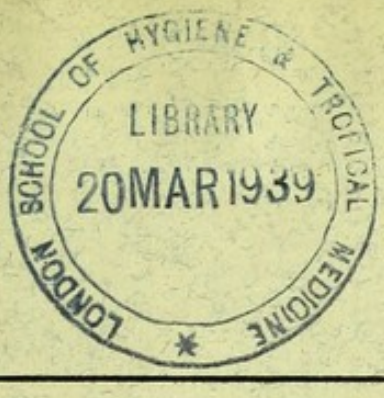
Dr. C. Creighton, M.D. and vaccination : a review / by J. McVail.



Dr C. CREIGHTON, M.D.

AND

VACCINATION

A REVIEW

BY

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DR CREIGHTON ON VACCINATION:

A REVIEW.

The Natural History of Cow-pox and Vaccinal Syphilis. By CHARLES CREIGHTON, M.D. London: Cassell & Company: 1887.

"Vaccination." Article in the *Encyclopædia Britannica*, Ninth Edition, Vol. XXIV. By the same Author.

Jenner and Vaccination: A Strange Chapter of Medical History. By the same Author. London: Swan Sonnenschein & Co.: 1889.

"It is the nature of an hypothesis, when once a man has conceived it, that it assimilates everything to itself as proper nourishment; and from the first moment of your begetting it, it generally grows the stronger by everything you see, hear, read, or understand. This is of great use."—*Tristram Shandy.*

PERHAPS no better instance could be imagined of the occasional correctness of Tristram Shandy's views on "the nature of an hypothesis" than is furnished by Dr Creighton's opinions on vaccination. Three years ago he published an interesting little work on *Unconscious Memory in Disease*,[1] in which he formulated the doctrine that the word "memory" has "a real application to unconscious organic phenomena," and is not merely "a figure of speech." Illustrations of this memory he found in connexion with such maladies as cancer, tubercle, syphilis, pellagra, leprosy, albuminuria, neuralgia, etc. When he began, in the same year, to study the subject of vaccination, in preparation for the article in the *Encyclopædia*, he soon found reason to conclude that, in reading the natural history of cow-pox, he had fallen in with a fresh and very perfect instance of "unconscious memory." The original cow-pox, as it occurred in the bovine animal, was a disease of a very severe character, and when inoculated on man often produced very severe symptoms—deep phagædenic ulcers, erysipelas, axillary abscesses, much constitutional disturbance, and even "secondary" symptoms, as roseola, lichen, pemphigus, and, in one case, an affection of the throat. By careful "management"

[1] H. K. Lewis, 136 Gower Street, London, 1886.

and selection of mild "strains" of lymph these characters of the "untamed" cow-pox have been gradually got rid of, and the result is the eight-day or "short cycle" pock of ordinary experience. Every now and then, however, we meet with reversions to the original type. Where the crust becomes detached and leaves an ulcer, we have been in the habit of attributing it to accident; where erysipelas has arisen, we have looked on it as entirely due to constitutional debility, or to foul instruments, or to direct infection from other cases; and where, on the Continent mainly, cases of "vaccinal syphilis" have occurred, we have accounted for them by heredity, or by insertion of the syphilitic virus from a specific sore, or by the possibility of inoculation from an infected vaccinifer. In all these suppositions we have been wrong, Dr Creighton tells us. The ulcer and the erysipelas were merely acts of memory on the part of the lymph used—a harking back to the original type of cow-pox as seen on the cow or on those vaccinated with "primary lymph." And the supposed syphilitic disasters were not due to syphilis at all—not even in the well-known case of the public vaccinator, who inoculated himself from a child with open syphilitic sores, and in due course developed the ordinary appearances of the disease. The symptoms, primary and secondary, were merely the result of a return to the first and worst characters of cow-pox. This brings us to the opinion that cow-pox has its analogue, not in small-pox, but in great-pox. It is not syphilis, but it is on all fours with it—it is a parallel disease. This being so, it naturally follows, in Dr Creighton's scheme of things, that cow-pox, having no relationship, either of similarity or of antagonism, to small-pox, has no preventive power against small-pox. It is not the case, and it never was the case, that vaccination had any inherent property of keeping away small-pox, nor of modifying an attack when it did occur. The popular traditions of the dairies, the variolous tests of the early vaccinators, the wide and prompt acceptance of the practice by the medical profession of Europe and elsewhere, the modern statistics both of mortality to population and of case mortality, and all the accumulated experiences of the world since the beginning of the century, form part of a gigantic delusion the like of which perhaps has never been seen before, while the key to the whole problem is to be found in Dr Creighton's theory, that organic phenomena, not merely mental but bodily, are endowed with unconscious memory, and that "memory" used in no metaphorical sense, but in the everyday meaning of the word, is as truly predicable of a disease like cow-pox as it is of the author of an autobiography.

It is with diffidence and misgiving that I have ventured to make this synopsis of Dr Creighton's thesis, for, if I am right in my statement of it, it follows that he sometimes uses English words in a sense which does not ordinarily belong to them. In the *Lancet* of 12th January 1889 he says,—"When I began it [the subject of

vaccination] in 1886 I had no other prepossessions than those which nearly all medical men have in favour of an established doctrine and practice. It was not until I had spent some months in a search among the authorities, pathological and other, at first hand, that I felt constrained to modify the opinions which I had hitherto implicitly accepted."

If I am right, or within a measurable distance of being right, in my interpretation of Dr Creighton's opinions, is it possible, by any common stretch of language, to describe these opinions as a "modification" of any previous opinions which were consonant with "the prepossessions which nearly all medical men have in favour of" vaccination? But, at any rate, I am safe in quoting his own words. In his latest book, addressed to the public rather than (as was his letter in the *Lancet*) to the profession, he speaks of vaccination as "a grotesque superstition."[1] Is this a "modification" of any ordinary views on the subject? If so, then a man who formerly held that the earth was a sphere, and now asserts that it is a level plane, may properly express his change of mind by saying that he has "modified" his views as to the shape of the planet.

It is obvious that, in endeavouring to square with his hypothesis all that is known of small-pox and cow-pox and syphilis, Dr Creighton sets himself a gigantic task, and one cannot but admire the courage which characterizes his effort to accomplish it. In attempting to review some of his principal lines of argument, I am met by the initial difficulty that his opinions are nowhere put in the form of definite propositions. I will, therefore, try to state each of them as necessity arises.

(1.) The first question to be taken up refers to the folk-lore of cow-pox. The belief in its protective power against small-pox, he says, was a localized one. It had never been heard of till rather late in the eighteenth century, and it had no foundation in fact. The only bond of connexion between the two diseases was the word *pox*. The "jingling sound of 'cow-pox—small-pox'" led the "officious gossips" of the countryside to make a legend out of it, just as the old herb-books alleged that for a person to carry about with him the herb hound's tongue, protected from mad dogs, and that the root of the dog-rose cured their bite. This, and this alone, originated the idle story.

Such is Dr Creighton's statement in Chapter II. of *Jenner and Vaccination*. Clearly it is of the very greatest importance, for Jenner dates his first thoughts on the subject from hearing in his master's surgery in Sodbury a young woman observe, "I cannot take that disease [small-pox], for I have had cow-pox;"[2] and during the next thirty years much of his attention to the matter was due to the constant reiteration of this opinion by the dairy people of the Vale of Berkeley.

[1] *Jenner and Vaccination*, p. 353.
[2] Baron's *Life of Jenner*, vol. i. p. 122.

Is, then, Dr Creighton's statement correct? In his last published work he makes constant references to the early volumes of the *Medical and Physical Journal.* He seems to have ransacked them from beginning to end (and well worth the labour they are); but, curiously enough, he has missed their very clear and ample evidence, that he is here entirely wrong. Vols. iii. and iv. (p. 503 and p. 425), published in 1800, contain letters on the subject from Dr John Barry of Cork. Dr Barry shows that the country people of his part of Ireland were well acquainted with the disease, that they knew it by the name of *shinach* or *shinagh,* and that they attributed to it the antivariolous power. One lady had had it about 1760, "and was then informed by some of her neighbours that she never would have the small-pox," which turned out to be true, contrary to her own expectation, and in spite of many opportunities of infection. A gardener "gave himself the disease purposely, by rubbing himself against some one who was affected with it, from a conviction that it would prevent the small-pox." Still more interesting is the following:—

"Johanna Sullivan, aged 50 (cookmaid at Dr Richard Walsh's of this city), when she was 13 years of age was brought with a number of other children to a dairy, for the purpose of being infected with a disorder of cows called the shinach, which by the general belief of the neighbours would secure for ever such as took it from the small-pox. She and the other children were made to squeeze the cows' teats till their hands and fingers were covered with the fluid matter of the disorder. . . . . When she was 20 years of age she was twice inoculated by Mr Godwin, an apothecary at Bantry, without effect; but on hearing from the mother that she had the disease above mentioned, he declined inoculating her a third time, alleging that there was not the smallest danger of her ever taking the small-pox, as he could aver from experience. She has since resided in Cork, where she was frequently exposed to small-pox, particularly about eleven years ago, when the grandchildren of the late Mr Attewell Hayes, with whom she then resided, were inoculated. In order, as she said, to be sure of herself, she lay with the children four nights in the height of the eruption, but did not take the disease."

Dr Barry gives another bit of evidence, of even more importance. It is an extract from a letter which he had received "from a lady of respectable connexions:"—

"It is thirty years since my mother had the cow-pock. . . . . She has been inoculated frequently since, and exposed to the infection of small-pox in various ways without taking it, which has been attributed to her having had the cow-pock, universally known among our farmers by the name of shinach. I was last night speaking to my grandmother on the subject. She had the cow-pock fifty years ago. Her account agrees with my mother's. . . . . She has never been inoculated, but was very often exposed to the small-pox without taking it. At the time she had the disease there was scarcely a spring that the cows were not affected with it; and it was so universally believed that those who took it were ever after exempted from the small-pox, that people exposed themselves as much as possible to it. My grandmother, who is about eighty years old, says that the same opinion always prevailed in this country."

These accounts go to show three things, all denied by Dr Creighton—(1), that the disease was known as a preventive of

small-pox long before "rather late in the eighteenth century;" (2), that the tradition was not confined to a few "English dairy-farming districts;" and (3), that as "shinach" does not jingle either with the English term "small-pox" or with the Irish term *Galra breac*,[1] the "jingle" theory will not account for the facts. It is *not* therefore a mere "case of the river in Macedon and the river in Monmouth." But, indeed, we don't need to go outside the covers of Dr Creighton's own book for proof. In a "note to page 21" (in which it is asserted that the legend is a local one), printed at the end of the work, he says that "there is an authentic record that the protective virtue of cow-pox had been talked of in the country near Gottingen previous to 1769." Here cow-pox was called *kuhpocken*, and small-pox *blattern*, so that again there was no "jingle" to give rise to the legend. But while Dr Creighton adds this note, he calls no attention to its bearing on his theory, and makes no attempt at a reconciliation of the fancy and the fact.

Thus one of the main contentions of the book is shown to be entirely without foundation, and in passing from it I have only to note of how much ingenuity the invention of the "jingle" doctrine shows the author to be possessed.

(2.) Dr Creighton asserts that the great bulk of the medical opinion of the country districts was dead against the antivariolous power of cow-pox. Indeed, in one place he conveys the suggestion (unintentionally, of course) that the jingle theory existed even in their day, and that they were believers in it. His words are that, when Jenner "used to air the popular fancy," "the medical men who had experience to guide them would good-naturedly produce case after case which showed that the popular belief, in so far as it was held even by the vulgar, was a mere verbal illusion."[2] Again, he says (p. 24)," the way was barred by the hard facts of experience, which country doctors, who knew far more of cow-pox than Jenner did, recognised in the way that sensible men always do recognise hard facts." This depreciation of Jenner, and magnifying of all who were opposed to him, or are imagined by Dr Creighton to have been opposed to him, is characteristic of the whole critique. Why the other medical men should have known "far more than Jenner" does not appear; and that they were "sensible men," Dr Creighton concludes simply because he supposes they agreed with his present opinions.

It happens that in 1798, shortly after the publication of Jenner's *Inquiry*, another *Inquiry* saw the light—the *Inquiry into the History of Cow-pox*, by Dr George Pearson, of St George's Hospital (who afterwards had a bitter quarrel with Jenner). Incited by Jenner's publication, Pearson wrote to a number of his medical acquaintances throughout the rural districts, asking what knowledge they had of cow-pox and its relationship to small-pox. The

[1] Reynold's *Medicine*, 2nd ed., vol. i. p. 223.

