr. Lubbock will not account for the saccharine quality of the fluid which is discharged by a diabetic patient, *per urethram*. Dr. Lubbock considers the emaciation of the patient as a leading feature of diabetes; but, in our opinion, the doctor is not more successful in explaining the cause of the emaciation, than he was in accounting for the saccharine quality of the liquid discharged by the patient. We consider the emaciation of a diabetic patient as the prominent *effect* of the saccharine quality of the fluid which he discharges *per urethram*.

On the 19th of January, 1804, we were requested to visit a patient, seventeen years of age, about seventeen miles from York, in Pennsylvania, the town in which we then resided. Upon examining our patient we ascertained that he was laboring under a well marked case of diabetes mellitus. His emaciation was great,

his appetite excessive, and he lived almost entirely upon *animal food*, as that kind of aliment was most agreeable to his palate. Thus we ascertained that our patient, *of choice*, lived upon *animal food*, an article of diet that Dr. Rollo considered *indispensable* in the therapeutic treatment of the disease. We had an opportunity of witnessing this fact, as in several of our visits we dined with the family and had ocular demonstration of the quantity and quality of the animal food which he eat to satisfy his *craving* appetite. We also often witnessed the exercise of parental, persuasive, and authoritative efforts to induce their son to take some vegetable with his animal food, He could not, however, be prevailed upon to comply with the request of his parents; for, although we have seen him put a *small* portion of bread in his mouth when desired to do so, yet when their attention was withdrawn from him, his bread was permitted to lie *untouched* beside his plate. The reader will bear in mind that the pathological description of the disease under consideration, which we give, has exclusive reference to our patient. We do not, for a moment, suppose that the same train of symptoms will appear in every case of diabetes mellitus. The skin of our patient was dry, the cuticle being scurfy. His thirst was great. The mind of our patient labored under the morbid irritability which is characteristic of his disease. From the history which he was able to give of his disease, it appeared that the emaciation was gradual in its commencement and that he dated it from a severe wetting which he received by a chilling rain, the preceding autumn. We decided upon a mercurial salivation as the only remedy that, according to the view which we had then taken of the disease, promised any relief to our patient. Accordingly we put our patient upon the use of small portions of calomel with a few drops of laudanum at bedtime. This plan was continued for a considerable time, without producing any mercurial action in the glands of the mouth, although the calomel had no evacuating operation upon the alimentary canal. We laid aside the calomel and prescribed the tincture of cantharides, with a few drops of laudanum at bedtime. We directed the tinct. of cantharides to be increased in the dose daily, a few drops, till a pain in the bowels, or a strangury, evinced that the tincture was given to a sufficient extent to test its curative powers upon our patient's disease. The tincture of cantharides was persisted in for at least

three weeks, without causing the system of our patient to acknowledge its action in any perceivable manner, notwithstanding the dose had been increased a few drops daily. We had full confidence in the tincture that we used, as we prepared it carefully from cantharides of the first quality.

Again we were requested to visit our patient, and upon examination could not perceive any favorable change in the action of his formidable disease. Foiled in every effort which had been made to arrest the progress of our patient's disease, we determined to try the effects of blood-letting, and immediately drew eight ounces of blood from his arm, in order to discover how his system would bear its loss. Encouraged by the experiment, we at once decided upon the plan of persevering in the use of the lancet, as long as the safety of our patient's life would warrant. For this purpose we prevailed upon his parents to place him near our residence, that we might have an opportunity of visiting him daily. Boarding was, therefore, procured for my patient at a farm house on the environs of York town. In three weeks, I took *seventy-two* ounces of blood from my patient, at five bleedings. I indulged him in the use of a mild diet, though not rigidly antiphlogistic. The bleeding produced a very flattering change in the action of my patient's disease. The quantity of fluid discharged, *per urethram*, was diminished *one half*. His skin, which had for many months been an entire stranger to perspiration, had now assumed a soft, moist and flexible appearance, and the cuticle was every where becoming smooth and free from scales. Under these encouraging circumstances my patient formed an unconquerable desire to return home. All my endeavors to prevail upon him to remain any longer under my care, were unavailing. His parents, to my regret, indulged his desire of returning home. Thus I was deprived of an opportunity of giving a fair trial to a plan of treatment, which I then thought bid fair to effect a cure of a formidable disease. When my patient left me, his pulse still justified a perseverance in the use of the lancet. The prevalence of an epidemic, at the time my patient left me, put it out of my power to attend to him at the distance of seventeen miles. In a few months he fell a victim to his disease.

