

JENNER AND VACCINATION.

CHARLES CREIGHTON.

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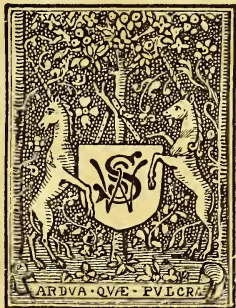
JENNER AND VACCINATION:

A Strange Chapter of Medical History.

BY

CHARLES CREIGHTON, M.D.

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P R E F A C E .

THE object of this book is to make clear to general readers the steps in the rise and acceptance of Jenner's doctrine and practice of vaccine inoculation. The assent of the profession both at home and abroad having been given within the first two or three years, the history has been followed most closely for those years. The subsequent establishment, endowment, and enforcement of the doctrine and practice are narrated with less minuteness in the concluding chapters. The history being a somewhat strange one, it has been thought desirable to authenticate the facts by full references. The events herein narrated and criticised are remote enough from our own day to have become fitting matter for historical treatment. In medicine new developments of theory and practice are so closely bound up with the legitimate professional standing and repute of their authors that it is always a matter of delicacy to subject them to contemporary criticism of the more rigorous kind. But there need be no such reserve in dealing with medical affairs that lie well within the limits of history. The medical profession of this

country, it is true, has offered no great encouragement to those who would touch even the history with a hand of criticism. But the public can hardly be expected to share that pious feeling so far as concerns a practice that is brought home to every one by the law; the historical origins, or the roots of authority, may here be laid bare without compunction. In most other affairs of the past it is not only permitted to historians, but even expected of them, that they leave no stone unturned.

Technical language has been avoided as far as possible, and has, indeed, been little needed in dealing with a subject which is a commonplace of every household. Some of the points the author has been enabled to pass over briefly with a reference to a former book written for his own profession. He has been enabled also to curtail where his immediate precursor in the history of vaccination, Mr. William White, has been most copious. Those who are acquainted with Mr. White's able and accurate historical inquiries will find that the present work for the most part covers new ground.

LONDON,

February, 1889.

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JENNERIAN DOCTRINE OF VACCINE.

CHAPTER I.

JENNER'S SCIENTIFIC CREDIT BEFORE VACCINATION.

WHEN Dr. Edward Jenner came before the world to recommend cowpox as an effective substitute for smallpox in the way of inoculation, he had been for nine years a Fellow of the Royal Society, with a considerable intimacy among leading men in London. When the evidence for his alleged discovery was challenged by Dr. Ingen-housz, a foreign physician and scientific writer of great repute, who happened just then to be on a visit to Lord Lansdowne, at Bowood, Jenner promptly stood upon his dignity as being himself a scientific personage not unknown, and thus wrote to his foreign critic: "Truth, believe me, sir, in this and every other physiological investigation which has occupied my attention, has ever been the object which I have endeavoured to hold in view."¹

What, then, were these earlier physiological inquiries

¹ Baron's *Life of Edward Jenner, M.D.* 2 vols. London, 1827-1838, i. 294.

to which Jenner appealed? and what were the grounds of his being taken seriously, as unquestionably he was taken seriously, by leading men in medicine and science whenever his first essay on cowpox issued from the press?

Jenner came of a prosperous family in the Church, who held benefices in succession, as well as a small landed estate in the Berkeley country of Gloucestershire. When he had finished his apprenticeship with an able country surgeon, Mr. Ludlow, of Sodbury, whose son and partner had been a pupil of John Hunter's, in London, Jenner was sent, at the cost of his elder brother, to become in like manner a boarder and pupil in John Hunter's house. Hunter's fee for each pupil was five hundred guineas, the pupil being bound for five years;¹ but as Jenner had already completed his indentures with Ludlow, he remained only two years with Hunter, and was probably received at an annual rate for so long as he cared to stay. In Hunter's house and workrooms, he was in a centre of great influence and of many opportunities. Among his youthful contemporaries were Everard Home, Cline, and others, who had become men of influence by the time that Jenner came forward as the advocate of cowpox six-and-twenty years after. Shortly after he went to board with Hunter, Banks came home in 1771 with a large collection of objects which he had made, with the help of Solander, as naturalist on Captain Cook's first expedition to observe the transit of Venus in the South Seas, and Jenner was set to work upon the specimens. There is nothing in his own

¹ Ottley's *Life of J. Hunter*, p. 34.

writings, or in any of his observations that Hunter made use of, to show that he ever acquired any technical skill in dissecting and preparing. The set of injected specimens, showing the stages in the development of the hen's egg, which he bequeathed to his executor, Dr. Baron, was probably one of the purchases which Hunter made for him at the sale of Hewson's fine preparations,¹ although Baron assumes without evidence that they were Jenner's own handiwork, and praises him, accordingly, for his anatomical skill. But there were other humble ways in which an apprentice of Hunter's could be useful to Banks ; and, at all events, Jenner made the acquaintance of the man who was destined to occupy the chair of the Royal Society for many years, and to be the Mæcenas of science. It is at this point that the Jennerian mythus begins in the pages of the biographer, Baron. Jenner, we are told, was offered the post of naturalist with Cook's second, or 1772, expedition to the Pacific. The facts are as follows : Banks failed to induce the Government to allow himself, and the assistants whom he had selected, to accompany the second expedition in 1772, although he had been so far led to expect their consent as to have his appliances all ready and a scientific staff chosen. In order that his preparations should not be altogether in vain, and his assistants unemployed, he himself fitted out a naturalists' expedition to Iceland ; but Jenner was not one of those who sailed with it. Jenner remained a few months longer with Hunter, and returned in the end of 1772 to Berkeley, where he com-

¹ Hunter to Jenner, 30th Aug., 25th Sept., and 9th Nov., 1778, in Ottley's *Life of J. Hunter*, pp. 70, 71.

menced practice in the house of his brother, the Rev. Stephen Jenner.

From the time of his return to Gloucestershire down almost to the death of Hunter in 1793, Jenner kept up a correspondence with the latter. Hunter's letters to Jenner have been preserved, and they are, indeed, almost the only letters of his that were available for printing in his biography. They are a conspicuous feature of Drewry Ottley's *Life of John Hunter*,¹ and of the earlier chapters of Baron's *Life of Edward Jenner*. There can be no question that Hunter had an unaffected liking for his old boarder, who was not only attractive to him by his imaginative qualities, but was also good-natured, although very dilatory, in getting him specimens.

"I don't know any one," Hunter tells him in 1776, "I would as soon write to as you. I don't know anybody I am so much obliged to." Again, on 18th January, 1776: "I have but one order to send you, which is, to send everything you can get, either animal, vegetable, or mineral;" and, on 17th December, 1777: "I am always plaguing you with letters, but you are the only man I can apply to,"—that is to say, for country specimens and observations.

After Jenner had been in practice two years (1775), Hunter made him an offer, which had been already declined by several. Hunter had a scheme for starting a school of anatomy and natural history in London, to a share of which venture he was willing to admit some one, with the rank of assistant, on receiving a premium

¹ London, 1835.

of one thousand guineas down. Jenner was asked to consider whether he was prepared to come to London and to find one thousand guineas. "I proposed it to L—— [probably young Ludlow] before he left London," says Hunter, "but his father objected, I believe, to the money." Jenner naturally objected to the money too, and on his declining the offer, Hunter replied that he had hardly expected it would suit Jenner.

Of the various naturalist inquiries which Hunter set his country correspondent to work upon, only two came to anything. One of these formed the subject of Jenner's own paper in the *Philosophical Transactions*, some fourteen years after, on the "Natural History of the Cuckoo"; another yielded a few meagre observations on the temperature of hedgehogs in their torpid and waking states respectively. There were also some fragmentary conclusions about the action of blood and other organic manures upon growing plants, in a letter to Banks. These, then, were his earlier achievements, which Jenner referred to when he wrote to Ingen-housz in 1798: "Truth, believe me, sir, in this and every other investigation which has occupied my attention," etc.

A great part of Hunter's correspondence with him relates to the hibernation of the hedgehog. Hunter's long memoir on the "Heat of Animals," etc., was read before the Royal Society in two parts, on June 19th and November 13th, 1777; for that research he had been collecting facts during several years, and had enlisted Jenner in the service, especially with reference to the temperature of the hedgehog and other winter-sleepers in their torpid state. On August 2nd, 1775, he writes to Jenner: "I thank you for your experiment on the

hedgehog ; but why do you ask me a question by the way of solving it? I think your solution is just, but why think? why not try the experiment?"¹ He then directs him to repeat all the experiments (as planned in detail by Hunter), and they will give him the solution. On 10th January, 1776, Hunter again writes: "Have you large trees of different kinds that you can make free with? If you have, I will put you upon a set of experiments with regard to the heat of vegetables. Have you any eaves where bats go at night? If you have, I will put you upon a set of experiments concerning the heat of them at different seasons?" On January 22nd: "You do not mention a word about bats"; and a few weeks later: "If you catch any bats, let me have some of them; and those you try yourself, open a hole in the belly, just large enough to admit the ball [of a thermometer] and observe the heat there," etc. In May, 1777, he sent Jenner a thermometer which he had got made specially for the purpose, and on the 6th of July wrote to him again with minute directions for using the ivory sliding scale attached to it.

But not even in the second part of Hunter's memoir on Animal Heat, which was read on 13th November, 1777, are the observations forthcoming on the hedgehog and bat, which he wanted Jenner to make for comparison with his own observations on the dormouse. On the 23rd November he writes to say that the hedgehogs sent by Jenner had arrived, and to ask him to go on observing these hibernators in the country. On December 17th

¹ This is all the warrant that Baron had in saying that Hunter used to advise his pupil, "Don't think, but try."

he writes to say that the hedgehogs sent had died : “therefore I want you to find out their haunts, and observe, if you can, what they do,” giving him full directions how to proceed. On 29th March, 1778 : “Have you made any experiments with the hedgehogs, and can you send me some this spring ? for all those sent me died, so that I am hedgehogless.”

In any circumstances it would have been no easy thing to carry out Hunter’s directions for taking the temperature of a torpid hedgehog by making an incision in its body when it was coiled up into a ball ; and Jenner was at that time in no mood for nice researches. Having written to tell Hunter of the disappointment in love which had just befallen him, he got answer on 25th September, 1778 : “Let her go, never mind her. I shall employ you with hedgehogs.” He then puts before him a number of points to be observed in the problem of winter-sleep, including the autumnal storing of fat, and the consumption of it during the winter, none of which does Jenner appear ever to have fully apprehended or at all events given heed to. The references to hedgehogs go on, in the same tenor, in the letters for several years following. In 1783 Jenner wrote for a thermometer, whereupon Hunter replied : “You are very sly, although you think I cannot see it ; you very modestly ask for a thermometer. I will send one, but take care those d——d clumsy fingers do not break it also.”

The sole outcome of all this dunning year after year was the brief record of four temperature observations made by Jenner on a hedgehog (two in winter, one in summer, and one at a season not stated), which Hunter

introduced in half a dozen lines into his paper on the Animal Heat when he reprinted it in 1786, nine years after it was read to the Royal Society, in his "Observations on Certain Parts of the Animal Economy." As late as 10th December, 1791, Hunter writes to Jenner: "Now that hedgehogs are gone to sleep, I could wish you would get some of them for me," and send them to London. Baron says that he found among Jenner's papers "a manuscript detailing many of the experiments which he had made at the instigation of Mr. Hunter on hedgehogs; but I deem it expedient to delay its publication till it is found convenient to collect and print all his medical and philosophical papers." The collection referred to was never published. Hunter in 1786 gave the four temperature-observations that Jenner had made for him; and, if there had been any others worth recording, it is tolerably certain that Hunter would have put them in to eke out his meagre data.

Jenner's "medical papers," previous to those on cowpox, were one on a mode of preparing tartar emetic, and an observation of calcified coronary arteries of the heart in a case of angina pectoris, which was used by Dr. Parry, of Bath. His "philosophical papers" are represented solely by the observations on the cuckoo, in the *Philosophical Transactions* for 1788. The instructive history of that piece will now be given, with the view of throwing some light on Jenner's habits of thought and of work before we come to his more famous labours on cowpox.

A farm near Berkeley belonging to Jenner's aunt Hooper was a favourite haunt of cuckoos, and Jenner as a boy was familiar, like other boys of the locality, with

the bird and its ways. The fact that it laid its eggs in the nests of hedge-sparrows and some other birds had been admitted by every one for centuries, having been noted as a piece of common knowledge by Aristotle. It was reserved for the *Philosophical Transactions*, or for the Hon. Daines Barrington writing therein, to call in question this familiar observation, which men and boys had made ever since European cuckoos had been observed by men and boys at all.

John Hunter, having no doubt that the common experience of mankind was to be trusted in that matter, proceeded to ask himself *why* the cuckoo should lay its eggs in the nests of other birds; and he endeavoured to find an answer by his favourite method of examining the internal economy, and its adaptation to the animal's habits.

Previous to 1771, or before Jenner, aged twenty-one, came to board with him, Hunter was known to have dissected hen cuckoos,¹ and had satisfied himself that there was nothing in the anatomical disposition of the viscera, as some before him had alleged, to prevent the bird from sitting on eggs like any other bird.

This conclusion was one of those which Jenner introduced as a novelty into his paper of 1788, along with an analogical argument identical with that which White of Selborne had developed from observations on the structure of the closely allied fern-owl in a letter to Barrington in 1776. But the biographer, Baron, mythological as usual, will have it that "all naturalists previous to Jenner were inclined to ascribe the peculiarity in the

¹ See Daines Barrington, *Phil. Trans.*, vol. 62 (1771).

economy of the cuckoo to causes of this kind," namely its structural disabilities, the truth being that Herissant's conjecture was discredited already. There were many other points in the cuckoo's problem needing elucidation, as Hunter well knew; and when Jenner left Hunter's roof to return to Berkeley, he would undoubtedly be in possession of the great anatomist's views and wishes on the subject. In one of his earliest letters, written a few months after Jenner's return to the country, he thanks the latter for sending a cuckoo's stomach; in another, of the same period, he writes: "I shall be glad of your observations on the cuckoo; be as particular as you possibly can."

Hunter was by no means disposed to deprive his correspondent of any advantage or credit that might accrue from his studies in natural history. At an early period of their correspondence he had written to him: "If in any of these pursuits you discover any principle worthy of the public, I will give it to the Royal Society for you."¹ However, Jenner's study of the cuckoo did not for years look as if it would amount to anything, any more than the observations on the hedgehog ever did. In 1783, or ten years from the date of his first cuckoo reference, we find Hunter still writing: "I should be glad to have a true and particular account of the cuckoo, as far as possible under your own eye"; and, in the same year: "I request the whole history of cuckoos this summer from you." Three years more passed; and at length, in 1786, Jenner drew up for the Royal Society his paper on the Natural History of the Cuckoo, in the

¹ Otley's *Life of Hunter*, Letter of 1776, p. 60.

form of a letter to Hunter. It was sent to the latter, who kept it beside him for several months before giving it to the Royal Society, as that body was so torn with inward dissensions that the moment was not favourable.¹ In May or June, 1787, the paper came before the Council, and was ordered for publication in the *Philosophical Transactions*.²

But Jenner had found reason to change his mind at the last moment on the most important part of this problem, which he had been at work upon for some fifteen years. He wrote to have his paper returned; and Banks, the president of the Royal Society, acceded to his request as follows, under date 7th July, 1787³ :—

“In consequence of your having discovered that the young cuckoo, and not the parent bird, removes the eggs and young from the nest in which it is deposited, the Council thought it best to give you a full scope for altering it as you shall choose. Another year we shall be glad to receive it again, and print it.”

Having at length got the Prince of Denmark into the play, Jenner sent it up, dated 27th December, 1787; it was read before the Royal Society on 13th March, 1788, and published in the *Transactions* for that year. On the strength of this achievement, Jenner wrote to Hunter suggesting that he should be proposed for election into the Royal Society; to which Hunter replied that Sir Joseph Banks, on being spoken to, “had not the least objection, and will give all his assistance, but

¹ Hunter to Jenner, 26th April, 1787, *Ibid.* p. 104.

² Jenner to Banks, in *Baron*, i. 77.

³ In *Baron*, i. 77.

he thinks the paper had better be first printed and delivered, and let the people rest a little upon it, for he says there are many who can hardly believe it wholly.”¹ It was accordingly in the February following (1789) that Jenner was balloted for and elected F.R.S.

The paper on the cuckoo, in its original form, contained a number of observations on the contents of the stomach in the young bird, on the small relative size of the cuckoo's egg (which was apparently not known at the time to Gilbert White), on the fierce behaviour of the young cuckoo when inspected in its nest, on the number of eggs or traces of eggs in the cuckoo's oviduct, and on the hedge-sparrow's or other foster-parent's habit of ejecting its own eggs from the nest after the cuckoo's had been deposited therein. Besides these observations, and one or two rather crudely devised experiments, the original paper had contained a speculation on the causes of the cuckoo not hatching and rearing its own young.

The basis of that theory was the observation on the number of eggs in the cuckoo's ovary in various stages of forwardness. Gilbert White, in his letters to Barrington, had already questioned the statement that the cuckoo lays only one egg, and proposed to examine the ovarium so as to settle the matter. That was what Jenner did. He found it like a hen's ovary, with eggs in all stages, and he concluded, as White said he would do if the fact were so, that the cuckoo laid “a great

¹ The following judicious puff was inserted among the leading paragraphs of the *World* newspaper for 8th April, 1788: “The *Natural History of the Cuckoo*, lately read to the [Royal] Society, is one of the happiest additions to this part of animated nature.”

number of eggs" in each year. Jenner's notion was that the cuckoo had "a call of nature to produce a numerous progeny"; and was also "compelled," for some reason not stated, to leave us early, being only "allowed" a short residence, and "instinctively directed" to migrate in July. It could only reconcile those two calls of nature by laying eggs up to the end of its stay and leaving them to be hatched by other birds.

The point that later theorists have dwelt upon is the (assumed) long interval between each of the cuckoo's eggs; it is that, and not the mere number of the eggs, which would "make the process of laying and hatching inconveniently long," as Darwin says (*Origin of Species*, 6th ed., p. 212), and would create also the inconvenience of eggs and young birds of different ages in the same nest. To entrust each egg, as it was laid, to the care of some other bird then sitting (and, as Gilbert White pointed out, another bird wisely chosen) would thus be a true maternal instinct, or an action done for the sake of the young brood severally and collectively. But the cuckoo's early migration can hardly be part of the cause; it is rather a correlated effect. The cuckoo leaves us early because its parental instincts or duties, as it construes them, do not serve to detain it. The young cuckoos do themselves remain until comparatively late in the year (September), or until they are strong enough to undertake their flight. What cuckoos of the first year could do, the same birds in their second and subsequent years could surely do also.

The after-thought which caused Jenner to ask for his paper to be returned to him formed that part of the communication, as published in the *Philosophical Trans-*

actions, which Banks must have had in his mind when he told Hunter that "there are many who can hardly believe it wholly." It is a highly coloured description of a young cuckoo, hatched since the day before, as seen in the act of ejecting its fellow-nestling, a young hedge-sparrow of its own age and size. Jenner's original view, based on observations and abundantly confirmed, was that the old hedge-sparrow turned out her own eggs from the nest after the cuckoo's was laid beside them, on the principle, perhaps, of cutting off its nose to spite its face; and that the old cuckoo somehow came and turned out the hedge-sparrow's brood after they were hatched. Such, at least, is the view that Banks professes to have read in the first edition of the paper; and that is a view which Jenner himself speaks of, in the second edition of it, as being erroneously held by some authors, although he does not say that he himself had held it until quite recently. The common-sense view, which he also refers to, was the one given some years before by Pennant in his *British Zoology*,¹ to the effect that the young cuckoo, growing much faster than the companion fledglings which started level with it, and soon requiring all the room, destroyed the young hedge-sparrows by overlaying them. (Their ejection after that would be a matter of course, and would naturally be done by the old bird.)

But on the 19th of June, 1787, Jenner saw a marvellous thing happen. The day before, a hedge-sparrow's nest had contained one cuckoo's egg and three of the bird's own eggs. Next day, the nest contained

¹ Fourth Edition, 1776, i. 201.

the newly hatched cuckoo (from an egg the size of a lark's) and one newly hatched hedge-sparrow, the two remaining eggs having disappeared. "The nest was placed so near the extremity of a hedge that I could distinctly see what was going forward, and saw the young cuckoo [less than a day out of the shell] in the act of turning out the young hedge-sparrow. The little animal, with the assistance of its rump and wings, contrived to get the bird upon its back, and clambered backward with it up the side of the nest, till it reached the top, where, resting for a moment, it threw off its load with a jerk, and quite disengaged it from the nest. It remained in this situation a short time, feeling about with the extremities of its wings as if to be convinced whether the business was properly executed, and then dropped into the nest again. With these (the extremities of its wings) I have often [how often?] seen it examine, as it were, an egg and nestling before it began its operations; and the nice sensibility which these parts appeared to possess seemed sufficiently to compensate the want of sight, which as yet it was destitute of"—being, in fact, a raw young thing hardly bigger than the small egg which held it the day before. He afterwards tried the experiment of putting in an egg beside this heartless young creature, when, "by a similar process, it was conveyed to the edge of the nest, and thrown out." These experiments he had since repeated several times in different nests, and always found the young cuckoo "disposed to act in the same manner." The "often" in a former sentence, and the "several times in different nests" in the last sentence, must not be taken too literally, inasmuch as this whole be-

haviour of the young cuckoo was, on his own admission, new to him on the 19th of June, 1787, by which time the hatching season was about over for that year, and his paper was sent in and printed before another season.

But these were not the only marvels introduced into the paper on second thoughts. The young cuckoo's back, it seems, is specially designed for the lodgment and ejection of eggs and young birds; "for, different from other newly hatched birds, its back from the scapulæ downwards is very broad, with a considerable depression in the middle. This depression seems formed by nature for the design of giving a more secure lodgment to the egg of the hedge-sparrow, or its young one, when the young cuckoo is employed in removing either of them from the nest. When it is about twelve days old, this cavity is quite filled up, and then the back assumes the shape of nestling birds in general." This unique and marvellous structural change, it need hardly be said, has no existence; nor did Jenner seek to establish his assertion in the only way by which it could be established, namely, by a series of dissections. Moreover, he himself inadvertently supplies the key to the illusion and the fanciful anatomy by his remark on the previous page of his wondrous tale of ejection, that the young cuckoo "makes a lodgment for the burden by elevating its elbows."

Not only does the peculiar structural depression disappear from its back after the twelfth day; also "the disposition for turning out its companions begins to decline from the time it is two or three till it is about twelve days old, when, as far as I have seen, it ceases. Indeed, the disposition for throwing out the egg appears

to cease a few days sooner ; for I have frequently seen," etc.

All this varied, rich, and marvellous experience of the behaviour of young cuckoos has to be crowded into a few days at the end of the breeding season of 1787, having eluded the observer's notice during all the years since 1773, when he first wrote to Hunter about his "observations on the cuckoo." Hunter's advice to him on that occasion was, "Be as particular as you possibly can"; and never was the advice more needed.

Jenner's graphic description of the newly hatched cuckoo clambering up the side of the nest with a young hedge-sparrow as big as itself in a specially designed hollow of its back, balancing itself on the edge of the nest, then throwing its burden over the precipice with an adroit jerk, and remaining there a short time to make sure that the catastrophe was complete, has been accepted by all ornithologists.¹ Pennant, who had originally given the sensible explanation that the young cuckoo "quickly destroys the genuine offspring by overlaying them, as its growth is soon so superior," altered the passage in his edition next following Jenner's paper to "quickly destroys them by ejecting them from the nest." Jenner's cuckoo paper contains a few credible and prosaic facts ; but the greater part of it, and all that part of it which is best remembered, is a tissue of inconsistencies and absurdities.

This, then, was the piece of scientific work which got

¹ Darwin (*Origin of Species*, etc., 6th ed., p. 214) says that Gould had "received a trustworthy account of a young cuckoo," etc., but he does not quote Jenner, the sole authority for the "strange and odious instinct."

Jenner elected into the Royal Society ; and this was chiefly what he had in mind when he wrote to Ingenhousz at the outset of the cowpox controversy : “ Truth, believe me, sir, in this and every other physiological investigation which has occupied my attention, has ever been the object which I have endeavoured to hold in view.”

CHAPTER II.

THE POX, THE SMALLPOX, AND THE COWPOX.

WHEN Jenner came up to London in 1788 or 1789 in connexion either with the reading of his cuckoo paper before the Royal Society, or with his election into that body, he brought with him a drawing of a peculiar affection on the hand of a dairymaid ; namely, a large bluish-white bleb about the size of a silver three-penny piece. The drawing was shown to the various old friends whom he met in town, including Banks, Home, and Hunter ; a rough sketch of the tumid bleb of a cowpox sore exists among the Hunterian drawings on the envelope of a letter from Jenner without date.¹ It was a pathological curiosity in London, but a tolerably familiar thing in the dairy farms of Dorset, Wiltshire, Gloucestershire, Norfolk, and other counties. Some of Jenner's professional neighbours knew a good deal about it, particularly Mr. Fewster, of Thornbury. Jenner himself at that time knew hardly anything more about it than he might have heard now and then ; and, in truth, the milkers' sores were more likely to be treated, if treated at all, by a cow-leech than by a surgeon. But Jenner's fancy had been arrested by some idle talk that

¹ Ottley's *Life of John Hunter*, p. 39.

he had heard of cowpoxed milkers being unable to take smallpox ; he thought he saw before him the materials for another Royal Society paper, and he kept cowpox in mind in the lazy and unmethodical way that was natural to him.

The cowpox had been so called as far back as the oldest inhabitant could remember, Jenner himself says "from time immemorial."¹ It had not occurred to any one to associate it in any way with the smallpox until rather late in the eighteenth century ; and those who did connect the two would appear to have been rather the idle gossips than those country people who had some real practical knowledge of either or both diseases. The single bond connecting cowpox with smallpox was the occurrence of the word "pox" in each name ; it was a case of the river in Macedon and the river in Monmouth. The jingle of the names had the effect that it often has upon credulous people, whose acquaintance with any matter is more verbal than real. Those who had been unlucky enough to catch the cowpox on their fingers from milking cows with sore teats had an instinctive notion why the affection had been called a pox ; but the officious gossips who knew no more than

¹ *The Origin of the Vaccine Inoculation*, 1801. In the *Inquiry* he says that the oldest farmers knew it by the name of cowpox as far back as they could remember, but it had not occurred to them to connect it with smallpox. Jenner's *Inquiry*, although it counts among the sacred books of the profession, is not much read now, even by the officials whose business is with these matters. Thus, a former Superintendent of Statistics for Scotland (Dr. Stark), in his report for 1870, p. 32, deplors the ignorance of the public about vaccination, and begins a homily to them with the remark that it was Jenner who first called the infection "cowpox."

the name of the thing must needs make a fine romantic legend out of it. You have had the cowpox, they said to the milkers ; therefore you cannot take the smallpox.

It is a mistake to suppose that this now famous legend had been a slow growth, with its roots deep down in experience. It was by no means universal in the grazing districts of England where cowpox was found ; Mr. Jacobs, a prominent Bristol solicitor about Jenner's time, came forward to testify that he had twice suffered from cowpox sores when he was a lad working upon his father's farm some forty years before, but he had never heard it said that the cowpox kept away the smallpox.¹ Dorsetshire is known to have been one indigenous source of the cowpox-smallpox legend about the year 1774 or earlier ; but it is not impossible, and, if we may trust the evidence collected by Pearson (Nov., 1798), it is probable, that a corresponding legend may have sprung up independently, and through the operation of the same legend-making causes, in other English dairy-farming districts. Those who professed to have discovered the same country legend in remote parts of Europe, such as Holstein and Provence, after Jenner's writings had become universally talked about, do not seem to have allowed for the possibility of its having been a mere after-thought on the part of their not very discriminating informants.² In several of these foreign

¹ *Contributions to Physical and Medical Knowledge*, edited by Beddoes. Bristol, 1799, p. 420.

² For an account of the Holstein cowpox and the legend, see the summary of essay by Hellway, in Hufeland's *Bibliothek der praktischen Heilkunde*, 1801. Hellway was the author who first in-

versions, it is sheep-pox that figures prominently, a disease of the cattle being but vaguely hinted at.¹ One German of credit, Dr. Heim, of Berlin, who performed the first vaccinations in that capital, distinctly tells us that he had in his youth heard from his father of milkmaids catching sores on their hands from the cows' teats, but there had been no mention of protection against smallpox.² In France they had no name for cowpox at all, and therefore no basis for the legend. The stories about deliberate cowpoxing in Beloochistan and the Peruvian Andes are hardly to be taken seriously as evidence of the world-wide prevalence of a legend depending upon the jingle of words, which Jenner dragged from obscurity in the western counties. Even in the eastern counties, where the affection was called papox, the legend of protection from smallpox was not indigenous before Jenner.

The origin of the legend is not difficult to account for. The notion of warding off, antagonizing, or charming away disease is old enough, and has pervaded the medical beliefs of the learned as well as of the vulgar. The special fitness of the charm or antidote depended usually upon some verbal jingle. The old herb-books are full of fanciful nonsense of the kind. Thus it afforded protection from a mad dog to carry the herb

vented the four spurious varieties ("yellow pox," "black pox," "white pox," etc.).

¹ It was so in the disease near Montpellier in 1781, vaguely reported by Rabaut, a Protestant pastor, and claimed long after, in 1821, by Husson (*Dict. des Sc. Med.*, art. "Vaccination") as the true source of Jenner's cowpox ideas.

² *Hufeland's Journal*, vol. x. pt. 2, p. 187.

hound's-tongue in a packet tied to the wrist ; and the root of the dog-rose was an antidote to the dog's bite.¹ The supposed antagonism of cowpox to smallpox was a verbal jingle of that kind ; it was founded on the similarity of names, and not on any alleged likeness between the two diseases. Certainly for those who knew by inspection what the pox of the cows' teats was, and most of all for those who had suffered the painful and often obdurate ulcers on their own hands, there would be no suggestion of real likeness to smallpox, or of the one disease being in any way related to the other. It was the jingle of the names that brought the two together in the first instance.

