

## **The unity or duality of syphilis historically considered / by J.L. Milton.**

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THE UNITY OR DUALITY OF  
HISTORICALLY CONSIDERED

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(From a Paper read before the Willan Society)

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# THE UNITY OR DUALITY OF SYPHILIS HISTORICALLY CONSIDERED.

BY

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*(From a Paper read before the Willan Society.)*

PRINTED BY OLIVER AND BOYD, EDINBURGH.

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THROUGH the kind permission of the Editor of the *Edinburgh Medical Journal* I am enabled to offer the reprint of a paper on a subject which, I believe, has not hitherto been treated specially from the point of view adopted in the following pages.

12 SUFFOLK STREET, PALL MALL, S.W.,  
AND  
SION HOUSE, KING'S ROAD, S.W.

# THE UNITY OR DUALITY OF SYPHILIS HISTORICALLY CONSIDERED.

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*(Reprinted from the Edinburgh Medical Journal for May 1886.)*

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NEITHER the sentence of death passed on duality at a well-remembered meeting of the Pathological Society of London, nor the experiments of the German physiologists, have availed to convince some men that the doctrine of two poisons must be given up, or even induced them to think that there is more reason now to doubt its vitality than there was ten years ago. Proof enough of this lies scattered through the various journals. In one of his last contributions to the literature of the branch which he so long adorned, Dr Bumstead says that such a statement as that unity is dead is "a fraud upon medical history, and a source of confusion to the student;" and even Professor Auspitz, one of the most thoroughgoing uncompromising defenders of unity, expresses himself in his latest communication on the subject rather less forcibly, it appears to me, than he did.

This justly famous pathologist asks the question, "Where are we to-day in respect to syphilis?" To which I should say the answer must be, "As far as concerns the unity or duality of the disease pretty much where we were some 350 years ago, when the reign of confusion began, and where we are likely to be 350 years hence, unless some more rigorous mode of investigation is adopted than I have ever had the good fortune to see put in force."

I propose, therefore, without entering upon any pathological questions whatever, to take what is, after all, indispensably necessary for a due examination of them, the historical part of the subject, and ask my readers, after weighing what I have put forward, to say whether they believe or do not believe that soft non-infecting sores were seen and described long before the epoch usually assigned to the great outbreak of syphilis? Secondly, whether they can point to a single authentic description of constitutional syphilis prior to the fifteenth century? And, lastly, whether, after having satisfactorily disposed of these two points, they feel inclined to affirm or to deny that syphilis, whatever be the deci-

sion now as to its simple or compound nature, was once double? The decision on these heads, be it which it may, will conduct us by a threefold path to a goal in which they naturally end,—to the first great crucial question which demands solution at the hands of the believer in unity, a question far too long overlooked, and that is whether a disease, once purely local and not including hard sore, not followed by secondary affection, all at once expanded into a malady which taints every organ and tissue of the frame, a malady from that date usually ushered in by hard sore; or if two diseases, hitherto totally distinct, suddenly became so fused together that they have ever since constituted one complaint; for, however impossible such a thing may appear, one of the two must have happened. It seems to me that it will be much better to try to get at a definite expression of opinion on this point than to make any attempt to embrace too wide a field. The bane of medical investigation has been, that only too often it did not sufficiently narrow the subject actually under discussion; that it admitted, almost encouraged, far too much laxity of expression, too much in the way of assumption; that it sacrificed to authority what ought to have been yielded only to argument; that it fostered the habit of allowing conviction to take the place of demonstration, and only too often elevated a high-sounding theory above fact. But if ever medicine is to attain to that accuracy which distinguishes the exact sciences, it can only be by means of that strict unsparing examination which these branches of study demand and will have. Consequently, if I appear to have dwelt with unnecessary minuteness on such a purely introductory part of the subject, I must plead guilty, but ask leave to justify doing so on the ground that I wish first of all to see the points just mooted thoroughly threshed out and sifted, so that the way may be made clear and straight, step by step.