When I determined to use the lancet in my patient's disease, I did not know that it had ever been employed in diabetes mellitus. I had not, at that time, read Dr. Rollo's treatise on the disease.

Blood-letting, however, did not form a part of the doctor's plan of treating diabetes. It is very evident that the very small portion of blood which he drew from his patient, Capt. Meredith, at two bleedings, was for the purpose of enabling him to investigate the pathology of the disease.

Doctor Watts, of Glasgow, published several cures of this disease, in 1808, in which the lancet was the most prominent remedy in his plan of treatment. Dr. Watts used the lancet in the case of his patient, Stephenson, under circumstances of the most discouraging kind. "The state of the pulse," says the doctor, "the dissolved appearance of the blood, and the emaciation of the patient, by no means indicate the propriety of blood-letting. The symptoms, however, have certainly given way a little, or at all events there is less hazard in the practice than is generally apprehended. It is on these grounds, and the intractable nature of the disease, that the farther prosecution of the plan of treatment can be justified." The doctor, in further describing the pathological condition of his patient Stephenson's case, states that "the pulse was slow, feeble, and not altogether regular—his strength and spirits were almost gone—the lower extremities had been edematous to the haunches, and were always cold and lifeless. When newly drawn, the blood was extremely dark; on cooling, the crassamentum was found to be as black as pitch, and totally devoid of tenacity. These were sufficient to have deterred me from trying this practice, had I not known from former experience, that many of them were ill founded." Dr. Watts did persevere in the use of the lancet, in his patient Stephenson's case, and the happy result was, the restoration of the sufferer to a complete state of health.

When I became acquainted with Dr. Watts' cases of diabetes mellites, after my removal to this city, I more deeply regretted my disappointment in not being permitted to continue my plan of treatment, in my patient's case. My patient was a very intelligent, observant youth, and was fully sensible of the improvement of his condition, under my treatment, and I have no doubt but he would have allowed me to continue the use of the lancet to any extent that my judgement would have directed, had it not been for the *officious* interference of some *gossips*, who produced an alarm in his mind, on being bled so much, in his emaciated con-

dition. To bleed a patient freely and *repeatedly*, in the condition in which my patient was placed, was certainly calculated to meet the disapprobation of those persons who are unacquainted with the animal economy, and are not observant of its functions. For my patient was *literally* a walking *skeleton*. I have never seen so *emaciated a human being* to possess the powers of *locomotion*. My patient, however, never possessed a pulse so discouraging to the use of the lancet, as was the pulse of Dr. Watts' patient, Stephenson, or that of the young student of the college of Glasgow. The blood of my patient did not exhibit so great a degree of morbid condition as was manifested in the blood of Dr. Watts' patient.

The encouraging effects of the use of the lancet in my patient's case, induced me to decide upon giving that remedy a fair trial, should another case of diabetes mellitus come under my care. A perusal of the cases of Dr. Watts, after my removal to this city, strengthened my decision in favor of the lancet as a *prominent* remedy in the cure of this formidable disease. I did not meet with a second case of diabetes mellitus, till the 1st of March, 1832, when I commenced my tour of duty as attending physician to the Baltimore Infirmary. The patient was a sailor, and he was put under the treatment recommended by Dr. Rollo, by my predecessor, as attending physician to the Infirmary, Professor Potter. As I considered the treatment adopted by my colleague had not been continued long enough to give it a fair trial, I made no change when I took charge of the patient. In a short time the patient was seized with varioloid, and was removed from the Infirmary to the Baltimore Hospital. I heard nothing respecting the issue of the case till the winter of 1833, when Mr. Smith, a member of my class, informed me that the patient had died, not of diabetes, but in consequence of a fall which he had sustained. The occurrence of varioloid caused the removal of this diabetic patient from under my care, and deprived me of an opportunity of instituting my plan of treatment for his disease. I do not undertake to say that I have formed a correct pathology of diabetes mellitus; but I have formed an opinion upon the subject—an opinion bottomed upon an attentive examination of my patient's condition.