The next step in the growth of the legend was also made on the part of the illiterate country people. After the middle of last century most persons in England knew the object of being "cut for the smallpox" ; it was thought better to be cut for the smallpox at one's own convenience than to run the risk of catching the disease when it was epidemic. In the year 1774² it occurred to Benjamin Jesty, a Dorsetshire dairy-farmer in good circumstances, that it would serve as well to be cut for the cowpox ; and accordingly he himself did actually cut his wife and two children for that disease ; that is to say, he inserted the matter of it from a cow's sore teats into their arms by incisions. What followed

¹ Gaidoz, *La Rage et St. Hubert*. Paris, 1887. Chapter i. § 2.

² This date is assigned to Jesty's experiment in the record of the fact on his tombstone in the churchyard of Worth Matravers. Jesty was made a good deal of by the vaccinists who separated from Jenner in 1801-2, and had his portrait painted for the Vaccine Pock Institution. He died in 1816.

is not precisely known, except that the doctor had to be called in ; but it does not appear that Jesty's odd freak found any imitators. However, when so much was being heard on every hand about preventing the smallpox by inoculation of mild varieties of itself, such as the absurdly named swinepox, the fancy about cowpox was pretty sure to be mentioned here or there. It was in some such casual way that Jenner first happened to learn of the existence of the legend. For some years he contented himself with sounding his professional colleagues about it, introducing the topic now and then at their convivial meetings at country inns. The man who knew most about cowpox sores in milkers was Fewster, of Thornbury ; and Fewster, as well as others, had unfortunately good reason to scout the milker's protection from smallpox as an old wife's fable. Whenever Jenner proceeded to air his fancy, he was met with instance after instance in which a cowpoxed milker had been attacked by smallpox like the rest.¹ It was clear that the legend as it stood would not work.

We have all heard how Jenner rose superior to difficulties, and how he resolved not to be baffled ; but few persons know what the difficulties were, and how he set about circumventing them. The way was barred by the hard facts of experience, which country doctors, who knew far more of cowpox than Jenner did, recognised in the way that sensible men always do recognise hard facts. Jenner, however, thought that he saw an opportunity of repeating the success of his cuckoo paper at the Royal Society. As may be seen from his preface,

¹ See Baron i. pp. 48, 49.

the task that he assigned to himself was to reduce a piece of rustic lore to scientific precision. It is highly improbable that he would have ever undertaken the subject, or persevered with it, if he had been as familiar with the nature of the cowpox, whether in cows or in milkers, as some of his medical and veterinary neighbours were. But his knowledge of the affection does not appear to have been more than an acquaintance with the name and the common talk, and with the superficial character of the milkers' sores. One can readily understand Jesty, the Dorset farmer, being misled by the similarity of names, and by superficial aspects of diseased processes. If we are to acquit Jenner of a much graver charge, we shall have to assume that he had no deeper insight into the real nature of cowpox, or the real significance of the name which it had borne for generations, than had Jesty himself. Jenner was, indeed, just the loose-thinking, imaginative sort of person to deal with the matter in a merely verbal way. While his prosaic medical neighbours saw no point of contact between cowpox and smallpox, and while they gave due heed to the abundant experience that cowpoxed 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, and that the cases of smallpox in previously cowpoxed milkers must have been exceptions which he would one day be able to account for.

Meanwhile let us see why cowpox had been so called

by the common people long before Jenner's time, and also why the variolous epidemic which had come across Europe from the East had been named smallpox. We can at the present day deal with these pathological and philological questions more easily than Jenner's contemporaries could; but 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. 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.

The first known occurrence of the name "small pockes" in English writings is in Holinshed's *Chronicle* (1577), under the year 1366, where it is applied to an epidemic called *pestilentia* and *lues* by Polydore Virgil. An epidemic of *pestis* in England in the latter part of the reign of Edward III. meant, in all probability, *pestis* in its technical sense of the plague. It seems to have been a verbal blunder of Holinshed to translate it smallpox; at all events, he gives no reason for departing from usage. But Holinshed's use of the word, although made in error, is evidence that smallpox was known by name in English speech in the Elizabethan period. It is not improbable, indeed, that the word was known in England before the thing itself became at all familiar; it may very well have been a direct importation into the language of the French *petite vérole*,¹ which is the only other European name of the

¹ Moore, in his *History of the Smallpox* (London, 1815, p. 81),

disease constructed on the same lines. There is every reason to think that France had an earlier experience of smallpox epidemics than England had; there were epidemics of it in Paris in 1536 and 1568, and over France generally in 1577 and 1586;¹ and, if we are to be guided by facts as chronicled, and not by vague prepossessions as to the ancient and universal prevalence of smallpox, these were the first epidemics of the disease in France. For the same period there is absolutely no record of the disease being epidemic in England, although it is probable from Holinshed's curious mis-translation of the *pestis* of two hundred years before as "small pockes," that the disease was being spoken of in England in 1577, which was the very year of a great epidemic all over France, and that the name in England was a direct adoption of *petite vérole*.

There may, of course, have been cases of smallpox in England at an earlier period, although that is hardly to be inferred from the mere use of the word *variola* by mediæval English compilers on medicine; the compilers all copied from each other or from some common Galenic-Arabistic source, their dealing with diseases being purely verbal, so much so that a confused observer and empiric practitioner like John Ardern stands out as a brilliant figure because he describes from nature. It is known, however, that smallpox cases did occur in London in the first years of the 17th

says that *petite* was prefixed to *vérole* in France "about the 15th century." But it appears to have been only in the last years of that century that *vérole* itself came into use. See also Littré, *Dict. de la langue Française*, art. "Vérole."

¹ Bohn, *Handbuch der Vaccination*. Leipzig, 1875, p. 7.

century, although probably not in great numbers. The London bills of mortality were published by Captain John Graunt¹ from 1629 onwards; there is no authentic record for earlier years, but even in that year, and for several years after, the deaths from smallpox are a mere trifle, except during the not very severe epidemic outbursts which came at intervals. It is not until 1667 that the total of deaths from smallpox in London becomes a large one every year.² That was the smallpox period which Sydenham lived through, and it seems reasonable to conclude that he was the first English writer on smallpox chiefly because he was the first who saw the disease on the great scale.

But the French had their *vérole* before they had their *petite vérole*, and the English had their pox before they had their smallpox. The sequence is made clear not only by the philology but also by the history. Syphilis, or the pox, overran all Europe as a strange and unfamiliar epidemic in the years 1494 to 1498, and continued with almost unabated virulence until about 1520. The names of the strange malady fluctuated for a time, and were various. In a proclamation of James IV. of Scotland, 22nd September, 1497, with reference to isolating the infected in Edinburgh to the island of Inch Keith, the disease is spoken of by the French name of *Grandgor*, "and the greit uther Skayth."³

¹ *Natural and Political Observations upon the Bills of Mortality*, 3rd ed. London, 1665.

² See the Tables compiled by Guy, *Journ. Statist. Soc.* London, 1882, p. 430.

³ From records of Town Council of Edinburgh, in *Phil. Trans.* xlii. p. 420.

But in France the name was soon fixed as *vérole*, and in England as *pox*. Thus in a petition of Simon Fish to Henry VIII., in 1530, against Romish priests, "the Pockes" is the term used.¹ The name had become a by-word in Shakespeare's time, and was clearly used with reference to the opprobrious disease that had been known in the country for a century. If it was now and then used for the smallpox in the 17th century, it was only a brief aberration from the common usage. A current notion of the present day, that "the pox" originally meant the smallpox, depends upon a curious error which I shall deal with in a note.²

¹ "These be they that corrupt the whole generation of mankind in your realm, that catch the Pockes of one woman and bear them to another," etc. Cited by Beckett, *Phil. Trans.* xxx. (1718), p. 845. In Fabyan's *Chronicle*, which is supposed to have been written not long before his death in 1512, it is stated (Ellis's edition, p. 653) that Edward IV., during an expedition against the Scots in 1463, "was then vysyted with the syknesse of pockys." Of course the name given to the king's malady by Fabyan is of no value as diagnosis; but he would hardly have used the word at all if it had not been then in men's mouths, as it well might be in the very years of his writing, the disease, which was certainly called "the Pockes" in 1530 (as above), having invaded England about 1495-7.

² In Webster's, Todd's, and other dictionaries it is stated that *pox* formerly meant the smallpox, and "was often employed in this signification in imprecations and exclamations." This absurd error is traceable to a note by Dr. Farmer, the commentator on Shakespeare. In *Love's Labour's Lost*, v. 2, a lady in waiting exclaims, "A pox of that jest!" whereon Theobald remarks that the language is unbecoming in a lady. Farmer replies, "But there needs no alarm—the *smallpox* only is alluded to," inasmuch as the jest to which the lady replied was, "Oh that your face were not so full of O's!" *i.e.*, pitted with the smallpox. Even if that be the

The *lues venerea* was called in English the pockys, pockes, or pox for a reason that the student of its history finds no great difficulty in making out, although the name cannot but seem inappropriate to those who have heard of the disease only in its modern forms. A striking character of the great epidemic which began in 1494 was the general eruption on the skin; in some of the contemporary accounts that eruption (now reckoned a "secondary") overshadows all else in the disease. The contemporary accounts were collected in two volumes published at Venice by Luisini in 1566.¹ In another

meaning, it was a special conceit for the occasion, or a play upon the well-known ordinary Shakespearian use of the word. Farmer supports his comment by two references to contemporary usage. Davison, he says, has a canzonet "on his lady's sickness of the poxe." Now, in all the three editions of Francis Davison's *Poetical Rapsodie*, published in his lifetime (1602, 1608, and 1611), the title of the poem is "Upon his Ladies sicknesse of the Small Pocks"; but in a pirated and careless reprint of 1621, from which Farmer had quoted, the word "small" is omitted by the printer, and the name of "Th. Spilman" is also omitted from the foot of the poem, so that Farmer assigned it to Davison, although that poet had been careful to distinguish it, along with poems by Sir John Davies and others in his volume, from his own compositions. That Davison knew the correct use of the French terms also, is clear from the heading to his translation of an epigram by Martial, about drinking out of the same glass—"À Monsieur Naso, vérolé." Farmer's other reference is to an undoubted use by Dr. Donne, in a letter to his sister, of the Pox for the Smallpox. I have found another similar usage by Donne in a letter to Sir R. D., in which he says of "my L. Harrington," that "now they know all his disease to be the Pox, and Measels mingled." But Donne's 17th century abbreviation is exceptional, and never became established.

¹ *De morbo Gallico*, 2 tom. Venetiis, 1566.

of them (by Le Maire) we read that the disease in Savoy was called "la clavela," from the eruption of hard knots, pimples, blebs, etc., on the skin, *la clavelée* being the modern French name given to smallpox of sheep for the same reason. In the monograph by Nicholas Massa, of Venice, which was long regarded as the most authoritative, although Hensler questions its value as first-hand testimony,¹ *pustulæ diversæ et aliæ infectiones cutaneæ* is the first line in his formal definition of the disease. In his fifth chapter, devoted to the *Pustulæ*, he says they occurred over the whole body—on the limbs, on the face and head, and amongst the roots of the hair. In his particular description we find such terms as elevated, tumid, moist; red, livid, whitish; small, dry, itching; broad, flat, soft. They came out comparatively early in the disease (second or third week even), and their outbreak was often the signal for the notorious pains in the head and limbs to abate. In many of the cases the pustules overshadowed everything else to such an extent that no primary lesion was thought of. It is clear that Massa thought the disease was of the nature of an eruption; and it is that theoretical bias which in part leads Hensler to distrust his account. But the term *pustulæ* is used by the contemporary writers generally;² from whom we learn also that the "pustules" broke and became foul, corroding or eating sores, that warty excrescences grew from the floor of the latter, and that fatal bleedings sometimes

¹ *Geschichte der Lustseuche*, Part I., 1783, p. 131.

² See the excellent summary of facts relating to the skin affection of the great epidemic in Häser's *Geschichte der Med. u. epid. Krankh.*, vol. iii. pp. 264 7, 3rd ed. Jena, 1882.

occurred from the sores about the face. It may be conceded that the same term (*pustulæ*) was applied also to the primary lesions, and that much of the description relates to the latter; but the occurrence of *pustulæ malæ per totum corpus* is too explicitly and circumstantially stated to be held in any doubt.

The secondary exanthem of the disease, as we now know it, is all that represents that terrible feature of the great epidemic; it was the original *pustulæ* on the skin that gave it its French name of *vérole* and its English name of pox. Therefore, when a disease of entirely different antecedents and pathology came into common notice,—namely, the contagious pustular skin disease which had been known in Arabia and the East for centuries, and in Europe bore the mediæval Latin name of *variola*,¹—it was called the lesser pox or the small-pox, because it had as its obvious feature an outbreak of true pustules resembling in their common and loathsome aspect, although by no means in minute or distinctive characters, those so-called *pustulæ* which had been the most obvious feature, especially on the face, of the great epidemic that first became notorious as syphilis, having determined the colloquial name of the latter.² In the subsequent history, syphilis lost its more horrible forms of skin eruption, but it retained in England its colloquial name of pox, which had a literal

¹ Used in that specific sense, it is said, first by Constantinus Africanus, who brought the Arabian medical teaching to Salerno about 1060.

² Beckett (*Phil. Trans.* xxxi. p. 56) says, "Great Pockes or Pustules on the surface of their bodies, from whence the Pox is denominated."

meaning only with reference to the original "pustular" type of its secondary on the skin. The pox, or a pox, meant from the first what it still means ; it did not as a rule mean *variola* unless it were qualified as the *small-pox* or *lesser pox*.

Accordingly, when common usage in the dairy districts of England gave the name of cowpox (at what precise date is not known) to a certain typical or characteristic malady of the cow's teats, that name was given in respect of certain well-understood "pocky" characters, in the Shakespearian sense of the word,¹—the foul, ulcerative, and corroding character of the sores on the teats, and their contagious property. It is, indeed, by no means unlikely that it was the sores acquired by the milkers from handling the teats which first led to the affection being named at all ; and there can be no doubt that common usage had fixed upon the salient characters and had recognised the true affinities of the malady when it named it a pox, although it had none of the opprobrious associations of the classical name. Cowpox was the pox of the cows' teats, which milkers were liable to catch ; in Norfolk the name was pap-pox. Its circumstances and mode of production are perfectly simple, and will be stated afterwards in the unpretending language of a Gloucester cow-doctor of Jenner's time (chap. iii. p. 56).

This disease was fancifully represented as an amulet or charm against smallpox, by the idle gossip of credulous persons who listened only to the jingle of the names. The milkers themselves must have had the

¹ As in Hamlet, Act v. Scene 1, 1st clown.

hard teaching of experience and the light of common sense to keep their credulity in check, while the medical men who were called to treat the milkers' sores, as well as the cow-doctors, would be puzzled to see where the resemblance to smallpox came in. A fancy of that kind could not exist along with real, even if empirical, knowledge of the two diseases, let alone the frequent experience that cowpoxed milkers could be inoculated with smallpox, or could catch smallpox like other persons. The fancy was the result of a merely notional, nominal, or verbal dealing with the matter. The kind of apprehension hardly deserves even to be called notional ; for, to a pathologist or epidemiologist, it is as truly nonsense to speak of cowpox becoming smallpox as it is legitimate nonsense to prove that a horse-chestnut is a chestnut horse.

It was reserved for Dr. Jenner to take up that surprising legend, and make it scientifically passable, despite the impatience and ridicule which his prosaic medical neighbours in the cowpox 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. 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 cowpox 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 cowpox, and that the classical pox and the cowpox are in that, as in other respects, closely parallel (see chap. v. p. 119). Jenner was with-

out these modern aids from experiment to keep him right, although his earliest critic, Moseley, saw quite clearly, in 1798, "solely on the ground of analogy and pathology," that cowpox was the *lues bovilla*, and that smallpox and cowpox were "radically dissimilar." But the mere common sense of the case, the obvious concurrence of evidence, the intuitive synthesis, the simple pointing of plain facts, would have kept him right, if he had not been caught up into a seventh heaven of verbal illusion.

The fact that Jenner carried a drawing of a milker's cowpox vesicle to London in 1789, is the first good evidence of his interest in the matter. Hunter's correspondence with him, which went on some two or three years beyond that date, contains no reference to cowpox; and there is no reason to suppose that Jenner dealt with this new subject otherwise than in the haphazard and indolent way in which he had dealt with the cuckoo problem, the hibernation problem, and the migration of birds.¹ From the year 1789, when he had got so far as a drawing of a milker's sore in its vesicular stage, there is nothing more heard of cowpox until 1794, in which year Jenner would seem to have been rather full of the subject. He spoke of it in his correspondence with Cline,² who mentioned it to Joseph Adams, one of

¹ He had promised a paper for the Royal Society on the Migration of Birds ever since 1787. It was printed posthumously in the *Phil. Trans.*, vol. 114 (1824). It is a rambling, rhetorical discourse of no scientific value. Baron gravely tells us that Jenner "ascertained the laws which regulate the migration of birds" (*Life*, vol. i. p. 118).

² Cline to Jenner, 11th Aug., 1796, in Baron i. 134.

the Hunterian set, who made a reference to the supposed antagonism of cowpox to smallpox in the first edition of his *Morbid Poisons* (1795), without mentioning where the idea had come from. He spoke of it also in conversation with his intimate, the Rev. Dr. Worthington, who wrote of Jenner's speculations to his correspondent, Dr. Haygarth, of Chester, a medical celebrity of the time. Haygarth's reply (15th April, 1794) is interesting: "Your account of the cowpox is indeed very marvellous, being so strange a history, *and so contradictory to all past observations on this subject*, [that] very clear and full evidence will be required to render it credible. . . . I trust that no reliance will be placed upon vulgar stories." It is proof, also, of the prevalence of a certain amount of talk on the matter in medical circles in the west country, that Dr. Beddoes, the leading practitioner in Bristol, should have made a passing reference to it among the "Queries respecting Inoculation," which he appended to the translation (London, 1795) of the Spanish treatise on Femoral Hernia by Gimbernat.

It was not until May, 1796, that Jenner took the first step to give effect to his ideas. Having heard of cowpox among the milkers at a farm near Berkeley, he took off some of the fluid from a large vesicle on the hand of a dairymaid, Sarah Nelmes, and on the 14th May inoculated it at two places on the arms of a boy, James Phipps, aged eight years. The experimental inoculation held, just as the accidental inoculation of milkers held, especially at cracks or scratches on their fingers. On the 2nd of July Jenner inoculated the boy with smallpox, by way of proving whether the previous cowpoxing had made him insusceptible of the variolous infection.

In the course of the autumn or winter he put together a number of statements which he had picked up about cowpox in cows or milkers, and several cases of cowpoxed milkers known to himself who had not taken epidemic smallpox subsequently, or who had resisted the artificial inoculation of it.

Out of these materials, along with the experiment on James Phipps, he constructed a paper, and sent it, perhaps accompanied by the drawing of the cowpox on the hand of the milker, Sarah Nelmes, to the Royal Society, either in the end of 1796 or early in 1797. It was handed about, perhaps in an informal way, and was shown by Sir Joseph Banks to Lord Somerville, president of the Board of Agriculture. The opinion formed of it, particularly by Everard Home, was unfavourable, so that after having been shown to the Council of the Society,¹ it was returned to Jenner. The subject was new to science, and the evidence for Jenner's contention must have seemed hardly strong enough to justify the referees in giving the paper a place in the *Philosophical Transactions*. Lord Somerville, however, stated that he had heard from a practitioner at Blandford, in Dorset, that the protective power of cowpox against smallpox was talked of in that county also, which was, indeed, the scene of the earliest known experimental cowpoxing by Farmer Jesty, and probably the native soil of the legend.

There is no exact record of the line taken in the original paper; but we know that it contained only the one cowpox experiment on James Phipps, and that it

¹ Jenner to Moore, about 1809, in Baron ii. 364.

contained neither the horse-grease cases nor the horse-grease experiment, which had no existence until March, 1798. It is probable, therefore, that the famous horse-grease doctrine of the source of all genuine cowpox, if it occurred in the original paper at all, was no more than outlined therein. The rejection of his paper by the Royal Society gave Jenner the opportunity of altering it considerably, before he brought it out in 1798, just as the return of his cuckoo manuscript (at his own request, however) had enabled him at the last moment to introduce the startling novelties described in the foregoing chapter. As no historical scrutiny of the great Jennerian legend can be too minute, it will be proper to consider, before we go farther, what had been the doctrine of cowpox, and the evidence for the same, which Jenner originally offered to the Royal Society.

Uncomplicated with horse-grease, the doctrine of cowpoxing was the simple country tale that milkers who had acquired the pox of the teats on their fingers were protected from smallpox. In dressing this up for the Royal Society, Jenner had, of course, to assume the airs of a man of science, and, above all, to bring in experiments. A century of English science had shown that any doctrine or project, no matter what its dialectical absurdity, was sure of an attentive hearing, and even a warm welcome, if there were a show of experimentation about it. It was not until Jenner had got some experimental evidence that he proceeded to put the vulgar cowpox legend into the form of Royal Society science; without a certain amount of experimental support, he would hardly have ventured to bring it forward at all, for the ordinary common-sense medical experience of

his neighbourhood was dead against the protective idea.

The experimentation was of two degrees: firstly, to inoculate old cowpoxed milkers with smallpox in order to see whether they would take it; and, secondly, to give the cowpox of purpose to a child, and then apply the variolous test. Why any one wanting to get at the truth should prefer experiment to casual experience in the case of old cowpoxed milkers, is beyond comprehension; the real but unavowed and perhaps unconscious object of experimenting upon them was, in fact, to circumvent experience, and to find a "scientific" reason for a comfortable illusion. Jenner accordingly kept silence about the cases of cowpoxed milkers subsequently smallpoxed, which he might easily have collected in considerable numbers from the experience of his own district. He confined his attention to such cowpoxed milkers as had *not* subsequently received smallpox either by accident or design; and these cases he adduced as experimental proof of the protective power of cowpox.

In two or three of them, the experimental test had been merely the "exposure" of the cowpoxed person to the contagion of smallpox—as if the majority of adults and elderly persons in those days had not been equally exposed with equal immunity. In a few others the experimental proof was discovered retrospectively in the failure to inoculate them with smallpox when others were being inoculated; but it was not attempted to prove that these failures in cowpoxed adults were more frequent than in adults not cowpoxed. Two or three more were variolated by Jenner himself with the particular intention of testing their resistance acquired

through cowpox. Quite elderly milkers were chosen, including worn-out paupers, in order to prove that the lapse of time did not weaken the resistance—as if advancing years did not also weaken the susceptibility to the smallpox virus.

But it is when we come to the ethical credit of Jenner's original proofs of protection by experimental test that we have most reason for amazement. If his logic was bad, his candour was worse. "I conceived it," he wrote, "to be of the greatest importance in conducting these experiments to attend to the state of the variolous matter previous to inserting it into the arms of those who had gone through the cowpox." The attention which he wanted paid to "the state of the variolous matter" was exemplified in his own dealings with case iii. John Philips, cowpoxed at nine, then aged sixty-two, was tested for protection by inoculating him with variolous matter "taken from the arm of a boy *just before the commencement of the eruptive fever.*" I must leave the full significance of this artifice to be made clear in chapter vi., on "The Variolous Test"; but I can anticipate so far as to say that the method of inoculation which Jenner warned his readers to use *in their tests*, if they would avoid "much subsequent mischief and confusion," was simply the extreme form of the bogus methods of Gatti and Daniel Sutton, whereby the effect of inoculation was reduced to the mere shadow or formality of smallpox. The matter for inoculation was not taken from a natural or accidental eruption of smallpox; it was taken from the local pustule alone of an artificial inoculation, and it was taken from the very earliest period of the local pustule

at which any fluid could be got at all, or "just before the commencement of the eruptive fever." By that means, as a French variolator of the time reports, "the smallpox becomes at length weakened to the point of nullity, so that the last inoculations are without effect."¹ The deliberate choice of the merely serous fluid from the merely local pustule of a previous inoculation on the arm made the absence of anything like effective variolation a certainty. That was how Jenner himself circumvented the damning truth of ordinary experience by the method of experiment, and that was how he earnestly desired that all others should try the variolous test after him. A mode of inoculation was coolly chosen, which was likely to produce the minimum of effect; and when the minimum of effect was produced, the previous cowpoxing of the individual got the credit of it.

It is not surprising that the Royal Society should have found Jenner's experimental proof of protection both meagre in quantity and doubtful in quality. But the paper might still have been made a valuable one by giving in it a precise account of the cowpox itself, which was a curious and hitherto undescribed disease. The paper contained no such precise account. It can hardly have been so dominated by crude theorizings about horse-grease as the later form of it, the *Inquiry* of 1798; but the opportunity of giving a full, candid, and scientific account of cowpox was not embraced. It does not appear that Jenner had ever any intimate first-hand knowledge of cowpox in the cow, such as

¹ Salmade, *La Pratique de l'Inoculation*. Paris, An. vii. (1798) p. 51.

Clayton, the Gloucester cow-doctor, had (see chap. iii.), or such as Ceely acquired by diligent observation in the Aylesbury district forty years later. He knew, however, that it was an ulcerous condition of the teats, which "the cow-leech usually kept in check by escharotics," that it was a local malady, and that it was catching only through contact with the matter both to other cows and to the milkers. Of the milkers' sores he may well have had a more particular knowledge, for they were not so very uncommon, and much more easily studied. He knew them to be of the nature of painful, phagedenic ulcers, which varied in severity or inveteracy, sometimes taking a long time to heal; they began in a big whitish or bluish-white bleb, almost the size of a six-penny piece, as his own picture of Sarah Nelmes' hand clearly showed; the tumid white skin shrivelled after a week or two, and either broke to become an open sore, or formed a crust (as the sores on the cow's teats were apt to do), beneath which a greyish foul ichor would continue to be produced for some time. It was not a nice disease, any way one might look at it; and Jenner ought to have known why the dairy folk had instinctively called it a pox.

Jenner's contribution to the scientific knowledge of it in milkers consisted of little more than the good coloured plate of the infection on the hand of the dairymaid. He does not even say whether the vesicle in that case became the painful ulcer that it usually became; he is content to let the reader go away with the impression, for the particular case which he illustrates, that the disease was a vesicular "eruption." When Ceely came to deal with the subject in a scien-

tific manner, he represented the successive stages of pimple, vesicle, and ulcer side by side ; and any one may see that the ulcers in Ceely's plates¹ have "specific" characters of the several types of indurated and inflammatory. No plate was given of the inoculated disease in the boy Phipps ; but the details in the text are rather more full than in the case of the dairy-maid from whom the infective matter was taken. I now give the sentence on which the emphasis was intended to fall : "The appearance of the incisions in their progress to a state of maturation were much the same as when produced in a similar manner by variolous matter."

That statement really amounts to little ; it merely tells us of appearances presented by the incisions in their progress to a state of maturation ; but the language is the old terminology of smallpox inoculation, and the impression left upon the not very critical reader is that cowpox was a form of smallpox. It is possible that Jenner may have so believed, notwithstanding the total unlikeness of the ulcers on the cow's teats or milker's fingers to the contagious skin-eruption of man ; it may never have occurred to him to ask himself why cowpox had been called a pox in colloquial speech long before his time. It is conceivable that his ambition to find a scientific basis for the legend of cowpox protecting from smallpox blinded him to obvious facts. But that can never justify him in coming before the Royal Society and the medical profession in the way that I have now to speak of.

¹ *Trans. Prov. Med. and Surg. Association*, 1840 and 1842.

The title of Jenner's cowpox paper is: "An Inquiry into the Causes and Effects of the Variolæ Vaccinæ, a Disease discovered in some of the Western Counties, especially Gloucestershire, and known by the name of the Cowpox." An objection might be taken to "discovered," but let that pass. The leading line in this full and learned title is Variolæ Vaccinæ, which is the only name in the short title. Now Variolæ Vaccinæ is Latin for *smallpox of the cow*. An affection of cows and milkers, which had been known to country people for generations as the cowpox, is suddenly introduced to the learned, who had never heard of it before, under a brand-new name. The new name is put in the forefront of the title, it overshadows the old country name both by its prominence and by its semblance of scientific precision, and, for purposes of short reference, it becomes the sole name. This startling novelty is on the title-pages, and only on the title-pages. Jenner never says, in the preface or text, that the name is a new one, hitherto unheard of in veterinary or medical writings; he never says a single word to justify its invention; he never once uses it in the preface or text at all. But there it stands in the title as the full, correct, and scientific name of the disease, to be copied in journals and repeated in a hundred ways when not another word of the essay would be copied or repeated, carrying with it, in short, all the power over the ideas that a descriptive or suggestive synonym for an unfamiliar thing does naturally carry with it.¹

¹ Jenner never publicly defended the innovation, but the following jottings were found among his posthumous papers, and printed by Baron (ii. 30):—

As one subterfuge entails many more, so Jenner's misleading title-page led him into the suppression of material facts and the suggestion of false issues throughout his text. Only one instance concerns us at the present stage, the great historical instance of his first vaccination upon James Phipps. The incisions on his arms, we are led to believe, went on at first very much as if he had been cut for the smallpox itself; on the ninth day he was perfectly well; there was some erysipelatous redness, "but the whole died away (leaving on the inoculated parts scabs and subsequent eschars) without giving me or my patient the least trouble." Very hearty and reassuring, no doubt; but the modest parenthesis about subsequent eschars is the cloven hoof peeping out. The meaning of eschars following the first encrustation of the cowpox sores on the arm is made quite clear to us by the narratives of more candid and honourable men who have vaccinated with matter direct from the cow's teats or the milker's fingers.