It is generally said that to Bassereau we owe the remark that the earliest writers on syphilis, after the great outbreak at the close of the fifteenth century, drew a clear and broad distinction between the sores on the penis resulting from connexion, so often described in foregoing times, and the new disease which had so suddenly broken in upon them, and that it was not the first observers, the men who actually saw syphilis make its great irruption, who confounded the two forms of it. But in point of fact the same opinion was set forth long before by Hensler, who had perhaps more thoroughly mastered the views of the old writers on this subject than any other man. "It is certain," he says, "that a lesion like the sore of impure connexion, and arising from the same impure cause, existed before syphilis." He might have put the case perhaps more strongly; still, even in this shape, unless this means preaching duality, I am at a loss to know what it does mean.

I leave on one side all such questions as whether this lesion, or the old form of syphilis as it might be called, existed from time immemorial in India and China, as maintained by Klein, Nelson,

and others. Such evidence would only obscure the argument. I pass by, too, not because it is untrustworthy but because it is superfluous, the description of venereal sores given by Celsus, though I should consider it of itself sufficient to settle the question; and Dr Joseph Adams, the contemporary and rival in fame of Home and Willan, whose work on morbid poisons attests the depth and accuracy of his judgment, points out that we have here every form of sore typified except the true Hunterian chancre, and that the sloughing phagedæna, *nigrities serpens* of Celsus, is the same disease as that described by Abernethy. I consider, too, the evidence of the votive tablets in an old Pagan temple near the Seine, found and described by M. Becquerel, equally decisive; it seems to me that it would never have entered the head of any one to sketch such things unless he had had the reality before him. But I will suppose that the reader may reject such testimony as savouring rather too much of antiquity, perhaps of obscurity—that he may prefer to demand the evidence of much more modern times—and I therefore proceed to deal with this.

Between the year 1270 and the great outbreak of syphilis in 1494, we find at least seven authors mentioning such symptoms as “ulcers of the yard,” “ulcers from pustules of the yard,” “mortification of the substance of the yard,” “imposthume of the groin,” all attributed to “lying with a foul woman,” “a foul, nasty, or cancrus woman,” “a woman having an ulcer of the womb,” and so on. We may read in at least one author of sores piercing between the “skin and flesh” of the penis, the very feature which Judd gives as characteristic of the black lion, “burrowing under the foreskin and dissecting between the integument and the body of the penis.” About the close of the fourteenth and early part of the fifteenth century several persons are spoken of as suffering under phagedæna arising from connexion. And, therefore, seeing that burrowing sores brought on by illicit intercourse, phagedæna, and bubo are in the present day constant and natural adjuncts of chancroid, the conclusion that soft sore existed in the times just referred to seems to me as justly founded as any conclusion in pathology can well be.

Attempts have been made to explain away the appearance of such sores. It has been asserted that they were merely due to irritation set up by connexion, an irritation fostered by dirt and debauchery on the part of the recipient. But I should say that there are plenty of patients to be seen every day in our large hospitals fully as dissipated and filthy as ever were the people of the thirteenth and fourteenth centuries. Mr Holmes Coote learned from some of the outcast girls, who used in his time to hang about the dark arches of the Adelphi, that it was quite a common thing for one of them to have connexion in a single night with eighteen or nineteen wretched profligate creatures like themselves. Yet though he more than expected to find syphilis generated in this