[2] Pp. 162–3, *op. cit.*

book contains the results of his investigations, and it is surprising to learn how many medical men, within a comparatively small radius of Berkeley, had formed on this point the same opinion as Jenner. The following I have noted: — Mr Bragge, Axminster; Mr Downe, Bridport; Dr Pulteney, Blandford; Mr Henderson, Wendover; Mr Giffard, Gillingham; Prof. Wall, Oxford; Mr Dolling, Blandford; Dr Croft, Staffordshire; Mr Rolph, Peckham (formerly of Thornbury in Gloucestershire); Mr Groves, Thornbury; Mr Wales, Downham (Norfolk); Dr Fowler, Sarum; and Mr Hughes, Stroudwater. Here is a sample of their evidence:—

"Mr Rolph says there is not a medical practitioner of even little experience in Gloucestershire, or scarce a dairy-farmer, who does not know from his own experience, or that of others, that persons who have suffered the cow-pox are exempted from the agency of the variolous poison. The late Mr Grove [Mr Rolph's colleague] was a very extensive small-pox inoculator, frequently having 200 to 300 patients at one time, and the fact of exemption now asserted had been long before his death abundantly established, by his experience of many scores of subjects who had previously laboured under the cow-pox, being found insusceptible of the small-pox, either by inoculation or by effluvia. While Mr Rolph practised at Thornbury, he thinks not fewer than threescore instances of failure in attempting to produce the small-pox inoculation occurred in his own practice, all of which were persons who had been previously affected with the cow-pox. In almost all of these cases, the uninfected persons associated with those who took the small-pox, and many were repeatedly inoculated. Although Mr Rolph has not, in his recollection, any instances of people taking the small-pox who gave admissible evidence of their having laboured under the cow-pox, he thinks such cases may, and have indeed occurred to others, where the cow-pox had only been local."

It is noteworthy that this same Mr Rolph, so early as 10th June 1795, or three years before Jenner's *Inquiry*, wrote to Dr Beddoes of Bristol on the same subject, and in a similar though less decided strain.

So, too, Mr Bragge, many years before, had inoculated over 50 persons, of whom three had had cow-pox, and these "he therefore charged with an abundance of matter, but to no purpose." Dr Pulteney stated that "an intelligent and respectable inoculator" had informed him "that of several hundreds whom he had inoculated for the small-pox, who had previously had the cow-pox, very few took the infection; and such as did, he had great room to believe, were themselves deceived in regard to their having had the cow-pox." Mr Downe said, "A few years ago, when I inoculated a great number for the small-pox, I remarked that I could not, by any means, infect one or two of them, and on inquiry I was informed they had previously been infected with the cow-pox. Some few families who had been infected with the cow-pox were repeatedly inoculated with the matter of the small-pox, and without effect."

Evidence of this sort could be largely added to, but enough has been given to show how far from the fact is Dr Creighton's opinion

that the legend was believed only by "credulous people," or "officious gossips," and not by those "who had some real practical knowledge of either or both diseases."

It is strange that all this adverse testimony receives no notice from Dr Creighton. But a still stranger thing remains to be stated regarding the evidence which he himself adduces. One man in particular he brings prominently forward as a witness. He says "some of Jenner's professional neighbours knew a good deal about it [cow-pox], particularly Mr Fewster of Thornbury" (p. 19). "The man who knew most about cow-pox sores in milkers was Fewster of Thornbury; and Fewster, as well as others, had unfortunately good reason to scout the milker's protection from small-pox as an old wife's fable" (p. 24). "Fewster, the chief authority on cow-pox" (p. 54). "Fewster and the rest knew there was nothing in it" (p. 55). Thus while Croft, Rolph, Wall, Giffard, etc., are all against him, our author can at least depend on "Fewster of Thornbury." We need not therefore quarrel with him if he tends to exaggerate a little the special knowledge and capacity of his chosen authority.

What, then, says Fewster, "the man who knew"?

Listen: "I can now with truth affirm *that I have not been able to produce the small-pox, in a single instance, among persons who have had the true cow-pox.*"

These are the *ipsissima verba* of "Fewster of Thornbury," italicised by himself! They occur in a letter from him, published in Pearson's *Inquiry*, pp. 102–3. And lest an isolated sentence should misrepresent his meaning, I give the whole passage:—

"In the spring of the year 1768 I came to live at Thornbury, where I have resided ever since. In that very year, from the following occurrence, I became well acquainted with the disease called cow-pox. The late Mr Grove and myself formed a connexion with Mr Sutton, the celebrated inoculator; and to inoculate for the small-pox we took a house at Buckover. We found in this practice that a great number of patients could not be infected with the small-pox poison, notwithstanding repeated exposure under most favourable circumstances for taking the disease. At length the cause of the failure was discovered from the case of a farmer who was inoculated several times ineffectually, yet he assured us he had never suffered from the small-pox, but, says he, '*I have had the cow-pox lately to a violent degree, if that's any odds.*' We took the hint, and, on inquiry, found that all those who were uninfectable had undergone the cow-pox. I communicated this fact to a medical society, of which I was then a member, and ever afterwards paid particular attention to determine the fact. I can now with truth affirm *that I have not been able to produce the small-pox, in a single instance, among persons who have had the true cow-pox*, except a doubtful case which you are acquainted with. I have, since that, inoculated near two thousand for the small-pox, amongst whom there were a great number who had gone through the cow-pox; the exact number of these I cannot tell, but I know that they all resisted the infection of variolous matter."

Then Fewster goes on to state that he "never knew one mortal or even dangerous case" of cow-pox, but that at the same time he thinks it "a much more severe disease in general than the

inoculated small-pox," and that therefore he does not see "any great advantage from inoculation for the cow-pox."

How are we to account for this extraordinary blunder on the part of Dr Creighton? Baron[1] says that Jenner had often told him that at the meetings of the local "Convivio-Medical Society," his brethren looked on the cow-pox rumour as a vague notion, and that most of them had met with cases in which persons "supposed to have had cow-pox had subsequently had small-pox." Dr Creighton quotes the passage. In the same page Baron gives the names of a few of the members, and among them he happens to include Fewster's. So far as I can make out, this seems the whole foundation for the misstatement. In general, Dr Creighton's distrust of Baron is only surpassed by his distrust of Jenner himself. He says (p. 3), "It is at this point that the Jennerian mythus begins in the pages of the biographer Baron," and on page 9 we read of "the biographer Baron, mythological as usual." As a rule, Baron's writings are of course strongly in favour of Jenner. Then, to Jenner's critic Baron is a mythologist. But when Baron makes the above statement, so little calculated even as it is to serve Dr Creighton's purpose, he seems eagerly to seize on it, and out of it to manufacture "the mythus," or "legend," or "old wife's fable" (there is a wealth of such terms in Dr Creighton's writings), that Fewster looked on cow-pox as entirely devoid of any anti-variolous property. So true it is, that "an hypothesis, when once a man has conceived it, grows the stronger by everything you see, hear, read, or understand."

Before passing from the evidence already adduced that a belief in the anti-variolous power of cow-pox was entertained by a number of medical men in the south-western counties of England, I wish to point out that Dr Creighton's lack of knowledge of the facts has led him astray, both in his reasonings on the subject and in his charges against Jenner. Having reached the curiously mistaken conclusion that Jenner's professional brethren were entirely opposed to his opinion, the doctor goes on to urge that this could not be otherwise. "We can hardly have a stronger or better founded conviction than they had that, whatever the similarity of names depended on, the diseases themselves were totally unlike" (p. 26, *Jenner and Vaccination*), and "a fancy of that kind could not exist along with real or even empirical knowledge of the two diseases" (p. 34). How, then, did Jenner think differently? Because "his knowledge of the affection does not appear to have been more than an acquaintance with the name and the common talk" (p. 25). "It was just because Jenner had no profound sense of these empirical realities that he went blundering into visionary nonsense in the first instance, and at length into systematic mystification and chicane" (p. 26). Further, "it was reserved for Jenner to take up that surprising legend and

[1] *Op cit.*, vol. i. p. 48.

make it scientifically passable, despite the impatience and ridicule which his prosaic medical neighbours in the cow-pox districts had met it with. It is difficult to acquit Jenner of recklessness or of culpable laxity, even in the very inception of his idea" that cow-pox "was an amulet or charm against small-pox" (pp. 33, 34). Indeed, it would be wrong to accuse Dr Creighton of making any serious attempt at the acquittal. On the contrary, the reader soon finds, with some amazement, that Jenner was guilty, not of ignorance, but of knowledge. At the time when "he rushed off to London to publish his *Inquiry*" he "was well aware that there were quite as many instances telling against protection as there were in favour of that popular fancy" (p. 76). Not only so, but "Jenner knew these differences between cow-pox and small-pox well enough,—indeed, he knew of far more striking differences, only he took care not to dwell on them" (p. 115). How and when Jenner, "lazy and unmethodical," acquired all this exact knowledge does not distinctly appear, but the assertion that he did know was clearly necessary to the proper setting forth of the charge regarding his comparative want of "honesty and candour."

In the light of the facts that many of Jenner's "prosaic medical neighbours" *did* believe that cow-pox prevents small-pox, and that they were "well aware" that there were few instances, real or apparent, to the contrary, and that the man who, according to our author, knew most about cow-pox, had implicit faith in its prophylactic power,—all this assumption that it was only Jenner, "loose thinking and imaginative," who held or could hold such an opinion, seems very ridiculous. Fortunately for succeeding generations, he differed from his neighbours in not stopping at the mere opinion.

(3.) We come now to the Latin name *Variolæ vaccinæ*, or small-pox of the cow, given by Jenner as an alternative to the English *cow-pox*. Dr Creighton waxes very wroth over the introduction of the new term. It is not merely that he believes it to be scientifically inapplicable to a disease whose relationships were not with *variola* but with syphilis. It was a "subterfuge" (p. 45), "the unblushing invention of a misleading name" (p. 76), an "artifice" (p. 81), a "foisted name" (p. 97), a "secret artifice" (p. 118), "disingenuous" (p. 239), and so on. The influence of the invention was remarkable. "The name of variolæ vaccinæ was accepted as proof enough that cow-pox *was* a sort of small-pox of the cow" (p. 73). "The invention of the new name was artfully concealed, and was never found out; and under the influence of the plausible idea which the new name covered, the evidence of protection was accepted" on unusually easy terms (p. 124). Indeed, in this connexion Jenner was an embodiment of cunning. For, "having found that the name on his title-page was adopted without suspicion, Jenner used it ostentatiously in the text of his second essay, although it is not used at all in the text of the first" (p. 79).

I am not about to enter into a pathological discussion on the

correctness or otherwise of the name variolæ vaccinæ; and that the medical profession, or any other body of sane men, was so very easily led away, hardly needs disproof. But, as a matter of fact, it was not found necessary to the acceptance of vaccination as a preventive of small-pox that cow-pox should be believed to be small-pox of the cow, for some of the most enthusiastic advocates of the practice held no such view. Dr Creighton, indeed, mentions one such—Dr Pearson, already referred to. But, in doing so, he minimizes the nature of the objection, which, he says, "was of the mild grammatical kind . . . . a catachresis of speech, as if one were to speak of the plumage of a bear" (p. 80). Pearson went much further. In one place he says, "We must distinguish the poisons as of distinctly different species;"[1] while the passage referred to by Dr Creighton is as follows:—"Now, as the cow-pox is a specifically different distemper from the small-pox in essential particulars, namely, in the nature of its morbific poison and in its symptoms,—although the cow-pox may render the constitution not susceptible of the small-pox,—it is a palpable *catachresis* to designate what is called the cow-pox by the denomination *variolæ vaccinæ*, for that is to say in English, *cow-small-pox*, and yet the cow is insusceptible of infection by the variolous poison." Dr Huggan, another supporter of vaccination, says, "It has been asserted, and that without the least shadow of proof, that the cow-pox is only the small-pox having undergone a certain modification by passing through the quadruped."[2] There was no more enthusiastic vaccinator than John Ring. Dr Creighton calls him "the most active of the cow-pox propagandists" (p. 191); but his activity had a different mainspring, for he insists that "it is certain that the cow can neither receive nor communicate the small-pox."[3] And finally, Odier of Geneva, described by Dr Creighton as "the great promoter of vaccination in Switzerland" (p. 262), invented the name "*la vaccine* or *vaccina*, rejecting as absurd the name of the English 'variolæ vaccinæ.'"[4]

Regarding Dr Creighton's charge against Jenner, that having quietly "foisted" the name on the profession, he ostentatiously paraded it in his second essay, I find the facts hardly as stated. As Dr Creighton mentions, the term "variolæ vaccinæ" occurs in the short title of Jenner's *Inquiry*. In his second paper he refers eight times to the *Inquiry*, and on seven of them he speaks of it as his "essay" or "treatise" on the variolæ vaccinæ. In addition, he mentions the words only once, while he uses the term cow-pox sixty times. But besides, we get a little insight here as to how deeply the much abused term had engraven itself on and influenced the medical mind. The paper contains a number of letters from doctors, and they make eighteen references to the bovine disease. On seventeen occasions they speak of it as cow-pox, and once only as variolæ vaccinæ. And eight months after the

[1] *Medical and Physical Journal*, vol. iii. p. 100.
[2] *Ibid.*, p. 244. [3] *Ibid.*, vol. vi. p. 487. [4] *Ibid.*, vol. iii. p. 100.

publication of his *Further Observations* Jenner issued his third paper, *A Continuation of Facts and Observations,* in which the term in question occurs only once, and that in a letter from a correspondent. What becomes here of the amazing craft and anxiety which Jenner displayed in forcing the title into use?