To take an instance from the very earliest vaccinations after Jenner's own, those described by Hughes, of Stroud:¹ William King, aged fifteen, was inoculated in December, 1798, with matter one remove from that taken by Jenner himself from a poxed cow at Stonehouse; on the tenth day the lad had the efflorescence or areola just as James Phipps had; on the eighteenth day "the

"The origin of smallpox is the same as that of cowpox; and as the latter was probably coeval with the brute creation, the former was only a variety springing from it. On this ground I gave my first book the title of 'An Inquiry into the Causes and Effects of the *Variolæ Vaccinæ*'—a circumstance which has since been regarded by many as the happy foresight of a connexion which was destined by future evidence to become more warranted."

¹ *Med. and Phys. Journ.* i. (1799), p. 318.

central scab put on the appearance of an eschar ;” on the twenty-ninth day the eschar came away, leaving an ulcer a quarter of an inch deep, which was treated with mercurial ointment and gradually healed up.

That is the meaning of the furtive parenthesis, “leaving on the inoculated parts scabs and subsequent eschars.” If we allow for the eschars coming into view after the superficial crusts had fallen, for the gradual exfoliation of the eschars (usually helped by poulticing), for the filling up of the ulcerous cavities by granulations, and for the covering over by new skin, we shall have to conclude that James Phipps, even if he were “perfectly well on the ninth day,” had sore arms for several weeks. The two places on his arms could have been no more than healed on the 2nd of July, when he was tested with smallpox ; according to the usual practice, the smallpox matter would have been inserted on the arms near to the place of vaccination ; and, under the circumstances, it would not have been surprising if the local pustule had failed to come to maturity, even assuming that Jenner had used a more certain means to inoculate the smallpox than the bogus method of Sutton which he advised his readers to use in their tests. We do not know that the local variolous pustule was actually kept back in the case of James Phipps ; Jenner does not say so ; he says merely that “the same appearances were observable on the arms as we commonly see when a patient has had variolous matter applied, after having either the cowpox or the smallpox.” When he was tested a second time, “no sensible effect was produced on the constitution.” “Poor Phipps,” as Jenner called him, was often tested and never “took” ; he was a poor,

ailing creature, suspected of phthisis, but perhaps only scrofulous ; he was not a fair subject for trying small-pox inoculation upon.

All things considered, it was not to be wondered at that the referees of the Royal Society declined to recommend Jenner's cowpox paper for publication in the *Philosophical Transactions*. There was not a very high standard of critical insight at the Society under the presidency of Mæcenas Banks ;¹ but there would have been at all events an appreciation of authenticated details, of plain matter of fact, of directness, and of all such qualities by which the good faith of a scientific worker would be guaranteed even if mistakes lay concealed in his observations and fallacies in his reasoning. Jenner had everything in his favour at the Royal Society. His previous communication had been received with favour and even indulgence ; he had been elected a fellow a few months after its publication ; Banks, the president, was his friend ; Everard Home (whom he blamed most of all for the rejection of the paper on cowpox) had been his fellow-boarder at John Hunter's five-and-twenty years before ; he had found a novel subject in an undescribed disease of scientific interest and of practical importance to milk producers and milk consumers. The reasons that could have led to the paper being returned to him can only be guessed ; but we shall not go very wide of the mark if we guess them to have been a certain meagreness in the original observa-

¹ See Whewell, *History of the Inductive Sciences*, with reference to the reception of Thomas Young's undulatory theory of light (1802).

tions upon cowpox, a certain secretiveness in the manner of their setting forth, a suspicion of irrelevancy or one-sidedness in the cases of protected milkers, and a pervading sense of something improbable in expounding the properties of such a disease as cowpox under such a title as Smallpox of the Cow.

CHAPTER III.

JENNER'S "INQUIRY."

THE historian of the Cowpox Legend has always a double thread to unwind: on the one hand, the secret history of Jenner's project, as we can now follow it by the help of posthumous documents; and, on the other hand, the history of it as it was presented to and received by the public and the medical profession at the time. If the profession and the public had been permitted to know then all that is known now (not reckoning the practical failure of cowpox to exterminate smallpox after ninety years' trial), they would probably have found out Jenner to be the vain, imaginative, loose-thinking person that he certainly was by nature, and they might have so acted as to prevent him from becoming the impostor and shuffler that the course of events made him.

After the refusal of his paper on Cowpox by the Royal Society, Jenner resolved to publish it on his own account. We know from his biography that he had resolved to do so in the autumn of 1797; so that it was not the sudden accession of new matter in March, 1798, that induced him to offer to the public that which the Royal Society had refused, although the fresh evidence doubtless served to hasten the execution of his resolve.

Jenner did not take his rebuff by the academical men of science in the chastened spirit which such rebuffs ought always to create in us. On the contrary he bore a grudge against Sir Joseph Banks and Sir Everard Home for years after. But he took the opportunity, all the same, of amending and fortifying the argument of his paper.

The year 1797, in which he had his manuscript returned, saw the real adoption of the famous doctrine of horse-grease as the one and only source of genuine cowpox. It is true that cases i., ix., and x. in the casual lot of old cowpoxed milkers are introduced as proofs of the horse-grease origin of cowpox; and these cases may have been in the original paper. If so, the evidence offered to the Royal Society on the origin of cowpox from the horse would have stood as follows:—

CASE I.—Joseph Merret remembered having been long ago, in 1770, at a farm where several horses began to have sore heels, which he frequently attended to. The cows soon became affected with the cowpox, and *soon after* several sores appeared on his hands. Conclusion: Joseph Merret carried horse-grease to the cows.

CASE IX.—Not quite so long ago, in 1780, William Smith, in this parish, was on a farm where a horse had sore heels, and it fell to his lot to attend him. The cows on the farm developed cowpox, "and *from the cows* it was communicated to Smith." In 1791 Smith again caught cowpox sores on his fingers at another farm, there being in that case no record of grease among the horses. In 1794 he acquired cowpox ulcers a third time in milking cows, the relevant circumstance of greased horses being again absent.

CASE X.—Sixteen years before, in 1782, Simon Nichol lived as a farm-servant with Mr. Bromedge. He had to apply dressings to the sore heels of a horse, and at the same time assisted in milking the cows. "Not until several weeks after he had begun

to dress the horse" did cowpox occur among the cows. He quitted Mr. Bromedge's service *without any sores upon him*; but, soon after going to his next place, his hands became affected with cowpox sores. "Concealing the nature of the malady from his master, he was employed in milking, and the cowpox was communicated to the cows."

These artless reasonings were hardly up to the standard of Newton's Rules of Philosophizing, or of any other rules of evidence which the Royal Society was in the habit of applying to the matters that came before it. Even Jenner himself must have been conscious that the evidence for horse-grease, assuming that he had given it a place in the original paper, wanted strengthening. Accordingly we find him, in the course of the year 1797, setting about new inquiries on horse-grease and its relation to cowpox. Biographer Baron's account of these researches is, that Jenner in 1797 "made many efforts to generate cowpox from the heel of the horse." The sole ground of this grandiose assertion is Jenner's own statement that he "sent a messenger to Bristol to procure virus [from the horse] in vain. I even procured a young horse, kept him constantly in the stable, and fed him with beans in order to make his heels swell; but to no purpose." This beautiful experiment having failed, the research was laid aside until February, 1798, when three cases occurred in Jenner's parish of stablemen with sores on their hands, caught, it was supposed, from dressing the heels of a greased horse. At the same farm cowpox occurred on the cows' teats about the same time; so that there was now an abundance of material within Jenner's reach.

On the 16th of March he inoculated a child on the

arm with matter from a horse-sore in one of the stablemen, and on the same day he inoculated another child with matter from a cow's teat. From the latter child he continued the succession of cowpox matter upon children's arms through four removes, and on the 24th of April he left Berkeley for London with the manuscript and drawings of the *Inquiry* in his pocket. The preface of the *Inquiry* is dated (from London) on the 21st of June, and in a week or two after it was in the hands of the booksellers, a quarto of some seventy pages, in the largest of type, and with the widest of margins, illustrated by four coloured plates, and costing seven shillings and sixpence.

The name of Variolæ Vaccinæ on the title-page, without any apology for it, or even a single repetition of it, in the text, was Jenner's master-stroke. Next to his title-page in effectiveness was his very adroit preface. Some few would read the book, more would read the preface, and most would get their impressions from the title alone. The preface is in the form of a letter to Dr. Parry, of Bath.

"MY DEAR FRIEND,—

"In the present age of scientific investigation, it is remarkable that a disease of so peculiar a nature as the Cow Pox, which has appeared in this and some of the neighbouring counties for such a series of years, should so long have escaped particular attention.

"Finding the prevailing notions on the subject, both among men of our profession and others, extremely vague and indeterminate, and conceiving that facts might appear at once both curious and useful, I have

instituted as strict an inquiry into the causes and effects of this singular malady as local circumstances would admit."

As coming from a fellow of the Royal Society located in the very centre of the cowpox districts, nothing could be more in character than that preface. The hour was come, and the man. A peculiar country disease had been long known, but had hitherto escaped particular attention ; but the scientific spirit of the age had penetrated to it in the person of Edward Jenner, M.D., F.R.S. ; and the extremely vague and indeterminate notions hitherto held about it by rustics and country practitioners were now to vanish before a strict inquiry, and to be replaced by scientific facts at once curious and useful. As a programme this was singularly in keeping with the fitness of things. It was exactly what we had a right to expect, what we all knew that it was the business of science to do. When a fellow of the Royal Society, adorning the vocation of a country doctor, promised to substitute strict inquiry for the extremely vague and indeterminate notions prevailing on a curious subject among men of his own profession no less than among others, there was every reason to expect that he would be as good as his word ; it was just the sort of thing that the Royal Society and its several fellows were specially constituted for and dedicated to. The modest and at the same time firm tone of this preface, from "My dear Friend" down to "as strict an inquiry as the circumstances would admit," could not fail to bespeak confidence in the author, the more so that he had already earned for himself the

highest scientific affix to his name by using well his rustic opportunities.

If we are ever disposed to complain of the laxity of criticism which allowed Jenner's nostrum to pass into currency as a good thing, let us remember what a *coup de main* he was able to execute. The fabric of things is based in a sense upon credit ; and the world was very willing to extend its credit to one whose pretensions as an innovator were justified equally by his scientific rank and by his unique opportunities. Nowadays we can bring historical scrutiny to bear upon these events ; and in the way of such scrutiny we may now proceed to inquire whether the pretensions of Jenner's preface were warranted by his text.

The notions concerning cowpox prevalent among his medical colleagues were, he says, "extremely vague and indeterminate." Now these are just the terms in which Jenner's medical neighbours in Gloucestershire were wont to characterize the popular fancy, that the cowpox of milkers protected them from smallpox. We have this important piece of evidence on the authority of Jenner himself, as reported by his biographer. Dr. Baron writes :¹ "Dr. Jenner has frequently told me that, at the meetings of this Society [the Convivio-Medical, which met at the Ship at Alveston in the southern division of the county, and was attended, among others, by Fewster, the chief authority on cowpox], he was accustomed to bring forward the reported prophylactic virtues of cowpox, and earnestly to recommend his medical friends to prosecute the inquiry. All

¹ *Life of Jenner*, i. 48.

his efforts were, however, ineffectual ; his brethren were acquainted with the rumour, but they looked upon it as one of those vague notions from which no accurate or valuable information could be gathered, especially as most of them had met with cases in which those who were supposed to have had cowpox had subsequently been affected with smallpox."

These were the very men whom Jenner, in his preface, included among those who held "extremely vague and indeterminate notions" on the subject of cowpox. But the vague notions were not theirs ; they were the mere idle talk and old wives' gossip of the country side, suggested by the jingling sound of "cowpox—smallpox," and suited to the general character of medical folklore, especially to the wide-spread belief in protection or cure by means of charms or amulets. Fewster and the rest knew that there was nothing in it ; and in course of time they came to regard Jenner as a bore, when he persisted in taking the protective virtues of cowpox seriously, against their own abundant experience to the contrary. Jenner, however, had one great advantage over them—he was a fellow of the Royal Society ; it was no less than his prerogative, as a man of science, to reduce the common notions about cowpox to the scientific scale. Only, he ought not to have led the world to believe that his professional neighbours shared these vague and indeterminate notions. They had good reason, as men of experience, for not sharing them ; and Jenner had good reason for knowing their invincible scepticism. So long, however, as he himself kept to his scientific task of instituting as strict an inquiry as local circumstances would admit, his rather unkind

imputation of vague notions in the minds of other medical men might pass.

Besides the medical practitioners in the cowpox districts, there was another class of men, the cow-doctors and horse-doctors corresponding to the subsequently organized veterinary profession, who had a knowledge of these matters, empirical perhaps, but certainly not vague and indeterminate. Whatever the state of education formerly among the veterinarians, there had never been lacking among them men of sagacity and natural powers of observing. One such practitioner, Clayton, of Gloucester, who attended at most of the dairy farms within a radius of ten miles of the city, was induced to put his experience of cowpox on record for publication in the *Contributions to Physical and Medical Knowledge*, issued by Dr. Beddoes, of Bristol, early in 1799, a volume which had the honour of publishing also the first researches of Humphry Davy. Clayton gave his evidence as follows¹ :—

That the chief diseases of the cow are the lough, swellings of the udder, and cowpox ; the two former are the most common, the latter being rarely seen except in spring and summer :—

That cowpox begins with white specks upon the cow's teats, which, in process of time, ulcerate ; and, if not stopped, extend over the whole surface of the teats, giving the cow excruciating pain :—

That, if this disease is suffered to continue for some time, it degenerates into ulcers, exuding a malignant and highly corrosive matter ; but this generally arises from neglect in the incipient stage of the disease, or from some cause he cannot explain :—

That this disease may arise from any cause irritating or excoriating the teats ; but that the teats are often chapped without the cowpox succeeding. In chaps of the teats they generally swell ;

¹ *L. c.*, p. 387.

but in the cowpox the teats seldom swell at all, but are gradually destroyed by ulceration :—

That this disease first breaks out upon one cow, and is communicated by the milker to the whole herd ; but, if one person was confined to strip the cow having this disease, it would go no farther :—

That the cowpox is a local disease, and *is invariably cured* by local remedies :—

That he never knew this disease extend itself in the highest degree to the udder, unless mortification had ensued ; and that he can at all times cure the cowpox in eight or nine days :—

That he is conversant with the diseases of the horse, and extensively employed, particularly in curing the grease :—

That he cannot recollect ever to have had horses with the grease and cows with the cowpox under care at the same farm :—

That he is very certain he has frequently had cows with the cowpox, where no horses whatever have been kept :—

That the grease is most prevalent in the winter, at which time he has never known the cowpox to occur.

These depositions of Clayton, the veterinarian, were taken down by Cooke, a surgeon apothecary in good practice in Gloucester, who adds to Clayton's statement : "There is little variation from this account in the information I have obtained from some of the most respectable dairy-farmers in this neighbourhood. Those who have seen the cowpox among their domestics all agree that, if they have been soon afterwards inoculated for the smallpox, they have had the disease very slightly ; but, since the late general inoculation, are as fully satisfied that many have had the smallpox in a more decided manner who some years before had the cowpox very severely." He gave also the notorious case of a cowpoxed farmer who had died of the smallpox.

More evidence of the same kind was soon forthcoming,

to which I shall refer in the next chapter, dealing with the reception that the *Inquiry* met with from the profession and the public. I have given here the experience of the best employed veterinarian in Jenner's own county merely to show what stores of information were at his service if he had cared to use them. Beddoes sent him a proof of the Gloucester evidence; he sent also another paper from his forthcoming volume, by Thornton, a surgeon of Stroud, who had been inoculating the cowpox independently of Jenner in 1798, and with rather startling results. Neither the veterinary experiences nor the medical, as we shall see later, were such as suited Jenner; and this is how he replied to Beddoes, on 26th February, 1799:

"I have neither the leisure nor inclination at the present moment to enter into an examination of their arguments, much less shall I attempt to refute the opinions [why "opinions" ?] of either of these gentlemen. . . . The same equitable tribunal [the public], perhaps, will not fail to discriminate between the man who sedulously employs the greatest part of his time in making experiments for the complete investigation of a confessedly complex subject, and him who appears peremptorily to decide on the truth or falsehood of a theory, on the supposed authority of a few solitary instances, which after all may have been mistaken or misunderstood."

Here we have the same lofty tone as in the preface of the *Inquiry*, coupled with a bold disparagement of evidence far more comprehensive on the veterinary side, and far more fully and accurately recorded on the medical side, than his own. This reply to Beddoes is the

beginning of the long course of bullying, and most effective bullying, by means of which Jenner bore down all honest experience adverse to his own pretensions. Every candid reader, every man of the world, who has gone through this chapter so far, will have begun to see that Jenner is not the sort of person who can be taken at his own valuation. Let us then scrutinize this inquiry, "as strict as the local circumstances will admit," these labours of "the man who sedulously employs the greatest part of his time in making experiments for the complete investigation of a confessedly complex subject."

The only real experiment in the paper on cowpox, as originally offered to the Royal Society, was the inoculation of James Phipps ; the results of it, as we have seen, were recorded with a brevity which enabled Jenner to suppress the true and suggest the false. It is absurd to claim the dozen old cases of cowpoxed milkers, who were subsequently inoculated with smallpox, as experiments ; there were many cowpoxed milkers, as Cooke's inquiries showed, who submitted to inoculation along with others, whenever a general inoculation was afoot ; and Jenner's cases were only a few, favourable to his contention, which he happened to have inoculated in the course of his own business or to have heard of. So far from "sedulously employing the greatest part of his time in making experiments for the complete investigation of a confessedly complex subject," he himself stands for the man who "peremptorily decides on the truth or falsehood of a theory, on the supposed authority of a few solitary instances."

As regards his great doctrine of horse-grease being

the only source of genuine cowpox, his paper in its original form did not contain a single experiment or even a single fact to show that horse-grease ever gave rise to the pox on the teats of cows. Sarah Nelmes, whose cowpox sore furnished the virus for the solitary James Phipps' experiment, is described simply as having caught cowpox from milking her master's cows, the disease having originated in a cow bought at a fair, and therefore spontaneously, on Jenner's own admission. Whatever experiments he made between the return of his paper from the Royal Society and the publication of the *Inquiry* were all done in the course of five or six weeks in March and April, 1798. It may be conceded, however, that it was the method of experiment which Jenner used, in so far as he used any method at all; whereas his country neighbours merely took facts as they came, and reasoned upon them in the ordinary way.

The ordinary experience of cowkeepers and cow-doctors, as we have seen, was that cowpox arose here or there, by some concurrence of circumstances, in a particular cow, and was transmitted to other cows by the matter on the hands of the milker. As Clayton, of Gloucester, said: "if one person was confined to strip the cow having the disease, it would go no farther." Cowpox, in fact, arose "spontaneously" in some one cow, on the top of some common affection, such as chapped teats, or an eruption of pimples brought out by the spring season or by an over-distended state of the gland; although chapped teats or pimples did not always end in cowpox. Neglect, as Clayton said, had a good deal to do with it; and, of course, the ruthless necessity

of relieving the turgid organ by "stripping" the teats tended to aggravate any small beginning of soreness upon the latter. That was the rational or common-sense view of how the pox of the teats arose in a cow here and there, and it was abundantly confirmed by Ceely forty years after. The cowpox was "spontaneous," as the phrase ran; but it became infective also, generally going the round of every cow in the same shed, and very commonly affecting the milkers with painful sores on the fingers, and with swollen and painful glands in the armpits, which caused them to go about having their shoulders raised in so characteristic a way that every one knew what was the matter.

Jenner, in a modest footnote to the first edition of the *Inquiry*, admitted that there was such a thing as spontaneous cowpox of the cow's teats, "and instances have occurred, though very rarely, of the hands of the servants employed in milking being affected with sores in consequence, and even of their feeling an indisposition from absorption." But why "very rarely"? Until Jenner appeared upon the scene, this was the only cowpox; the past experience of the country related to that, and to that alone. If the milkers "felt an indisposition from absorption," the pox was the real thing, according to one of his own tests; therefore these awkward cases had to be admitted, but made "very rare." The motive of this deliberate sophistry comes out in Jenner's second pamphlet: "Whether a disease generated in this way [spontaneously] has the power of affecting the constitution in any *peculiar* manner, I cannot presume positively to determine. It has been conjectured [why "conjectured" ?] to have been a cause of the true cow-pox,

though my inquiries have not led me to adopt this supposition in any one instance ; on the contrary, I have known the milkers affected by it, *but always found that an affection thus induced left the system as susceptible of the smallpox as before.*"

It is impossible to show more *naïveté* in begging the question. The "genuine" cowpox of Jenner was, in short, whatever should not be followed by an attack of smallpox, whereas that cowpox was "spurious" which the smallpox contagion gave no heed to ; and that distinction was called for in the first instance by way of confronting the testimony of Jenner's medical neighbours, that they had known many cowpoxed milkers (or, as Baron puts it, "milkers supposed to have had cowpox") who had fallen victims to smallpox in the usual way.

The need having thus arisen to make out some cowpox genuine and some spurious, it remained to take off the arbitrary edge of the distinction by facts or theories. The old spontaneous sort was on the whole spurious, so many milkers having received no protection from smallpox by its means ; the genuine would have to be something special, and obviously it must not be spontaneous. To carry it to a source one remove off from the cow was enough ; and here was the grease of the horse's hocks as if ready for the occasion. That Jenner believed in his heart the grease itself to be of spontaneous origin, we know from his comical attempt to induce it in a young horse by keeping him in the stable and feeding him upon beans.

The curious and possibly misleading thing was that a farrier's or stableman's sore on the finger, caught from a

greased horse, was almost the same as a milker's sore caught from a poxed cow. For an interesting illustration of that fact Jenner was indebted to his experienced neighbour, Fewster, surgeon, of Thornbury, whose narrative is printed in Jenner's second pamphlet.

“William Morris, aged 32, servant to Mr. Cox, of Almonsbury, in this county, applied to me the 2nd of April, 1798. He told me that, four days before, he found a stiffness and swelling in both his hands, which were so painful, it was with difficulty he continued his work; that he had been seized with pain in his head, small of the back, and limbs, and with frequent chilly fits succeeded by fever. On examination I found him still affected with these symptoms, and that there was a great prostration of strength. Many parts of his hands on the inside were chapped, and on the middle joint of the thumb of the right hand there was a small phagedenic ulcer, about the size of a large pea, discharging an ichorous fluid. On the middle finger of the same hand there was another ulcer of a similar kind. These sores were of a circular form, and he described their first appearance as being somewhat like blisters arising from a burn. He complained of excessive pain, which extended up his arm into the axilla. These symptoms and appearances of the sores were so exactly like the Cow Pox, that I pronounced he had taken the distemper from milking cows. He assured me he had not milked a cow for more than half a year, and that his master's cows had nothing the matter with them. I then asked him if his master had a greasy horse? which he answered in the affirmative; and further said that he had constantly dressed him twice a day for the last three or four weeks or more, and remarked that the smell of his hands was much like that of the horse's heels. . . .”

Jenner's account of the grease is condensed into a few vague and useless lines: “It is an inflammation and swelling of the heel, from which issues matter possessing properties of a very peculiar kind, which seems capable of generating a disease in the Human Body (after it has undergone the modification I shall

presently speak of), which bears so strong a resemblance to the Small Pox, that I think it highly probable it may be the source of that disease."

It is one of the evils of making a man a fellow of the Royal Society, that people will be apt not to recognise any subsequent nonsense that he may write, in the name of science, for what it really is. The horse-grease has so strong a resemblance to smallpox, that he thinks it highly probable it may be the source of that disease! But it is only after it has undergone a certain modification that horse-grease resembles the smallpox and may be the source thereof. Here, then, is a malady of a kind that may possibly be induced by feeding a horse with beans to make his heels swell; next, a stableman, "having been appointed to apply dressings to the heels of a Horse affected with the grease, and not paying due attention to cleanliness, incautiously bears his part in milking the Cows, with some particles of the infectious matter adhering to his fingers; a disease is communicated to the cows, and from the cows to the dairy-maids, which spreads through the farm until most of the cattle and domestics feel its unpleasant consequences. Thus the disease makes its progress from the Horse (as I conceive) to the nipple of the Cow, and from the Cow to the Human Subject"—in the form of the epidemic smallpox of history?

No one in 1798 could suppose that there was anything vague and indeterminate in this account of cowpox, and its relation to smallpox, Jenner having stated in his preface that these were just the qualities which he was going to banish from his treatment of the question, by instituting as strict an inquiry as the local

circumstances would admit ; moreover, he had silenced the most experienced veterinarian in the county of Gloucester (who had never met with cowpox and horse-grease together at the same farm, but had often seen cowpox where no horses were kept) with a reminder that he had better not attempt peremptorily to decide the truth or falsehood of a theory on the supposed authority of a few solitary instances, so long as he, Jenner, a fellow of the Royal Society, was sedulously employing the greatest part of his time in making experiments for the complete investigation of a confessedly complex subject.

But the only experiment on horse-grease that he had made, besides that of the young horse kept in the stable and fed with beans to make his heels swell, was the experiment to inoculate a child with the virus of a stableman's horse-sore. Knowing what Jenner did of the nature of horse-sores on stablemen's and farriers' hands, it was a reckless, not to say an unjustifiable thing in him to seek to induce the same on a young child. Moreover, what was the experiment designed to prove more than the inoculations by accident had already taught him? He knew from his quite recent cases of horse-sores on the hands of stablemen, in February, 1798, what the inoculated grease was like ; he knew also that two out of three horse-greased farmers or farriers (cases xiv. and xv. of the *Inquiry*) had received smallpox infection afterwards, the plain inference being that the one infection was nothing to the other. An experimental trial after such an experience could only mean that he was dissatisfied with the experience, that he meant to circumvent the plain

teaching of it if he could, and to tie knots upon the common-sense issues, by a resort to so-called scientific method. As it turned out, his experimental inoculation of the horse-grease virus upon a child resulted in rather more than he found it expedient to disclose.

On the 16th of March, 1798, Jenner took virus from a sore upon the hand of a stableman, Thomas Virgoe, who had been infected while washing the heels of a greased mare, and inoculated it upon the arm of John Baker, aged five years. The record of the experiment is sufficiently brief: "He became ill on the sixth day with symptoms similar to those excited by cowpox matter. On the eighth day he was free from indisposition." So far as the text is concerned, that is all. A coloured plate is given of the boy's arm, representing a stage of the infection probably later than the eighth day, although we are left to guess the date; the large, whitish vesicle has fallen in, there is evidently a sore of some depth beneath the brown sloughing cuticle, and there is an angry, brick-red zone of erysipelas for some distance around. If the child was free from indisposition on the eighth day, it was only because the full force of the filthy infection had still to be felt. A mere look at the collapsed vesicle in the picture will satisfy any practised eye that sloughing ulceration was imminent, and the brick-red colour of the skin around is equally ominous.

It is beyond all mere guessing, however, that the vesicle did become an ulcer; we know that, not from anything that Jenner himself ever disclosed, but from what his biographer, Baron, who seems to have been a simple-minded enthusiast, inadvertently published

long after. In explaining and justifying the horse-grease hypothesis, Baron printed from among Jenner's papers an enumeration of six points wherein inoculated horse-grease resembled inoculated cowpox;¹ one of the points was the "disposition of John Baker's pustule to run into an ulcer." The same enumeration had already been given by Jenner in his second pamphlet (April, 1799); but in the six points, as Jenner had printed them from the copy, the reference to John Baker's "pustule" is merely to its "progress and general appearance," the original clause about the ulcer being conspicuously omitted. That authentic evidence, then, will carry us beyond the eighth day of the case, when the child was "free from indisposition."

There is no doubt that Jenner intends the narrative of this child's inoculation with horse-sore virus to conclude with the reassuring statement that, on the eighth day, he was free from indisposition. It is only in a footnote on a subsequent page, inserted to explain why John Baker was not tested with smallpox after being horse-greased, that we read: "the boy was rendered unfit for inoculation from having felt the effects of a contagious fever in a workhouse soon after the experiment was made." The child, it appears, was rendered unfit for inoculation by unhappily becoming a corpse; he felt the effects of a contagious fever, soon after the experiment was made, to some purpose, for he died of it.

After a year's interval, Jenner wrote of John Baker's case without any euphemisms such as "felt the effects

¹ Baron, i. 248.

of a fever." Having occasion to mention the case in a note in his next pamphlet, he says, with simple inadvertence, that the boy "unfortunately died of a fever at a parish workhouse"; it is not even a "contagious fever." If the fever had been typhus, or scarlatina, or measles, why did he not remove all ambiguity by saying so? Reading between the lines, with the help of horse-grease pathology and Jenner's own plate, we may safely conclude that this child of five, lent for the experiment by poor parents under some cajolery or other, had an ulcerated or sloughing arm from the virulent matter inserted into it, that he had erysipelas (which is both a fever and contagious), that he was sent to the parish workhouse, that he died there, and that this village tragedy was all enacted within a period "soon after the experiment was made." Such is the one experiment with horse-grease which Jenner introduced into the *Inquiry*, and such is the candour of it.

On the same day (16th of March, 1798) that he inoculated horse-grease on one child, he inoculated matter from a poxed cow's teats on another child. He will have us believe that one of the three horse-greased stablemen, John Haynes, carried the infection to the cows. The evidence is of the most flimsy kind; no exact dates are given, nor any full statement of the circumstances. We are merely told that Haynes was daily employed as one of the milkers at the farm, and that cowpox "began to show itself among the cows about ten days after he first assisted in washing the mare's heels." Of course there may have been a number of other things relevant to this outbreak of cowpox, but we are told nothing more; we are not even told anything

about the disease upon the fingers of Haynes, whether he caught it when he first "assisted" in washing the mare's heels, or, as in every one of the cases (i., ix., and x.) of the *Inquiry*, only after the cows had become affected. The concurrence of the two diseases at the particular farm probably meant that there was no more care and cleanliness in the stable than in the cowhouse. Jenner seems to have had a larger experience of these double events than any one else; they reflect somewhat on the ignorance and slovenliness of Jenner's parish, but they do not establish the origin of cowpox from horse-grease.