way, he never could, with all the pains he took, trace it to excesses of this kind. For a long time I had unusual opportunities of seeing the out-patients in two large hospitals, and I can safely say that I saw very little indeed of phagedæna and bubo accompanying sores on the penis which did not in the end turn out to be syphilis. One author has denied that such lesions were sores at all, arguing that because the old writers believed and taught that gonorrhœa, the source of which they could not see, was due to an ulcer, therefore they must be equally mistaken about these sores—the sanies which they supposed to be issuing from an ulcer being only the product of a balanitis. This seems to me strange reasoning, and rather like fighting for a lost cause. It is most probable that men first saw pus coming from an ulcer, and then very naturally inferred that there must be an ulcer in a canal from which they saw pus coming; also it is most probable that sores piercing “between the skin and flesh” were ulcers and not simply balanitis. Besides, the old writers recognised a like sore in the female giving the infection; they mention the signs by which it could be detected in her through the medium of the sight and touch, *visui et tactui manifesta* are the words employed, and according to one author such a sore would be revealed by the speculum when it could not be made out by the naked eye.

During all this time we hear nothing of hard sore; I have entirely failed to meet with any description of such a lesion. Auspitz, indeed, says he has shown that it was described at a time when it is stated by the dualists to have been non-existent. I would always rather be on the same side with this distinguished teacher, but here I must range myself in opposition to him. The evidence on which he relies is a passage in Valescus of Tarentum, who says “he has known some patients die because they did not obtain medical assistance in time. The penis was entirely surrounded with a cancerous ulcer having a certain degree of hardness, and also round like a turnip, and the patient was already livid and half dead.” But this is a description of strangulation from paraphymosis neglected and mismanaged; it has nothing in common with hard sore, little even with chancrous erosion, while it may be seen almost daily under a milder form in our hospitals as a result of chancroid. The earliest writers on the morbus gallicus never allude to such a state. If there had been any necessary or even frequent connexion between such cases and true syphilis, shrewd observers like Leonicens, experienced army surgeons like John de Vigo, who wrote so fully and ably on this disease, would almost certainly have mentioned it, would have recognised the new malady in the description given by Valescus, with which they, the former of them especially, were sure to be familiar, seeing that he was a well-known author. Even supposing it had been hard sore, the historical evidence would still have been as strong as ever in support of the view I have ventured to

recommend. While numerous accounts attest the almost constant occurrence of local sore during more than two centuries, all that the industry of a man of such extraordinary acquirements as Auspitz can bring forward on his own side is one solitary case of very dubious nature. Auspitz also maintains that a passage from the same author must be construed as indicating a knowledge of chancre in the urethra, a lesion nearly always, if not always, followed by constitutional infection. With regret I must again range myself on the opposite side; the passage is not very clear, but it is to this effect, "The ulcers and pustules which form on the penis are sometimes in the outer skin, sometimes as far as possible within it," and my contention is that Valescus meant by the latter part of the sentence, not the canal of the urethra, but the reflexion of the prepuce. The reader can, however, see the original words quoted in Auspitz's work on *Syphilitic Contagion*, page 26, and judge for himself.

The authors spoken of as describing soft sore in the thirteenth and fourteenth centuries never allude to this as a constitutional affection at all, and one of them distinctly says it is purely local. But when syphilis first began to alarm Europe, the fact that the disease generally arises from chancre on the penis only dawned very slowly on the minds of even the best observers; in the case of the sores described by their predecessors we find no trace of such obscurity. They never doubted about the matter; so far as it went their pathology was certainly concise and most probably correct. Their treatment was suited to their pathology; they employed only topical means, and never dreaded secondary disease from restricting themselves to such simple measures. Professor Auspitz, indeed, quotes from Gerardus de Berry, an author of the thirteenth century, whose work I have not seen, testimony which he looks upon as conclusive, that at that early date secondary disease was ascribed to sores derived from impure connexion. I refrain from expressing any opinion as to whether the evidence will commend itself unreservedly as conclusive to his readers, but in reality it amounts to no more than the author he quotes from saying, that "the penis becomes infected, and that the health sometimes suffers in consequence." Why, the same thing was habitually said for ages about gonorrhœa, and there is not a single symptom, not a sign of secondary disease mentioned; in short, I cannot understand how such a simple phrase of four words is to be interpreted as meaning any distinct disease whatever, especially one like syphilis, which men described forcibly enough at a time when we know that they really did see it—that is to say, at the close of the fifteenth and beginning of the sixteenth centuries. But granting—and with all deference to Professor Auspitz it is granting a great deal—that true syphilis is meant in the passage from De Berry, it would merely show that this disease was occasionally seen at the early date mentioned, only to disappear again for