Finally, thirty years later, we find in the *Medical Repository* a reference to "What physicians have long laboured to discover—a satisfactory and simple explanation of the protective power of cow-pox against small-pox." Such a statement is clearly incompatible with the supposition that the profession had adopted the theory of cow small-pox, and it is quite evident that Dr Creighton "lays on the Latin term a burden of responsibility which it has no right to bear."[1]

(4.) *The Variolous Test.*—One of the most difficult tasks which Dr Creighton sets himself, but one which it was absolutely necessary for him to face, is the explaining away of the numerous small-pox inoculations by which the early vaccinators tested the protection afforded by the cow-pox. That he fails in his attempt is not to his discredit, for it is an attempt in which no man could have succeeded. At the same time, the theory which he sets up is interesting, both in its originality and its ingenuity.

When inoculation was first introduced from the East by Lady Mary Wortley Montagu, the disease produced was in a certain proportion of cases so severe, and even fatal, that the practice fell into desuetude for twenty or thirty years. After its gradual resumption, a change in the method of performance came into vogue, and was accompanied by lessened risk and mortality. A change was badly needed. Here is the method as practised by James Burges, who wrote in 1754:—

"Let a slight incision, of about an inch long, be made on each arm through the cuticle into the skin, but not through it so as to wound the cellular membrane; let a thread saturated with variolous matter be laid along the whole length of the wound, and covered with a pledget of digestive ointment, fastening it on with an adhesive plaster, and binding it on with a thin linen roller. Let this dressing continue on two days."[2]

But Sutton and Dimsdale (afterwards Baron Dimsdale) introduced a milder method.

The English practice, as Woodville tells us in 1796,[3] was Dimsdale's, whose instructions, published in 1766, had "almost, without exception, deservedly continued ever since to regulate the practice of inoculation." Dimsdale's method was as follows:—

"The patient to be infected being in the same house, and, if no objection is made to it, in the same room, with one who has the disease, a little of the variolous matter is taken from the place of insertion, if the subject is under inoculation; or a pustule, if in the natural way, on the point of a lancet, so that both sides of the point are moistened. With this lancet an incision is

[1] *Glasgow Herald*, January 9th, 1890.

[2] Woodville's *History of the Inoculation for the Small-pox in Great Britain.* London, 1796.

[3] *Ibid.*, p. 375.

made in that part of the arm where issues are usually placed, deep enough to pass through the scarf skin, and just to touch the skin itself, and in length as short as possible, not more than one-eighth of an inch. The little wound being then stretched open between the finger and thumb of the operator, the incision is moistened with the matter, by gently touching it with the flat side of the infected lancet. This operation is generally performed in both arms, and sometimes in two places in one arm, a little distance from each other. For, as I have not observed any inconvenience from two or three incisions, I seldom trust to one. . . . . Neither plaster, bandage, nor covering is applied, or in any respect necessary."[1]

This brings us to Dr Creighton's contention. He holds the new method to have been a sham. But it was this method that was advised by Jenner in testing his cow-poxed cases. Therefore the test was a sham. Not only so, but the whole medical profession, all over the world, agreed to deceive itself and the public in a quite unheard of fashion. Up till Jenner's introduction of vaccination, a single pustule, or even a little redness of the skin, had been held sufficient evidence of success. When vaccination cases were tested, the pustule was produced as often as before, or nearly as often. But now, acting with one mind, medical men turned round, and said that the single pustule and the slight inflammation were *no* evidence of variolation; that, indeed, they were the very opposite, and that unless an eruption of small-pox resulted, with the accompanying fever, it could not be said in any case that vaccination had failed.

It is, of course, to be recollected that vaccination did not at once supplant small-pox inoculation. Many doctors practised both. What, then, are we asked by Dr Creighton to believe? This: That medical men went on inoculating in the same manner as before, that they inoculated the vaccinated and the unvaccinated alike, and that in both classes alike they obtained the same average result as before—this being in a great many cases a local pustule, with or without a few additional pimples. Supposing, now, that a practitioner had with this result practised variolous inoculation on 100 persons, 50 of whom had never been vaccinated, and the other 50 had been vaccinated, then to each member of the unvaccinated group he would say,—"This pustule on your arm is the true small-pox—you have been successfully infected, and are now safe against the disease;" while to each of the vaccinated 50 he would say,—"This pustule on your arm is no evidence whatever of small-pox; I have been unable to infect you, because you have already been vaccinated." Not only would each medical man say this to his patients—he would also say so to his medical *confrères*, and his medical *confrères* would with equal solemnity say so to him—they had also experimented, and with a similar happy result. Indeed, besides seriously telling his patient and his professional brother this contradictory tale, he would with equal sincerity repeat the story to himself, and he himself would

[1] *The Present Method of Inoculating for the Small-pox*, 7th ed., 1779, pp. 23–25.

swallow it as easily as his patient and his colleague had done! And not only would this happen in London, where Jenner's treatise was published; it would happen wherever the inoculation test was tried,—all over England, and all over Europe, and in India and America, and throughout the world.

This in effect is the theory that Dr Creighton sets up to explain away the variolous test. Do my readers think it incredible? To me it is incredible that they should think it anything else. Nevertheless it is so. Here is Creighton's summing up:—

"We come, then, to this extraordinary result, that the very same degree of small-pox infection, namely, the local pustule alone, or the local pustule followed by an abortive fever and a few abortive pimples, which had come to be reckoned a sufficient manifestation of the disease when inoculation was an end in itself, was now reckoned an insufficient manifestation, and, in fact, an evidence that the infection had not taken at all, when inoculation was done after cow-poxing and with a view to test the alleged antagonistic power of the latter against small-pox."—(Pp. 146, 147.)

It is clear that the Doctor himself sees the halting nature of the thesis. Very oddly does he strive to make it pass muster, however, trying to turn even its weakness into strength. He says,—"I am aware of the *gravity* [the italics are mine] of that accusation against the common intelligence and moral prudence of the medical profession." The assertion that the charge is a "grave" one is a stroke of real genius. Its characteristic is not gravity, but absurdity.

In developing his argument, Dr Creighton, as usual, falls foul of Jenner. He desired not only to mislead the profession by the name Variolæ vaccinæ, but to guide it craftily into a method of testing which would be itself a fraud. He quotes Jenner's statement in the *Inquiry*, that "In some of the preceding cases I have noticed the attention that was paid to the state of the variolous matter previous to the experiment of inserting it into the arms of those who had gone through the cow-pox," and he italicises Jenner's words, "*This I conceived to be of the greatest importance in conducting these experiments.*"

Then he goes on to say--

"It is only in one of the 'preceding cases,' not in 'some,' that any notice is taken of the point; but that notice is quite significant enough of what this super-subtle genius wanted to hint to his readers. Case III.—John Philips, a cow-poxed milker, aged 62, was tested with small-pox, the matter having been 'taken from the arm of a boy just before the commencement of the eruptive fever.' Just so, the variolous test was applied in the most mitigated form of Gatti's and Sutton's 'new method;' the matter for inoculation was taken from the local pustule of a previous case of inoculation, not from a general eruption of natural small-pox; it was taken at a very early stage, before it had undergone the supposed 'putrefactive' change which made it spurious; and it was inserted, not by a deep incision, but by a superficial puncture, as well as in small quantity."

Part of this, it need hardly be said, is mere assumption. Jenner does not say that he made a puncture (like Gatti) instead of an

incision (like Dimsdale), nor that he introduced only a small quantity of matter. But, in the first place, I must amplify a little the quotation from Jenner. Here it is,—"*I inoculated him, and was very careful in selecting matter in its most active state.* It was taken from the arm of a boy just before the commencement of the eruptive fever, *and instantly inserted.*" The words which I have italicised are those which Jenner's critic did not think it necessary to include. Dr Creighton's accusation, therefore, comes to this—that Jenner had the audacity, or rather the folly, to describe as "matter in its most active state" that which the medical men to whom he was writing (and who were thoroughly acquainted with everything relating to inoculation) were well aware would produce variolation only "in the most mitigated form." But surely even Dr Creighton will not deny that limpid matter should be used alike for variolation and vaccination. Indeed, Jenner's advice as to the latter is almost exactly similar. "The limpid fluid should be taken for the purpose of [cow-pox] inoculation, as soon as the vesicle appears sufficiently prominent for that purpose."[1] Then, in regard to the matter being taken "from the local pustule of a previous inoculation," Geo. Lipscomb, to whom Dr Creighton refers as a well-known inoculator, deliberately aiming at getting a "mere formality of small-pox," says that matter should be taken "either from the inoculated part, or, what is preferable in the opinion of the most experienced, from the natural small-pox pustule." And Dimsdale[2] says, "It seems of no consequence whether the infecting matter be taken from the natural or inoculated small-pox as soon as any fluid can be obtained from it." Thus there was no advantage, in the way of mildness, to be got by using the local pustule, nor any depth of craft on Jenner's part in selecting it for Case III. Nor is Dr Creighton correct in asserting that it was only in this case, and not in "some" that Jenner had referred to the state of the matter used. In Case V. he says, "I inoculated her with active variolous matter." Case XX. was inoculated "with variolous matter immediately taken from a pustule." Case XXIII. "was inoculated with variolous matter from a fresh pustule," and "to convince myself that the variolous matter made use of was in a perfect state, I at the same time inoculated a patient with some of it who never had gone through the cow-pox, and it produced the small-pox in the usual regular manner." In the course of his references to the subject, Jenner argues as follows:—(1.) That variolous matter for inoculation should be taken early (and therefore in a limpid state), in order to get it not only in a condition of variolous activity, but also free from the risks incident to the insertion of pus; (2.) That it should not be preserved on cloth, corked up in a vial and carried in the warm pocket, so as to induce putrefaction, but, as he says in the

[1] *Medical and Physical Journal*, vol. vi. p. 64.
[2] *The Present Method of Inoculation*, 7th ed., p. 26.

words omitted by Dr Creighton, should be "instantly inserted;" (3.) That in performing the operation the adipose tissue should not be wounded; and (4.) That matter should not be inserted by lodging a thread, dipped in the virus, in the part operated on.

Will any medical man excepting Dr Creighton see in these precautions any attempt to falsify the variolous test, or anything which does not meet with his heartiest approval? Indeed, Jenner's correctness of view as to the practice of vaccination itself is nowhere better displayed than in connexion with the parallel subject of the choice of lymph. Dr Creighton has great admiration for Woodville, and some even for Pearson, as compared with Jenner. But at a time when Jenner was insisting on "a rule never to inoculate [vaccinate] with matter after the eighth or ninth day," as stated in G. C. Jenner's evidence before the House of Commons' Committee, Woodville declared that "he had never been able to discover any difference" between the effects of "vaccine matter taken on the eighth, ninth, tenth, or eleventh day,"[1] and Pearson held that "no difference is perceived between the effects of matter taken before the red areola appears and that taken when it is distinctly formed, notwithstanding the 'golden rule' [Jenner's] that has been laid down, never to use matter when such areola is distinctly formed."

Even, however, if Dr Creighton's opinion of Jenner's intention were correct, the question would remain, Were the means adopted at all likely to carry out the intention? Is it conceivable that medical men, having hunted through Jenner's book, as Dr Creighton has done, in search of the "preceding cases" which contained his references to the state of the variolous matter, and having found them, or rather having found the particular case which Dr Creighton says is the only case, should thereby be guided into the application of a fallacious test to the new inoculation? To a country practitioner advocating the prophylactic properties of a cow disease, of which they knew nothing, they might indeed listen. But were they likely to pay heed to the same man when he asked them not merely to reverse their views as to what constituted successful variolous inoculation, of which they conceived they knew everything, but to do so for the very purpose of using the reversed doctrine as a standard by which to judge of the success or failure of the Jennerian method of preventing small-pox?

Surely the "super-subtlety" is not with Jenner, but with his latter-day critic.

There is indeed no mystery about Jenner's observations on the proper manner of performing variolous inoculation. He knew that at a late stage of the pock, pus might be inoculated as well as small-pox, and he had reason to think that preservation of the matter for days at the temperature of the human body, as in the pocket of the inoculator, might have a deleterious effect. What he desired was that his new practice, which he hoped would be

[1] *Pearson's Examination of the Committee's Report*, pp. 91–93.

"essentially beneficial to mankind," should be tested by the subsequent insertion of variolous virus alone, which would have every chance of producing small-pox in an unprotected person, but would have no effect on a system rendered incapable of receiving it. He did not want vaccination, by which his reputation was to stand or fall, to be subjected to a fallacious test, as by the inoculation of the products of purulent decomposition, which might, even in a person proof against small-pox, set up symptoms capable of being confused with some of those resulting from successful variolation. His statement was that due "attention to the state of the variolous matter previous to the experiment of inserting it into the arms of those who had gone through the cow-pox . . . . . might prevent much subsequent mischief and confusion." His attitude needs no justification, for his request simply was that his new project should have a fair field and no favour.