We come, then, to Jenner's actual experiments with virus from the cow's teats, leaving the theory or reasoning as it stands:—

"William Summers, a child of five years and a half old, was inoculated the same day with Baker, with matter taken from the nipples of one of the infected cows, at the farm alluded to in page 35. He became indisposed on the sixth day, vomited once, and felt the usual slight symptoms till the eighth day, when he appeared perfectly well. The progress of the pustule, formed by the infection of the virus, was similar to that noticed in case xvii. [James Phipps], with this exception—its being free from the livid tint observed in that instance."

Here again the experimentee was perfectly well on the eighth day; but if the "progress of the pustule" was similar to that of James Phipps, the boy Summers can hardly have been perfectly well during the days following the eighth. James Phipps, as we have seen, had subsequent eschars, which meant deep ulcers, which meant slow healing and a good deal of disturbed health for several weeks. Ulceration of the arm has been the almost uniform experience with cowpox virus direct

from the cow's sore teats or from the milker's sore hands, as we shall see in subsequent chapters.¹ Jenner, who was something of an exquisite, spared his readers these unsavoury details whenever he could; only he will persist in dropping hints about eschars and the like, when he might just as well have suppressed the disagreeable facts altogether.

The child Summers ought to be even more famous than the child Phipps, because he is the first vaccinifer on record. On the 28th of March, being the thirteenth day of his infection, matter was taken from his arm and inoculated upon William Pead, aged eight years. Again the narrative touches lightly on some of the aspects of cowpox, while it emphasizes others:—

“On the sixth day he complained of pain in the axilla, and on the seventh was affected with the common symptoms of a patient sickening with the smallpox from inoculation, which did not terminate till the third day after the seizure. So perfect was the similarity to the variolous fever, that I was induced to examine the skin, conceiving there might have been some eruptions, but none appeared. The efflorescent blush around the part punctured in the boy's arm was so truly characteristic of that which appears on variolous inoculation, that I have given a representation of it. The drawing was made when the pustule was beginning to die away, and the areola retiring from the centre.”

The “dying-away pustule” is still a big, whitish bleb with a fallen-in, brownish centre; it is quite probable, from the look of it, that it became an ulcer, but of course Jenner dislikes mentioning things of that kind. We are told of the fever or constitutional disturbance, which was safe neutral ground whereon to make a comparison

¹ See also my *Natural History of Cowpox and Vaccinal Syphilis* (London, 1887), chapters i. and v.

with smallpox ; and about the efflorescent blush, which might also be common ground in almost any inoculated infection of the kind. But of those features wherein the infection was wholly unlike even the local pustule of inoculated smallpox, we are told nothing.

From the arm of the boy Pead "several children and adults were inoculated" on the 5th of April, or the ninth day. It is clear, from the text, that most of these did badly, although we have no details ; but from one of them, Hannah Excell, a child of seven, matter was taken on the 12th April, or the eighth day, and some of it inoculated upon four children, three of whom did badly (no particulars), while the other, Mary James, whose vesicle "scabbed quickly without any erysipelas," became the vaccinifer of J. Barge, a child of seven. The date of Barge's vaccination is not given (nor indeed any other particular information about the case) ; but it must have fallen between the 19th and 24th April. On the latter date Jenner left Berkeley for London, taking with him his MSS., his drawings, and a sample of vaccine lymph dried upon a quill, being part of that which he had taken from Hannah Excell (third in order from the cow) on the 12th of the same month.

He remained in London until the 14th of July, getting the *Inquiry* printed, and otherwise looking after the interests of his strange project. The preface of the *Inquiry* bears the date of 21st June, so that we may assume that it was ready for delivery at the end of that month, or early in July. Within a week or so of its publication, Mr. Cline, surgeon to St. Thomas's Hospital, used the dried vaccine matter, which Jenner had brought to town, for the inoculation of a boy with hip-joint

disease, having the ulterior purpose of turning the cow-pox sore over the hip into an issue.

These are the facts, and this is how the biographer Baron moralizes upon them: "It is a strange circumstance that the author of that practice, a man known in the highest circles of medical science as worthy of all credit and as an accurate and enlightened observer, should have been unable, *notwithstanding the proofs which his 'Inquiry' contained* of the safety and importance of vaccination, to prevail on one individual to submit to the operation during his stay in London." It was, indeed, very natural that Cline and all Jenner's old set should wait until they had seen the proofs which the *Inquiry* did contain. They knew that the business had been discredited by the Royal Society the year before, notwithstanding the strong interest that Jenner had with Banks and others; and they would have been told by Jenner, when he came to town to have his essay printed on his own account, that it now contained important additions, which might lead his old friends to take a more favourable view of it after they had it in print with the three new coloured plates. Now, when Cline wrote to Jenner on the 2nd August, to tell him of the result of the vaccination in London, he carries the narrative of it to the 11th day of the vesicle, to its subsequent ulceration, to the testing inoculation with smallpox, and to the effects of the latter for three days longer; so that the first trial of the vaccine in London must have taken place not later than the middle of July, or within a week or so of the publication of the *Inquiry*. It was on the 14th of July that Jenner left London.

We have now brought the narrative of events down

to the date of publication, and have anticipated one small fact in the reception of Jenner's project by the medical profession. But, before we enter on the full history of its reception, there still remains to consider the evidence which he offered in the *Inquiry*, that cowpox, whether caught accidentally or given experimentally, did, as a matter of fact, anticipate and ward off the attack of smallpox. It was upon that evidence, and the subsequent corroboration or refutation of it, that the vaccination controversy mainly turned. The name of Variolæ Vaccinæ was accepted as proof enough that cowpox *was* a sort of smallpox of the cow, the doctrine of the origin from horse-grease being passed over with indifference by practical men. The questions that really interested people were whether inoculation with the variolæ vaccinæ, whatever that disease might be, was as good a protection from smallpox as inoculation with variola itself; whether it was unattended by a general eruption; whether it was a milder and safer disease than variola proper; and whether it was free from the great and growing objection to the latter of being a source of aërial contagion. On two of these questions Jenner was pretty safe to get a verdict; on the question of protecting from smallpox he wanted all the ingenuity of his very imaginative and unscrupulous mind to carry him through.

Jenner led off, at page 6 of the *Inquiry*, with a bold statement, which his medical neighbours knew very well to be untrue: "What renders the Cow Pox so extremely singular is, that the person who has been thus affected is for ever after secure from the infection of the Small Pox; neither exposure to the variolous

effluvia, nor the insertion of the matter into the skin producing this distemper. In support of so extraordinary a fact, I shall lay before my Reader a great number of instances." But first the reader is treated to the innocent-looking footnote about genuine cowpox and spurious, the full significance and historical importance of which plea I shall deal with in chapter vii.

Of the "great number of instances" of the variolous test, I have already examined those that were in the original evidence prior to March, 1798. Jenner's opportunities in that and the following month were really considerable; and, in so far as his variolous test was a valid test at all, it was much more to the point to try it upon young vaccinated children than upon a number of old cowpoxed milkers. We are loftily told, however, that it was superfluous, after all that had been said, to try the variolous test upon each of the children whom he had succeeded in infecting with cowpox: "After the many fruitless attempts to give the Small Pox to those who had had the Cow Pox, it did not appear necessary, nor was it convenient to me, to inoculate the whole of those who had been the subjects of these late trials."

It was not convenient to Jenner, because he rushed off to London as soon as he had made these few experiments on children, and without waiting to ask the great question, whether they were, as a matter of fact, insusceptible of smallpox inoculation, or what proportion of them were insusceptible. He asserts, however, that the boy Summers, the first of his series, was tested with smallpox and that "the *system* did not feel the effects of it in the smallest degree"; but he does not say when this was done, nor by whom, nor does he give

any other details. He tells us, further, that two other children, Pead and Barge, were inoculated with smallpox by his assistant (after Jenner had gone on his London visit), who wrote as follows: "On the second day the incisions were inflamed, and there was a pale inflammatory stain around them. On the third day these appearances were still increasing and their arms itched considerably. On the fourth day the inflammation was evidently subsiding, and on the sixth it was scarcely perceptible. No symptom of indisposition followed." This is not very precise and determinate; but, even if we admit that the variolous matter failed of its usual effects upon children, we must bear in mind, not only that Jenner's method for the variolous test was the bogus method of Sutton, but also that the attempt was made at a time when the cowpox sores were quite recent, being either in a state of scabbing, or filled by eschars, or in open ulceration, and that any such active process on the skin, together with the clogging of the absorbent glands by the inflammatory action of cowpox virus itself, would be a sufficient hindrance to the full action of smallpox virus inserted near the same spot, or a cause of irregularities, at least, in its evolution and extent.

We may now sum up the contents of the famous *Inquiry into the Causes and Effects of the Variolæ Vaccinæ*, which Jenner published to the world in the end of June, 1798. The programme of it was one thing, and the execution another. Extremely vague and indeterminate notions were to give place to the results of "an inquiry as strict as local circumstances would admit"; so, at least, the world was modestly told in the

preface. In the text, the unblushing invention of the misleading name of Variolæ Vaccinæ is never once reverted to; the novelty, which none knew to be a novelty, is on the title-page and in the short title of the fly-leaf, but elsewhere it is passed over in discreet silence. The proofs that there existed both a genuine cowpox and a spurious, and that the former came from horse-grease while the latter was spontaneous, were both disingenuous in motive and puerile in effect. The proof of the main thesis, the protection from smallpox, was disgracefully scamped, even assuming that experiments were valid for proof. The average experience of Gloucestershire milkers was ignored, Jenner being well aware that there were quite as many instances telling against protection as there were in favour of that popular fancy; only such cases as supported the notion were adduced, and these were set forth in such loose and meagre fashion as to be worthless according to any strict standard of evidence. Of all the children vaccinated by Jenner, only one was subjected by himself to the variolous test, the result being stated in evasive or ambiguous language. He rushed off to London to publish his *Inquiry*, without waiting to see whether his vaccinations of March and April, 1798, would stand the test, such as it was: only two, or perhaps three, of them were subsequently smallpoxed by his assistant. The test used and recommended was the bogus method of Sutton. Lastly, the similarity of cowpox to smallpox is craftily implied, not in regard to the vesicle and pustule respectively, but in regard to the constitutional disturbance and the efflorescence; while the ulcerous course of the cowpox infection beyond its

vesicular stage, which would have put all affinity to smallpox out of the question and would have inevitably suggested the pox proper, was systematically suppressed. The same suppression was practised in the case of the child whom Jenner inoculated with virus from a horse-sore on a stableman's hand.

It has to be kept in mind that Jenner's contemporaries had not the means which we now have of detecting all this laxity and dishonesty in the form and matter of the *Inquiry*. In considering what reception they gave to the book and to the project, we must endeavour to put ourselves in their place.

CHAPTER IV.

THE RECEPTION OF THE "INQUIRY."

"BEFORE the publication of Dr. Jenner's treatise," writes Denman, a leading physician of the time, "the cowpox was unknown, even by name, to the generality of physicians in the kingdom."¹ When they did come to hear of the disease, it was under the name of Variolæ Vaccinæ (smallpox of the cow), which Jenner had thought fit to give to it for reasons best known to himself. The name was accepted in good faith by the profession in all countries. The first French writers uniformly spoke of the new disease as *petite vérole des vaches*; the Germans at once adopted the synonyms *Kuhblattern* and *Schutzblattern* ("cow-smallpox" and "protective smallpox"); and in Italy it was called *vajulo vaccino* ("vaccinal smallpox"). These terms were gradually superseded by the new word *vaccine*, introduced at Geneva in 1799, which simply meant something pertaining to a cow, and carried no suggestion of disease in general, or of a pox-disease in particular. In colloquial English speech, cowpox continued in use for some time, and was then changed into cow pock. A reason for the change was given by

¹ *Med. and Phys. Journ.*, iii. (1800), p. 292.

a London surgeon, in the preface to an account of a notorious series of vaccinal ulcers at Clapham:¹ he says that he prefers cow *pock*, "as I conceive the word 'pox' to be inapplicable, being the exclusive appellation of syphilitic affections." Not exclusive; for usage in the western counties had given the name of pox to the loathsome affection of the cow's teats for generations past. The same unwarrantable liberty had been taken with the old English name by a German writer shortly before, on the ground that pock was a "milder and more convenient" name than pox.² In the United States, the liberty first taken with cowpox was to make it kine pox, as being "more delicate";³ and, shortly after, kine pox became kine pock, which was doubtless more delicate still.

If it should be said that these changes in an old name were not Jenner's doing, that he was not responsible for them, and that the leading title of the *Inquiry*, Variolæ Vaccinæ, was not put there with any such deliberate purpose as I have asserted, those who so contend are invited to follow closely what was said by critics of the *Inquiry*, and what Jenner said, or caused to be said, in reply. 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. He took some pains to secure its currency, and jealously watched any reference to his innovation in the naming

¹ Pears, *Lond. Med. Rev.*, Jan., 1801, p. 276.

² *Neues Hannövrishes Magaz.*, 1800, p. 58.

³ Waterhouse, *History of the Variolæ Vaccinæ, etc.* Boston, U. S., 1800.

of the disease. The earliest public friend of Jenner's project was Dr. George Pearson, F.R.S., physician to St. George's Hospital, a scholar, and an honourable if not a very clear-headed man. His *Inquiry concerning the History of the Cowpox*¹ followed Jenner's in less than six months, and endorsed it. Pearson, however, was getting dangerously "warm" on more than one side of the mystification; in particular his "Remarks upon the use of the term *Variolæ Vaccinæ*" were such as Jenner could not but read with alarm. Pearson's objection to the Latin name was of the mildly grammatical kind; *variolæ vaccinæ*, or smallpox of the cow, he opined, was a catachresis of speech, as if one were to speak of the plumage of a bear; for it was not ascertained that the cow, or the bovine species, was subject to variolous disease at all.

In his private correspondence Jenner wrote with some asperity about Pearson's exceedingly mild objection to the name; and for public purposes he put forward his clerical neighbour, the Rev. T. D. Fosbroke, to overbear all such inconvenient remarks by a prodigious display of philological and other learning. The clerical scholar wrote as follows in reply to Pearson, signing himself "T. D. Fosbroke, *Vacco-variolist*":² "Every schoolboy knows that the meaning of *variola* is 'freckle' or 'pimple,' and therefore that its modern and forced application to smallpox by no means destroys its original latitude of signification, and indeed real and only one; and, of course, that it may be therefore allowably so used. The Latins knew nothing about

¹ London, 1798 (November).

² *Lond. Med. Rev.*, ii. 482.

smallpox ; how, therefore, could they appropriate the term to the disease in question ? ”

This rubbish was allowed to pass in the medical journal which was specially set apart for the work of criticism ; no one replied, or was permitted to reply, that *variola*, in its technical sense, ever since the middle ages had meant smallpox, and nothing but smallpox, and that cowpox blebs or crusts or sores or ulcerations were neither pimples nor freckles. A “ vacco-variolist ” also came forward about the same time in the columns of the *Gentleman’s Magazine* ;¹ and a second time in the *Medical Review*,² to contradict a London physician (Dr. Hooper), who had pointed out that milkers’ cowpox sores were larger than the pustules of smallpox and otherwise unlike them, Jenner’s advocate taking care to parade the term “ vacco-variolism ” and to denounce the “ malignancy ” of objectors.

It must seem strange to any one who reads Pearson’s *Inquiry* now that it should not have sufficed at the time to show up the artifice of Jenner in re-naming cowpox “ variolæ vaccinæ,” or smallpox of the cow. Pearson made out very clearly, by the evidence which he collected, that the milkers’ cowpox took the form of “ painful phagedenic sores,” often lasting for weeks or months, which pointed to cowpox being a pox in the classical sense of the word. But he was too much captivated by the idea of a substitute for variolation to read the true lesson of these facts. In August of the year following (1799) he had progressed so far in his easy-going assent to Jenner’s teaching that he practi-

¹ 1799, ii. 664.

² August, 1799.

cally withdrew his objection to the name *variolaë vaccinaë*, which "I formerly endeavoured to show to be unjust and tending to mislead by giving erroneous notions." Pearson's final view seems to have been that cowpox and smallpox were "varieties of the same species"; but he never quite lost his early impression of their unlikeness. When the Clapham cases of vaccinal ulcers were making a stir in 1800, he wrote¹ that cowpox might indeed have something loathsome in its nature, but then it was "useful"; it was one of those things

"Which, like the toad, ugly and venomous,
Wears yet a precious jewel in his head."

Another London physician of repute, who got dangerously "warm" on the side of the new name and old nature of cowpox, was Dr. John Sims, a man of liberal tastes, who edited the *Botanical Magazine* for many years. Sims, in the innocence of his heart, thought that any information on the nature of cowpox would be welcomed. Accordingly he gave an account, in the very first number of the new London medical journal² (13th February, 1799), of the case of Mr. Jacobs, a prominent solicitor of Bristol, who had begun life in the humble position of a milker on his father's farm, and had twice caught cowpox on his hands. Mr. Jacobs was perhaps the only one of the large number of cowpoxed milkers who could now make himself heard in the learned world on a matter of vulgar experience, which had assumed a sudden and wholly unlooked-for importance. "What this gentleman remarks," wrote

¹ *Med. and Phys. Journ.*, v. 87.

² *Ibid.*, i. p. 11.

Sims, "of the loathsomeness of the disease, although a circumstance entirely overlooked in Dr. Jenner's account, appears to be in itself a formidable objection to its introduction," not to mention the fact that Jacobs had twice taken smallpox afterwards. When Jenner read this, he wrote to a friend,¹ calling Sims a "snarling fellow," and accusing him of "harsh and unjustifiable language." Private remonstrances were made to Sims, and he wrote again on the 20th April, that Jenner's doctrine would appear to have been based upon "suitable inquiries." A paragraph in the same number announces that Sims had acknowledged the Bristol case to be "spurious." In a year's time Sims had progressed so far as to let his name appear near the top of the list of metropolitan physicians and surgeons who recommended cowpoxing to the public.²

The veterinary criticism, also, was highly inconvenient. That of Clayton, the Gloucester veterinary surgeon, has been given at length in the foregoing chapter; two other testimonies of the same kind have now to be noticed. A well-known entertaining writer on veterinary and rural subjects was John Lawrence, of Bury St. Edmunds, the author of a *Philosophical and Practical Treatise on Horses and on the Moral Duties of Man towards the Brute Creation*, and of other works. Lawrence promptly came forward to tell what he knew of cowpox in the eastern counties. "When the public ardour for the present topic," he wrote,³ "shall have

¹ Letter to Gardner, 7th March, 1799, in Baron, i. 321.

² July, 1800.

³ *Med. and Phys. Journ.*, i. 114.

become a little cool and satisfied, I hope it will be turned by enlightened men towards another perhaps of nearly as great consequence, namely the prevention of the original malady in the animals themselves. Those who have witnessed, or only reflected upon the excessive filth and nastiness which must unavoidably mix with the milk in an infected dairy of cows, will surely join me in that sentiment." Lawrence was hopelessly before his time; it was not likely that any one would listen to a person so absurdly Quixotic as to propose that cowpox, the source of Jenner's "guardian fluid," should actually be eradicated from among the diseases of the brutes. It was not until 1886-88 that we began to find out that "the filth and nastiness which must unavoidably mix with the milk in an infected dairy of cows" was a not uncommon cause of scarlet fever in those who used the milk.

Another criticism of cowpox, from the cows' side of the question, was published soon after in an anonymous pamphlet.¹ The author begins with some cautions to milkers not to handle the teats of cows too roughly, and then proceeds to inquire a little farther into the nature and extent of "this most horrible contagion." These filthy ulcers, he points out, never arise except on the teats of a cow in milk; there is no such disease of the bull, the ox, the maiden heifer, or the calf; the disease was, in fact, incidental to the "stripping" of the teats by the hands of milkers. This bold rationalist objected altogether to inoculating disease in order to ward off

A Conscious View of Circumstances and Proceedings respecting Vaccine Inoculation. London, 1800.

disease ; smallpox was subject to the same laws as the plague and the sweating sickness, which had had their day in England. The author of the *Conscious View* was severely handled by the medical critics. One journal gave him half a dozen contemptuous lines:¹ "It is impossible for a candid mind to read this illiberal and, we may say, scurrilous pamphlet without feeling the most lively indignation." The other and more critical London organ gave a full summary of the essay, and concluded that it was written with too much acrimony and prejudice to have any influence on the practice of cowpoxing, which was by that time in full swing.² It does not appear who this anonymous writer was. His line is much the same as that taken by Lawrence, except that the latter was not wholly opposed to the old variolous inoculation.

The realities of cowpox and the utter unlikeness of it to smallpox were also dwelt upon by Moseley and others ; but as these opponents were destined to carry on a long warfare against the Jennerian project, I shall put off what I have to say of them until chapter xiii. on Dissent.

The most formidable of Jenner's antagonists, judged by scientific or professional standing, was Dr. Ingen-housz, of Vienna, who happened to be residing in England when the *Inquiry* was published.

Dr. John Ingen-housz, born at Breda in 1730, came to England in his youth and learned the art of inoculat-

¹ *Med. and Phys. Journ.*, iv. 567.

² *London Medical Review*, v. I have had to depend on the extracts from the pamphlet given by this Review, as the original is not to be found in libraries.

ing smallpox under Dimsdale. On the recommendation of Sir John Pringle, he was summoned in 1768 to the court of Vienna by the Empress Maria Theresa, who had shortly before lost two of her children by the smallpox. After an obstinate struggle with his countryman, De Haën, who was then all-powerful in the Vienna medical school, he succeeded in introducing inoculation into Austria and devoted a great part of his energies to it in after years. He excelled, at the same time, as a botanist, chemist, and physicist, and his name will be found honourably mentioned in the history of vegetable physiology and of electricity. His *Miscellaneu Physico-medica* was well known both in German and Latin editions.

In the autumn of 1798, being then in his seventieth year, he came on a visit to the Marquis of Lansdowne at Bowood. Jenner's *Inquiry*, which was just out, came naturally under the notice of so leading an authority on smallpox inoculation; and he took advantage of his residence in Wiltshire to make inquiries about the "extraordinary doctrine" of protection by cowpox, as he knew that the cowpox was well known in that county. He first applied to Mr. Alsop, surgeon, of Calne, and was taken by him to a farmer of the neighbourhood named Stiles, who had seen the cowpox go through the cows in his father's time thirty years before, and had himself caught the infection in a very severe form; when his cowpox sores were all healed, he had been inoculated with smallpox by Mr. Alsop; the disease was produced, many pustules came out, and he gave the smallpox to his father, who died of it. This was the information elicited by Ingen-housz on the very

first attempt. He heard of several other facts of a similar kind, which tended to overthrow Jenner's idea of protection. He advises Jenner to think it over "before you finally decide in favour of a doctrine which may do great mischief should it prove erroneous." He prefers to approach Jenner privately rather than to draw him into a public controversy, "always disagreeable to a man so liberal-minded and well-intentioned as your treatise indicates you to be."

Ingen-housz himself gave Jenner the cue for his reply. The famous inoculator of Vienna had noticed in passing the digression in the *Inquiry* about smallpox virus losing its properties, owing to some subtle imaginary putrefactive change, and producing a disease which was "certainly not smallpox," although it had all the look of it: it was not smallpox, because those who had been thus inoculated caught the smallpox naturally afterwards. No sensible and honourable man could endorse stuff of that kind, however much he might wish to excuse the failures of his own art. Spurious smallpox was afterwards disclaimed by Pearson, Woodville, and other inoculators who knew their business. It was a point which Ingen-housz could not let pass, and he tells Jenner that if he will inquire more particularly, he will find that he is in error in setting up a spurious variety of smallpox; there was no such thing known. Whether from mere momentary irritation or from deliberate design, Jenner answered Ingen-housz by extending the accusation of spuriousness to those very cases of *cowpox* which the Viennese doctor had heard of in Wilts. There was an offensive stench from the cows' udders, therefore the putrefactive process had been going on;

therefore the cowpox was spurious, and no wonder Farmer Stiles had taken smallpox after it.¹ Hitherto spurious cowpox, in Jenner's estimation, had been such varieties of it as did not come from horse-grease; the necessity of replying to Ingen-housz showed him how to extend the domain of the spurious, which he did still farther in his next essay. As Ingen-housz had the temerity to object to Jenner's spurious smallpox, that worthy, whom no one would have accepted as an authority upon smallpox, replied not only by re-affirming his statement, but by throwing spurious cowpox also at his adversary's head. The spuriousness in both cases had no other ground than the failure to ward off smallpox. Jenner did not allege that there was anything in the look of the one disease or of the other by which its "spurious" character might be known. Jenner's spuriousness, in the language of metaphysics, was a subjective, not an objective quality.

A man of the world, as Ingen-housz was, must have very quickly seen that there was no use controverting the arguments of such a person as this, who was palpably either a fool or a knave. He told an emissary of Jenner's, a surgeon, Paytherus, who called upon him in London on the 13th December, 1798, that "nothing would have kept him from answering Dr. Jenner's letters but the desire of satisfying his mind on the subject." He also "spoke very handsomely" of Jenner,

¹ One of the cases published by Abernethy for the Rev. R. Holt, of Finmere, was of a servant so dangerously ill with cowpox ulcerations that medical help was necessary for more than three weeks, the effluvia being so offensive as to penetrate every room in the house.—*Med. Phys. Journ.*, ii. 401.

and sent him the advice to be in no hurry to publish a second time on the cowpox. He took no farther part in the controversy, and died during his next visit to Bowood in September of the year following (1799).

The impression made by Jenner's book upon leading medical men throughout the country was a somewhat mixed one. Beddoes, of Bristol, told one of his colleagues, who was a friend of Jenner's, that he thought the *Inquiry* would do its author much credit;¹ but in writing about the same time to Hufeland, of Berlin, he spoke disparagingly of it.

The letter is worth quoting as an instance of the kind of criticism current just before Woodville came to the rescue:²—

“You know Dr. Jenner's experiments with the cowpox. His idea of the origin of the virus appears to be quite unproved, and the facts which I have collected are not favourable to his opinion that the cowpox gives complete immunity from the natural infection of smallpox. Moreover the cowpox matter produces foul ulcers, and in that respect is a worse disease than the mildly inoculated smallpox. With all this suppuration, the system remains on the whole unaffected; accordingly nothing is gained thereby for the smallpox. They are occupied at present with experiments upon it at the Smallpox Hospital in London.”

Percival, of Manchester, congratulated Jenner on his publication, and went on to say:³ “But a larger induc-

¹ Hicks to Jenner, 3rd October, 1798, in Baron, i.

² Beddoes to Hufeland, 25th February, 1799, in Hufeland's *Journal*, vii. (1799), pt. iii. p. 168.

³ Letter to Jenner, 20th November, 1798, in Baron, i.

tion is yet necessary to evince that the virus of the *variola vaccinae* [he had clearly no suspicion of the name] renders the person who has been affected with it secure during the whole of life from the infection of smallpox."

Francis Knight, a court surgeon in fashionable London practice, who was connected with Gloucestershire, wrote, on the 10th September, 1798, that the plates were correct, and that he "knew the facts to be well supported: at least it was a general opinion among the dairymen that those who had received the cowpox were not susceptible of the variolous disease. . . . It is sufficient for me to have proof that a lighter disease may be uniformly substituted for a greater one." And, to show his confidence in the discovery, he asks for a supply of lymph, adding: "I know some people of fashion who are well disposed to let me make the experiment on some of their children."

It never for a moment occurred to Knight to suppose that Jenner was not then practising his new method, or that he had not as much matter for inoculation as he cared to use. An intimate friend of Jenner's, Dr. Hicks, of Bristol, was equally in the dark. Writing on the 3rd October (three months after the *Inquiry* was published), he says: "I do not see that you need hesitate to accept of the invitation given you to inoculate with the cowpox, convinced as you are that it will secure the person so inoculated from ever being infected with the smallpox." Jenner was "hesitating" for reasons that have now to be made clear.

When he came to London in April to publish the *Inquiry*, he left his series of vaccinated cases in the

hands of his nephew and assistant, who would seem not to have carried the succession farther. Jenner took a sample of cowpox matter with him, and gave it to Cline, who produced an ulcer with it in his first case, and failed altogether at the next remove. Cline then wrote to Jenner for a fresh supply of cowpox, never dreaming that there was any lack of it. He sent a handsome testimony of his own and Dr. Lister's belief in the new protective, along with the account of his trial of it. When Jenner afterwards used that letter, he struck out Cline's words, "The ulcer was not large enough to contain a pea, therefore I have not converted it into an issue as I intended," and inserted in their place the words, "There were no eruptions."¹

When Jenner went back to Gloucestershire in July, he heard of the prevalence of cowpox at a farm near Berkeley, and inoculated four or five of the farm servants with matter from a cow's teat. These inoculations, which were in adults, all failed; but, within a month, the same servants acquired cowpox accidentally from milking the infected cows. The accidental sores on their hands were, of course, a perfectly available source of matter, but Jenner does not say that he resorted to it. In September, Dr. Pearson was urging him to begin the practice in earnest, and Jenner excused his want of cowpox matter by blaming Cline for failing to continue the old April stock in London.