centuries, as certainly no other writer at all near that time was acquainted with any constitutional disease arising from sores on the genitals; nor can the syphilis which De Berry is supposed to describe be connected, by any stretch of argument, with the primary lesions spoken of by Lanfranc, Gordon, Arnald of Villanova, and others. That a few scattered symptoms of constitutional disease, such as boonhaw (node), suspicious ulcerations of the face and throat, are mentioned now and then by some writers of the dark or mediæval ages is quite true; but these distant allusions only appear to fade away again for a long series of years; they reveal no consciousness on the part of the writer that he was dealing with a severe individualized constitutional disease; whereas the first writers on true syphilis have given us the portrait of a malady in which we distinctly recognise the syphilis of our day, and the unbroken descent of which down to our time I need not stop to point out.

Until we come to the time when men were so startled by the great outbreak of syphilis, we never find writers branding a disease derived from sexual intercourse with such epithets as were speedily bestowed upon syphilis. No doubt a great deal of this was exaggeration—as, for instance, in the case of Grünbeck, so often quoted as an authority, whose spasmodic style of writing, with his allegorical description of the spectre of humanity and pedantic comparison of the encroachments of the disease to those of a hostile force making its way into a citadel, is not calculated to gain our confidence, interesting as his work is. But behind all objections of this kind lies the great fact that we never in earlier times find a trace of this exaggeration itself, that not even leprosy is spoken of in this manner. And when we see one author after another impregnated with such views of syphilis, and even a man of so moderate and practical a bent of mind as de Vigo speaking of it as “thys shamefull and abhominable dysease,” and all this without the least allusion to its having been known in the days of Lanfranc and Valescus, we may be sure that to them, at least, it was new as well as abhorrent.

We must not omit from consideration the extraordinary ignorance on the part of men, so well able to judge as the first authors on syphilis were, of the newest and, I suppose, still predominant theory about chancroid, namely, its origin from the inflammatory products of chancre. Such an idea as that of the ulcers on the parts of generation, described by a long succession of authorities, being simply a local form of the morbus gallicus or pudendagra, never occurred to men like Schellig and Pinctor, like Marcellus Cumanus, present with the Venetian and Milanese army in 1495, or John de Vigo who, though writing later, had evidently mingled with the actors on the scene, and might be fairly spoken of as one of them. Misled by no specious theories, these men naturally treated as distinct two diseases which, viewed by the light of history and

judged by their visible signs, were entirely different in their origin, course, and result. They were, perhaps, not quite so familiar with chancroid as Bassereau and Chaballier would have us believe; but so far as they were acquainted with it, they appear to have seen it from the same point of view as their predecessors. For all practical purposes the old soft sores, the ulcers between the foreskin and the glans—*puncturæ intra corium et carnem*—were in their eyes as distinct from the new pestilence as one disease could be from another. With the arrival of the latter we encounter a totally different pathology and practice. The search for the cause of the morbus gallicus in the baneful influence of planetary conjunctions or atmospheric agencies upon the humours, especially the bilious, the immediate recognition of the wide-spread infection of the system, and the necessity for general treatment, particularly by means of mercury, appear with the general appearance of syphilis and no sooner. In the eyes, then, of the authors who flourished at the close of the fifteenth century and the opening of the sixteenth, syphilis was essentially twofold; and the least reflecting person can understand that they were not likely to descant upon either the distinction or connexion between two affections which as yet no one had ever dreamed of confounding, or upon a genesis of chancroid, the discovery of which was reserved for modern ingenuity.