This, however, is not Dr Creighton's whole argument on the variolous test, and it will be necessary to follow him into some further details. He complains that the *Inquiry* contained only three or four cases in which the variolous test had been applied after vaccination. But, in addition, Jenner mentions about twenty cases to which the test had been applied sooner or later after accidental cow-pox. Then Dr Creighton takes up and animadverts on Jenner's first case of vaccination from a human subject. James Phipps was vaccinated in 1796, and subsequently tested by variolation, which "produced no effect." Our author's remarks on this are as follow: "Poor Phipps, as Jenner used to call him, was inoculated some twenty times after that, and never 'took;' he was Jenner's show case of resistance to small-pox; he was a poor consumptive or scrofulous youth, with his lymphatic glands so clogged (after the cow-pox ?) that any subsequent inoculation of virus on the arm had no chance of being absorbed" (p. 126). And (p. 149) he speaks of "scrofulous children with clogged absorbent glands," of whom "his own show case James Phipps was a good instance." This statement as to Phipps seemed to me singularly at variance with Jenner's description of him in his *Inquiry* (p. 32) as "a healthy boy, 8 years old," and I therefore turned with interest to the reference given by Dr Creighton, "Baron, ii. 304." Here is the passage, which is detailing the incidents of a country drive: "A short time afterwards we passed Phipps, his first vaccinated patient. 'Oh, there is poor Phipps,' he exclaimed, 'I wish you could see him; he has been very unwell lately, and I am afraid he has got tubercles in the lungs. He was recently inoculated for small-pox, I believe for the twentieth time, and all without effect.'" It is not possible accurately to get the date of this incident; but the narrative immediately proceeds to mention "a subsequent visit" in October 1818, and the occurrence in question could hardly have been very long previous—not many years previous, at any rate. Now, in 1818 Phipps was a man 30 years old, and we may safely take it

that he had reached the age of manhood at the time when Jenner feared he had got tubercles.

What Dr Creighton has done, therefore, is this:—(1), To absolutely ignore Jenner's statement that Phipps was a healthy boy; (2), to alter Jenner's *fear* of tubercle into the positive statement, "he was a poor consumptive or scrofulous youth;" (3), to transfer the illness from (almost certainly) manhood to childhood; (4), to introduce the entirely new statement that the axillary glands were diseased; (5), to suggest that this (imaginary) disease of the glands was due to vaccination at the age of 8 years; (6), to assert that it was so permanent as to make successful small-pox inoculation impossible, even when attempted for the twentieth time; and (7), to buttress the whole story by stating that Jenner was in the habit of speaking of "poor Phipps," whereas there is no evidence that he ever employed the expression except on the occasion narrated.

Comment is needless. But I may be allowed to point out that it is Dr Creighton who complains (pp. 107, 108) that "the suspicion of having been edited"—by Jenner himself—attaches to much of Jenner's writings.

The reference to clogged glands leads us to another argument, namely, "That the vaccine infection itself caused a swelling and obstruction of the absorbent glands in the armpit and neck, and to that extent made them incapable for the time, and in some cases for long after, of taking up and passing into the lymphatic circulation another virus inoculated under the skin at the same place" (p. 149).

This, of course, finds its reply in the passage quoted above from Dimsdale (whose instructions, Woodville tells us, were followed in Jenner's day), that small-pox inoculation "is generally performed in both arms." Even in 1806, Lipscomb speaks (in condemnation) of what he describes as "the practice" in question. But even if only one arm was sometimes inoculated, then, to give Dr Creighton's argument any weight, it is necessary to assume that the operator fatuously selected for his test that one which had newly been vaccinated. The question of how long any such barrier would exist need not detain us. Neither need we delay over the unimportant contentions (1), that as cow-pox was often "a discharging sore," it would prevent for the time successful variolation; and (2), that the vaccinal exanthem, "which was a frequent incident of the early days of vaccination," would have a similar effect. But I may note in passing that the latter point furnishes Dr Creighton with the opportunity of falling into another blunder, in the course of his curiously unsuccessful endeavours to "authenticate the facts by full references."[1]

He says that in Woodville's practice at the Inoculation Hospital, the vaccinal exanthem got mixed up with the true small-pox eruption, which many of the patients had. "But it was often

[1] Preface to *Jenner and Vaccination.*

observed in the country practice of vaccination, where concurrent small-pox was out of the question. Thus, of seventy cases vaccinated by Evans of Ketley, near Shiffnal, commencing in May 1799, no fewer than thirty-nine had an eruption." But on verifying the reference, what do we find? That there was an abundance of "concurrent small-pox;" for Evans says, "At the same time I inoculated fifty patients with variolous matter, that I might have an opportunity of observing the different effects of the two diseases. And whenever I had it in my power, I inoculated one part of a family with vaccine and the other part with variolous virus" (*Med. Phys. Jour.*, vol. ii. p. 312).

Dr Creighton further gives a series of cases to show that even where small-pox was successfully inoculated on vaccinated subjects, the profession was so blinded for the time that the very man who performed the inoculations which proved the uselessness of vaccination "congratulated mankind" on the success of cow-pox. The cases are a series of fourteen, published by M. Ward, Surgeon to the Manchester Infirmary. They are given individually by Dr Creighton (pp. 130, 131, *Jenner and Vaccination*), and having given them, he comments on them thus:—

"Ward was highly pleased with this record of the variolous test. What are we to think of the temper of the profession at this time, when a respectable practitioner congratulates the world upon a great discovery, with failure staring him in the face from the record of his own experience? Only one of all his cases resisted variolation after being cow-poxed, namely Case VIII.; three cases took small-pox in the clearest way after being cow-poxed (Nos. I., II., and IX.); four cases resisted vaccination, and likewise resisted variolation; one case resisted vaccination, and received the subsequent variolous infection; and two cases were apparently variolated in the first instance by misadventure."

Dr Creighton's notes of these cases are so much at variance with what seem to me to be the facts, that I must trouble the reader to follow the details of at least the first case, as condensed from Ward's paper in the *Medical and Physical Journal*. It was that of a girl, aged 7, whom Ward operated on on 16th April. She had a very serious illness. On the 4th day the punctures considerably inflamed, one discharging slightly. 8th day—Efflorescence round punctures lessened. 12th day—Feverish from the morning; had a restless night. 13th day—One puncture healed, the other consisting of an oblong vesicle with limpid contents and increased efflorescence; breath offensive, tongue white, anorexia, eyes dull and heavy. 14th day, 12 noon—Some spots appeared on face, "which relieved her." 15th day—Plentiful crop on every part of body. 16th day—Pustules on tonsils and mucous membrane of throat; eruption confluent in some places on face and arms. 17th day—Rather hoarse. 18th—Very weak. 19th—"Pustules become more confluent daily;" several on tongue; eyes nearly closed. 20th—Maturation nearly complete; eyelids closed; inoculated arm loaded with pustules; puncture covered by small

dry scab. 21st day—Somewhat better; throat well. The improvement continued irregularly, till on the 31st day "her health is restored." That was on 17th May. Then on 4th July she was "inoculated with variolous matter," and on 7th July it is noted, "she has not taken the infection." He adds, in reference to the eruption above mentioned, "As nearly as I can ascertain, she had not fewer than from 1600 to 1800 pustules."

That is Ward's statement; here is Dr Creighton's—

"CASE I.—16*th April*—Girl, aged 7, successful vaccination (oblong vesicle on 13th day, full of limpid fluid and surrounded by areola); was thereafter inoculated with small-pox, and had the disease in the confluent form (1600 to 1800 pustules)."

Thus Ward tells us "she has not taken the infection," while Dr Creighton says she "had the disease in the confluent form."

Ward's statement does not seem difficult of interpretation. He was deceived as to the nature of his first inoculation. He had used small-pox matter, not cow-pox matter, and the result was a somewhat delayed attack of confluent small-pox; therefore the second inoculation failed. But Dr Creighton says the girl was successfully vaccinated, and then she was variolated, and to make the history complete, he transfers the 1600 or 1800 pustules from the first to the second inoculation, though Ward tells him the latter failed. That he has got fairly lost as to the sequence of events is shown by his statement that "two cases were apparently variolated in the first instance by misadventure." But these two cases were X. and XI., inoculated on 30th April (the 14th day) from Case I. How possibly could they be *variolated* from the "oblong vesicle" of a "successful *vaccination*"? Thus, after all, Dr Creighton seems at one moment to recognise the fact that the original infection was small-pox, and at the next moment he forgets all about this, and speaks of it as cow-pox. Indeed, in his notes on Case X., he speaks of it as "inoculated from Case I., evidently with the co-existent small-pox matter mistaken for cow-pox." It is not worth spending time over the other thirteen cases. Cases II. to VIII. were inoculated from the same source as Case I., and the other five were inoculated from Case I. There was indeed no vaccination in the business. Ward, who wrote in July 1799, had doubtless been misled into expecting pustules after cow-pox by the publication, in May of the same year, of Woodville's experience in the Small-pox Inoculation Hospital, where, as Jenner afterwards pointed out, he had for the time confused the two diseases.

Blind as Ward was to the nature of his cases, he does not deserve the further misreading to which his critic subjects him. Dr Creighton says Ward "sent the following series of cases, and 'congratulated mankind' on the success of cow-pox." As a matter of fact, Ward's congratulations are not based on his own experience, but on the opinions which he gives from Woodville, "that those persons who have had this disease by inoculation are thereby

secured from having the small-pox," and "that the inoculated cow-pox is seldom accompanied by pustules" if the virus be taken from mild cases. He then says, "Considering the above observations as established facts, and should it also appear that the cow-pox is not so liable to be propagated by contagion as the small-pox, may we not indulge a hope that the era is probably not far distant when we shall be able to congratulate mankind at large on their having a fair prospect of being exempted at no very remote period from that most destructive malady." But Dr Creighton's reader is led to believe that Ward's congratulations of mankind are based on his own experiments.

Regarding another series of cases, which occurred at Stroud, Dr Creighton says,—"The Stroud doctors put all their ten cases religiously through the test, with the singular result that the only one of the ten, an adult, whose vaccination had not held, was the only one who stood the test, while the other nine all had the small-pox in one degree or another, in the usual inoculated form—the two who had the worst vaccinal ulcers having stood the test rather better than the others" (p. 127).

Dr Hughes, of Stroud, along with Mr Darke, had to do with five of these cases, and Dr Thornton with the other five. The first-named gives a fairly full account of what occurred. In T. V. there was a slight inflammation the day following the vaccination. This diminished, and on the sixth day hardly any appearance of the puncture remained but a small scab; in the evening a slight redness came on, with a small pimple or two near the puncture, which disappeared next day. Cases II. and III. were practically similar. Dr Creighton accepts this as successful vaccination, and the subsequent successful variolation of the same cases is part of his argument in the above quotation. Dr Hughes thought differently. He speaks incidentally of Cases II. and III., "who did not take the infection of the cow-pox." The other two cases, a child 2 years old and a servant, W. K., 15 years old, reported on by Hughes, had very considerable local symptoms. The child was decidedly the worse of the two. They had been reported by Thornton to Dr Beddoes of Bristol, who first published the cases, as "alarming and dreadful." But Hughes says No—the more severe case was only "troublesome and disagreeable." Hughes, moreover, gives the details, which Dr Creighton condenses, chiefly by omitting the favourable points (as that "from this time the induration lessened," and that "K.'s arm was so little troublesome that he went on with the usual business of the house the whole time"), and in conclusion he accepts the description which Hughes had rejected, that both cases had "alarming" and "dreadful" symptoms. And finally, as to the variolous inoculation, Hughes states that,—"In both, the punctures in the right arm inflamed earlier than they usually do after a first inoculation (as is common where a second inoculation is made so soon after the first, where variolous matter

is used): they became small pustules, without any previous accompanying illness, without any affection of the axilla or subsequent pustules." Thus Hughes says in effect that the variolous inoculation failed, while Dr Creighton is of the opposite opinion. In regard to the symptom spoken of by Hughes—the early appearance of inflammation after variolation—I may mention that the literature of the period shows it to have been an accepted evidence that the operation would not succeed.

In connexion with this letter from Hughes, Dr Creighton makes another attack on Jenner. Hughes sent his letter dated—

"9th May 1799 to Jenner, who forwarded it to the *Medical and Physical Journal*, with the explanation that it had arrived too late for him to include in his second pamphlet. But he had already been told the main facts by Darke; and in the second pamphlet he had deliberately omitted all reference to them, merely stating that 'Mr D., a neighbouring surgeon,' had taken some matter from the arm of the child on 13th December. The trial, however, had made some noise in Stroud, Gloucester, and Bristol, and it would have been too risky for Jenner to have suppressed the second and fuller relation of facts by Hughes" (p. 95).

The preface to *Further Observations* is dated April 5th, 1779, and the work is reviewed in the June number of the *Journal*, which also contains Hughes's letter, as Dr Creighton mentions, of May 9th, 1799. Clearly, therefore, Jenner is likely to have been telling the truth in saying that it had arrived "too late for insertion in his work." Indeed, Hughes seems to have expected this, for he writes to Jenner, "You are at liberty to insert it in your next publication, or transmit it to the editors of the *Medical and Physical Journal*." But why it should be supposed that Jenner desired to suppress a communication which, as we have seen, amounted to a defence of cow-pox against published assertions as to its "alarming and dreadful" results, probably no one but Dr Creighton knows.

In the course of his chapter on the variolous test, Dr Creighton naturally endeavours to lessen as much as possible the importance due to the very extensive way in which the test was applied. He says, "The total of some two thousand successful English tests, which got extensively quoted abroad and helped greatly to recommend the new practice, was made up of Woodville's hundreds, of Marshall's two hundred odd, of Pearson's scores of cases, and of other large aggregates for which the details were never given," and that where scrutiny of the event is possible, "in most cases a full and correct variolous pustule" resulted; and further, that the test "was not applied at all generally in England after the first weeks of cow-poxing in the spring and summer of 1799." These statements are not borne out by the facts.