In the end of September cowpox appeared in a dairy

¹ Cline's original letter was published by Baron (i. 152), who does not seem to have known that Jenner had already used it—and tampered with it.

at the village of Stonehouse, on the Stroud road, not far from Eastington, where his friend Mr. Hicks had a factory. Hicks knew all the circumstances of the publication of the *Inquiry*, and was ready to have his own two children inoculated in the new way. It would seem that no word of the cowpox at Stonehouse had reached him until after it had been prevalent several weeks; for it was not until 26th November that Jenner procured some of the matter, and next day inoculated with it the two Hicks children. The result is indicated vaguely in a letter to Woodville: an inflammation in the arms, the constitution unaffected, the local effects lasting more than a week, a small scab left behind. On the 2nd of December, a portion of the same lymph which had been dried upon a quill was inserted into the arm of Susan Phipps, a child of seven. On the twelfth day the areola was out, and there were a number of very minute confluent pustules round the big cowpox vesicle. "So exact was the resemblance of the arm at this stage to the general appearance of the inoculated smallpox," that Drake, a surgeon from Stroud, who had never seen the cowpox before, declared he could not perceive any difference between it and smallpox. However, Drake took some matter from the child's arm and inoculated some cases of his own, with a result, as we shall see, that must have opened his eyes to the difference between cowpox and smallpox.

The utter unlikeness of cowpox to smallpox, and its singular generic resemblance to the great pox, became obvious in Jenner's own case in a few days; the vesicle dried to a crust, the crust was cast off, and disclosed an ulcer, which continued to spread until it reached a size

“nearly as large as a shilling”—not very like *smallpox*, one would suppose. With matter which had been taken from Susan Phipps’ arm on the twelfth day, Jenner inoculated Mary Hearn, aged twelve. She had the areola on the fourteenth day, and an ulcerous state of the arm for some time after, which had to be treated with mercurial ointment. These facts are given by Jenner himself, although he prefers to call the ulcer a “pustule.”

It so happened that Thornton, of Stroud, got matter from the same Stonehouse farm on the 1st of December independently of Jenner; and both he and Hughes, of Stroud, who reported Drake’s cases done with matter from the arm of Jenner’s case on the 13th December, have left full narratives of their experience. These are in striking contrast to Jenner’s ordinary equivocal and secretive manner in reporting his results. If vaccination at the outset had been left in the hands of men as candid as Thornton and Hughes, the public and the profession would have declined to adopt it; the immediate results were too uniformly alarming for the vaccinated children, and the subsequent test too adverse to the theory of protection from smallpox.

Thornton’s experience is historically important as being the first independent evidence that the *Inquiry* elicited. On the 1st of December, 1798, he found a milker at the Stonehouse farm, with sores upon his hands; one of these was still in the unbroken form of a pock, being “the only one that was not degenerated into a sordid and painful ulcer.” The vesicles were seen first on the fingers five days before, having been preceded by pain in the axilla, headache, cold shiverings,

fever and weakness. The same evening, on which he took the cowpox matter from the milker, Thornton went to Stafford's Mill and inoculated Mr. Stanton and four of his children, from ten years of age to ten months. On the third day the arms of the four children were affected with a kind of erysipelatous efflorescence above the point of insertion. About a fortnight after, the punctures began to be covered with a thick crust, from which some ichor was discharged for several days. The inflammation subsided and the scabs fell off about the twentieth day. "From the long-continued local excitement," Mr. Thornton began to hope that the virus might imperceptibly have crept into the habit and proved a security against the variolous infection; but it was not so, for when they were tried to see whether the cowpox had made them insusceptible, all the children "received the infection and passed through the stages in the usual slight manner"; the father, whose vaccination had failed altogether, was the only one of the five who *resisted* the smallpox.

This damning experience of cowpoxing, from a source used by Jenner himself and authenticated with full particulars, ought to have raised a suspicion that there was something wrong. It was communicated to Jenner by Beddoes, in whose *Contributions to Physical and Medical Knowledge* it was about to appear; and Jenner replied to it, and to the equally damning veterinary experience of Clayton, of Gloucester, which was sent to him at the same time, by a bouncing declaration of his own superior credit as a man of science.¹

¹ See chapter iii. p. 58.

There remained, however, the other experience at Stroud, with cowpox matter taken by Jenner himself from the child vaccinated from the Stonehouse cows, and given by him to Drake on the 13th December. Drake sent the results of the five vaccinations and subsequent smallpox tests to Jenner shortly after ; but the facts were suppressed, and nothing would ever have been heard of them but for the following circumstances : The subjects offered for vaccination were the three young children of the Rev. Mr. Colborne, of Stroud, a lad in his employment, and another lad employed by Mr. Drake. Mr. and Mrs. Colborne requested another Stroud doctor, Hughes, who was a connexion of the family, to witness the operations by Drake and to follow the results. Hughes wrote out a tolerably full account of the five cases, from notes that he had taken, and sent it, under the date of 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 Drake ; 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, as he had already done the more summary statement of failure by Drake. We now come to the cases as narrated by Hughes.

In three of them, a lad aged seventeen and two of the Colborne children (one four years, the other fifteen months), the cowpox

vesicles came to early maturity and were scabbed under the usual time. The lad was inoculated with smallpox on the 20th December, being the eighth day from his vaccination, and the two children on the 21st, being again the eighth day. They all developed smallpox, both the local pustule and the general eruption with fever. The remaining two cases—a lad aged fifteen and the third Colborne child aged two years and a half—were also variolated on the 21st December, or the eighth day of their vaccination; but these two developed the local pustules only. The reason why they did not have the consecutive fever and general eruption of smallpox will perhaps appear from the peculiar history of their cowpox sores.

In the lad, W. King, the areola appeared on the tenth day and continued spreading until the fifteenth. On the eighteenth day the scab, which now occupied the centre of the vesicle, put on the appearance of an eschar, with much induration of the tissues around; on the twenty-ninth day the eschar separated, and left a sore one-quarter of an inch deep, which, under treatment with mercurial ointment, filled up and skinned over in due course. He had meanwhile been tried a second time with smallpox, on the 1st of January, but resisted it entirely, his cowpox sore being on that day and for a week longer in its eschar stage and his lymphatics doubtless clogged. The case of the child E. Colborne was somewhat similar. On the tenth day her cowpox vesicle was the size of a sixpenny-piece, being mostly a scab with a narrow ring round the margin containing matter. On the fifteenth day the crust was thrown off, and left a small superficial eschar, which increased in depth in the next few days; much inflammation followed in the skin around, and "two small suppurations" broke out a little above the original vaccine puncture, each of which reached the size of a shilling, one of them communicating with the original sore. On the 4th of February, being the fifty-second day from vaccination, the sores were all healed and the induration gone. Meanwhile this child had also been tried a second time with smallpox on the 1st of January, entirely without effect.

It was a not incorrect summary of this experiment which had reached Jenner: "Two of them had alarming ulcerations on their arms, and these two, whose arms

were so dreadfully affected, did not take the smallpox, while the other three received it."

At the end of 1798, or six months after the *Inquiry* was published, the case for cowpoxing as a substitute for inoculation with smallpox stood as follows: Nearly all the children's arms had ulcerated, some of them to an alarming extent, just as the milkers' hands nearly always ulcerated. Jenner neglected the variolous test in some of his cases, and got a rather equivocal result in others. The variolous test, when applied by Drake and Hughes in one set of cases, and by Thornton in another, gave a result which was as far as possible from bearing out Jenner's confident assurances. In some medical circles these adverse facts were as well known then as they are now to us in the retrospect; and it is the strongest possible evidence of the good-will, nay, the welcome, extended to Jenner and his innovation, that the fatal objections were not pressed.

Ulceration was so clearly written in the December experiments, both in Jenner's own hands and in the hands of Thornton and Drake at Stroud, that the artifice of the title-page, the foisted name *Variolæ Vaccinæ*, looked as if it were going to be found out. It was probably thought imprudent to continue a stock of matter from the ulcerating Stonehouse source, or perhaps the attempt to continue it failed, as all Jenner's attempts to raise a stock had failed. At all events, neither Jenner himself nor the two surgeons at Stroud had any matter to go on with; and the great cowpox project might have come to an end there and then if it had rested with Jenner to give practical effect to it. At this point in the history of the substitution of cowpox

for smallpox inoculation, the end of 1798, the scene changes from Gloucestershire to London. Jenner had made at least two attempts, subsequent to the publication of the *Inquiry*, to raise a stock of cowpox matter on the human arm, and had failed; so that he was unable to supply those who applied to him. The most urgent of his correspondents was Dr. George Pearson, who had entered into the question far more methodically, but not less confidently, than Jenner himself. The results of his numerous inquiries by correspondence all over the country, and of his own investigation among the London dairies, were published in November, 1798.

In consequence of Pearson's bustling zeal, the dairymen in London were induced to report any cases of the pox among their cows; and on Sunday, the 20th of January, 1799, the news was brought to Woodville that the disease was among the cows at a dairy in Gray's Inn Lane. On Monday, Woodville repaired thither along with a veterinary student, who belonged to Jenner's parish and professed to know about cowpox. In a day or two the milkmaids had the blebs on their fingers, exactly as Jenner had figured in his first plate. The original sceptics and rejectors of Jenner's innovation, Sir Joseph Banks, Lord Somerville and others, were fetched to the cowhouse, and Jenner's book was produced. Scepticism gave way to belief; for there, sure enough, was the identical large bluish-white vesicle on a milkmaid's hand which Jenner had pictured—indeed, "a more beautiful specimen of the disease than that which you have represented in the first plate." Having satisfied themselves that there was such a malady as cowpox, and that Jenner's picture of it in milkers was true to

nature, they concluded that there was a *primâ-facie* case for giving it an independent trial. No body of Englishmen would have acted otherwise; whatever the irrationality or dialectical absurdity of the project, they would put it to an experimental test.

Matter was at once taken to the Inoculation Hospital, and a number of the applicants at that institution had it inserted into their arms, instead of the smallpox matter which they had come to receive. The succession of inoculations was kept up from arm to arm, and vaccination was established on the grand scale. From that perennial source Jenner himself was supplied with matter on the 15th February, and thenceforward circulated it as the "true Jennerian lymph."¹ It was just at this juncture that Jenner got the proof sheets from Beddoes of the damaging experiences of the Gloucester veterinarian and of the Stroud doctor; and it is no wonder that he replied (26th February): "I have neither the leisure nor inclination at the present moment to enter into an examination of their arguments." Cowpoxing was now a going concern, and all the theoretical objections in the world could not bring it to a stop.

Woodville had come to the rescue with his *solvitur ambulando*. Nothing is more striking than the effect that this practical solution of the question had upon objectors; within a few months they either withdrew and apologised for their scepticism, or they kept silent. In the month of June, 1799, within three months of the first diffusion of lymph and within a year of Jenner's first publication, the editor of the *Medical and Physical*

¹ See *Natural History of Cowpox*, pp. 18-21.

Journal wrote: "There is not, perhaps, in the annals of medicine, to be found an example of an experiment or inquiry where the life and health of such numbers already born, and of all to be born, were implicated, that has been taken up more generally, received more candidly, or conducted more prudently than this concerning cowpox."

But Woodville did not merely supply a stock of lymph for all and sundry, learned and simple, to try their hand with; he succeeded, by a mixture of luck and skill, in presenting cowpox to the profession in the subdued form which it has ever since retained in average practice, a form which surprised Jenner when he saw it, and was indeed very unlike the original ulcerous disease. The latter achievement, along with the actual supply of cowpox matter for all who wanted to try it, gave the new substitute for smallpox an irresistible vogue. *Omne tulit punctum* may be said of Woodville: he provided vaccine lymph, while Jenner was still talking about it; and he made the lymph comparatively innocuous, while Jenner was still floundering in the difficulties of erysipelas and phagedenic ulceration. We have now to see how cowpox came to acquire the rather mild type which it had when the profession and the public first made trial of it and accepted it.

CHAPTER V.

COWPOX MADE MILD AND ACCEPTABLE.

DR. WILLIAM WOODVILLE, who started cowpoxing on the great scale and supplied all the world with vaccine lymph, was one of the most practised inoculators of his time. He had been a favourite pupil of Cullen at Edinburgh, and had come to London after trying country practice for a few years. In 1791 he was elected physician to the Smallpox and Inoculation Hospitals. He was a botanist of no small repute, having published a *Medical Botany* in three quarto volumes in 1790 (subsequently edited by Sir W. J. Hooker), and laid out two acres of ground around the Smallpox Hospital (then at King's Cross) as a botanical garden, which he maintained at his own expense.

In 1796 he published the first volume of a *History of the Inoculation of the Small-pox in Great Britain*, in which he has the following remarks (p. 7) on cowpox: "It has been conjectured that the Small Pox might have been derived from some disease of brute animals; and if it be true that the mange, affecting dogs, can communicate a species of itch to man; or, that a person, having received a certain disorder from handling the teats of cows, is thereby rendered insensible to variolous infection ever afterwards, as some have asserted, then

indeed the conjecture is not improbable." This was either taken from the paragraph in almost identical terms in Adams on *Morbid Poisons*, published the year before, or was derived from the same source; namely, Jenner's private correspondence with Cline. Pearson speaks of Jenner's *Inquiry* as having been long expected; the rumour of its main contention, that cowpox protected from smallpox, had reached Adams, Beddoes, Woodville and others two or three years before the essay appeared. The proposed substitute for variolous protective inoculation would thus have caused a flutter among all the specialist inoculators, determining some of them perhaps towards opposition, and others of them towards giving the new plan a trial whenever it was ripe.

Among the latter was Woodville. He was approached by Jenner in London in the summer of 1798, when he came up to print the *Inquiry*, and gave the advice that horse-grease should be struck out from the text altogether.¹ On the 17th of June, 1798, four days before Jenner wrote the preface to his *Inquiry*, Woodville was present at the Smallpox Hospital while his friend Pearson tried the variolous test upon three formerly cowpoxed milkers from Willan's farm adjoining the New Road, Marylebone.² It will thus appear that Woodville, along with Pearson, had become interested in the new protective inoculation, owing to private communications with Jenner in London, before the *Inquiry* was through the press. The variolous test on the three old cowpoxed

¹ H. Fraser, *Med. and Phys. Journ.*, 1805, p. 10.

² Pearson's *Inquiry*, pp. 14, 15.

milkers was confirmatory, so far as it went, of Jenner's position ; none of them received the infection, while two men from the same farm, not previously cowpoxed, received it in the usual way. Pearson and Woodville were, accordingly, eager to begin cowpoxing on the great scale, and the former made several applications to Jenner for a supply of lymph in the course of the ensuing autumn.

Jenner had no lymph to give to Pearson in September or November ; nor did he succeed in cultivating a stock, supposing that he tried to do so, from the two cases of phagedenic cowpox ulcers in children after inoculation from the Stonehouse cows in December. The only other persons who had tried the new practice up to that time were Cline, in July, with matter from Jenner ; Thornton, of Stroud, on December 1st, with matter taken by himself from a Stonehouse milker ; and Drake, of Stroud, with matter from Jenner, on the 13th and 14th December. From none of these inoculations with cowpox was a stock of matter raised ; they had all turned to ulceration, like Jenner's own cases ; and at the opening of the year 1799 the project of cowpoxing, which had been recommended to the world by Jenner six months before and is commonly supposed to date in practice from that recommendation, was represented by some half-dozen children at Stroud and Eastington slowly recovering from cowpox ulcers on their arms.

It is at this juncture that Woodville comes on the scene. On Sunday, the 20th of January, word was brought to him at his house in Ely Place that the cowpox had appeared among the cows at a dairy in Gray's Inn Lane. On visiting the cowhouse next day (Jan. 21st)

he found three or four cows affected with "pustulous sores on their teats and udder." A veterinary student then in London from Jenner's country, named Tanner, whom he had sent for, took matter from one of the cows "which appeared to be most severely affected with the pustular complaint"; and with that matter Woodville the same day inoculated seven persons at the Inoculation Hospital, "by a single puncture in the arm of each, or rather by scratching the skin with the point of the lancet till the instrument became tinged with blood."

The affection existed in only three or four of the cows when Woodville inoculated from it, but eventually it spread through the whole herd of some two hundred animals, those cows which were not in milk escaping. The infection was accordingly fresh, or recently started, or in the making, when Woodville first heard of it and obtained a supply of its virus. Calling again at the cowhouse two days after, on Wednesday, the 23rd January, he found two or three of the milkers with the beginning of cowpox on their hands. For only one of these are there details given, namely, Sarah Rice, who had four cowpox vesicles on her fingers, wrist, and forearm; this milkmaid became an object of scientific curiosity, and on Thursday, the 24th, being the fifth day since she had noticed the whitish blebs on her hand or arm, her cowpox was inspected at the cowhouse by Lord Somerville, Sir Joseph Banks, Sir William Watson, Dr. Willan, Dr. Pearson and several others, and compared with the plate in Jenner's *Inquiry*.

Two of Sarah Rice's four vesicles were a third of an inch or more in diameter on that day, and already

acquiring the bluish-white tint; she had then some uneasiness in the armpit, and a degree of headache subsequently; but none of the vesicles were painful, and they all gradually went off *without producing ulceration*. She had been infected when the diseased process on the teats had hardly gone beyond the cow which first started it, and had presumably not yet acquired that type of specificity which a longer duration and successive reproductions would give to it; 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 or scabs.

From one or more of the vesicles on her hand or arm Woodville had inoculated two men on the 23rd of January, being the fourth day since the vesicles had been noticed, and six other subjects at the hospital on the 24th, or the fifth day. He thus got matter for inoculation at what we must call an early stage of the cowpox vesicle. It is even more important to remark that the vesicles on the milker, whence the matter for vaccination was taken, were not destined to become painful open sores at all, having been caught from the first cow, or the first two or three cows, in a series that extended by successive transmissions of the infection until it reached to nearly two hundred animals, and must have lasted weeks or months.

These circumstances had necessarily some significance for Woodville's success, as compared with the failure of Jenner and of Thornton with the Stonehouse cowpox in the month of December preceding. The failure to raise the much-demanded stock of lymph from that source was owing, so far as we know, to the ulcerous

type of disease transmitted in the first remove from the cow direct, or from the milker; and with that alarming type we may connect the fact that the cowpox had been passing from cow to cow at the Stonehouse farm in successive transmissions since Michaelmas, or for more than two months. The severity of type, which would have been thus cultivated by neglect or inveteracy, was shown to exist in fact, in the case of the man-milker from whom Thornton, of Stroud, took matter for his five inoculations at Stafford's Mill: the man's sores were supposed to be of the same age (fifth day) as in the case of Woodville's milkmaid; and yet there was only one of them "which had not degenerated into a sordid and painful ulcer" at even that early date, whereas Sarah Rice's cowpox never became ulcerous at all.

The pedigree of the world's vaccine, which is the pedigree of Woodville's stock, was thus derived from an exceptionally mild type of cowpox in the cow and in a milker, or from a stage of the particular outbreak at which the worst features of the infection had not had time to develop through neglect and aggravation. Woodville succeeded in passing cowpox matter for inoculation into common currency, after Jenner had several times failed in attempts to do the same; and we have to associate with his success not only a certain superior skill as an inoculator, but also a large element of luck.

We have now to see how his experiments at the Inoculation Hospital came out; and how his practical success was achieved. It will appear that Woodville at the outset had as little of a reasoned and steady perception of the advantages of *early* cowpox as had Jenner

himself. It was his good fortune to get early intelligence of an outbreak, and he made use of the matter for inoculation as soon as he knew of it. Thus blindly led by fortune, he overcame initial obstacles that had baffled Jenner, and were to give trouble to most of those who started new stocks of lymph in later years. Luck alone enabled him to lead off with a type of cowpox vesicle which hardly differed from the standard vaccine of to-day; but so little did Woodville know the law of the pathological process with which he was dealing, that he actually allowed the cowpoxed arms in some instances to proceed to the fifteenth and even nineteenth day of their development before he inoculated from them; and his venture was only saved from failure by a kind of empirical selective instinct which led him, in a wide field of choice, to continue his stock with matter that happened to stand for the early stage and the short cycle of cowpox. The smallness of the vesicle thus induced, the shortness of its cycle, and the mildness of its effects all served to divert the attention of Woodville from the true analogies of cowpox, and to fix it upon the false analogy which had been put into men's heads by Jenner's new-fangled name "*Variolæ Vaccinæ*."

Woodville has traced the pedigree of inoculated cowpox through a number of generations, and has given in a table the names, ages, and other particulars of about four hundred and fifty cases. For the first two hundred cases, he professes to do more; he gives information in the text under the name of each of them, but the information is often meagre as regards the state of the cowpoxed arm. The record is on the whole an authentic one, and is at all events free from the suspicion of hav-

ing been "edited," which attaches to everything of the same kind published by Jenner. I can give here only a few results gathered from a study of his book.¹

To begin with the strain of cowpox from which Jenner himself was supplied, and which became in his hands the source of "true Jennerian lymph:" at the first remove from the cow it was taken off as tenth-day lymph, at the second remove it was eighth-day lymph, and at the next remove it was taken off and sent to Jenner at the tenth day; so that the vesicle was as if habituated to yield fluid from the eighth to the tenth day, and, as we learn from Woodville's narrative, to have the efflorescence out on the ninth day, and the first appearance of the scab about the tenth.

Of Woodville's numerous other concurrent strains of lymph, several came to an end, probably because the ripening of the vesicle got later and later; whereas those which survived and sent out the most numerous branches were strains with a consistent record of early maturity. Thus, to take one from the same parent stock as Jenner's own: Collingridge (direct from the cow); Butcher (10th day); Jewell (7th day); Fisk (9th day); Murrell (7th day); Hatt and Playford, each the vaccinifer of many more, on dates not stated. A parallel strain to this had an obnoxious interlude, but came back, at the next remove, to a safe type: Collingridge (direct from the cow); Butcher (10th day); Jewell (7th day); Reed (10th day); Webb (15th day, had severe erysipelas); S. Timms, H. Timms, and Lee (10th day),

¹ *Reports of a Series of Inoculations for the Variolæ Vaccinæ or Cowpox.* London, 1799.

each the vaccinifer of numerous others on dates not stated.

These and other strains claimed descent from the cow direct. But Woodville had also a stock of matter in currency which he took from the dairymaid's hand; and there is something to be learned in following the fortunes of that, the more so that the original infection on the milkmaid's hand and arm never came to the usual painful ulcers of cowpox at all. Two men were inoculated from the dairymaid's vesicles at the fourth day, having been inoculated with smallpox the day before. Both infections ran their course independently of each other, and the cowpox vesicles proved to be of so early a type that they had actually scabbed before the variolous pustules did. Six others were inoculated from the dairymaid's vesicles when these were a day riper, of whom we have a somewhat different history. Three of them are unaccounted for altogether; of the other three, only one, James Crouch, aged seven, was used to continue a supply from. Let us take the three in order:

William Harris, aged twenty-one: on 5th day vesicle began; on 9th day it had prominent callous edges and depressed centre, but hardly any areola; on 12th day areola going off; on 14th day vesicle dry at the centre, but its surrounding edges of a bluish tinge and still abounding with ichorous matter; 19th day the cowpox infection has become a dry scab, with a finely polished surface of a mahogany-brown colour—the standard or classical termination of vaccination, and a termination reached within a day or two of the usual time. We hear of no strain being continued through this highly favourable case.

The next case from the dairymaid's vesicles at the fifth day is William Bunker, aged fifteen: 8th day, vesicle has grown rapidly, pain in armpit with headache; 10th day, vesicle already scabbing,

the areola extensive ; 12th day, the areola nearly gone ; 17th day, a dry scab all over ; 20th day, complete smooth brown scab.

The case from which the stock was continued is James Crouch, aged seven : 9th day, vesicle full of ichor, little areola ; 11th day, the efflorescence extensive, vesicle drying at centre ; 14th day, pain in armpit, drying process extending. From this case one person was vaccinated on the twelfth day, and two on the thirteenth ; the former, aged twenty-five, had a mild form of cowpox, but was not used as a vaccinifer ; one of the latter, a child of twelve months old, had a very severe illness, and was likewise not used as a vaccinifer ; while the remaining one, Edward Turner, aged twenty-four, was used to continue the stock of the milker's cowpox from. On the 12th day his two vesicles began to dry in the centre, but the margins were of a dark-red colour (areola), and studded with minute vesicles, same time pain in armpit ; 14th day, the inner edges of the vesicle distended with ichorous fluid. From that arm six persons were vaccinated on the *seventeenth* and *nineteenth* days ; the results are given with far too much brevity to be intelligible, but none of the six became the vaccinifer of others.

Thus the strain from the dairymaid's hand would have come to an end, only that the strange experiment had been tried of inoculating from it at the first remove (James Crouch) back to a cow's teat. It was through that indirect channel that the dairymaid's lymph passed into the main current of English vaccine ; the cow was infected (and gave infection to a man who milked her) ; from her, three persons were inoculated, and from two of these a numerous race of vaccinifers arose, whose several lymphs corresponded to the 8th, 9th, or 10th day of the cowpox cycle.

It will thus appear that no lymph in Woodville's practice was passed into general currency if it was older than the tenth day. For some unexplained reason he allowed cowpox vesicles in several instances

to go on to the thirteenth, fourteenth, fifteenth, sixteenth, seventeenth, eighteenth, or nineteenth days, before he took matter from them; but in all those instances (excepting one that reached the fifteenth day, but was brought back at the next remove to the tenth day) the stock failed or was discontinued for one reason or another.

Woodville's earliest vaccinations were on the sixth day; and that early lymph was got from two of his cases directly inoculated from the cow. That matter was procurable from them on the sixth day, means exceptionally early maturity. The sixth-day lymph produced good vesicles, which ended in the characteristic polished mahogany-coloured crust; and no doubt lymph would have been continued from that good stock had it not been that the cases at the second remove were badly complicated with smallpox, which had been inoculated the day before the cowpox, and ran its course concurrently.

After Jenner himself had made trial, in twelve cases, of the cowpox matter which Pearson sent him from Woodville's stock, he wrote to Pearson (13th March, 1799): "The character of the arm is just that of cow-pox, except that I do not see the disposition in the pustule to ulcerate as in some of the former cases." In his letter to Woodville, on receipt of the London lymph, he had spoken grandiloquently about his own trials, evidently for the purpose of making Woodville believe that he had been experimenting largely, and had as much vaccine lymph of his own raising as he wanted. As a matter of fact, he had none, having been baffled, every time he tried, by the ulceration of the

children's arms. He used the same disingenuous tone when he referred to Woodville's lymph in the *Further Observations*, which came out in April following.

The complications with smallpox, which troubled Woodville for the first few weeks of his vaccination practice at the Smallpox Hospital, gave Jenner his opportunity. He is, of course, addressing the public, who knew nothing of the private history of all these transactions as we now have it in letters and memoirs. He does not let them know that he was without cowpox matter until the 15th of February, when he got some from Pearson; nor does he say that it was from the letter which came with the London vaccine lymph that he first heard of the eruptions. "You will be astonished," Pearson had written, "at our talking of eruptions." Jenner wishes the public to believe that it was merely for the purpose of comparing it with his own (non-existent), that he tried Woodville's lymph: "The matter they made use of was taken, in the first instance, from a cow belonging to one of the great milk farms in London. Having never seen matured pustules produced either in my own practice among those who were casually infected by cows, or those to whom the disease had been communicated by inoculation, I was desirous of seeing the effect of the matter generated in London on subjects living in the country." That was the only reason for his making use of Woodville's matter—the only reason except that he was without lymph of any kind, having uniformly failed to continue a stock of his own.

The same reason turns up again in his third pamphlet, under equally disingenuous circumstances. Having

come to London to secure his rights in the spring of 1799, and having found Woodville's lymph distributed universally, he saw the necessity of initiating a stock of lymph which might become the true Jennerian. The veterinary student, Tanner, who had assisted Woodville, was employed to get some cowpox matter for Jenner in London, if he could. This, Tanner is said to have succeeded in doing some time in April; he brought it to Jenner, who—proceeded to raise his stock forthwith? Not so; he sent Tanner at once with it to Marshall of Eastington, who was carrying on the vaccine practice in Jenner's absence, and had at that time done more than a hundred vaccinations with Woodville's lymph. The matter was intended to be the source of the historical "true Jennerian lymph"; and it was sent off to a remote part of the country, where no one ever knew what happened to it, except Marshall himself. But this is all that Jenner says of his despatching it to the country and his declining to raise the true Jennerian stock from it, with his own hands or under his own eye, amidst the abundant opportunities that the population of London afforded: "On the supposition of its being possible that the Cow which ranges over the fertile meadows in the vale of Gloucester might generate a virus differing in some respects in its qualities from that produced by the animal artificially pampered for the production of milk for the metropolis, I procured, during my residence there in the spring, some Cow Pock virus from a cow at one of the London farms [Clarke's, in Kentish Town] It was immediately conveyed into Gloucestershire to Dr. Marshall, who was then extensively engaged in the inoculation of the Cow Pox, the general result of which,

and of the inoculation in particular with this matter, I shall lay before my Readers in the following communication from the Doctor.”¹ Then follow two letters from Marshall, the first dated 26th April, 1799, and the second with the date [8th September] omitted. Marshall’s only reference to the cowpox matter taken from the cow artificially pampered for the production of London milk, so as to compare it with the corresponding virus taken from the animal which ranges over the fertile meadows in the vale of Gloucester—a virus of which Jenner’s own experience was by no means idyllic—is contained in a postscript to the second and undated letter, wherein this country doctor coolly observes that 127 vaccinations out of a total of 423 (or exactly 30 per cent.) were done with “the matter you sent me from the London cow.” That is the whole evidence; as if the establishment of a stock of lymph from original cowpox in the cow were an easy thing, an everyday occurrence, and as if Jenner had not failed every time he tried! He goes on: “I discovered no dissimilarity of symptoms in these cases from those which I inoculated from matter procured in this country.” Procured in this country! Why, it was procured by Woodville from the cow in Gray’s Inn Lane. “Artificial pampering” would have been much the same sort of thing in Kentish Town as in Holborn. It was wholly irrelevant to the problem in any case, and was a mere “blind.”