And here let me request the reader to recollect what I stated in a former part of this communication—any pathological opinions that I advance, have *exclusive* reference to the only case of dia-

betes mellitus which I have had under treatment.* For I am far from supposing that every case of the disease will exhibit the same assemblage of pathological symptoms. Dr. Lubbock considered that "it is the emaciation that claims the first attention of the physician, for without the removal of this symptom, neither the reduction of the urine, the alteration of its quality, nor the appearance of considerable remaining muscular power will prove that the disease has yielded to any curative process." Now, I view the *emaciation* of the patient as the prominent *effect*, or *consequence*, of the *saccharine* quality of the urine, and if this characteristic quality of the urine is completely removed, the emaciation will be arrested in its progress. In my opinion, therefore, the *saccharine* quality of the urine which ought to go to the support of the bodily system, in its integrity of size and dimensions, is thrown out, per urethram, by the pathological action of some part of the corporeal structure. We, therefore, differ in opinion with Dr. Lubbock, and consider the *saccharine* quality of the urine as intimately connected with the emaciation of a diabetic patient. When I saw my patient take an unusual quantity of aliment of the most nutritious kind—when I saw that his stomach bore with unruffled calmness, this excessive amount of nutriment—when I saw the sensorium unclouded, though so heavily drawn upon by the stomach for sensorial influence—when I saw an unchanged action in the arterial system—when I saw a completely elaborated fecal matter discharged from the alimentary canal—when I was informed by my patient that he enjoyed sound and refreshing sleep, which was only interrupted by calls to unload his bladder, I could not avoid concluding that my patient's digestive organs produced an excessive quantity of chyle—a very diminutive portion of which per-

* In a preceding part of this communication I have stated that I saw a *second* case of diabetes mellitus, in the Baltimore Infirmary. But I do not consider that *I had that case under treatment*. The patient was admitted into the Infirmary about *two weeks* before the termination of my predecessor, Professor Potter's tour of duty, as attending physician. When the case came under my view, I found that my colleague had prescribed the plan of treatment recommended by Dr. Rollo. Considering that the treatment had not been continued long enough to test its utility, I made no change. In *less than a week* the patient became covered with such an extensive crop of *varioid pustules*, that he was removed from the Infirmary. I never had *that patient under treatment* and can with propriety say, that I have had *only one case* of diabetes mellitus under *therapeutic management*.

formed the round of the pulmonic circulation. The lungs of my patient were perfectly sound. Their functions, therefore, were correctly performed. And here I must say that the pathological doctrine of Dr. Cullen is completely prostrated, in my opinion. You will recollect that the doctor considered the cause of diabetes to be, a defect in the "assimilating powers." By the terms "assimilating powers," I suppose the doctor meant that finishing part of the digestive process, in which the chyle is exposed to the influence of atmospheric air, in its passage through the pulmonic circulation. I have expressed it as my opinion, that a very diminutive portion of chyle passed through the lungs of my patient. Indeed I am disposed to conclude, from an attentive examination of my patient's case, that not a *particle* of chyle entered his pulmonic circulation after the emaciating process was completely established. Hence the *extreme degree* of emaciation in his system before *death* closed the scene. The only medicine that I gave to my patient, after I commenced the use of the lancet, was a tonic astringent powder, composed of equal parts of the powder of angustura bark and powdered sulph. alumin. Of this tonic and astringent powder, he took a small portion three times in the twenty-four hours. The reader will observe, that in speaking of the fluid discharged by my patient, per urethram, I have designated it by the term *urine*, a word uniformly used by those who have described the disease under consideration. That the fluid discharged by my patient contained urine, I have no doubt; for I had every reason to believe that his kidneys were in a perfectly healthful condition. It is not correct, in my opinion, to denominate a fluid urine, which neither possesses the *smell* nor *taste* of that peculiar secretion. *Urine*, therefore, formed a *very small* portion of the fluid which my patient evacuated, *per urethram*.

Baltimore, January 30th, 1840.