In the first place, Woodville's cases, which in 1799 were hundreds, ultimately amounted to thousands. To the Committee of the House of Commons he stated that, up to 1st June 1802, there had been done in the Hospital "7500; about one-half of which was since inoculated with small-pox matter, *in none of whom did*

the small-pox produce any effect."[1] The italics are his own. By this time he seems to have got over his initial difficulty, for in answer to a question about "pustules like small-pox" following vaccination, he says, "I believe they never do over the whole body; I have seen in some instances a few pustules in the neighbourhood of the inoculated part, but these instances are very rare, one in five hundred."

To show how the test was applied, I may extract from the half-yearly volumes of the *Medical and Physical Journal* a few jottings referring, like most of Woodville's cases, to dates subsequent to "the summer of 1799:"—

(1.) *Jan.* 31*st*, 1800.—Mr Stewart, surgeon, Plymouth, reports three cases. Case I.—*Nov.* 1*st*, 1799.—"Active variolous matter" inserted on forty-sixth day after vaccination, followed by inflammation, which at first suggested small-pox, but disappeared in five days. No indisposition nor pustules. Case II.—On the twenty-second day "inoculated with variolous matter without effect," and on the thirty-fifth day "limpid variolous matter was inserted into both arms, but no disease ensued." Case III.—On eighteenth day "inoculated with active variolous matter," and (twenty-fourth day) "she has not taken the infection."—(Vol. iii. p. 236.)

(2.) *March* 11*th*, 1800.—Rev. W. Finch, on 25th and 26th February, "inoculated with variolous matter twenty children," who had been vaccinated in November and in the beginning of December. "All entirely resisted its infection." Inflammation began on the second day, and began to disappear in three to five days In two cases a small-pock resulted, similar to "what nurses who have had the small-pox frequently experience."—(Vol. iii. p. 419.)

(3.) Writing on May 19th, 1800, Mr T. M. Kelson, Seven Oaks, records forty cases in a workhouse, "inoculated with the most virulent matter I could procure, but nothing ensued, except local superficial inflammation for the first six or seven days. I then introduced a wretched family, just recovered from very bad small-pox, their dirty clothes unchanged, and divided them in different beds among them, but to no purpose."—(Vol. iv. p. 23.)

(4.) Mr William Fermor, in his "Reflections on the Cow-pox," reviewed in the *Medical and Physical Journal* of September 1800, gives a list of 173 persons who were (after vaccination) inoculated with small-pox "without receiving the infection in any instance." Some of these, however, may have been previous to the summer of 1799.—(Vol. iv. p. 260.)

(5.) Mr Custance of Kidderminster, in September 1800, inoculated for small-pox Mrs W. and two of her children, who had been vaccinated by Dr Jenner himself fourteen months before, and subsequently tested, without result, by variolation. Custance's inoculation "totally failed in all three," though an unvaccinated infant, "who was inoculated with the same matter, sickened, and had an unusual number of pustules."—(Vol. iv. p. 421.)

(6.) The Parisian Vaccination Committee record that "more than seventy-two have been inoculated for the small-pox, yet none have taken the infection."—(Vol. v. p. 357.) That in certain cases there was a slight local effect is seen in the following extract from an earlier report (p. 101). The inoculation was done three months after the vaccination.

"The nineteen subjects submitted to the operation have been inoculated with fresh pus, taken every time from a variolous infant who was present. The Committee, for the purpose of rendering their experiments more decisive, employed in many of the subjects very deep incisions, such as, according to the inoculators, necessarily occasion a large eruption of pustules. They even proceeded so far as to introduce at different times a great quantity of variolous

[1] *The Evidence at Large*, etc., by the Rev. G. C. Jenner. London, 1805.

matter into the incisions, notwithstanding which, not one of nineteen who were inoculated had anything like a general eruption. In fourteen, the incisions were soon obliterated, without any symptom of complaint. In the remaining five, the inflammation can be considered in no other light than as the effect of local irritation, produced by the puncture of the skin. The inflammation began the very day of the insertion. This process has been more rapid and less regular than that of ordinary inoculation. Besides, instances of the same effect occur in persons who, after having had the small-pox, have again submitted to inoculation. In a word, if some preservative effect did not operate by the vaccine inoculation in those who have submitted to it, how should it happen that the variolous matter introduced into their incisions for the inoculation of the small-pox should have excited (in some at least) only a local and partial affection, whilst taken from this source to be transmitted to infants not before inoculated with the cow-pox, it has produced in the latter all the usual symptoms of general infection ?"

(7.) "Cit. Ané, Paris, the well-known inoculator, in the presence of many medical practitioners," inoculated in three places each seven children vaccinated three or four months previously by Colon. The small-pox virus was taken from a child labouring "under a copious eruption of the natural small-pox." "Not one took the small-pox."—(Vol. v. p. 556.)

(8.) Mr Clement of Shrewsbury, who was "prejudiced against the vaccine inoculation," vaccinated three children in the autumn of 1799. Early in the following spring he tested the children "both by inoculation and exposure" during the prevalence of small-pox, and "without the least effect, excepting local inflammation."—(Vol. vi. p. 5.)

(11.) Lord Berkeley reports two cases in which the "inoculated part looked red and angry, as if it would fester, for a few days, but then died away, without producing any effect."—(Vol. vi. p. 105.)

(12.) Dr Cogan communicates a letter received from several medical men in Rotterdam. "In twenty cases variolous inoculation was superadded without producing the disease. In all these the wounds were inflamed on the first or second day, on the third day they were more inflamed and swollen ; and, in some instances, to such a degree that we began to suspect a general infection would succeed ; but these were, on the fourth day, less inflamed and swollen, and they disappeared on the fifth."—(Vol. vii. p. 251.)

(13.) Mr Thomas, Pimlico, tested sixty cases, "and never was able, in any one instance, to produce the least symptom of small-pox."—(Vol. viii. p. 168.)

In the above cases I have inserted such notes regarding local inflammation, etc., as are in part depended on by Dr Creighton to support his view that the small-pox inoculation had succeeded as well as it could do under the mild method of inoculation. But it will be seen that the method was not always mild ; that there was not "in most cases a full and correct variolous pustule," and that the operators, even when led by the first appearances to look for variolous infection, had no hesitation in concluding that it had not occurred. And Willan's treatise "On Vaccine Inoculation," published in 1806, contains ample proof of the extensive way in which the test was applied. Besides making special mention of several series of tests—embracing in one case 35 persons, in another 50, and in another 60—he states that he himself had witnessed 180 such inoculations, and he gives a plate of the "greatest effect" produced. He gives also a list of medical men whose combined vaccinations amounted to over 18,000 cases, none of which had been subsequently affected by small-pox, "though most of them

were afterwards either inoculated with variolous matter or exposed at different periods to contagion." It need hardly be insisted on that no such body of evidence could have come into existence had vaccination been nothing more than "a grotesque superstition."

(5) We come now to the very heart and soul of Dr Creighton's doctrine—to his views on the true nature of cow-pox, and its relationship to syphilis. The subject is dealt with mainly in his work, *Cow-pox and Vaccinal Syphilis.* His opinions are rather difficult to collect from among the rhetorical passages of his essays, but the following summary expresses what we learn concerning them.

Cow-pox he holds to be an entirely artificial disorder. It has its origin in an eruption on the teats and udder of hard pimples, which, if left to themselves, "maturate" to a very limited extent. These pimples, even maturated in a natural way, do not, he urges, constitute cow-pox. "Their 'natural' development, in so far as it can come into the pathology of infective disease, does not exist, for the reason that the pimples on the cow's teats, if they were saved from the 'merciless manipulations of the milkers' [the words are Ceely's], would simply run the course of pimples, and would never become pox; it is the perpetual 'insult' of an ailing part, the forcible traction on the pimply skin three times a day, the creation of hæmorrhagic crusts, and the ever-renewed displacement of these, that now and then sets up the inveterate and communicable process which we know as cow-pox."

"Its characters are deep or spreading ulceration (sometimes phagædenic to a degree that destroys half the udder), with slow healing, induration of the base and roundness of the edges, and a deep permanent scar, often smooth and regular, but not rarely puckered and irregular, such as follows any ulcerative destruction through the whole thickness of a vascular and almost erectile skin."

The parallelism of the disease thus described with syphilis he endeavours to make out by a reference to Ricord's plates of experimental chancres, and by the narrative of one of Ricord's cases as given by an eye-witness. On the 6th day there was a little pimple at each inoculated spot, on the 7th day the pimples had become vesicular, on the 10th crusts began to form, on the 15th ichor oozed from beneath the crusts, on the 22nd this had become purulent, and on the 29th putrid. On the 30th the crust separated, revealing an ulcer, with raised hard edges, and ultimately a cicatrix formed. Mr Henry Lee's plates (*Med. Chir. Trans.*, 1861) are said to show the same characters, and the ulcers above described "are exactly parallel to the Stroud inoculations with cow-pox" to which I have already referred. Jenner's and Ceely's descriptions of the sores on the milkers' hands, got by accidental infection from the cow's teats, are also brought in to show the resemblance to syphilitic sores. In a case of Ceely's, the

patient stated that about ten days after discovering the disease on the cows he observed two pimples on his hands, followed by axillary pain and tenderness, constitutional symptoms, the pimples ending in ulcers with a central slough, the bases being surrounded by a red elevated induration. Such sores may cause accidental auto-inoculation of other parts of the body. Occasionally, too, the local sores are accompanied by "papular, vesicular, and bullous eruptions." Then we have a description of Ceely's vaccinations direct from the cow. In them, fluid remained in the margin of the vesicle till about the 16th or 18th day, the crust being often retained till the 4th and 5th week, leaving a deep cicatrix, or a yellow foul excavation. Often the vesicles burst, and in bad subjects secondary inflammation followed, with perhaps cellular and axillary abscesses, etc. All this soon subsided, however, leaving an unexpectedly small cicatrix. In succeeding removes from the cow very remarkable improvement took place, even in three or four removes. The facts as to the important Wurtemberg collective investigation on cow-pox, reported by Hering in 1839, do not easily fall in with Dr Creighton's views, Hering having no corroding nor phagædenic ulcers in using primary lymph. Dr Creighton tries to take the edge off Hering's statement by suggesting that his inquiries were biassed, and his reports incomplete. Bousquet's account of the Passy cow (1836), and Estlin's Gloucestershire practice,[1] originating with 11th day lymph from a girl "domestically inoculated from a milker's vesicle," are also adduced in support of Dr Creighton's doctrine.

Having considered these accounts of the results of cow-pox in its early removes from its source, the doctor goes on to discuss "humanised cow-pox and its anomalies." And here we find him guided by his views on unconscious memory. The effects of vaccine in every-day practice "are a more or less remote reproduction of the natural history of cow-pox in the cow, of accidental cow-pox in the milker, and of the infection set up by primary lymph experimentally in the child." The cycle of events is shortened, the ulcerous termination being left out, and repair is earlier completed, the resulting scar being also modified. The experiments with primary lymph are the key to "so-called anomalies and complications" of vaccination. "However far the vaccine may travel from its source, it can but 'drag a lengthening chain.'" Vaccine roseola, as occasionally seen, "is really the secondary exanthematic effect" of the vaccine fever, and vaccinal erysipelas is simply an exaggeration of the normal areola, and is therefore not to be guarded against as if it were due "to foul lancets or extraneous infection," as to which it is said "we need have no hesitation in dismissing the theory."

Some causes which determine the return to the original character of cow-pox are referred to incidentally. The chief appears

[1] *London Medical Gazette*, 1838-39.

to be taking lymph after the areola has appeared. Another is the use of backward or retarded vesicles furnishing a scanty supply. A third is the existence of a large number of vesicles on the arm of the vaccinifer (p. 140), and a fourth, "the drainage of lymph to the last drop." In these we see points of contact with the orthodox belief. The facts, of course, have been long known, and Dr Creighton accepts and adopts them by saying that we are here dealing with causes which rouse to activity the dormant memory. The retardation of vesicles is equivalent to a prolongation of an early stage, and belongs to a reversion to the long cycle of untamed cow-pox. It is not so clear, however, why draining a vesicle should stimulate this memory. And as for the appearance of an areola on or before the 8th day—that is surely a quickening of the process—it is too soon, not too late. Why then should it also revive the recollection of a lengthened cycle?

I need not here wait to dilate on what every medical man must see at a glance—how these opinions differ from those ordinarily held on this subject; but I may note in this connexion what Dr Creighton nowhere refers to, even in his most recent writings, namely, Dr Buist's valuable cultivation experiments, and the reasonable basis they afford to the recognised practice of using only limpid 8th day lymph from vesicles without an areola.

Last of all, we come to the so-called vaccinal syphilis. Dr Creighton does not believe that true syphilis can be implanted from a vaccine vesicle—not even, as I have already mentioned, in the well-known case of the public vaccinator, who after two or three failures, produced the ordinary symptoms of primary, secondary, and tertiary disease by vaccinating himself from an obviously infected child. In this and in all other cases the effects produced were not, we are told, due to venereal syphilis at all. They were merely the manifestations of cow-pox returning to its original characters on the cow's teats and the milker's hands. They were examples of "unconscious memory." In this connexion our author quotes, apparently with approval, from some of the early anti-vaccinators,—from Moseley, for instance, who looked on the symptoms of diseases following cow-pox as "totally new, and differing in every particular from established nosological definition;" and from Birch, who "saw new and anomalous eruptions following this disease, eruptions which in the whole course of his former practice he had never met with." In passing, I may observe that these statements give about as flat contradiction as could be conceived of, to Dr Creighton's view that the appearances in question were on all fours with those due to so common a disease as syphilis.