The special services of Woodville in making vaccination practicable were recalled in 1802, when Jenner was about to receive ten thousand pounds from Parliament;

¹ Collected edition of the three essays, 1800, p. 151.

it was Pearson, and not Woodville himself, who strove, in vain, to clear up the historical sequence of events and the respective merits of parties. One of Pearson's remarks is as follows:¹ "The acuteness of Dr. Woodville, and the obligations of the public to him, will be fairly appreciated by considering that he was led to expect, from Dr. Jenner's account, a quite differently appearing pock from what, I suppose, all the world now knows to be the fact." The *circular figure*, he explains, the *smooth surface*, the *less pointed shape*, and the *peculiar scab* were first noticed as distinctive of the cowpox by Woodville and by Pearson himself. These are, indeed, great and sufficient distinctions of cowpox from smallpox, even if there had been no gulf separating them in their clinical history, and a still more insuperable barrier in the whole epidemiological history of smallpox, which Jenner knew nothing of.

But Woodville's greater "acuteness" was nothing more than his greater honesty and candour. Jenner knew these differences between cowpox and smallpox well enough, indeed he knew of far more striking differences; only he took care not to dwell upon them. If any one scans his writings closely, he will find how dexterous Jenner had been in suggesting the identity or likeness of the cowpox with smallpox on indecisive or irrelevant points. It is the fever that is the same in the two, or the efflorescence, or the early changes in the appearance of the incisions.

¹ *An Examination of the Claims, etc., containing a Statement of the principal Historical Facts of the Vaccinia*, p. 104. London 1802.

Two references to the identity-question in Jenner's second pamphlet (*Further Observations*. April, 1799) are as follow: "Seeing that these sores [of cowpox] bear a resemblance to the Small Pox, *especially the confluent*, should it not encourage the hope," etc.; and, "In my former cases [*i.e.*, previous to getting matter from Woodville] the pustule produced by the insertion of the virus was more like one of those which are so *thickly spread over the body in a bad kind of confluent Small Pox*. This [with Woodville's lymph] was more like a pustule of the distinct Small Pox, except that I saw no instance of pus being formed in it, the matter remaining limpid till the period of scabbing."¹

Woodville, then, passed into common currency a type of cowpox which was less unlike the smallpox pustule than Jenner's had been; and, at the same time, he recognised the differences between his own cowpox and smallpox with more "acuteness" than Jenner (as Pearson said), or with more candour and honesty. By good fortune, as much as by technical skill in inoculating, he got rid of the ulcerous termination of cowpox. Jenner himself admitted that the cowpox produced by Woodville's lymph differed principally in not having "the disposition to ulcerate as in some of the former cases"; and Woodville said: "We have been told that the Cow-pox tumour has frequently produced erysipelatous inflammation and phagedenic ulceration; but the inoculated part has not ulcerated in any of the cases which have been under my care, nor have I observed inflammation to occasion any inconvenience, except in

¹ Ed. 1800, p. 136.

one instance, where it was soon subdued by the application of aqua lithargyri acetati. It would seem, then, that the advantages to be derived from substituting the Cow-pox for the Small-pox must be directly in proportion to the greater mildness of the former than the latter disease."¹

These are the words with which Woodville ends his *Reports of a Series of Inoculations*, the authentic historical narrative of the establishment of cowpoxing on the great scale. His own good faith and genuine belief are everywhere apparent; in those respects he represents, at the outset of vaccination, the state of mind which has been the common one among medical men regarding this practice. Cowpox is a milder disease than smallpox, and equally efficacious; that is the sum and substance of the vaccinator's creed. The efficacy, as proved by the early evidence, falls to be considered in the next chapter; we have here still something to say as to the real meaning of the mild type, which was as conspicuously present in Woodville's lymph as it had been conspicuously absent in Jenner's.

The freedom from risk in vaccinations done on the great scale is, of course, a remarkable fact, when we bear in mind what sort of disease cowpox is. Of the eight hundred thousand infants infected every year in this country with cowpox virus, the vast majority escape very lightly. The mildness of type which Woodville accidentally found in cowpox, or skilfully gave to it, became a cloak for Jenner's numerous inconsistencies and evasions; above all, it served to cover, much more

¹ *Reports*, p. 155.

successfully than his own practice had done, or ever would have done, the unwarrantable liberty he had taken in changing the name of cow's pox into smallpox of the cow. We shall never understand the merits of the vaccination controversy until we understand how the practice came to be adopted by the medical profession, on the recommendation and by the practical endeavours of so honest a man as Woodville. It was not until forty years after Woodville's time that pathological experiment brought to light facts which explain how the illusions about inoculated cowpox had arisen; although these facts have remained unnoticed in this connexion until I adduced them in a book on *The Natural History of Cowpox and Vaccinal Syphilis*.¹ The pathological experiments in question were those made by Ricord in Paris to inoculate the virus of syphilitic sores, or venereal pox, on the skin. If these and other experiments of the same kind had existed in 1798, the secret artifice of making cowpox first known to the profession under the name of smallpox of the cow would have been obvious at least to the pathologists, and would have been exposed in due course; for it would have been shown conclusively that the affinity of cowpox was to the *great* pox of man. In the exercise of that scientific method, the pathologists would only have given proof to the profession of an affinity that the vulgar had originally recognised, without reasoned argument, when they called the sores on the cows' teats and on the milkers' fingers by the name of cowpox: an affinity that Moseley also recognised by his

¹ London, 1887, p. 34.

natural shrewdness when he sought to stigmatise the new inoculation with the name of *lues bovilla*, in his first reference to it in 1798.

One of the fullest narratives of the inoculation of venereal pox by Ricord is given in a communication to the first number of a German periodical called *Syphilidologie*, edited by Behrend. The narrative is from the pen of Dr. Selke, a German who was then following the hospital practice of Ricord, and who enjoyed exceptional opportunities.¹

A young man with multiple primary sores, three of them in the state of small whitish blebs, came into the hospital for venereal disease at Paris on the 4th of May, 1835; he was inoculated next day (5th) on the skin of each thigh with matter from the primary malady—on the left thigh with matter from an unbroken bleb, and on the right with matter from one of the blebs which had meanwhile become an open sore. On the 6th a little pimple appeared at each spot, which was soon surrounded by an areola or zone of redness an inch in diameter. On the 7th the pimples changed into vesicles or pustules, and became seated upon a hard and elevated base. On the 9th each pustule was an eighth of an inch in diameter; and on the day after they began to change into brownish crusts, which on the 11th were a quarter of an inch in diameter. Day by day the crusts grew thicker and broader, and on the 15th an ichor or watery matter was found oozing from beneath them; on the 22nd and 23rd the ichor was a thin brown pus, and on the 29th was of a putrid odour. On the next day the crust on the left thigh, an inch and a quarter broad, came off after poulticing, and revealed a round ulcer, three-quarters of an inch

¹ "I give this case," he says, "as I have seen it, and been enabled to note the successive changes in the patient's condition, from day to day. An English doctor studying in Paris, Dr. A. Thomson, and Dr. Vernois, *interne* under Ricord, have also kept accurate journals of the case, which have been used by me to correct my own notes."—Behrend's *Syphilidologie*, vol. i. 1839.

in diameter, with raised, hard, bluish edges, and a few large yellowish-red granulations in the centre of its dry yellow floor. The day after, a new crust had formed upon it, which was again detached by poulticing on the 1st June.

Meanwhile the crust on the inoculated spot of the right thigh had remained adhering ; on the 5th June it was loosened round the edges, and on the 8th it came away, disclosing, not an ulcer, but another thin, reddish-brown crust or eschar, beneath which was an elevated growth or *bouton* three-quarters of an inch in diameter ; the thin under-crust or eschar became firmer and darker when it was exposed, and in the days or weeks following (for which the daily record is not given), it came away, and left a cavity to be filled gradually by granulations.

The left sore had preceded it in the same course of healing ; and in both instances the induration had disappeared (without mercurial treatment) and the healing had progressed to a cicatrix by the 20th July. The right inoculation was complicated by a secondary sore on the skin near it, which began as a small pustule on the 8th of June, and was the last part of the ulcerative process to get healed.

These are sufficiently typical instances of the behaviour of an ulcerous specific infection when reproduced on the skin by deliberate inoculation ; and they are exactly parallel to the Stroud inoculations with cowpox, in the last chapter (p. 96). The inoculated spot is first a pimple, which becomes a vesicle or bleb or pustule, and quickly passes into a scab. It is under the scab that the active process goes on for some time ; the scab may be removed (with the help of poulticing, if need be), a new scab will probably form, or an eschar be disclosed under the original crust, and the defect of substance will be at length filled up with granulations.

For five-and-twenty years after that date numerous experiments were made in the inoculation of venereal virus upon the skin of the same subject, or in the way

of practising the foolish craze for "syphilisation"; and much was learned of the behaviour of a specific type of ulceration when so inoculated. The vesicular stage often reproduced the figure and colour of the cowpox vesicle almost exactly: that is to say, a large whitish spot of skin, tumid around the edge and less elevated at the centre, which became a scab; in due time the scab would be thrown off and reveal either an eschar filling the cavity, or an open excavation discharging a thin, stinking ichor. Mr. Henry Lee carried his inoculations through several removes, and in a number of instances got the whole process to end with the scab, just as it does in ordinary inoculation with the pox of the cow's teats. The ulcerative phase might, in fact, be got rid of in the course of successive reproductions of the venereal pox, as in the cowpox; and it is significant that the cases of the former which Lee got to dry up from the vesicular stage, without ever passing into a phase of open ulceration, were cases where he had taken the matter for inoculation at a very early stage of the original sore.¹ Those who may desire to see how exactly an inoculated venereal sore in its vesicular stage can resemble the vaccine vesicle have only to look at Lee's plates.²

Ricord's plates³ show a great variety of similar appearances; and we have that experienced syphilographer's recorded opinion,⁴ that the vesiculae pustular

¹ *Med.-Chirurg. Trans.*, xlii. (1859), p 439.

² *Ib.* xliv. (1861), especially fig. 2 of Plate II.

³ *Maladies Vénériennes*. Paris, 1851. Plate I. figs. 6 and 7; Plate III. figs. 7, 8, and 9.

⁴ Reported by Diday, *Traité de la Syphilis des Nouveau-nés et*

stage of a syphilitic infection, produced artificially on the skin, might easily be mistaken for a pure vaccine vesicle produced under the same circumstances.

In the series by Lee, among which there occurred his typical bluish-white vesicle with depressed centre, the succession was kept up to the third remove, "and the poison appeared quite as active and virulent at last as at first." What makes cowpoxing so unique a thing among inoculated infections is that it has been kept up through some thousands of removes, has been steadied, as it were, to a particular eighth-day type, and cultivated into an artificial malady called vaccinia. It cannot be doubted that this was begun by Woodville in the boldness of ignorance, and under the illusion that he was really dealing with smallpox of the cow. It is singular that the boldness of ignorance should have come out so well as it has done; but, with all the average safety of cowpoxing in infants, there have been many reminders during these ninety years that the original type of cowpox is a foul ulceration and not a mere cutaneous eruption. These occasional reversions of type, in contrast to the average mild type of mitigated cowpox, have been dealt with by me in my former book on the *Natural History of Cowpox and Vaccinal Syphilis*; I advert to the subject here only in so far as it serves to explain how Woodville could have gone on in good faith propagating cowpox by inoculation, being misled by the name *variolæ vaccinæ*, or smallpox of the cow.

The advocates of horse-grease, in 1800-1803, as we shall see, were under the same illusion from their want of pathological knowledge. The tumid, whitish vesicle or bleb on a farrier's or stableman's hand was just the same as that upon a milker's, although the cow's pap-pox was far from resembling the horse's "greased" hocks either in causation, or in development, or in issue.¹ The point in common between them was inveterate soreness through filth and neglect; and the infective discharges of each, summing up as they did the history or antecedents of the disease as it then was, brought out a process of infection on the human hand which began in each like the white blister of a burn, and in each generally became in due course a painful and corroding ulcer. Such being the admitted character of each animal disease at the fountain-head, it seems well-nigh incredible that medical men, with some pretensions to a discriminating knowledge of the processes of disease, should have allowed Jenner's bold invention of a "smallpox of the cow," derived from horse-grease, to pass into current professional teaching.

The fact that so unreasoned and nonsensical a doctrine did become current suggests various reflections and vain regrets. Had there been in medicine some encouragement for the logical or dialectical qualities of mind which are the ground of authoritative position in the law, there would have been such a force of critical scrutiny brought to bear upon the project of cowpoxing as would

¹ Hering (*Ueber Kuhpocken an Kühen*, Stuttgart, 1839) speaks of the "slight similarity" between the two diseases, although the infection of the human hand from each of them was the same.

have effectually unmasked the illusion about "smallpox of the cow," and brought the evidence on the protection against smallpox afforded by the ulcerous infection of the teats to its proper bearings. Of such critical scrutiny in the most authoritative circles, there was none. The invention of the new name was artfully concealed and was never found out; and under the influence of the plausible idea which the name covered, the evidence of protection was accepted on terms which will seem incredibly loose to all who have not hitherto made acquaintance with the standard of logic in the medical profession.

CHAPTER VI.

THE VARIOLOUS TEST.

THE one great question which the profession had to satisfy themselves upon, after a stock of cowpox matter had been found by Woodville and distributed by Pearson, was whether vaccine inoculation warded off smallpox. There were other points supposed to be at issue, such as whether vaccine caused an eruption, whether vaccine inoculation spread contagion, and whether the operation was attended with risk to life; but the main question was whether it answered the purpose that Jenner recommended it for. There were two ways proposed of getting an answer to the question, the way of experiment and the way of experience. Experience was, of course, the best test, but it was not usually the quickest. The profession wanted to know the value of the new protective as soon as possible, and they proceeded to test their first cowpoxed cases by inoculating them soon after with smallpox. That was the famous variolous test.

No one seems to have discussed the validity of the variolous test as a proof of the protective power of cowpox for practical purposes. Jenner resorted to it as if it were a matter of course to do so; and his example was implicitly followed. The principle of test-

ing the force of one inoculation by means of a second was part of the current inoculation doctrine of the time. Men of the stamp of Daniel Sutton were accustomed to reassure their clients that they were safe, by showing them that a second, or third, or fourth inoculation produced either no smallpox at all, or a less amount of smallpox than the first inoculation.¹ The assurance given on these experimental grounds proved not unfrequently to be fallacious; when the real trial came, it was not unusual for the inoculated to take the epidemic disorder just like their unprotected neighbours. There was no lack of sobering experience of that kind; so that in Paris the inoculators lost credit, and in course of time almost ceased to find employment.

After describing in a letter to a friend his first case of cowpoxing (James Phipps, 1796), Jenner goes on to say: "But now listen to the most delightful part of my story. The boy has since been inoculated for the smallpox, which, as I ventured to predict, produced no effect."² He was tried again a few months after, when no effects were produced "on the constitution." 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 smallpox; he was a poor consumptive or scrofulous youth, with his lymphatic glands so clogged (after the cowpox?), that any subsequent inoculation of virus on the arm had no chance of being absorbed.³

¹ W. Langton, M.D., *An Address to the Public on the Present Method of Inoculation*. London and Salisbury, 1767.

² Jenner to Gardner, Baron, i.

³ Baron, ii. 304.

The *Inquiry* contained only two, or at most three other variolous tests done on vaccinated children; Jenner rushed off to London to get it printed, without waiting to apply the test himself, but his assistant applied it in two or perhaps three of the cases. Even careless readers could hardly have been satisfied with the evidence, so far, that the vaccinated were "for ever after secure from the infection of the smallpox," as Jenner boldly alleged. When he went back to the country, after launching his discovery, he applied the test to one other vaccinated child, and produced the local pustule, the eruptive fever, and a transient eruption on the wrists. Jenner did not inoculate his Stonehouse cases (in December, 1798) with smallpox; but 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 smallpox 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.

By way of a control upon Jenner, the independent evidence from Stroud was not encouraging. The next evidence was that which Woodville's continuous series of cases enabled him to supply on the grand scale. He carried on his vaccinations at the old Inoculation Hospital, in an atmosphere of smallpox contagion; so that cowpox had a great opportunity of showing its protective power. Jenner had hastened to advise Woodville to inoculate those patients with smallpox who might "resist the action of the cowpox matter"; and

Woodville did inoculate several a few days after they had been vaccinated. Even those who did not resist the action of cowpox, but on the other hand developed full and correct vesicles, acquired the smallpox also, either by inoculation or by contagion. Thus, Ann Bumpus, from whose vaccine vesicles Jenner's own stock of lymph was procured, sickened for natural smallpox on the tenth day from her vaccination and had 310 pustules of that disease on the fifteenth day. Her immediate vaccinifer but one, Jane Collingridge, was inoculated with smallpox on the fifth day after vaccination, and had an eruption of 100 to 200 variolous pustules. These experiences were frequent in the first weeks of Woodville's practice with cowpox in the atmosphere of the smallpox hospital. Woodville was so sure of the protective power of cowpox that he tried to explain the eruptions in every way but the right one. "I did not conceive it to be possible," he wrote, "till after I had made repeated trials of the new inoculation out of the hospital; nor is the fact to be easily explained," etc.¹

At length it was admitted that cowpox did not prevent smallpox if both infections were received together, or if the cowpox had no more than a few days' start; the protection from smallpox was only established in the system when cowpox had run its course, and produced its full constitutional action. The system was supposed to have been, as it were, touched profoundly by the virus of cowpox, and to have been rendered insensible to the action of smallpox for ever after. No

¹ *Med. and Phys. Journ.*, Dec., 1800.

one was able to explain how an infection that was so unlike in kind to smallpox as to run its full course while the latter ran its full course also, could become an antagonistic influence in the years succeeding, when nothing remained of it but a scar ; indeed, the prophylaxis was candidly acknowledged, as we shall see, by those who thought about it at all, to be of the nature of a mystery.

Woodville put all his cases through a variolous test shortly after the cowpox infection was over, including even the large number of his cases who had gone through the smallpox itself concurrently with the cowpox. They all stood the test equally ; and these cases, to the number of several hundred, formed the nucleus of the great body of English variolous tests which the Germans and others were fond of adducing as a grand total of evidence, behind which it was unnecessary for most doctors to go. Pearson also wrote that he had "inoculated many scores with smallpox matter after the vaccine disease, and never with the effect of exciting the smallpox." He adds, however : "I have, indeed, been desired to see even some of my own patients who, I was acquainted, had taken the smallpox ; but these cases turned out to be either those in which the cowpox had not in reality preceded, or they were cases of merely local affection from the inoculated smallpox."¹ Jenner's friend, Marshall, of Eastington, wrote that he had tested 211 out of his total of 423 (as nearly as possible fifty per cent.), but found that every one was protected.² This is

¹ *Med. and Phys. Journ.*, ii. (Oct., 1799), p. 216.

² *Lond. Med. Rev.*, iii. (March, 1800).

the same Marshall who declared that 127 out of his total of 423 (as nearly as possible thirty per cent.) were vaccinated with lymph of his own raising.

Evidence from various parts of the country soon began to appear in the medical journals. Evans, of Ketley, near Shiffnal, had vaccinated successfully sixty-eight persons, of whom thirty-nine had vaccinal eruptions and several had troublesome ulcers; of these he tested twelve with smallpox, and found that they resisted it.¹ Even when the vaccinated did not resist smallpox inoculation, it was not thought of much account. M. Ward, surgeon to the Manchester Infirmary, sent the following series of cases,² and "congratulated mankind" on the success of cowpox:—

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

CASE II.—Infant aged nine months, brother of No. i. Successful vaccination at two points (one healed on 15th day, other covered by crust, which became a superficial ulcer after the 21st day and yielded ichor up to the 32nd day). Caught the smallpox from his sister, and had about 50 pustules, mostly on his face, which began to show about the 35th day from his vaccination.

CASE III.—Aged 5 months. Vaccination did not hold. Variolation did hold.

CASE IV.—Aged 5 years. Did not take vaccine. Did not take smallpox after two trials by inoculation.

CASE V.—Aged 9 months. Did not take vaccine. Did not take smallpox at twice.

CASE VI.—Aged 3 years. Did not take vaccine. Did not take

¹ *Med. and Phys. Journ.*, ii. 310.

² *Med. and Phys. Journ.*, ii. 134, paper dated 12th July, 1799.

smallpox. Variolation had failed, beyond local inflammation, when tried four months before.

CASE VII.—Aged 5 months. Vaccination failed, though tried twice. Variolation failed, but arm swelled.

CASE VIII.—Aged 16 months. Successful vaccination (areola on 11th day, very extensive, with much fever). Variolated on the 29th day, without result.

CASE IX.—Aged 19 weeks. Successful vaccination (slight vaccinal eruption on arm). Variolated on the 12th day, local pustule on the 19th day, eruption (of thirty pustules) on the 22nd day.

CASE X.—Aged 14 weeks. Inoculated from Case i., evidently with the coexistent smallpox matter mistaken for cowpox. Sickened on 7th day, eruption of smallpox on 10th, full burden on 12th, but not confluent. Variolated on the 14th day without result.

CASE XI.—Aged 9 months. Also inoculated from Case i. (complicated with smallpox), with same result as in Case x.

CASES XII., XIII., and XIV.—Results not known.

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 cowpoxed, namely Case viii.; three cases took smallpox in the clearest way after being cowpoxed (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.

Some practitioners were shaken in their faith when the variolous test failed. One of these, Shorter, of Bloxham, near Banbury, wrote to Jenner that he had succeeded in producing the correct variolous pustule at the place of

insertion in recently vaccinated cases; but Jenner's "gentleman-like letter removed all my doubts, and I have again resumed the practice."¹ Another, Boddington, found, in the case of his own child, that the test produced not only the local pustule, but also the general smallpox eruption. Jenner, in reply, gave him a tremendous wiggling:² "How a gentleman, following a profession the guardian angel of which is fame, should have so committed himself as to have called this a case of smallpox after cowpox is not only astonishing to me, but must be so to all who know anything of the animal economy." This, as Baron says, is a good illustration of Jenner's "method of dealing with rumours of that kind." Beyond all question, the eruption was the due consequence of inoculating with smallpox virus. Jenner's bullying attempt to refer it simply to the child's tender skin should be pondered by all who believe his reasoning powers to have been masterly.

The variolous test, with such validity as it had, was not applied at all generally in England after the first weeks of cowpoxing in the spring and summer of 1799. 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. Whenever we have the opportunity to scrutinize the actual sequence of events, we find that the inoculation of smallpox was

¹ *Med. and Phys. Journ.*, iii. 348. The letter is a good sample of Jenner's wheedling manner.

² Jenner to Boddington, 21st April, 1801, in Baron, i. 445.

nearly always followed by some degree of local action and in most cases by a full and correct variolous pustule. It was chiefly the eruptive fever and the general eruption which aborted or remained in abeyance. A common experience, probably the average experience, was thus stated in a letter to Jenner by W. Forbes, of Camberwell, who had applied the test a good many times :¹

“Although the variolous inflammation perfectly succeeded, and, I have no doubt, would have infected others inoculated with the fluid secreted, yet the constitutional progress was as completely arrested as if the patient had gone through the smallpox before.”

In their indifference to the production of local smallpox in testing the virtue of cowpox, and to any slight show of eruptive fever, the profession betrayed a singular facility for dropping all at once a mode of reasoning that they had made great account of under the old inoculation *régime*. The slight effects of the smallpox inoculation, mostly the local effects, which were thought nothing of when the inoculation was done to test the protective virtue of preceding cowpox, had been held of the utmost account, had been carefully noted and liberally appraised, when inoculation was an end in itself, or when it was done in the ordinary way of pre-Jennerian protection. In order to make clear to modern readers this use of a double standard of what constituted effective variolation, it will be necessary to go back some thirty years prior to the advent of Jenner. The digression will be of some length, but the importance of the subject will perhaps be found to justify it.

¹ *Med. and Phys. Journ.*, vi. 314.

The years from 1764 to 1767 saw the rise of a "new method" of smallpox inoculation in England, and a lively controversy thereon. Whatever may be urged against the discretion and wisdom of the medical men who took up the practice of inoculation after it was first introduced from Turkey by Maitland, at the instance of Lady Mary Wortley Montagu, in 1721, it can hardly be said that they scamped their work. They seem to have given a considerable dose of the inoculated smallpox either unavoidably or of choice,¹ with the intention of anticipating the attack of it in the ordinary way of endemic or epidemic contagion. The severity of the engrafted disease was a serious bar to the general adoption of this Turkish and beauty-saving artifice; and in a few years it fell into considerable disrepute. From the depths of its unpopularity it emerged shortly after to enjoy a qualified success; and from the year 1764, when the Suttonian method was introduced, it continued in vogue in England until it was superseded by Jenner's cowpox. Of the method as practised before Sutton's and Dimsdale's time, we may take our impressions from the well-written essay by James Burges.²

The obvious thing in the essay of Burges is his anxiety to bring out the eruption, to give it facilities for coming out, and to obviate whatever would tend to repress it or "repercuss" it. The Boerhaavian pathology of the time was used as the scientific justification of

¹ See Nettleton (*Philos. Trans. of Royal Soc.*, 1722) and other authorities cited by White in *Story of a Great Delusion*. London, 1885, p. 30.

² *The Preparation and Management necessary to Inoculation*. London, 1754.

these common-sense aims. Warmth in bed was advised, lest the external air should "obstruct the exclusion of the infectious matter." The apartment was not to be "so open and cool as to produce a degree of chilliness sufficient to check those sweats that are in this state quite necessary for bringing out the eruption." Again, the cold of winter, "by obstructing the pores and constringing the vessels, brings such an overcharge on them that they become unable to get rid of the load"; and the extreme heats of summer had somehow the same obstructive effect in another way. One great risk of "repercussing" or driving back the eruption was that the habit for long after would be clogged or encumbered with "obstructions." Another common-sense risk was thus technically expressed by Richard Holland in 1728: "In a genuine and complete eruption, the matter of the disease is entirely evacuated, and therefore there is no possibility of a return. But in an imperfect crisis, part of the original cause may remain."¹

It was only when the general eruption of the engrafted smallpox was checked by a chill or other accident that the early inoculators counted it a failure; if the pustules died away, or came to nothing when due care had been taken, it meant that the habit was a favourable one, or that there was a natural disposition to take the disease mildly. But in Burges we may notice also the first insidious beginnings of a mode of reasoning which was carried to great lengths a few years later. The original practice in England was to insert

¹ *Observation on the Smallpox and a more effectual Method of Cure.* London, 1728.

the smallpox matter at a quite large and deep incision in the arm, and, in some instances, to keep open the sore so produced as a rendering issue for weeks after. Even if no effort were made to keep the original incision an open sore, it might go on rendering for some time. In such cases, as Burges says, "the extent and duration of the primary variolous insertion would seem to have checked the general eruption"; and he gives a particular instance where the primary spot sloughed, gradually widened, and continued to discharge for six or seven weeks, so that no general eruption took place at all. But now let us carefully observe the significance that Burges puts upon this abeyance of the eruption:¹ "But if the sores keep open, and the feverish symptoms come on at the usual time, though not a single pustule should appear, I am convinced that the patient is as secure from ever having the smallpox as if there had been a plentiful eruption; at least there is no instance that has ever been produced where it has happened; even though the utmost endeavours have been used to procure a second infection on a supposition that the first had been imperfect."²

Other means, preferable to an open sore on the arm rendering matter for weeks, were soon found to keep back the general eruption, without impairing the value of the protection given by the single representative local pustule. This was the "new method" of inocula-

¹ *L. c.*, chapter xv. p. 41.

² The stock instance in the books is that of the Hon. John Yorke, inoculated (without eruption) by Mr. Sergeant Hawkins at the age of twenty, and re-inoculated in vain.

tion, begun in France by the famous inoculator Angelo Gatti, practised in England to their own enrichment by Daniel Sutton and by Dr. Dimsdale, of Hertford, and, after a vain remonstrance by two or three staunch men, generally approved by Sir George Baker, Sir William Watson, and other leaders of the profession in the latter half of the eighteenth century.

Gatti found respectable or scientific reasons for the vulgar charlatanism of old women in the Levant. Sir George Baker approvingly quotes him to this effect (1766):¹ "In the Levant old women have inoculated ten thousand people without an accident. They only ask: Is the person prepared by nature? Is the breath sweet? Is the skin soft? Does a little wound heal easily? Then the inoculation may be undertaken without the least fear of danger." These were the only conditions made by the old witches of Mussulman countries; but the conditions were the quintessence of craft, whether Sir George Baker knew it or not. They meant no less than a selection of such subjects as experience had shown to be likely to have the milder form of smallpox by inoculation ("prepared by nature," forsooth!). Gatti, in the exercise of his art in France, could hardly pick his cases by the divination which old women in the East were privileged to exercise; but he always endeavoured to make the inoculated disease as mild as possible. Instead of making a large incision, and laying the matter therein upon a thread, he made a small oblique puncture with a lancet point and in-

¹ *Inquiry into the Method of Inoculating the Smallpox.* London, 1766.

served the minutest quantity of matter. Moreover, he took the matter from as early a stage of the natural smallpox as any fluid could be got at all, and from the mildest case ; and, improving upon that, he at length transferred matter from the early vesicle of one inoculated arm to another person's arm, and so on through a series of cases from arm to arm. He dropped the old treatment that had been used to "bring out" the eruption ; and, by keeping the patient's hand immersed in cold water, he often succeeded, with the help of his other arts, in limiting the whole process to the primary smallpox pustule at the place of insertion.¹

For some time Gatti did a large business in his "new method" of inoculation, amassing a fortune and acquiring fame. At length an accident happened in the case of a great lady, the Duchesse de Boufflers. Two years and a half after being inoculated by Gatti, and assured by him of protection, she had an attack of smallpox, which made a great noise. Her inoculated protective disease had consisted of the local pustule, some abortive pimples round it, an abortive fever on the eleventh day, and one large pustule on the forehead which left a mark visible for long after. About the same time, many other persons in Paris, who had been inoculated by Gatti, confidently exposed themselves among the sick during an epidemic of smallpox, with the result that they caught the disease in large numbers, and not a few of them died of it. These accidents made an end of Gatti's credit, and the practice of inoculation,

¹ See the summary of his practice in Bohn's *Handbuch der Vaccination*. Leipzig, 1875, p. 82.

by whatever method, was forbidden in Paris by a statute of the Parliament.