I believe disease to be as unchangeable in its nature as the elements around us. It gains or loses hold upon the human frame; its severity is lessened or aggravated, mostly by causes which we cannot fathom, but to the last it wears the same radical characteristic features as at first. The scarlatina of this century seems to be more fatal than that of Sydenham's day, but it is still simply scarlatina; leprosy is less formidable in Iowa and Minnesota than in Norway and Guiana, but in no other respect does it change one jot. The sweating sickness of 1517 was more fatal and rapid than that of 1485, but, I suppose, no one ever doubted their identity in nature. The cholera of 1854 was more deadly than that of 1849, yet cholera has not altered in one essential characteristic since it first issued from the jungle. Many other instances of these mutations of severity might be cited, but not one of them points to a fundamental change like that which we must admit if we are to accept the unity of syphilis.

It seems to be in the order of things, that old opinions, however well founded, must, for some reason or other, yield to those which possess the charm of novelty—that the generation of to-day must overlook or reject something which a previous age had received with general assent. Thus it proved with respect to syphilis. By the time that the pathology of the disease had begun to assume completeness of outline under the hands of Paracelsus, 1527, and Fernelius, 1556,—that the dependence of the constitutional symptoms upon a specific primary affection, itself produced by a poison

acting at the point of contact, had come to be recognised, although still encumbered with much that was erroneous as regards the nature of this lesion,—the old historical landmarks had almost been lost sight of, and very soon after the time of even the first of these two writers the epoch of confusion had fairly set in. As this is the turning-point in the history of our subject, I propose to examine it rather fully.

MM. Bassereau and Chabaliér date the change in men's opinions as to the quality of syphilis from the account of this disease given in 1508 by Vella. If so, his readers were very easily convinced, for, I think, a piece of worse reasoning could scarcely be found; and if he can be supposed to have proved anything, it is that he has utterly confuted himself. He saw the disease both before and after the outbreak of the morbus gallicus, and expresses his surprise at finding it from that date followed by constitutional symptoms, because the initial lesions seemed to him identical. "The way in which these patients are infected," he says, "is the same as that in which the virile members were infected before this disease (the morbus gallicus) existed—that is to say, by the act of coition, by the same organ, and, so far as the power of sight enables one to judge, by the same pustules. For which reason, those skilled in the art (of medicine) not being able to see any distinction between the one and the other, proceeded in the old way with the treatment of the new disease." Unless this means that chancreoid not only existed, but was fairly common before the assumed date of the coming of syphilis, I am utterly at a loss to know what it does mean. If the fact of an author, writing on the spot, treating one disease as new and never alluding to the other in any such way, be not evidence of the pre-existence of that other, pray, of what is it evidence? what does it go to prove? Here, too, we see scattered to the winds all the arguing about the soft sores of the thirteenth and fourteenth centuries being common ulcers, for Vella tells us that he could not distinguish them from sores which were clearly the first lesions of syphilis. Therefore, if the older sores were not chancreoids, chancreoid has never existed at all, and must be treated as a myth. Of course, Vella's mistake was that he converted an occasional resemblance into a rule. The occasional resemblance we know to be an established fact, seeing that some of the best observers, even M. Ricord himself, have not hesitated to confess that they could not always make out the nature of a sore from its visible signs.

Here, then, we see that the confusion between the two forms of syphilis, soon afterwards to be openly taught, is fairly hinted at on authority, and had already begun to disturb men's opinions; but for all this, I consider that both Bassereau and Chabaliér have erred in ascribing the beginning of the belief in the unity of syphilis to the weight of any arguments employed by Vella, for I am inclined to think that he never had influence enough, either