Bohn's statement is also noted, that "the origin of the syphilis that occurs as a sequel of vaccination is shrouded in mystery," and Dr Creighton urges that "inculpated vaccine matter could hardly ever be traced to a syphilitic constitution in the vaccinifer." Between "the common 'vaccinal ulcer'" and those cases in

which syphilitic contamination is alleged, "the distinction," he says, "is arbitrary." In short, he claims "the phenomena of so-called vaccinal 'syphilis' as in no respect of venereal origin, but as due to the inherent, although mostly dormant, *natural history characters of cow-pox itself*," and in this belief he makes a detailed examination of the various outbreaks of alleged vaccinal syphilis that have occurred since the first reported epidemic at Udine in Italy in 1814. The value of this theory to the professed opponents of vaccination is manifest. It forms a much more useful weapon than does the assertion that syphilitic virus may be inoculated along with vaccine lymph. The latter danger is, to begin with, contingent on the presence of syphilis in possible vaccinifers, while the former is inherent in every drop of lymph that is used for vaccination.

In attempting to review Dr Creighton's doctrine, I need hardly say that I do not pretend to speak as a professed pathologist. The criticism which I have to offer is mainly suggested by a careful reading of his own writings, and by references to the original authorities on whom he founds.

The first thing to strike the reader is the curious view taken regarding the artificial nature of cow-pox. The natural disease is said to consist merely of a few unimportant and non-infective pimples. Starting with these on the udder or teats, the milker may in a few days create a malady which is infectious from cow to cow, from cow to milker, from milker to child, and from one child to another, through generation after generation, for an unlimited length of time. As the malady gets further removed from its source, its duration becomes shorter and its characters milder, until it gets to the standard of ordinary vaccination, at which point, as a rule, it remains stationary. But after any number of generations, the lymph of any vesicle if taken on the ninth or tenth day, or after the areola has formed, may perform an act of unconscious memory, resulting in the production on the arm of the inoculated person of an exact picture of the milker's sore, from which possibly half a century before it had been evolved. This memory, once revived, is not so easily quieted. It may show itself in every one of thirty or fifty vaccinated children. And the sores thus produced may be followed by mucous tubercles, loss of hair, copper-coloured scars, and all the symptoms of constitutional syphilis, the parallel being further carried out in the fact that mothers suckling such children may also be infected with symptoms, primary or secondary, indistinguishable from those of venereal disease.

It is important to observe that the inveteracy produced by milking is the sole alleged cause of the infectivity, and that therefore the cow whose disorder forms the starting point of any outbreak must first itself have been the subject of the process in

question. Unintentionally, on its teats and udder, the milker has successfully performed the experiment of creating out of a few occasional pimples, a severe and specific contagious disease, capable of being maintained and cultivated by inoculation through all time.[1]

The evidences which must be produced in favour of the acceptance of a thesis so remarkable should well-nigh be overwhelming. Let us look at them, or for them.

Dr Creighton states that his views are founded on the accounts of cow-pox given by Ceely in his *Observations on the Variolæ Vaccinæ*, "for we are all alike dependent on Ceely's information." We must next, therefore, turn to Ceely (pp. 302–313, *op. cit.*)

By "natural" cow-pox, that author means the disease as found in the first case in any outbreak, and by "casual" cow-pox, he means the malady as found on cows, or on milkers infected by contact with a preceding case.

Regarding the "condition of the animal primarily affected," there is, Ceely says, through want of skilled observation, seldom any exact knowledge. In one observed instance, the cow "appeared out of condition; it had heat and tenderness of teats and udder as the first noticed signs." The other two were affected in about ten days. In another case "the only symptoms noticed were that the udder and teats were tumid, tender, and hot, just before the disease appeared." Such conditions, when observed by the milkers, continue for three or four days, and "are followed by irregularity and pimply hardness of these parts, especially about the bases of the teats and adjoining vicinity of the udder; these pimples, on skins not very dark, are of a red colour, and generally as large as a vetch or pea, and quite hard; in three or four days many of these have increased to the size of a horse bean, milking is generally very painful to the animal, the tumours rapidly increase in size and tenderness, and some appear to run into vesications on the teats, and are soon broken by their hands; milking now becomes troublesome and occasionally dangerous." When first seen by a skilled person, the cow originally attacked usually presents on the teats, vesications, some entire, others broken. Those broken have a central depression with marginal induration; those entire, on being punctured, effuse a more or less viscid, amber-coloured fluid. On the udder, especially near the base of the teats, and on the teats themselves, are dark crusts; some, in the latter locality, partially or wholly detached, leaving a raw surface and central slough. The crusts on the udder are circular or ovoid, acuminated or depressed; on the teats they are less perfect.

It is not, of course, necessary for me to prove a negative, but I confess that I look in vain through Ceely's writings for any sufficient foundation to Dr Creighton's doctrine.

In the *natural* disease it will be noted that on the udders, which of course are not subject to the milking process, the disease does not stop with the formation of a few pimples. On the contrary, though the details of the cases are hardly ever seen by competent

[1] To the obscurity of a footnote I relegate the really ridiculous inquiry of a friend looking over my shoulder, who asks—If Mrs Brown, or Jones, or Robinson were found to have unexpectedly developed symptoms of syphilis, would Mr Brown, or Jones, or Robinson be content with the explanation that some innocent acne pimples had been converted into chancres by "the merciless manipulations," etc.?

witnesses, there is ample evidence that the process goes on to the stage of crust formation—"dark brown or black uniform crusts . . . imbedded or surrounded with more or less indurated integument." The milking will unquestionably indirectly affect even the udders, especially close to the teats, but only very trivially as compared with its action on the teats themselves. And there is no proof that either on teats or udder the disease would in any circumstances have stopped short at the papular stage.

Reverting now to a consideration of the conditions under which the *casual* cow-pox, as above defined, comes to be witnessed, the character of the matter, which is often the vehicle for conveying it, will, from Ceely's description, be easily understood. In cows receiving the infection from an animal which itself has been already under the disease for two or three weeks, blood and pus, and samples of all the filth incident to a cow-house, are practically certain to be introduced in addition to any elements of definite contagious disease. Indeed, the reason for the disorder being entirely, or almost entirely, confined to milk cows, seems to be that it is propagated solely by contagion, and that the only likely contagion is by means of the milker's hands, so that the cows are infected not by a specific virus alone, but by various extraneous matters, including the products of inflammation, suppuration, and putrefaction. Dr Creighton has formed the belief that it is on the operation of milking, and on these various and varying foreign constituents, that the whole infective process depends, and that it is they, diverse as they are, that in all cases end in the evolution of a malady whose characters are, not severity but mildness, not variety but uniformity.

Ceely knew quite well that the milker's operations caused great inflammation and induration. He speaks both of "the general mildness of the disease," and of the fact "that its topical severity depends almost wholly on the rude traction of the milkers." But his interpretation of the facts is the reverse of Dr Creighton's: the mild disease was in his estimation the true cow-pox, and the topical severity was an accidental complication. With the very subjects of the disorder before his eyes, he never hints at any other notion.

Coming now to the details of the casual disease, which Ceely had a much better chance of following out on the cows, we find again that "there is rarely any manifestation of fever or constitutional disturbance." There is, of course, the usual variation of type depending on season and on individual idiosyncracy.

As to the topical symptoms Ceely notes—"It is very rarely that any indications of contagion, after undoubted exposure, are manifested before the sixth or seventh, sometimes not till the eighth or ninth day; but a vigilant observation of thin-skinned animals, with chaps and cracks on the teats, will exhibit small red, rather tender papulæ near the udder and on the body of the teats about the fifth day. . . . Between the tenth and eleventh days the

disease in general reaches its acmé. On the udders the tumours are often from eight to ten lines in their largest diameter, and in white skins the centres and central edges of the intumescent margin are of a deeper blue or slate colour, and the areola, which is usually of a pale rose colour, is seldom more than four or five lines in extent, under which the integuments are deeply indurated. Lymph, which two days before was difficult to procure from beneath the cuticle of the central depression of some tumours, is now so copious that it raises the cuticle, forming a globular or conoidal vesicle, or freely flows out from its rupture. . . . On the teats the few tumours which remain unbroken undergo similar changes, but appear to have less extent of areola, and less circumferential induration. . . . On and after the twelfth day, on the udder nearly all is passive ; . . . the marginal indurations . . . have nearly disappeared on the spontaneous separation of the crusts, which takes place on the twentieth or twenty-third day. . . . On the teats about this period—the twelfth day and onwards—around their base the tumours and vesicles which are left entire exhibit the like appearance ; . . . on other parts of the teats, out of the way of the milkers, and where the tumours or vesicles have been small, few, or solitary, the same may be observed." Then follows what amounts to a description of the effects of milking,—crusts with hardened base, raw bleeding surfaces, ulcers, sloughs, heat and tenderness, etc. Next it is noted that there is "not unfrequently . . . an odour strongly resembling that which emanates from a patient in the last stage of small-pox." In some animals, under some circumstances, the condition described continues little altered till the third or fourth week. . . . In many, however, little uneasiness seems to exist ; the parts gradually heal. . . ."

In summing up his observations, Ceely says, "The normal course of the natural and casual disease is completed in about twenty or twenty-three days, viz., four days in the natural form, from the probable period of invasion (in the casual, three or four from the presumed period of incubation) to the appearance of the eruption ; six or seven from this period to the full development and perfect maturation of the vesicle ; five or six from its decline to perfect dessication ; five or six from this period to the spontaneous separation of the crust, and the formation of the cicatrix." Irregularities, however, are frequent —*e.g.*, shortening of the first two stages in the natural disease ; prolongation of the first stage, and shortening of the second in the casual disease ; frequent shortening, and occasional prolongation of the third stage in both diseases, and so on. Regarding the seat of the vesicles, "they are found principally on the teats, but are often seen on the udder, especially on the lower and naked part."

In the casual disease, as in the natural, we thus again see the importance of observing the facts about the udder. On it, on the tenth and eleventh days, "lymph is so copious that it raises the cuticle, forming a globular or conoidal vesicle, or freely flows out from its rupture."

I have now to call attention to what seems to me a very serious objection to Dr Creighton's doctrine. While we have noted the character of the material conveying the disease to the majority of the cows in a byre, we have also noted that, from the time when the earliest symptoms of the natural disease are observed on a cow, only ten or twelve days elapse till some others are visibly affected by contagion. We now see that this interval embraces the period—four days apparently—of tenderness and swelling preceding the eruption on the first cow, and also the incubation period of three or four days belonging to the casual disease of the animals infected from the primary case. Between these two there is an intermediate period

of only from two to five days. Now, on Dr Creighton's hypothesis, it must be in this interval that the great change takes place—that the eruption of ordinary pimples is altered into an inveterate, specific, highly contagious malady, parallel in its local manifestations, and in its after consequences, to the worst species of human venereal disease. Ceely always speaks of two milkings per day, though Dr Creighton writes as if there had been three. Probably, however, Ceely knew best, as he was on the spot, and the events are now fifty years old. Therefore, from four to ten repetitions of the milking process must be sufficient to bring about the transformation. And if we allow, what Dr Creighton does not suggest, that a beginning may have been made a day or two before the pimples appear, we still have a period which, even to a pathologist willing to grant that it could take place at all, would surely seem absurdly inadequate for so remarkable a metamorphosis, which, in the words of its sponsor, "owes its existence to provocation, or to neglect and indifference to the reparative process, carried beyond the safe limit." Clearly, too, in this short period there has been no time for the formation of sloughs, deep ulcers, etc., and all the conditions of wildness which, in Dr Creighton's estimation, play so prominent a part in the infective process. Yet, independently of these, the disease *is* infective. Have we not, then, here the very virus of cow-pox, and are not all the sloughs and ulcers mere accidental complications, having nothing to do with the real essence of the infection?

Further, in order to agree with Dr Creighton, it seems necessary either (1) to give up the modern belief, with all the evidences on which the belief depends, that infectious diseases owe their existence to living organisms; or (2) to accept abiogenesis, and to acquiesce in the proposition that these few acts of milking are capable of bringing into existence a germ whose life history involves all the phenomena of vaccination. From Dr Creighton's writings, I judge that he would accept the former alternative.

The sores on the milkers' hands and those resulting from the use of "primary lymph" next demand attention. The former furnish the strongest evidence that Dr Creighton can adduce in support of his thesis. "The best proof that the ulcerating and indurating part of cow-pox is no mere appendage that may be lopped of, is the fact that on the milkers' hands, and even on their faces, the vesicles pass into the stage of slow-healing ulcers, with a uniformity that is practically decisive for making ulceration the full and unmodified type of cow-pox as a communicable infection" (p. 83). The position, indeed, he thinks so unassailable that opposition to it hardly merits consideration. "It is idle," he says, "for Hering and Seaton to urge 'that the phagædenic or indurative ulcerous process has been, in England, *superinduced* upon, or added to the original characters of cow-pox by the remorseless traction of the teats in milking the animals three times

a day.'" We will turn now to the facts, not using those which everyday experience of calf vaccine would give, but keeping strictly to Ceely himself.