In England it fared otherwise with the new method of "buying the smallpox" on the easiest terms. The method was much the same as Gatti's, with some additional conjuring by means of secret pills and powders, which were at length found to be calomel and antimony, the theoretical Boerhaavian "antidotes" to smallpox. According to Daniel Sutton's advertisement, which he put out in the form of a sermon (with appendix) by a chaplain in his pay, the patients in his establishment at Ingatestone "have in general little or no sickness; their indisposition is so trifling that they are ashamed to complain, and in a few days they are perfectly well. Here is no confinement, no keeping of bed. All is mirth, and all seem happy. If any patient has twenty or thirty pustules, he is said to have the smallpox very heavy."¹ According to Chandler, however, this was merely the tempting bait; for some of Sutton's patients had a more copious eruption, despite all his efforts to keep it back.

Daniel Sutton quickly made money, and in 1766 he was followed by Dr. Dimsdale, of Hertford, who also made a fortune and became a banker in Cornhill. Dimsdale gave a tolerably candid account of his practice. He inoculated many of his patients a second time, and produced the same local pustule as before, but without fever. Others had symptoms of the eruptive fever (on the first trial), but no pimples. "In

¹ Rev. R. Houlton, *Sermon in Defence of Inoculation*. Chelmsford, 1766. Appendix, p. 40.

many instances of cases related by the doctor, on going home the smallpox seeds were brought forward, and the disease appeared in the usual stages.”¹

The “new method” of making inoculated smallpox easy not only took the fancy of the public, but soon found vindication in the profession. Ruston, Giles Watts, and others published on it; according to Watts, “a most extraordinary improvement is made, and the art of inoculation is enabled to reduce the distemper to almost as low a degree as we could wish. . . . There is now an opportunity of seeing what a very small number out of the multitude of persons of all ages, habits, and constitutions, who have been inoculated in these parts [Sussex and Kent] have been ill after it.”²

The opposition to the new method was confined to a few, notably William Bromfeild, an eminent court surgeon, and Dr. Langton, of Salisbury. Bromfeild, in an essay dedicated to the Queen, reminded his colleagues of the general tendency in medicine towards credulity, and remarked that even the French had passed through a fit of the same; for it was mere credulity on their part “to have given credit to a man [Gatti] who should assert he would give them a disease which should not produce one single symptom that could characterize it from their usual state of health.” He was “afraid that inoculation, though hitherto a great

¹ W. Bromfeild, *Thoughts on the Method of Treating Persons Inoculated for the Smallpox*. London, 1767.

² Giles Watts, M.D., *A Vindication of the Method of Inoculating the Smallpox*. London, 1767, p. v.

blessing to our island, will in a very short time be brought into disgrace," if people went on believing "that health and security from the disease can be equally obtained by reducing the patients so low as only to produce five to fifteen pimples." He had been told (what was the fact) that "many lost their lives in Paris, after the epidemical phrenzy for inoculating in the new way there, which in general neither occasioned fever nor eruptions." If they could see the new method *only* in the light of its mildness, it would be unpardonable prejudice to oppose it; but did it really give protection from smallpox? ¹

Dr. Langton was even more alive to the illusory nature of the new practice. He issued an *Address to the Public on the present Method of Inoculation, proving that the Matter communicated is not the Smallpox, because Numbers have been Inoculated a second, third, and fourth Time, that therefore it is no security against a future Infection.*² After quoting the case of the Duchesse de Boufflers, he says that "not above one in ten have so many variolous symptoms as may be re-

¹ Bromfeild, *l.c.*, 1767, pp. 43-5. His own inoculation practice at Court was attended with disasters. Prince Octavius, youngest child of George III., died of inoculation. In other cases within the Court circle, the inoculation by Bromfeild, severe though it was, proved to be no security. See *Court and Private Life of Queen Charlotte*, being the Journals of Mrs. Papendiek. London, 1887, i. 41, 70, 270. In a letter to James Moore, who was writing a history of vaccination, Jenner says: "The late Mr. Bromfeild abandoned the practice of inoculation in consequence of its failure. Is not that a precious anecdote for your new work?"—Baron, ii. 401.

² London and Salisbury, 1767.

marked in her case.”¹ Besides the local pustule, there were usually only one or two pimples, or watery vesicles, which never matured.

Bromfeild and Langton were not supported by the academical leaders, who, as usual, found it politic to go with the stream. The principal spokesman was Sir George Baker, afterwards president of the College of Physicians, who had no objection to the practice of Daniel Sutton provided it were not kept a secret. He waxed eloquent on the invariable plea of the more stolid Englishman, the plea of giving the novelty a trial. “He is an enemy to improvement,” said Baker, “and is no philosopher, who fastidiously and upon mere speculation rejects what he has not brought to the test of experiment.” To the test of experiment, accordingly, they all set about bringing Sutton’s quackery; with the usual result that, in a very short time, their self-love got involved in the issue of the experiment, and a course of dogmatics and apologetics, or what is commonly called hard swearing, was entered upon so as to circumvent the teaching of common sense.

The mere formality of smallpox, as gone through by the new method of inoculation, was held to be a sufficient protection from the epidemic contagion. It became at least the object of inoculators, even if they did not always succeed, to attenuate smallpox to the shadow of its real self. Such was the respectable practice in England during the latter part of the eighteenth century. In 1796, only two years before cowpox came on the scene, Woodville published the first (and only) volume of his *History of the Inoculation of the Smallpox*

¹ *L. c.*, p. 18.

in Great Britain, wherein he carries the developments of the practice down to the adoption of Dimsdale's mild methods. A sentence in the preface gives us a glimpse of Woodville's own aims; new researches, he says, are needed, because "the established process [Sutton's and Dimsdale's] will, in certain cases, not only fail of success, but evidently produce the disease in an aggravated state." We have further evidence of the contemporary mode of variolation in the handbook published as late as 1806 by Lipscomb, the third in succession of a noted family of inoculators.¹ Matter should be taken, he advises, as soon as any fluid can be obtained from the eruption in a mild case of natural smallpox; the patient should be kept out of bed and as much as possible in the open air, particularly during the eruptive fever. If these precautions be observed, "the complaints are usually very trivial during the eruptive stage; the patient eats and sleeps well; a few pustules may appear, irregular and dispersed."

Such was the kind of protective smallpox inoculation deliberately aimed at, and in the majority of cases accomplished, under the *régime* of the "new method," which dated in England from 1764. There is no reason to suppose that the earlier and severer type of inoculated disease was ever re-introduced of purpose into English practice, although there may have been old-fashioned inoculators here or there; and there probably were always a few cases which turned out more severe than the inoculator had intended or had thought necessary. In Jenner's time the type and mode of inoculation were those of Sutton and Dimsdale; his near neighbour,

¹ *Manual of Inoculation*. London, 1806, p. 8.

Fewster, of Thornbury, had actually been a pupil of Sutton in the inoculation business.

But we have clear enough evidence of what Jenner himself understood by variolous inoculation, and wished others to understand by it, in its new use of testing the strength of cowpox. The *Inquiry* of 1798 contains a few pages devoted to the subject of smallpox inoculation, which are introduced with a very definite purpose, although that purpose is nowhere explicitly stated. We suddenly find ourselves reading about "varieties of smallpox," from which we pass to one variety that had occurred in the inoculation practice of "a medical gentleman, now no more," which variety was "certainly not the smallpox" at all. The deceased inoculator had some special way of managing matter; "so strongly persuaded was he that he could produce a mild smallpox by his mode of managing matter that he spoke of it as a useful discovery until convinced of his error by the fatal consequence which ensued."¹ The matter produced the local pustule or pustules, swellings of the glands in the armpit, the ninth-day fever, and "sometimes eruptions"; but it so happened that epidemic smallpox broke out in the locality, and "many unfortunately fell victims to it who thought themselves in perfect security."

Jenner recalls the incident (which was of a kind common enough everywhere) in order to suggest that these inoculations had been spurious: "But what was the disease? Certainly not the smallpox." This is very like the staunch language of Langton and Brom-

¹ Jenner's *Further Observations*, ed. cit., p. 84.

feild, who protested against all modes of "managing matter" with a view to make the inoculated disease a mild or formal affair. That, however, was not Jenner's suggestion; the spuriousness and inefficacy that he wishes to guard his readers against, with a view, however, to the variolous test of cowpox, and not to inoculation for its own sake, were due to the fact that the variolous matter had not been "managed" enough; it had undergone some (purely imaginary) "putrefactive" change from being taken at too advanced a period of the smallpox pustules. He points out how careful he had himself been to avoid such causes of "spuriousness" in trying the variolous test upon cowpoxed milkers:—

"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 cowpox. *This I conceived to be of the greatest importance in conducting these experiments.*" No doubt of the greatest importance. And what was the attention that he paid to the state of the variolous matter previous to using it for the proof that cowpoxed milkers could not take smallpox? 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 cowpoxed milker, aged sixty-two, was tested with smallpox, 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 smallpox; it was taken at an 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.

Very few modern readers of the *Inquiry* will see the drift of those pages in it devoted to the modes of variolous inoculation unless they read them with especial care. The subject was not introduced for nothing; these portentous warnings about "much subsequent mischief and confusion" (p. 56), if attention were not paid to the state of the variolous matter used for inoculation, were merely Jenner's way of creating a preference for the ultra-Suttonian method of inoculation, when the variolous test was to be applied to his new project. It was in that way that he had himself applied the variolous test; it was in that way that he wished others to apply it; and there can be no doubt that it was after the Suttonian method that it was generally applied in proving the protective power of cowpox.

We come, then, to this extraordinary result, that the very same degree of smallpox 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 cowpoxing and with a view

to test the alleged antagonistic power of the latter against smallpox. I am aware of the gravity of that accusation against the common intelligence and moral prudence of the medical profession on the occasion, when they were asked to deliver judgment on Jenner's novelty. Every allowance should be made for the position that they were placed in with reference to the new protective disease. As Denman tells us, hardly any of them had ever heard even the name of cowpox before; it was sprung upon them by a practitioner of the dairy-farming districts, who was treated with unusual deference because he happened to be a fellow of the Royal Society, and had the air of being a modest and honourable man; it was sprung upon them under the invented name of *variola vaccinae*, or "smallpox of the cow," which, for all they were ever told, might have been an ancient designation. The profession were undoubtedly mystified and hoodwinked about the true nature of cowpox; they were started off on an entirely false analogy by Jenner's adroit title-page. But I can find no excuse for their conduct over the testing inoculation with smallpox, on the result of which it was generally agreed that the verdict was to turn. If any of my readers or critics, having taken the trouble to go over the evidence at first hand, will make out a case more favourable to the leaders and editors of medical opinion at this juncture, I shall be ready to amend the result of my own investigation, finding it somewhat incredible as it stands. The conclusion, as it stands, comes to this: that the same effects of smallpox inoculation which were counted good enough when the object was to give protection to their patients from the sub-

sequent risk of contagious smallpox, were reckoned as nothing at all when the object was to test whether their patients had been made safe by cowpox. I do not know any uglier incident in the history of medicine than that astounding *volte-face*.

We have now brought the evidence in favour of the variolous test down through several degrees. Firstly, the test, as applied naturally or accidentally at Woodville's hospital, broke down palpably; secondly, in a number of the early trials for which we have full particulars, it was followed by a fair average amount of smallpox as inoculated; thirdly, in the ordinary run of cases, it was often followed by about as much of smallpox as the bogus inoculation practice of the time was calculated to produce. But if there be still a margin of abortive inoculated smallpox standing at the credit of the previous cowpox, there is ample explanation of the fact without assuming any specific antagonism in the vaccine. This is the last remaining point to be considered in connection with the variolous test.

In the first place, for the ordinary purpose of inoculation, it was not enough to take a child and merely insert the smallpox matter under the skin. Some trouble had to be taken so as to ensure that the inoculation would produce any result at all. We find this frankly admitted by an enthusiastic vaccinator, Trotter, the well-known author of *Medicina Nautica*:¹—

“When my practice formerly lay much in this way, and finding my incisions often fail in communicating the variolous infection, particularly with very young

¹ *Med. and Phys. Journ.*, iii. 525.

children, I was in the habit of ordering the arm to be *well bathed* with warm milk and water; which, when wiped with a *rough towel*, would excite such a temporary inflammation of the spot that I never failed afterwards." Success in variolation, he concludes, depended, for one thing, upon the state of the skin at the seat of puncture. As a practical comment upon this, it may be stated that Sutton and others frequently succeeded in smallpoxing the vaccinated, after the believers in cowpox protection had failed.¹

Jenner himself, in the section of the *Inquiry* where he specifies the sorts of persons for whom cowpox was best suited, mentions a class of children who were apt to resist the inoculation of smallpox altogether. Scrofulous children, with clogged absorbent glands, were of that kind; and his own show case, James Phipps, was a good instance. A large proportion of the variolous tests, especially abroad, were done upon the inmates of orphanages and foundling hospitals, who are notoriously subject to chronic swelling of the lymphatic glands.

But the most obvious consideration, which should have been familiar to those who first tried cowpox and tested it, was 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. It was in Paris that this point was chiefly urged

¹ See Moseley's *Commentaries on the Lues Bovilla*. London, 1807.

by the critics of the variolous test, and the point was at length conceded. In England, it does not appear that the early vaccinators gave any heed to it.

Apart from the swollen and clogged state of the absorbent glands after cowpox, the mere presence of a sore of any kind on the arm served to divert and obviate the full action of a new infection. It was a common remark, in the earlier period of inoculation with the smallpox, that the insertion of the matter by a large and deep wound, which suppurated, and either became an issue or was made one, tended to keep back the general eruption. According to Burges, not a single pustule might appear "where the sores keep open"; and again, "the extent and duration of the primary variolous insertion would seem to have checked the general eruption." The same experience is stated by Ruston in an inverted form, which shows that he did not understand the significance of it: "We sometimes find the wounds even of those who have afterwards very few smallpox, except just in those parts [*i.e.*, around the wound], exceedingly foul and very ill-conditioned."¹ It is difficult to understand why the original incision should have ever been deliberately encouraged to become a rendering sore, unless it had, as a matter of fact, helped to abrogate the eruption, the fanciful theory of the time being that such an issue was an outlet for the infectious matter pervading the system.

Now, the cowpox upon the arm was often such a discharging sore in the early practice. Most of Jenner's original inoculations resulted in eschars and sores that

¹ T. Ruston, M.D., *Essay on Inoculation*, p. 55. London, 1767.

went on for weeks, and some of them resulted in quite large phagedenic ulcerations. Cline, who made the first trial of cowpox in London, actually intended to turn the resulting sore into an issue for the benefit of the chronic hip-disease of the child on whom the experiment was tried. In the Stroud experiment with matter from the same source (Stonehouse), which produced Jenner's cases of phagedenic ulceration, we have a striking proof by the method of difference: in the three cases which had mild and non-ulcerative cowpox, the variolous test at the eighth day gave both the local pustule and the ordinary fever with eruption, or, as the narrative says, the patients "went through the smallpox in the usual way"; in the other two cases, which had severe initial cowpox and open sores for weeks after, the variolous test at the eighth day gave the local pustule only, and when it was repeated after the cowpox vesicles had actually become eschars or ulcers, it gave nothing at all. Such eschars and ulcerations in the early practice were not uncommon, being the natural effects of cowpox matter in the early removes from the cow.¹ Thus in the series published by Addington, of West Bromwich,² there were ulcerations among his first eleven cases, but none in the remaining fifty; and the same event occurred uniformly in the establishment of new stocks of lymph from the cow by such experimenters as Estlin, Bousquet, and Ceely forty years after. Now, these first vaccinations were just the cases upon

¹ Henry Hicks (of Eastington), *Observations on Dr. Pearson's "Examination of the Report."* Stroud, 1803, p. 43.

² *Practical Observations on the Inoculation of the Cowpox.* Birmingham, 1801.

which the variolous test was systematically tried in Jenner's time; after it had been tried upon a few cases at the outset, with an apparently satisfactory result, it was tried more intermittently on those that followed, and it soon ceased to be tried at all. Thus the early cases had often a condition of the arm or arms which sufficed, according to analogy, to render the variolous infection nugatory, apart from anything specifically antagonistic in the nature of the sore arm.

In order to bring out all that is here asserted, let us imagine a parallel case. Let us suppose that the glowing end of a cigar is firmly applied to an infant's arm; an eschar and an indurated sore will result, which may be called cigar-pox.¹ Let the variolous test be now tried, and there is every reason to expect, assuming the lymphatic glands to be touched, that the result will be the same as after cowpox. Of course the experiment can never be made; but the cigar-pox is in its pathology just as relevant to the smallpox as cowpox is.

Two other things in cowpox infection, besides the state of the lymphatic glands and the sore, helped to check or render abortive the evolution of the inoculated smallpox. One of these was the extent of the areola and the degree of constitutional upset; the other was the occasional presence of the general vaccinal exanthem, or eruption proper to cowpox. In the series of cases by Ward, of Manchester (see p. 130), the only one which did stand the variolous test after cowpox was a case in

¹ This artifice is actually practised with success by Belgian soldiers undergoing imprisonment, so as to get themselves placed on the sick-list for venereal disease. See De Broën, *Gaz. des Hôpît.*, 14 Aug., 1880.

which the areola began on the eleventh day, became "very extensive" and was accompanied by "much fever," the test having been applied on the twenty-ninth day. An illness such as that, in an infant of sixteen months, would hardly have ceased of its effects in eighteen days; the system would have been sufficiently preoccupied to make the evolution of a new virus improbable. That is a very common type of case; and it exemplifies one of the most obvious reasons why the variolous test produced either no smallpox at all or a degree less of smallpox than the variolation of the time produced in ordinary.

The vaccinal exanthem or skin-eruption proper to cowpox was a frequent incident of the early days of vaccination, as it was afterwards found to be in Estlin's experience with matter in the first removes from the cow.¹ In Woodville's cases at the Inoculation Hospital it got mixed up with the true pustular eruption of smallpox which many of the patients had, and the significance of it was not made out for a time owing to that confusion. But it was often observed in the country practice of vaccination, where concurrent smallpox 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.² Evans applied the variolous test in twelve cases only of his series of seventy, which had doubtless included a proportion of the eruption cases. In one of the first German trials, at Bremen, a third part of the cases had

¹ *Lond. Med. Gazette*, xxii. (1838), p. 977; xxiv. (1839) p. 153.

² *Med. and Phys. Journ.*, ii. 310.

a rash or eruption.¹ Now, an eruption after cowpox has the same significance as an eruption after the pox proper ; it is a secondary, or a sign that the constitution has been touched by the infection. A person still under the influence of the secondaries of cowpox would not be a likely subject for smallpox engrafted on the top of it.

But even if we attach no constitutional significance to the proper eruption of cowpox, the mere presence on the skin of spots or pimples or vesicles or blebs would hinder the full evolution of smallpox by inoculation. In the essay by Burges on the *Preparation and Management necessary to Inoculation* we read that "cutaneous eruptions render a child an improper subject for inoculation until those disorders are removed." That meant that the inoculation would either not take at all, or would miscarry ; and in the time of Burges, they did not wish it to miscarry. It is hardly necessary to accumulate evidence on the point. The presence of any common eruption, even itch, was well known to prevent the cowpox itself from taking. Jenner began, about the year 1804, to explain the failure of cowpox by an ambitious doctrine of "herpes," which the profession gave no heed to ; but it had this grain of truth in it, that an infection inserted under the skin would not have a fair chance of being absorbed if the skin were already engaged with an eruption even of the most ordinary kind. In so far as that was a plea for the failure of cowpox, it was a plea for the failure of inoculated smallpox. Only in those enthusiastic days the homely maxim of "sauce for the goose, sauce for the gander," was unhappily lost sight of.

¹ Hufeland's *Journal*, xiv. pt. i. p. 66.

CHAPTER VII.

THE FIRST APOLOGIES FOR FAILURE.

WHEN vaccination was passing through a storm of adverse criticism during the smallpox epidemic of 1805, Jenner wrote to one of his friends,¹ that nothing of that kind ever shook his faith in cowpoxing. "And why? I placed it on a rock, where I knew it would be unmoveable, before I invited the public to look at it." The metaphor is too pure to express the whole complex truth. Jenner placed his doctrine on a rock in one sense, and on a shifting sand in another; and its security was just because it was on a mobile basis. That foundation was laid, as he correctly states, before he invited the public to look at his invention.

The apogetics of vaccination began in the mind of Jenner before his project was given to the world. The years of patient observing and proving, which have been the subject of so much rhetorical nonsense on the part of so many otherwise sane persons, were really a few years of indolent casting about by Jenner for the means of meeting the obvious objections to the scientific whitewashing and professional adoption which he intended for the vulgar cowpox legend. All Jenner's

¹ Letter to Dunning, 9th March, 1805, in Baron, ii. 29.

medical neighbours knew that there was nothing more in the legend than the verbal jingle of cowpox-smallpox, just as dog-rose and hound's-tongue were charms against mad dogs or remedies for their bites. The alleged immunity of poxed milkers from smallpox they knew to be a mere popular delusion, which did not find the smallest justification in the experience of any medical man who had seen much practice among the class of milkers. That was the common-sense obstacle to Jenner's fanciful ambition to see cowpox inoculation substituted for the ordinary inoculation of the time. Jenner resolved to circumvent that obstacle, and all other obstacles of the evidential sort, by calmly asserting that the ordinary spontaneous cowpox was spurious, and that the sort of cowpox which alone gave immunity from smallpox was a derivative of horse-grease.

It is not easy to make out at what period of his "researches" Jenner called in the aid of horse-grease. The *Inquiry*, as published in 1798, was pervaded by horse-grease everywhere, but it is improbable that the equine source of true cow's pox had figured to the same extent in the paper when it was sent to the Royal Society some eighteen months before. At all events, the only experiment that Jenner included in the first edition of his paper had been made from a milkmaid, who had caught the pox from cows, which cows had been infected from a cow (with "overstocked" udder) bought at a fair, which circumstance was claimed afterwards by Jenner himself, although not with reference to his own first case, to be one of the common occasions of generating spontaneous or spurious cowpox. He could hardly have failed to see that Sarah Nelmes, and consequently

James Phipps, would be found out to have had cowpox of the spontaneous market-cow spurious sort, if the doctrine of true and spurious had appeared in the first draft of his paper in all the precision of clean-cut dogmatism which it afterwards wore.

It seems more likely that the year 1797 saw the first serious adoption of the great genuine-and-spurious plea, and of horse-grease as the one and only authentic source of the genuine. The opening of the *Inquiry* is on horse-grease, and so is the conclusion of it. We may take it, then, that the doctrine of spurious and genuine, and of a horse-grease origin as the correct mark of the latter, was slowly developed, and not really formulated until it appeared in the *Inquiry* in June or July, 1798. Even in that essay itself, although the horse-grease origin of cowpox pervades the argument, yet the doctrine of true and spurious, or horse-greasy *versus* spontaneous cowpox, is relegated to a quiet footnote on the seventh page, and not once referred to again until the last page, when *true* and *spurious* cowpox are thus printed, with a reference to "page 7" in a note, the evidence of genuineness in that connexion being the marvellous case of the sucking colt with erysipelas and abscesses of his thigh. "Thus far have I proceeded," he concludes, "in an inquiry founded, as it must appear, on the basis of experiment; in which, however, conjecture has been occasionally admitted in order to present to persons well situated for such discussions objects for a more minute investigation."

The last clause was Jenner's way of offering to shift his ground, if the course of events and the vulgar prejudices of mankind should make it desirable for him to

do so. Woodville had, in fact, warned him, before the *Inquiry* went to press, to cut out everything in it relating to horse-grease,¹ no doubt for the reason which Pearson afterwards gave, that "the very name of Horse-grease was like to have wrecked the whole concern." But these ingenuous Londoners did not know how essential the horse-grease doctrine of cowpox was in Jenner's private calculations. One of his veterinary critics saw clearly that it was a factitious doctrine, and concluded that Jenner had adopted it "rather out of compliance with the ideas of the people he was obliged to consult, than as the result of his own mature reflection."² But it was against the wishes of others that he retained it, in all the prominence that he had given to it, as the stamp and seal of the genuine form; it was "of the greatest consequence to point it out here, lest the want of discrimination should occasion an idea of security from the infection of the smallpox which might prove delusive." And therewith began the long chapter of vaccination apologetics.

The course of events soon bowled over Jenner's poor strategic plea that all genuine cowpox came from horse-grease. Woodville supplied the world with vaccine after Jenner had failed to do so; and not only did Woodville and Pearson disclaim the horse-grease doctrine, but it was quite out of the question that the outbreak at the cowhouse in Gray's Inn Lane, whence they got their vaccine, should have had such an origin. Their cowpox was the ordinary "spontaneous" cowpox,

¹ Fraser, *Med. and Phys. Journ.*, 1805, p. 10.

² Lawrence, *Med. and Phys. Journ.*, i. 115.

which had been the only kind known until Jenner suddenly denounced it as spurious, and has been the genuine cowpox of Ceely¹ and all other modern authorities. Clearly, then, Jenner's original distinction between spurious and genuine could not be maintained; otherwise he would himself be open to the charge of using spurious cowpox, inasmuch as he was using matter given him by Woodville, having none other to use.

Jenner's next appearance in print was in April, 1799, when he came to London and published his *Further Observations*. Spurious cowpox is the grand theme of the second essay; but the doctrine of spuriousness is no longer the simple major premiss of "all non-horse-grease cowpox is spurious." Indeed, any one reading Jenner's newest publication rather carelessly, and without having read the *Inquiry*, would not unnaturally suppose that horse-grease was itself a source of spurious cowpox. He gives "the sources of a spurious cowpox" as four in number:—

1. Pustules on the cow's nipples or udder, which pustules contain no specific virus.

2. Matter which had originally possessed the specific virus, but had suffered decomposition either from putrefaction or from some other cause less obvious to the senses.

3. Matter taken from an ulcer in an advanced stage, which ulcer had arisen from a true cow-pock.

4. Matter produced on the human skin *from some peculiar morbid matter generated by a horse*.

Now, a careless reader, or one of those readers who take

¹ See *Natural History of Cowpox*, p. 60.

their impressions from glancing over the leaves, might easily go away thinking that No. 4 was the obnoxious horse-grease itself. It is difficult to say what the fourth source of a spurious cowpox really was ; most probably it was horse-grease which had not undergone the mystical modification by being grafted on a cow. At a later part of the essay, Jenner touches on the objections to his horse-grease doctrine of genuine cowpox. He does not now fight strenuously for it, although he fights more strenuously than he had ever done for the radical separation of genuine cowpox from spurious. But while he insists upon a genuine cowpox, he forgets to say what it is, or how it is defined. He hints that he might have been mistaken in deducing cowpox from horse-grease ; he is willing to consider all the objections that had been taken to his hypothesis ; he will merely repeat there the six considerations that had weighed with his scientific and candid intellect in giving horse-grease the prominent position in the doctrine of cowpox which it occupied in the *Inquiry*. In the third pamphlet, *Continuation of Facts and Observations*, which came out eight months later (December, 1799), horse-grease is not once mentioned ; and in the short historical sketch which Jenner drew up, of the dawning, development, and perfecting of the great vaccination idea in his mind during years of quiet and fruitful work in the peaceful retirement of Berkeley (*On the Origin of the Vaccine Inoculation*, 1801), there is not one word said about horse-grease. The thousands who took their ideas from that manifesto, or from the exact repetition of it in the form of evidence before the Parliamentary Committee of 1802, would never have believed that horse-grease was

the original corner-stone of the whole project and doctrine of cowpoxing. The truth is, that the notion of "genuineness" and "spuriousness," which was all that he ever wanted to establish through horse-grease, was soon able to run on its own legs without support from pathology or from anything else. "Spurious" became a cry; and, as a cry, it could be used with far more freedom and far more effectiveness if it were uncommitted to definitions, which, as Jenner's old master, John Hunter, has said, are, "of all things on the face of the earth, the most cursed."

But while Jenner himself dropped horse-grease, a number of persons, who were simple enough to take his *magnum opus* seriously, spared no pains to show that the horse-grease doctrine was right.¹ These partisans were more Jennerian than Jenner himself; and one can only guess at their queer state of mind when they found their hero telling the story of his many years of patient and laborious research, and saying not one word about horse-grease from first to last. This story, which is the principal epitome and canonical writing, as it were, of vaccination apology, will now be given.

The paper *On the Origin of the Vaccine Inoculation* is dated from Bond Street, the 6th of May, 1801. Jenner was now a great personage, had been presented to the King a year before, and at the time of his writing was in full career as a lion of London society. It would be charitable to assume that vanity had turned his head

¹ Sir Christopher Pegge, of Oxford, *Lond. Med. Rev.*, v. 76 (Oct. 10, 1800); J. H. Grose, of Winslow, *Med. and Phys. Journ.*, iii. 294; John G. Loy, M.D., of Aislaby, *Experiments on the Origin of the Cowpox*. Whitby, 1801.

and made him untruthful ; at all events, the piece is a tissue of lies.¹ He professes to give a "concise history" of the origin of vaccine inoculation, the conciseness being enhanced by a charming *naïveté* and heartiness of manner. The reader is reminded by many simple touches of the long period of anxious thought that this admirable man endured until he came before the world with his beneficent discovery ; if dates or other particular circumstances are seldom given, that is merely the writer's artlessness and modesty.