with his contemporaries or with later writers, to gain currency for his views. He seems to have scarcely ever been mentioned by either class of authors, and his practical knowledge of the disease to have been in rather a crude state, for he was evidently not acquainted with the Hunterian sore, common, according to what de Vigo says, after the invasion of Italy, seeing that he could not very well have mistaken it in a developed state for one of the old soft chancres. It is not true, too, that those skilled in medicine treated both kinds of primary sore—that is to say, the lesion seen before, and that first noticed after, 1494—in the same way; not a passage can be produced from any standard writer on syphilis to such effect. The purely local method pursued by the practitioners of the fourteenth and fifteenth centuries is as distinct as can well be from the constitutional means employed almost at the first appearance of syphilis, especially as regards the use of mercury, recommended as early as 1495; and for my part I cannot understand any person maintaining the identity of the two systems. Vella's evidence, therefore, in support of the unity of syphilis, which he is trying to enforce, must, I contend, be rejected, and, as well as I can make out, was rejected for long after.

It was not even a novelty on his part to trace a likeness between the old sores and the primary lesions of syphilis. Six years previously Almenar had described the new disease as beginning with a corrosion on the penis like the caroli. But I must totally dissent from the sweeping statement of a modern writer, that when syphilis first broke out the authors of that day, Marcellus of Como, Leonicens, Beniveni, Cataneus, de Vigo, Vella, and even Fracastoro, found no difference between the old caroli pudendorum and the pus sores on the genitals, which they described as an accidental symptom preceding syphilis! Fracastoro is not entitled to a hearing, as he could only have learned about the matter from others. With Vella I have already dealt, and de Vigo must be eliminated, for his classical description of hard sore is the earliest thing of its kind; no such portrait of Hunter's chancre exists before his time. As regards three of the first four names I can say nothing; the author in question does not quote his references, and I have searched in vain for a single passage to which such an interpretation could be given. The evidence of Cumanus I must read the other way, for though he saw the two diseases side by side, he does not seem to have thought of confounding them; and his description of sores the size of a millet seed, whether it refer to itch or to follicular chancre, would be as difficult to find in any author before his era as would that by de Vigo. But, indeed, the question is one, not of resemblance, for that with some reservation may be admitted, but of identity. On the latter point the evidence of the earlier writers after 1494 is decisive; they saw no identity. And as I understand Vella, some of his contemporaries were anything but convinced that the two affections were one and the

same, for in substance they were asking the question, "Why, if this be old, did we never see such results till now?" And the reason why is, according to him, a new state of the atmosphere.

The mystification was not long to be delayed, and the task fell into the hands of two of the most incompetent and blundering observers of that or any other age, Massa and Bragsavolus; the first effectually began, and the other thoroughly completed the work of confusion. Massa, a person of narrow, superstitious views, who in an age of inquiry had not advanced a step beyond the crude opinions of the earliest observers, added, 1532, chancrous bubo to the body corporate. Bragsavolus, 1551, joined to the two forms of syphilis, now fused into one, gonorrhœa, recognised as a local complaint at least as far back as the time of John Arden. They carried the day. Though they were opposed by men like Fracastoro, Scaliger, and Fallopius, the tidal wave of obscurity which they had raised buried for a time at least all dissent, and has made itself felt often enough in the present century. Such men, having adopted an idea, seem incapable of any further impression from either arguments or facts, and adhere to the first conviction in much the same way as the bivalve clings to the rock on which it has anchored itself.

It will be unnecessary to reproduce the opinions of Fernelius, Fallopius, and others. They effected no particular change in the general belief. It will suffice to say that the reign of chaos, having fairly set in, went on with unvarying monotony, reaching, perhaps, its lowest depths in the pages of Sydenham, who wrote, I should say, the worst description of syphilis in the whole world,—the catastrophe in which, according to him, this disease naturally closes being a picture utterly untrue to nature, though quite in keeping with the rest of his account. "The limbs," he says, "rot away piecemeal, and the mangled carcase, having at length grown hateful to the living, is buried in the earth." The views of Turner and others who followed Sydenham need not detain us, as they go for nothing in this question, the first real step towards the solution of which was reserved for Hunter, with whom history ends and pathology begins.