That author says—and the words may apply alike to the natural and to the casual disease—"The best lymph is to be obtained from perfect vesicles before the period of acumination," and should be sought for "where the vesicles are least exposed to injury, viz., on the lower or naked parts of the udder and the adjoining bases of the teats;" or, in other words, the best cow-pox virus is to be sought for on parts which have not been subjected to those manipulative processes which, Dr Creighton tells us, are alone capable of producing it.

Not often, however (and this is of essential importance), did Ceely use "the best lymph," or, indeed, liquid lymph of any kind. He was too soon, or he was too late, or if at the right time, then the vesicles had been accidentally ruptured, so that in detailing the results of "primary lymph" he usually means, not liquid lymph at all, but crusts. Dr Creighton himself mentions this. He says (p. 63), that "for the practical purpose of inoculation, Ceely had to be content with crusts." But when he comes to urge, in support of his theory, the severity of the symptoms produced by Ceely's primary vaccinations, he makes no reference to this very pertinent fact. After dilating on sloughs, abscesses, and ulcers, he goes on to say, "such being Mr Ceely's experience with primary lymph;" and the headline of the page in which the passage occurs is, "Ceely on effects of direct cow lymph." But surely Dr Creighton knows that, from the point of view of those who differ from him, the whole question consists in what he here ignores—that there is need to differentiate between the effects of uncontaminated lymph on the one hand, and of crusts containing all manner of dirt and debris on the other. Further, the doctor should have observed that Ceely himself makes mention of an additional cause for the severity of the milker's sores. He writes as follows about their *liability to rupture*, "Although there can be no doubt of the greater severity of the local and constitutional symptoms attending the casual cow-pox on the hands, etc., it is equally clear that these symptoms are greatly aggravated by the rupture of the vesicles on parts so vascular, tense, and sensitive, and subject to motion." Woodville, forty years before, had unintentionally illustrated almost the same point in his forty-fourth case,[1] where he vaccinated the same person both on the hand and the arm, and found that "the difference was very evident, for the tumour upon his hand was much more extensive, of a more livid colour, and attended with more inflammation than the other."

Jenner himself indicates views essentially similar. Speaking of ordinary humanized lymph, in his *Further Observations* (p. 32), he

[1] *Reports of a Series of Inoculations, etc.*, p. 66.

points out "that the most material indisposition, or at least that which is felt most sensibly, *does not arise primarily from the first action of the virus on the constitution, but that it often comes on, if the pustule be left to chance, as a secondary disease.*" The italics are his own. Again, arguing, as was his wont, by analogy, he says, "Is *pure pus,* though contained in a small-pox pustule, ever capable of producing the small-pox perfectly? I suspect it is not." And yet again, as Dr Creighton quotes (p. 75), Jenner says, "I am more and more convinced of the extreme mildness of the symptoms arising *merely from the primary action* of the virus on the constitution."

So that Jenner, Woodville, Ceely, Hering, and Seaton are of one mind as to what is essential and what accidental in the vaccine disease.

But we need not go to these authorities for proof that Dr Creighton is wrong. The following narrative, taken from his own pages (114–6), shows, when examined along with the associated facts, a condition of things entirely at variance with his whole theory:—

"The first vaccination done in America, with lymph from Woodville, was upon Dr Waterhouse's own child, who suffered from axillary swellings, an efflorescence from the shoulder to the elbow, and what would seem to have been an ulcer; 'a piece of true skin was fairly taken out of the arm by the virus, the part appearing as if eaten out by a caustic' (*Op. cit.*, i. p. 19). His own subsequent cases were milder, and, in fact, regular; but in the autumn of that year (1800) a great many misadventures occurred through the incautious use of vaccine matter from open sores or from vesicles late in their development. 'I have known,' says Waterhouse (ii. p. 8), 'the shirt sleeve of a patient, stiff with the purulent discharge from a foul ulcer, made so by unskilful management, and full three weeks after vaccination, and in which there could have been none of the specific virus. I have known this cut up into small strips and sold about the country as genuine kine-pock matter coming directly from me. Several hundred people were inoculated with this caustic morbid poison.' At a later part of his second essay we come upon the more precise details of these vaccinations with caustic virus: 'All those cases where there were violent inflammations, deep-seated ulcerations, eruptions, and heavy febrile symptoms were not the true kine-pock, but a malady generated by a highly acrid, putrid matter; or, in one word, poisonous matter taken from under a scab, or from an open ulcer *long after the specific virus was annihilated.*' The explanation printed in italics is, of course, sophistical; the scientific explanation is, that the use of the virus from a late period of the vesicle or ulcer reproduced and gave fixity to that section of the natural history of cow-pox, which is ordinarily kept latent by careful attention to the period of maturation. . . . 'At another time [says Waterhouse] the angry pustule shows no disposition to scab, the aperture in the skin increases, the inflammation blazes forth afresh, and the illness keeps pace with the progress of the ulceration; a transparent glairy fluid fills the cavity, which granulates very slowly.' This transparent fluid had been used to vaccinate with: 'It is the most virulent of all the discharges of cow-pox.' This is the caustic matter which is apt to produce in patients of certain habits a crop of eruptions and a heavy weight of constitutional symptoms.'

"When Jenner heard of the American disasters of the autumn of 1800 and of the end of 1801, he wrote to Waterhouse that he had been longing for a speaking-trumpet that would carry these words on the rapid wings of the wind

across the wide ocean: *Take the virus before the efflorescence appears.* That is, no doubt, the golden rule of safe vaccination. All the same, the disastrous effects of taking late virus, or of allowing vesicles to become ulcers, were neither more nor less than natural and inherent possibilities of all and every inoculation with the products of the disease on the cow's teats."

In this quotation we have the whole case in a nutshell. Let it be remembered that Woodville's lymph *was mild from the beginning.* As he says, and as Dr Creighton quotes (pp. 27, 28), "We have been told that the cow-pox tumour has frequently produced erysipelatous inflammation and phagædenic ulceration, but the inoculated part has not ulcerated in any of the cases which have come under my care, nor have I observed inflammation to occasion any inconvenience except in one case, where it was soon subdued." Dr Creighton also calls particular attention to the fact that the vesicles of Sarah Rice, the milker from whose hands Woodville's first lymph was taken, "*gradually went off without producing ulceration.* . . . She had caught the disease, in fact, mildly, and it never came to painful open ulcers with her at all, but healed under the crusts and scabs."[1] Thus, as Dr Creighton says, Woodville "led off with a type of vesicle which hardly differed from the standard vesicle of to-day."

Here, therefore, there was no ulceration, *and there could be no memory of ulceration—no reversion to an "untamed" condition which had never existed.* Yet we find, on Dr Creighton's own showing, that in the subsequent history of this lymph there were "violent inflammations, deep-seated ulceration, and heavy febrile symptoms." Still further, heaping contradiction on contradiction, he says of these occurrences that their "scientific explanation" is that they "reproduced" an antecedent condition.

I need hardly point out here that Dr Creighton cannot take refuge in the supposition that this Gray's Inn lymph may have had ulcerous characters in some previous herd from which it had been transmitted. He asserts that the disorder is one which begins *de novo* from simple papules, and in endeavouring to account for the mildness of this particular "strain" of Woodville's, he argues on the assumption that it had such a commencement. But the inveteracy, and the consequent specificity of cow-pox come into existence on the cow's teats and udder, and it is entirely owing to the "perpetual insult of an ailing part" that the disease in any case becomes transmissible. Who, then, would ever have supposed that, in Dr Creighton's scheme of things, lymph which, to start with, "hardly differed from the standard lymph of to-day," could possess all the properties of contagion? We have now learned, however, that it had these properties; and we further learn, from this episode in its career, that it was capable of developing every one of the phenomena, which, he insists, can only be accounted for by initial severity—a severity which it never possessed! But, adopting the

[1] *Jenner and Vaccination*, p. 105.

doctor's own airy phrase in the above extract, it is "all the same" to him.

Turning from this medley of contradiction to the plain facts of the case, we find that this lymph, which had been so mild at home, caused bad symptoms on the very first case abroad. But in the interval, we have the significant fact that it had been preserved on a thread through the long sea and land journey. Next we have Waterhouse's realistic description of the foul shirt-sleeve and the poisonous matter "from an open ulcer long after the specific virus was annihilated." This, to Dr Creighton, "is, of course, sophistical." Of course! And of course, too, his own theory of reversion to a type which had never existed is not sophistical. It is the "scientific explanation." Science, forsooth!

And what a contrast does the story afford us between the "science" of the critic, Dr Creighton, and the "sophistry" (save the mark) of the much-criticised Jenner, who, living in days long before telegraphs or steamboats were heard of, longed for a speaking-trumpet to carry across the wide ocean that message of advice which to this day is the golden rule of vaccination practice.

Following Dr Creighton in his relation of some of the bad results obtained by the early vaccinators, the observations which I have already made continue applicable. Indeed, in Jenner's early practice we find pretty much what might be expected if his early procedure (before he had discovered the "golden rule") were adopted by any one in the present day. To begin with, the best lymph direct from the cow is apt to produce a more severe disease than humanized lymph, the effects on the arm being relative to the soil on which it had been raised. Next, the filth, the pus, the decomposing blood, the ichorous discharge from neighbouring ulcers on indurated bases, were so intimately mixed up with the liquid lymph itself, or with what had been liquid lymph but had dried into a crust, that the disease produced could not but have characters which were the compound effect of a compound cause. Nor is it surprising that such characters were not got rid of at the first or second remove. Then we have the late period at which the lymph was taken. In Darke's cases, specially referred to by Dr Creighton as being of great severity, we find that the lymph used had been taken on the *twelfth* day from a vesicle, as to which Dr Creighton himself tells us "the areola was out, and there were a number of very minute confluent pustules round the big cow-pox vesicle." In another example, "with matter taken on the *twelfth* day Jenner inoculated Mary Hearn," the consequence being "an areola on the fourteenth day and an ulcerous state of the arm for some time after" (p. 93, *Jenner and Vaccination*). Again, Dr Creighton enlarges on "Thornton's experience." That practitioner found a milker with sores on his hands, one of which "was still in the unbroken form of a pock," all the others having degenerated into sordid and painful ulcers, though only five days

had elapsed since the vesicles first appeared. [In passing, I cannot avoid pointing out once more how such a narrative, on the very face of it, is at variance with Dr Creighton's theories. The milker's ulcers, he tells us, were a late stage of the long cycle of untamed cow-pox, and were representative of the same stage on the cow's teats, that stage being itself analogous to the true syphilitic ulcer, with indurated base, hard edges, etc. But here we have the "sordid and painful ulcers" within *five days* of the first appearance of the vesicular stage. Surely that was not a long cycle, and surely the ulceration is abundantly explained by the fact that the vesicles occurred on the hands of men engaged in such daily manual toil as falls to the lot of general servants on a farm. But to resume.] From this vesicle a Mr Stanton and four children were inoculated, and "on the third day the arms of the four children were affected with a kind of erysipelatous efflorescence above the point of insertion," and, finally, all the children were inoculated with small-pox, and all took the disease. Such is Dr Creighton's version. But on turning to the original we find two differences, which, had they been discovered by Dr Creighton in any narrative of Jenner's, would have probably evoked a protest against "editing." In the first place, Thornton describes the matter used as "purulent,"—a most pertinent fact in the estimation of ordinary readers. And in the second place, not four children, but only one, had erysipelatous symptoms. Thornton's words are—"On the third day all their arms appeared to be under the influence of a very active virus; the arm of the youngest child was affected with a kind of erysipelatous inflammation, the size of a half-crown piece, without any elevation of the cuticle." And, finally, as to subsequent variolation proving successful in cases which had shown such early local symptoms, the experience of Woodville may be quoted (p. 34, *Observations on the Cow-pox*). "When a considerable tumour and an extensive redness take place at the inoculated [*i.e.*, vaccinated] part within two or three days after the infectious matter has been applied, the failure of inoculation [vaccination] may be considered as certain as where neither redness nor tumour is the consequence. This rapid and premature advancement of the inflammation will always be sufficient to prevent the inoculator from mistaking such cases for those of efficient inoculation."

Leaving Jenner's practice, we turn to Bousquet's experiences with lymph from the Passy cow (1836), which are also cited by Dr Creighton as evidence of the great severity of the results of primary lymph, and of its consequent analogy to the virus of syphilis. Very opportunely, Professor Crookshank has included in his work just published (in November 1889, though the preface is dated April 1889), a translation of part of Bousquet's paper. Dr Creighton mentions that the "inoculations were made from the milker's vesicles or pustules, which were large, semiglobular, yellow-

ish blebs, without central depression." Their diameter, Bousquet says, was "three or four lines," they were "well circumscribed,' and regarding the fact that they did not resemble the vaccinal eruption, he points out that "these differences were naturally explained by the advanced state of the vesicles." Further, when we come to the condition of the matter used for vaccination (a point not referred to by Dr Creighton), we need have no difficulty in accounting for some severity of symptoms: "On puncturing the vesicles with a lancet, thick, white, purulent matter escaped, as if it were an abscess discharging;" and besides, "it was none too soon; in a few hours all chance would have been lost." Regarding the choice of lymph in general, Bousquet held that even when from the 10th to the 12th day, "the areola was large and vivid," and "the subjacent tissue much infiltrated," and "the lymph beginning to be turbid," yet "it is none the less suitable for inoculation." Does it need a new doctrine of cow-pox to explain that an operator holding such opinions should occasionally find in his practice inflammation and ulcers? But withal, the impression produced by reading Bousquet is hardly that his observations "are in close agreement with those made by . . . . Ceely two or three years after," as Dr Creighton thinks. Bousquet's average results seem to have been milder than Ceely's, even though, in one particular case vaccinated in three places by another practitioner, the violence of the inflammation explained to him "les frayeurs de Jenner." The same remark applies to Estlin's Bristol experience, which our author brackets with Ceely's; and in Estlin's case, too, there is the relevant fact, already noted (and included in Dr Creighton's synopsis),[1] that his stock of lymph was taken on the eleventh day from the arm of a child that had been domestically inoculated from a milker's hand.

In addition to what I have already urged in opposition to Dr Creighton's views on cow-pox and syphilis, I have now to call attention to his references to that outbreak of vaccinal syphilis which is best known in this country, and the report of which, by Mr Jonathan Hutchinson, did most to arouse the attention of the profession to the possibility of syphilitic infection by means of the vaccine lancet. Dr Creighton's account of these cases, which occurred in London in 1871, is as follows:—

"In the first series, twelve persons were successfully vaccinated with lymph taken on the eighth day from a specially healthy-looking child with five good vesicles. When this child was examined two months after, it was found to have correct scars; but it had five small condylomata *circa anum*. There was no imputation on the soundness of its parents. Of the twelve per-

[1] But Dr Creighton says Estlin used the lymph the same day on which he took it, while Estlin merely states that, after his return from the farm where the disease existed to Bristol, he began operations "as soon as practicable," having first called in the assistance of two experienced vaccinators.

sons successfully vaccinated from the child's arm (most of them in three places, some in four), the first two had no ill effects, but in each of the remaining ten the scars broke out after having come to rest in the correct manner, and in the eighth week presented the appearance of indurated chancres. Under mercurial treatment the induration soon became soft, and the sores healed. Besides headache in some, there was hardly any constitutional disturbance while the sores were present; only two (Nos. 4 and 5) had ulcerated tonsils, and not more than half had a well-marked secondary eruption on the skin."

Dr Creighton holds that these cases were not syphilitic at all; they were simply a reversion to the untamed cow-pox. Taking his own synopsis of the facts, I have to ask special attention to this sentence referring to the vaccinifer,—"When this child was examined two months after, it was found to have correct scars; but it had five small condylomata *circa anum*." This statement is nothing less than destructive of Dr Creighton's position. He continually asserts, as I have already over and over again mentioned, that in such cases there is a return to the original local characters of cow-pox—to a prolonged cycle of inflammation, ulceration, etc. The secondary symptoms, he urges, are relative not to syphilis, but to this specific cow disease, whose primary appearances are local induration, severity, delayed healing, etc. But in this child the vaccination pursued its ordinary career. There was no return to an aboriginal condition. Its course locally was normal throughout. *And yet the secondary symptoms followed.* There were secondaries without any primary! The remorseless manner in which Dr Creighton's theory of unconscious memory drags him from paradox to paradox could have no better illustration.

This brings us to the argument from the statistics of syphilis, to which Dr Creighton devotes Chapter IX. of "Cow-pox and Vaccinal Syphilis." He gives a table headed "Increasing Infantine Death-rate from Syphilis," and showing how from 1847 till 1884 the number of deaths (which he sets down, by mistake, as death-rates) under one year increased, in England and Wales, from 255 in 1847 to 1733 in 1884. He makes a special point of the fact that the Compulsory Vaccination Act came into force in 1853, and that in the table the greatest proportional increase in infantile syphilis was in 1854. But in a valuable article in the *National Review* of June 1889 Mr Preston-Thomas replies:—"The rise between 1853 and 1854 is easily explicable. In the analysis of the death certificates of 1854 (as will be seen on examination of the Registrar-General's reports) a new plan of tabulation was adopted, as the result of the Statistical Congress which had been recently held at Brussels; and a large number of deaths, which had before been unclassed, now began to be relegated to particular diseases. Thus we find that the deaths under the heading 'causes not specified,' which had been 6900 in 1853, declined to 5663 in 1854, and that the

difference of 1237 was distributed over different headings. It was in this way that the figures in the column for 'syphilis' were swollen." As to the gradual rise in infantile syphilis exhibited in the table, the figures on which it is founded form a favourite anti-vaccination argument. But they are usually given alongside of other columns showing the corresponding "public vaccinations" performed in England and Wales. I need not give the figures here, but, as I have said elsewhere, taking the years 1852 to 1882 (with the exception of 1872–3, which for certain reasons are not comparable), and comparing each year with its predecessor, "it will be found that on twelve occasions an upward or downward movement of vaccination was accompanied by a movement of similar direction on the part of syphilis, and that on seventeen occasions the reverse was the case—that when vaccinations rose syphilis fell, and that when vaccinations fell syphilis rose."[1]

In *Vaccination Vindicated* (pp. 136–8), and recently in the *Lancet*, I have proved from the Registrar-General's figures—(1), that in Scotland, vaccination not being compulsory under six months, no less than 65 per cent. of the deaths from syphilis at all ages take place before the age of vaccination, and that in the second half-year of life, or immediately following vaccination, the deaths fall to 11·6 per cent., or less than one-fifth of those in the prevaccination half-year; (2), that the figures for England are almost identical, and therefore that the earlier performance of vaccination in England than in Scotland has no effect whatever on the comparative infantile mortality from syphilis in the two countries; and (3), that while the percentage under one year of age of the syphilis deaths at all ages has increased from 70 previous to compulsory vaccination to 77 since compulsory vaccination, the whole excess of 7 per cent. is accounted for by infants under three months of age—the three months of non-vaccination. I then proceed as follows (p. 139):—"What, then, is the meaning of the increased registration of syphilis as a cause of death? In elucidation of this question I have to point out that there is one other period of life besides infancy in which deaths from syphilis show a great proportional increase. That period is advancing age. If we divide life into three epochs—(1) childhood, (2) adult life up to 55 years, and (3) all ages over 55—we find that while in the last period the total syphilitic mortality is much smaller than in the others, yet in the last, as in the first period, deaths from syphilis have considerably increased. Obviously, vaccination can have nothing to do with this. And there is only one answer that will satisfy all the facts of the case—namely, that the change to a very large extent depends on improved knowledge of the disease by medical men. It is, again, a question of diagnosis. The symptoms of primary and secondary syphilis in young adults were about as well known thirty years ago as they are now; not so the mani-

[1] *English Mechanic*, 7th December 1888.

festations of congenital syphilis in children and the obscure tertiary affections of later life. One of the commonest results of congenital syphilis is premature birth. That fact is better understood than formerly, and some fraction of the enormous decrease (from 1043 per million in 1850–4, to 476 per million in 1875–9) in deaths registered from this cause is doubtless due to the substitution of the term syphilis. So, too, congenital syphilis may cause brain disease ending in convulsions ; and here, again, part of the diminution consists of a transference of deaths from convulsions to syphilis. These views are in exact accord with what we have already discovered—that it is in the first three months of life that the great bulk of increase has appeared."

Let us suppose now, for a moment, that Dr Creighton, instead of finding an analogy between cow-pox and syphilis, had found it between small-pox and syphilis. If he had suspected that inoculated small-pox, and not inoculated cow-pox, had its parallel in great-pox, would it have been possible for him, by research into the writings of the old small-pox inoculators, to have got any facts in support of his theory? In the history of inoculated small-pox could he have discovered any occurrences which might have been described as results of unconscious memory—as reversions to an untamed condition exhibiting great severity of symptoms? Let us try.

As I have already mentioned, Woodville cites Dimsdale as the authority whose method of inoculation, introduced long previously, had by its inherent value held its own even to Woodville's day. Turning to Dimsdale, I find the following:—

(1.) " . . . there are some cases wherein the incisions continue to discharge a purulent matter longer" (p. 41).

(2.) "Another deviation, of still more consequence, which sometimes happens towards the end of the eruption, and is often, though not always, accompanied by great sickness, is an erysipelatous efflorescence" (p. 43).

(3.) "Another irregularity, deserving notice here, is that sometimes, upon the abatement of the fever and other symptoms, after the appearance of several pustules, and when the eruptive stage of the disease seems completed, it nevertheless happens that fresh eruptions come out, and continue doing so daily, for four, five, or even six days successively" (p. 51).

(4.) "Every one who has had any share in this practice according to the common or old methods will allow, that after passing through the disease in a very favourable manner, their patients (children especially) were frequently liable to abscesses in the axilla and other parts, tedious ophthalmies, and troublesome ulcerations in the place of insertion" (p. 56).

(5.) "In a few instances also there has been a slough in the incised part" (p. 57).

Take, again, Dr Gatti's book, as translated by Dr Maty:[1] "In

[1] *New Observations on Inoculation.* London, 1768.

the usual method . . . . the humor . . . . necessarily brings on an ulcer. This must be dressed for a fortnight at least; and whilst the principal disorder employs the inoculator but two or three days, the incisions require his attendance for several weeks. Thus a complaint is produced, both tedious and painful; . . . . It even happens that the ulcer will not heal up, but grows so deep and foul that the surgeon cannot conquer it in many months; and the patient must suffer a thousand times more from this than from the small-pox itself. It is well known that sometimes inoculation leaves other bad remains—such as erysipelas, tumours, and abscesses, which are very troublesome, and may become fatal." Here we have deep and foul, slow-healing ulcers, tumours, abscesses, and erysipelas—all as in cow-pox. It is true that in accounting for them Gatti says, "You must take in the effects of the thread and of the putrid matter itself, not merely as conveying the infection, but as extraneous and offensive bodies; and to these are to be added the action of the plaister and of the air. These last causes may produce an inflammation . . . . which often becomes erysipelatous, as in any other wound covered with a greasy plaister." But to all this it might be replied, in Dr Creighton's words (p. 103), "We need have no hesitation in dismissing the theory, which can always be plausibly urged for apologetic purposes, that the erysipelas of vaccination [inoculation] is owing to foul lancets or extraneous infection introduced," and (p. 166) that "the explanation [by Gatti] is, of course, sophistical."

There is another very singular feature in Dr Creighton's writings to which I must call attention. His promulgation of the doctrine as to the resemblance between cow-pox and great-pox lays him open to charges almost exactly similar to those which he launches against Jenner for *his* belief in the resemblance between cow-pox and small-pox. I will endeavour to illustrate this by parallel columns:—

"One can readily understand Jesty, the Dorset farmer, being misled by the similarity of names [cow-pox and small-pox], and by superficial aspects of diseased processes.

"While Jenner's prosaic medical neighbour saw no point of contact between cow-pox and small-pox, and while they gave due heed to the abundant experience that cow-poxed milkers had not escaped the common epidemic of the time, Jenner persuaded himself that the one kind of pox was somehow related to the other, that there was a scientific or pathological basis for the rumoured antagonism between them,

One can with difficulty understand Dr Creighton, an able pathologist, being misled by the similarity of names, great-pox and cow-pox, and by superficial aspects of diseased processes, as ulcers, etc.

While Dr Creighton's prosaic medical neighbours saw no point of contact between cow-pox and great-pox, and while they gave due heed to the abundant experience that vaccinated children had not been, through their cow-pox, attacked by the common venereal disease of the time, Dr Creighton persuaded himself that the one kind of pox was somehow related to the other, that there was a scientific or patho-

and that the cases of small-pox in previously cow-poxed milkers must have been exceptions, which he would one day be able to account for.

"It is difficult to acquit Jenner of recklessness, or of culpable laxity, even in the very inception of his idea. There is just one thing that may be pleaded as having misled him in an excusable way, and that is the form of vesicle which cow-pox assumes in the first few days of its development on the milker's hand. We know now, since the experiments of Ricord, Henry Lee, and others, that a sore of the pox proper, or of syphilis, when inoculated on the skin begins in the same kind of whitish vesicle as the milker's cow-pox, and that the classical pox and the cow-pox are in that as in other respects closely parallel."

logical basis for the parallelism which he alleged to exist between them, and that many cases of syphilis in previously cow-poxed children must have been occurrences which could only be accounted for in this way.

It is difficult to acquit Dr Creighton of recklessness, or of culpable laxity, even in the very inception of his idea. There is just one thing that may be pleaded as having misled him in an excusable way, and that is the form of ulcer which cow-pox often assumes after the first days of its development on the milker's hand. We know now, since the experiments of Ricord, Henry Lee, and others, that a sore of the pox proper, or of syphilis, when inoculated on the skin, develops into an ulcer with indurated base and raised edges not unlike that which, owing to dirt, to exposure, and to the mobility of the parts, often occurs in milker's cow-pox, though the classical pox and the cow-pox are not in any essential respects at all parallel.

In view of these "parallel readings" may we not now form the opinion that Dr Creighton belongs to the class of people described in Hudibras, who

> "Compound for sins they are inclined to,
> By damning those they have no mind to."

JOHN C. M'VAIL.