Jenner's first difficulty, in approaching the great cowpox-smallpox problem which he afterwards solved to his own and the world's satisfaction, was one that might well have deterred a better-instructed and more sensible man. He found that some cowpoxed milkers had taken smallpox, just as if their previous cowpoxing were purely irrelevant. In his concise narrative, he would have us believe that he knew that very well, of his own knowledge, and candidly admitted it ; it was this that "led me to inquire among the medical practitioners, who all agreed that cowpox was not to be relied upon as a certain preventive of the smallpox." The real sequence of events was that Jenner, more imaginative than his medical neighbours and colleagues, used to air the popular fancy about cowpox-smallpox at their medico-convivial meetings ; whereupon the medical men who had experience to guide them would good-naturedly produce case after case which showed that the popular

¹ The *Edinburgh Review* (1806, October, p. 35) says that this "simple and interesting narrative" is the "best and most authentic account of his discovery."

belief, in so far as it was held even by the vulgar, was a mere verbal illusion ; Jenner, however, was so persistent in arguing against the facts that, as he told Baron, the members of the Society threatened to expel him as a bore.¹ That was how Jenner came to know so well that all cowpoxed milkers had not been protected from smallpox.

For a while, the concise narrative goes on, these exceptional cases damped his ardour, but did not extinguish it. If we are to believe the story that he had occupied his thoughts with this question ever since he was a pupil at Sodbury, the statement means that his ardour was damped a very long while, something like five-and-twenty years. At length he "had the satisfaction to learn that there were some varieties of spontaneous eruption, all of which produced sores on the milkers." Only one of these was the *true* cowpox ; the others were *spurious*, as they possessed no specific power over the constitution. Here, then, was the obvious explanation of some cowpoxed milkers taking smallpox ; their cowpox sores had been spurious.

Of all the many sly and impudent tales that Jenner told to the medical profession and to the public, the short sentence just quoted is the most sly and the most impudent. He trusted, and rightly trusted, to general readers, and even medical editors, having short memories. Before we state the real development of doctrine about *true* and *spurious* cowpox, let us see how this concise narrative was received by the principal London organ of medical opinion. We are apt, says the editor,

¹ *Life of Jenner*, i. 48, 49.

to forget the throes and travail of a discovery after we have become familiar with the perfected achievement : "Who now wonders at the discovery of America, or the Circulation of the Blood? There is, however, a period between the conception of a discovery and its mature birth fraught with more pangs than war or women know ; and there is no light in which the human mind can be viewed more interesting than during this anxious period."¹ Rhetorical rubbish, instead of sober criticism.

The very same editor had reviewed or analysed the *Inquiry* only two years before ; the *Inquiry* was Jenner's *magnum opus*, his deliberate production, the mature birth of his discovery, after "more pangs than war or women know"; and the ever-recurring burden of the *Inquiry* is that true cowpox was *not* a spontaneous disease of the cow at all, but an infection derived from the horse ; while the spontaneous cowpox is mentioned by name in two places, and a third time by implication on the last page of the essay, only to be dismissed as spurious. And now, in the concise narrative of the slow incubation of his ideas and the gradual perfecting of his researches, Jenner calmly informs the world that he long ago discovered the true cowpox *in one of the varieties of the spontaneous malady*, while he keeps silence about the elaborate doctrine of the *Inquiry* of 1798, that *all* spontaneous cowpox was spurious, and the only true cowpox a derived infection from the horse. The audacity of this proceeding will show all the more if we recall the fact that his second essay, the *Further Observations* of 1799, actually reveals the disingenuous workings of his

¹ *Med. and Phys. Journ.*, v. 505.

mind in choosing a new ground for the doctrine of "spuriousness" under the pressure of his own and other failures at Stroud, in December, 1798, of criticism by Ingen-housz, of the dislike of horse-grease generally expressed, and of his having to adopt Woodville's spontaneous cowpox as the "true Jennerian" for want of any stock of his own.

The "concise history" then proceeds: "But that was not the worst obstacle and check to my fond and aspiring hopes." Of course fond and aspiring hopes carry us back to the obscurity and discouragement of early days. This worst obstacle was that some milkers who had been infected even with the *true* cowpox had caught smallpox afterwards. Now in Jenner's classical work, in the great *Inquiry* itself, as well as in the succeeding essays, there is not one word said of any milker having been infected with *true* cowpox and afterwards with smallpox, even if we allow Jenner to have as many definitions of "true" as there are points in the compass.

What he is pleased to describe as the "worst obstacle to his fond and aspiring hopes" is so absolutely an invention to serve a doctrinal apologetic purpose in after years, that he cannot adduce a single illustration of it among the original cases of cowpoxed milkers upon which his theory and project were based. But he knew of such cases, all the same, in the days of his fond and aspiring hopes; and they led him to reflect "that the operations of Nature are generally uniform, and that it was not probable the human constitution (having undergone the cowpox) should in some instances be perfectly shielded from the smallpox, and in many others remain unprotected. I resumed my labours with redoubled

ardour. The result was fortunate: I now discovered that the virus of cowpox was liable to undergo progressive changes, from the same causes precisely as that of smallpox; and that when it was applied to the human skin in its degenerated state, it would produce the ulcerative effects in as great a degree as when it was not decomposed, and sometimes far greater; but having lost its *specific properties*, it was incapable of producing that change upon the human frame which is requisite to render it unsusceptible of the variolous contagion." This was none other than the main argument of *Further Observations* (April, 1799), a plea which he had to trump up in order to answer Ingen-housz and to cover his own ulcerative failure with the Stonehouse cowpox in December, 1798, and the still more awkward, because more notorious, failures of Thornton and Drake, of Stroud, with matter from the same source.

Having thus carried us over the many years of preparation and parturient travail, Jenner next brings us, in the concise history, to his first experimental trial of cowpox, the famous case of James Phipps on 14th May, 1796. Again vast labours and mental anxieties interpose (represented, in reality, by the memorable attempt to give a young horse the grease by keeping him in the stable and feeding him on beans), and we come down to the month of March, 1798, to the case of the child Baker, who was horse-greased, and died in the workhouse, and to the half-dozen or more cases of inoculated cowpox in other children. The narrative then proceeds: "The result of these trials gradually led me into a wide field of experiment, which I went over not only with great attention but with painful solicitude. This became uni-

versally known through a treatise published in June, 1798."

In so far as any wide field of experiment, subsequent to the trials of March and April, 1798, comes into the *Inquiry* published in June, 1798, the above statement is the mere coinage of his brain. He hurried off to London in April, 1798, with the draft of the *Inquiry* in his pocket, without waiting even to ask the great question of his cowpoxed patients, namely, whether they could stand the variolous test. If there were any painful solicitude made known through the *Inquiry*, it was the solicitude of having to consider the importunity of Woodville, after he had read the manuscript of that work, that horse-grease should be entirely cut out from the programme.¹

Woodville was one of the few men in the profession who knew almost as much of the secret history of Jenner's discovery as we know now; and he could easily have shown up the concise narrative for the romance which it certainly was. But he was a man of quiet disposition, far more inclined to efface himself than to enter into a controversy with such a man as Jenner; and he never wrote anything in the vaccination discussions beyond a dignified and candid explanation of the smallpox eruptions which befell his first vaccinated cases at the Inoculation Hospital, and had been diligently turned to account by Jenner in order to discredit Woodville's share in the discovery.

The "concise narrative" ends with the publication of

¹ H. Fraser, M.D. (Woodville's pupil and successor at the Smallpox Hospital), in *Med. and Phys. Journ.*, 1805, p. 10.

the *Inquiry*; it says nothing of the *Further Observations* of 1799—for the sufficient reason that it refers back the extemporized argument of that work to the mythical period of the discoverer's early wrestlings with his great idea.

The cry of "spurious lymph" was the great excuse for the failures of cowpox to protect from smallpox, as well as for the ulcerous and other bad effects of that infection itself. It is unnecessary to show that the plea of "spuriousness" was a transparent piece of sophistry,¹ elastic enough to cover all failures and disasters whatsoever. What we are here concerned with is the way in which the profession received this plea, the scrutiny and discussion they applied to it in general, and the evidence they required for each spurious case as it arose. Let us remember that cowpoxing was then a new thing on its trial, and that there is always a presumption, in the minds of a later generation who take a thing on trust, that a new project, especially if it be a scientific one, had been thoroughly tested and debated on all sides before it received the general assent of its own age. We have already seen what they made of the variolous test; we have now to inquire into the reception which they gave to the apologetic plea of spurious cowpox and spurious lymph.

The first person to bring Jenner to his bearings about "spurious" and "genuine" was Dr. Ingen-housz, although it was *spurious smallpox* that Ingen-housz had occasion

¹ So far as concerned cowpox in the cow, Jenner was plainly told in the report of the College of Physicians (1807), that his "spurious" doctrine was one by which "the public have been misled, as if there were a true and a false cowpox."

to protest against. This episode between Jenner and Ingen-housz might well have been forgotten, had it not been the first suggestion of Jenner's later doctrine of *spurious cowpox*. In replying to Ingen-housz, both by private letter¹ and in identical terms in the *Further Observations*, he not only re-affirms the absurd doctrine of smallpox made spurious and ceasing to be smallpox from some imaginary putrefactive change, but he has the assurance to say that the cowpox in Ingen-housz's case of the Wiltshire farmer who had afterwards taken smallpox, must have been also spurious owing to the same putrefactive change, for it was stated that the cows gave out a stench from their ulcerated teats. Ingen-housz, as we have remarked in a former chapter, saw that his correspondent was either a fool or a knave, and took no farther notice of him. But it was his sharp rebuke over the spurious-smallpox doctrine that led Jenner on to his equally audacious doctrine of cowpox made spurious by putrefaction or "some other change less obvious to the senses." Jenner does not say that the putrid smallpox virus failed to produce an infection with the objective characters of smallpox, any more than he says that spurious cowpox matter failed to produce the correct vesicle and other developments of cowpox. He merely says that the disease so induced failed in both cases to *protect from future smallpox*. He is guilty of so transparent a begging of the question that it is really hard to decide whether folly or knavery entered most into his excuses.

The same answer that Jenner made to Ingen-housz's

¹ Baron, i. 294.

case of the Wiltshire farmer was made, on his behalf, to the notorious case of Mr. Jacobs, of Bristol, which at one time staggered the faith of Dr. Beddoes and of Dr. John Sims. That case received far more publicity than the case adduced by Ingen-housz, and it was answered by Jenner's nephew and assistant, Henry Jenner, in an *Address to the Public*, a quarto pamphlet of twenty pages, which contains, along with other puerile matter, the following statement of the doctrine of spurious cowpox:—

“Every case that has been brought forward to undermine the theory we defend, we can prove to a demonstration was not one of the *genuine* kind. There are *three* diseases called cowpox, only one being genuine. Animals exposed for sale acquire inflammation of the udder, which terminates in eruptions on the teats and udder and affects the milkers with a loathsome disease on the hands, arms, and shoulders. [The very circumstances in the case of Sarah Nelmes, who supplied James Phipps.] The forehead sometimes does not escape, from the servants leaning against the udder. This disease may affect the same person several times, but it will *never* prove a preventive for smallpox. A case of this kind occurs in the city of Bristol: a Mr. Jacobs, attorney-at-law, was extensively affected *twice* with this disease (which, from his total ignorance of *real* cowpox, he has called by that name), but it did not prevent his being afflicted with a subsequent severe smallpox.”

The public and the profession were vastly impressed with the idea that the Jenner family alone knew what real cowpox was. Sims had been approached privately before this, and had acknowledged that Jenner's nephew

had proved the case of Mr. Jacobs to be spurious. Beddoes, who first took up the Jacobs case, was even brought to book by Jenner's parson, Fosbroke. The cowpox, said this bold cleric,¹ which Mr. Jacobs had contracted in youth, was undoubtedly spurious: "I speak from actual observation [of Jacobs?], Dr. Beddoes from description only." The same clerical advocate had the assurance to reply to the case of the Oxford college porter, published by Dr. Hooper, physician to the Marylebone Infirmary.² Fosbroke knew that the cowpox which the porter had contracted in Wiltshire, five years before he died at Oxford of confluent cowpox, was spurious, because Hooper had stated that the sores on the hands "were larger than those of the smallpox, and ended in a brown crust." The clerical proof that this, the correct mark of cowpox, indicated the spurious disease, was an indirect one; Fosbroke's own dairymaid had caught cowpox only two months before at Mr. Walkeley's farm, whither she had been sent to learn the art of milking; in her case also the pustules were larger than those of smallpox, and ended in a brown crust. "That I should err in supposing this a spurious disease is impossible. My own children were at that very period of time infected with the *true* cowpox, the inoculated pustule being then in a state of complete maturation. The points of difference between the two diseases were visible, though unnecessary to be repeated here, as they are clearly described in Dr. Jenner's publications." Fosbroke wrote again,³

¹ *Lond. Med. Rev.*, Aug., 1799. ² *Ibid.*, Letter of 12th July, 1799.

³ *Med. and Phys. Journ.*, iii. (1800) p. 249.

on 11th Feb., 1800, and disclosed a fact which he must have known at the time of his writing in July: "I have mentioned virulence in cowpox. Owing to the neglect of advice, my own child had it exceedingly severe," the "pustules" requiring to be treated with vitriolic acid; so that the points of difference between the spurious form on his dairymaid's hand and the genuine on his child's arm were perhaps not so very great, all things considered.

Another member of the family employed to urge the "spurious-or-genuine" plea was the Rev. G. C. Jenner, of Burbage, Wilts. He wrote a paper¹ on Spurious Vaccine, "with an ardent wish that my remarks may throw a ray of light on a subject which so intimately concerns the dearest interests of humanity." In two instances he had seen the *perfect* and the *spurious* cowpox co-existing in the same person, for which strange thing he did not presume to assign a cause. He had vaccinated himself fifty times before he produced any result, and when he did succeed at the fiftieth time, the vesicle was *spurious*. It was by pricking the back of his hand with a lancet, in order to show a young lady of timid disposition how simple the operation was, that he eventually raised the genuine cowpox vesicle. After these curate-like experiences, he comes to the larger view of the question, which so intimately concerned the dearest interests of humanity:—

"From whatever source the spurious pustule may arise, there is this satisfaction, that it is very easily distinguished from the perfect disease by those who

¹ *Med. and Phys. Journ.*, vii. 201.

have paid any attention to the vaccine practice. The features of the genuine disease are strongly marked, and require but little discernment to be familiarly acquainted with them."

The medical profession, however, were not quite so clear about the differences between genuine and spurious, having less discernment than this reverend cowpoxer. In the summer of 1801 the editor of the *Medical and Physical Journal*¹ intimated that "ingenious artists are now at work, in the hope of being able to give accurate representations of the true and spurious pustules." In a later number, Dr. Stokes, of Chesterfield, wrote² that he was glad to hear of the artists being at work, for such pictures were much needed; there were two forms of inoculated cowpox, the *vacciola scutellata* and the *vacciola leprosa*, and it was not easy to distinguish them; nine persons at Chesterfield had taken smallpox after being vaccinated with *vacciola leprosa*, and two of them had died. In the beginning of 1802 the editor of the *Medical and Physical Journal* again writes:³ "We cannot help regretting on this occasion that Dr. Jenner's engagements prevent him from giving to the public those very accurate and beautifully coloured plates which he is now preparing to accompany the next edition of his works. Those plates would indeed be a rudder and a compass by which the practitioner might steer with safety."

Whether the plates of spurious and genuine cowpox

¹ Vol. vi. 201.

² Under date 3rd Oct., 1801.

³ Vol. vii. 187.

really were very accurate and very beautiful, there was never any means of judging ; they were never published, nor did the projected new edition of Jenner's works on vaccination ever see the light. The profession went drifting on without the rudder and compass which the sapient editor thought these plates would have supplied them with. The rudder and compass which the profession really needed were the rudder of pathological principles and the compass of rigidly scrutinized facts ; these together would have guided them to the conclusion that the project for exterminating smallpox by means of cowpox was an imposture on Jenner's part and an illusion on theirs.

The use made of the plea of "spurious" to stop all free inquiry into the merits of Jenner's claims is illustrated in the medical journals of the period. Thus Dr. John Fawssett, of Horncastle, Lincolnshire, sends to the *Medical Journal*¹ three cases of children who took smallpox after being vaccinated ; whereupon the editor, in the exercise of his privilege, designates the cases "spurious" in the headlines and title. The same liberty is taken with cases immediately following, by Dr. John Stevenson, of Kegworth, Leicestershire. In a subsequent number,² Stevenson remonstrated, under date 17th November, 1801 :—

"May I be permitted to solicit your *reasons* for denominating *my* cases of cowpox, as related in a former number, *spurious*?" He then offers some "cursory observations" on the vague use of the epithet "spurious

¹ *Med. and Phys. Journ.*, vi. 117.

² *Ibid.*, vii. (Jan., 1802), p. 9.

or imitative, as expressive of a deceptive species of cowpox, and on the absolute want of its diagnosis." Stevenson writes like a scholar and a practised logician; his masterly criticism of the loose usage of the Jennerians and their high-placed abettors ought to have opened the eyes of the profession to the illusions that were being practised with names, only that the profession unfortunately had no great wish to have its eyes opened.

Two instances of this prevailing temper will serve to show how irretrievably the long career of apologies had been entered upon. Dr. Denman, who had come forward to give cowpoxing his weighty support in March, 1800, wrote another letter in June following:¹ "Since that time," he says, "there have been many vague reports of cases, in which it was asserted that several persons who had been inoculated for the cowpox had afterwards been actually infected with the smallpox. Presuming that some error in the nature of the matter inoculated, or in the conduct of the operation, must have been the cause of such opposite conclusions (if there was any foundation for the reports)," he begs to send for publication a letter from the Earl of Derby intimating the successful vaccination of two of his lordship's own children. Denman knew well that, in a country like England, these two infant Stanleys were the very best form that logic could take; and in the same number of the *Journal* the editor triumphantly refers a sceptical Newcastle correspondent to the "exalted and respectable names which appear in the first pages of this number." The *Newcastle Advertiser* had

¹ *Med. and Phys. Journ.*, iv. p. 1.

published a paragraph against vaccination, which the editor of the London professional organ advises his northern colleague to treat after the following analogy : "A paragraph or letter very similar appeared a few weeks ago in a London paper ; but the Faculty here treated it with the silent contempt it merited ; being persuaded that the declamatory effusions of such writers, when opposed to the opinions of Jenner, Woodville, Pearson, Denman, Saunders, Cline, Keate, Ring, Knight, Abernethy, and many others equally respectable, have no weight with a discerning public."

The other sample of the professional mood shall be taken from an article by Dunning, of Plymouth, in the same *Journal* for January, 1802 :—

"Reports the most fatal to its interests continue daily to accumulate, and are circulated with much earnestness, and even apparent satisfaction, by at least the sceptical, the anile and the foolish, uncontradicted and unchecked." There had been misrepresentations at Plymouth, and these had spread through Cornwall ; they had been counteracted, but only for a time, by the strong testimonial in the *Medical Journal*, and by others in the Sherborne and Exeter papers. "Let the public mind be no longer distracted by the circulation of dreadful accidents and numerous failures [why not ?] which are so eagerly caught at, edited, and improved by the ignorant and the prejudiced." He then raises the grand issue with a preciseness that leaves nothing to be desired : "The *genuine vaccine* lymph does, or does not, possess an absolute preventive power against variolous contagion. Such power is, or is not, a law of Nature. The protection, if it affords protection, cannot be casual,

it must be regular and determined." ¹ Dunning had no doubt at all that such protective power *was* the law of Nature; if the lymph were genuine, it would protect; whatever lymph failed to protect must have been spurious.

One point remains to be made clear, before we leave this doctrine of spuriousness, by which the profession deceived themselves or allowed themselves to be deceived. It had not one great application, but two. In the years 1801 and 1802 the doctrine was mostly in request to explain away failures to protect, or, in other words, cases of smallpox caught in the ordinary way by those who had been cowpoxed; for the epidemic was then reviving a little from the periodic lull which had happened to correspond with the first trial of Jenner's nostrum. But in 1799 and 1800 there had been another use for the cry of "spurious"; it was then wanted to silence the clamour which threatened to arise owing to the number of ulcerated arms. These were a very common experience, if we may judge from the narratives of the more candid. Thus, Addington, of West Bromwich, one of the first to publish his experiences of Woodville's lymph, had five ulcerated arms in his first eleven cases.² Evans, of Ketley, near Shiffnal, who was supplied by Addington, says:³ "Those few patients whose arms were most inflamed were of the first that were infected, which I attributed to the cold N.E. winds, as they were disposed to become troublesome ulcers."

¹ *Med. and Phys. Journ.*, vii. 3.

² *Practical Observations on the Inoculation of the Cowpox*. Birmingham, 1801.

³ *Med. and Phys. Journ.*, ii. 310.

Brown, of Hatton Garden, wrote to the *Gentleman's Magazine* (May, 1800, p. 433), that "nasty, ugly, and inveterate ulcers have remained in the arm long after." Stromeyer, of Hanover, one of the first to try the new inoculation on the continent, got matter from Woodville, and apparently also from Jenner: "The Gloucester matter frequently occasioned ulcerations of the inoculated parts of a tedious and long duration, which the former matter never did."¹ He had therefore given up Jenner's stock, which was, after all, only Woodville's altered for the worse in character in a series of transmissions. Wilke, of Brandenburg-an-der-Havel, had numerous cases of ulcers with elevated edges and a bacon-like floor, sometimes larger than a half-thaler piece.² Cappe, of York,³ admitted that "in some instances the crust separates and leaves a spot unhealed, like that of an issue. In some of the earlier inoculations, these sore places became troublesome, but at that time the proper treatment was not discovered."

The most famous series of ulcerated cowpox arms happened among a rather poor and querulous set of people in Thunderbolt Alley, Clapham, in the fall of the year 1800;⁴ the parents of the poxed children were "much prejudiced, full of invective, and refused to converse reasonably." The lymph was of correct pedigree, and had been taken from the arm of a gentleman's

¹ Letter of 14th March, 1800, in *Med. and Phys. Journ.*, London, iii. 474; also *Hufeland's Journal*, x. pt. 3, p. 106.

² *Med. Chirurg. Zeitung*, 1801, ii. 424.

³ *Med. and Phys. Journ.*, iv. 434. See also *Ibid.*, v. 25 (letter to *York Herald*).

⁴ *Lond. Med. Rev.*, 1801 (Jan.), p. 276.

child ; only it had been taken after the crust began to form on the vesicle, and it therefore represented a late type or a full cycle of cowpox, coming near to that of cowpox on the cow herself or on the milker. The effects were erysipelas, rapidly spreading ulceration, and sloughing ; a woman, aged thirty-five, had a large, irregular, oval sore, with elevated edges of a livid colour. We now know that such effects can be produced at will by systematically using lymph from a late period of the pox, or, in other words, by using the infective matter in a state fully representative of the cow's ulcerous affection.

But let us observe how such an untoward incident was explained away. Blair, the editor of the *Medical Review*, said that it arose from "this spurious sort, or from a violent matter derived from the cow." Dr. Lettsom, a leading physician, and a fussy or influential person among the charitable, rushed to the help of the endangered cowpox project with a letter¹ dated 25th November, 1800 : "The disease," he assured the public, "was not cowpock, but morbid ulceration, originating from the purulent matter formed under the scab or dried pustule of the cowpock." Lettsom, whose writings prove him to have been something of a windbag, did not know what he was talking about.² If the subject had been a suitable one for conundrums, Lettsom and such as he would have been in their element. When is the cowpox not the cowpox ? Answer : (1) When it fails to protect

¹ *Med. and Phys. Journ.*, iv. 567.

² *Observations on the Cow-Pock.* By John Coakley Lettsom, M.D., LL.D., 2nd ed. London, 1801.

from smallpox; (2) When it produces "morbid ulceration."

Besides the apologetic plea of spurious lymph, the excuse was sometimes put forward that the smallpox ensuing was not smallpox but something else. Thus, Bevan of Stoke-on-Trent sends two cases of children who had been vaccinated on the 12th of January because their mother had confluent smallpox, and had themselves sickened for that disease on the 23rd and 24th respectively, the one having sixty pustules on the 28th, and the other, twenty on the 29th, "exactly like smallpox in every respect." To this perfectly credible recital the editor of the medical journal coolly appends a note: "We think this eruption was not variolous."¹ The common explanation of an eruption of the milder sort was that it was really chickenpox, even if the circumstances of infection should have suggested smallpox.² At a later period that excuse grew into the doctrine of varioloid or "modified" smallpox, especially in connexion with the epidemic in Scotland in 1818, described by Thomson.³ In the Vienna school the same mode of reasoning was carried so far that varicella, the learned name of chickenpox, actually came to be used as the equivalent of discrete smallpox, or varioloid, or "modified" smallpox (*e.g.*, in Hebra's writings), and continued to be so used down to recent times.⁴

¹ *Med. and Phys. Journ.*, v. 455 (11th Feb., 1801).

² Forbes, *Ibid.*, vi 314.

³ See chapter xiii.

⁴ In 1871 the writer had an attack of illness in Vienna, caught while attending the smallpox wards of the Allgemeine Kranken-

Other and more subtle excuses for failure were made in Germany (see chapter ix.); but the two stock English pleas were, either that the lymph was spurious, or that the ensuing disease was not smallpox. The nearest approach to the refinements of the Germans occurs in a case in which Sir Joseph Banks played a part. Being personally interested in a child in the country who had caught smallpox six months after vaccination, he wrote to the medical attendant, Dr. Harrison, of Horncastle, and received the following explanation: The child had been vaccinated successfully, and others in the house had in turn been vaccinated from her. Now these latter did *not* take smallpox on the occasion when their vaccinifer did, although they were in the same house; "hence it appears that Fanny communicated a security against the smallpox to others, although she herself remained liable to its influence." With this mystical reasoning the good president of the Royal Society would appear to have been well content, for he allowed the letter which his inquiry had elicited to be published in the *Medical Journal*.¹

Such, then, was the programme of excuses which came to be generally adopted for the failure of cowpox.

haus. The late Professor Skoda, who made the diagnosis during the eruptive fever and when the eruption was appearing, used the puzzling term "varicella," which, to an English student, had no other meaning than chickenpox. The eruption developed into the ordinary pustules of smallpox and ran the ordinary course. The diagnosis was so made, doubtless, on account of the existence of an obvious vaccinal mark on the arm.

¹ *Med. and Phys. Journ.*, v. (1801), p. 108.

As Jenner said, he had placed it on a rock before he invited the public to look at it. The patient thought, etc., which he gave to the subject before he wrote on it was merely to invent the plea of "spurious." That plea occupied a great part of the *Inquiry*, in connexion with the horse-grease doctrine of genuine cowpox; and it occupied the whole of the *Further Observations*, in connexion with an entirely new and hitherto unheard-of doctrine of what was genuine and what was not. As he employed some of his following to give plausibility to his invented name of "smallpox of the cow," so he employed others of them to spread abroad the doctrine of spuriousness. In both matters he found the profession only too willing to be deceived. With the very first trials of cowpox begins the long course of hard swearing in defence of a radically mistaken and erroneous doctrine, which the medical profession has been able to pass off for expert testimony, on the strength of the excellent maxim, *Cuique in arte sua credendum est*. We have next to see how deeply the English profession was committed, by its leaders, to Jenner and his doctrines within the first year, or the first two years, of the novelty being tried.

CHAPTER VIII.

GENERAL ASSENT IN ENGLAND.

WE have thus far seen what kind of evidence the profession had before them, on the protective power of cowpox, and what kind of apologies they were prepared to make for failures and disasters. They never went deep enough into the anatomy and pathology to realize what sort of pox the cowpox actually was, and they had none of the milkers' experience to teach them in the most forcible of all ways. Their behaviour over the variolous test was incredibly stupid and careless. Their chief apologetic plea of spuriousness was wholly alien to the spirit of logical investigation, and a flagrant example of the art of circumventing the unwelcome teachings of experience.

It is hard to believe that the many educated and conscientious men, who belonged to the medical profession of Britain in those years, had given their reasoned assent to a doctrine and practice so full of frauds and fallacies that a later generation will hardly bear to have the naked facts exhibited to the public gaze.

It is by no means certain that the active spirits in the new project were either many, or fairly representative of the best professional qualities. The evidence of Dr.

Moseley, an opponent, may be thought tainted in regard to the quality of the early backers of Jenner ; but, if we allow for his love of stating a case in a few hyperbolic strokes, his testimony is not without its value and significance. Writing in 1808, he said :¹ "The mere operative practice of vaccination has been chiefly carried on by lady-doctors, wrong-headed clergymen, needy and dependent medicasters, and disorderly men-midwives. No man of letters, or of the least pretensions to science, Dr. Pearson excepted, has lately been concerned in it. It has been, and now is, in the hands of the most ignorant of medicine." That is Moseley's exaggerated way, which has always caused large deductions to be made from his credit.

Vaccination, very likely, was most affected by the class of amateurs and fussy novelty-hunters to whom he refers ; but it was tried and countenanced quite early in the day by a certain number, perhaps not a large number, who were by no means among the ignorant of the profession. It is true that some second or third-rate persons, such as Ring, Huggan, and members of the Jenner family, do come up again and again in the vaccination writings of the time, like the stage-army, which makes a great parade by going off at one wing and on again at the other ; but there were some distinguished names among its early patrons, without whose support the new doctrine and practice would hardly have made way.

It is even more important, for understanding how

¹ *A Review of the Report of the Royal College of Physicians on Vaccination.* London, 1808, p. 11.

the general assent was secured, to observe the nature of the opposition. Moseley was the only considerable person who put his foot down at the outset (September, 1798), while it was still possible to have killed the imposture by criticism. When Woodville's activity set men talking, several more made a feeble show of hesitation, or called attention to the evidence against Jenner's theories. Of such was the eminently respectable Dr. John Sims, who was speedily denounced by Jenner as a "snarling fellow," and thereby brought to his proper bearings within the next month or two. The charges of jealousy, malignity, and the like were so freely flung about on speculation by the personal following of Jenner that it needed some strength of conviction, as well as an established reputation, to remain indifferent to them. None but the staunch conservatives of the old inoculation continued to make a firm stand; and although many medical men must have withheld their assent for a long period, and some even for their whole lives, yet, like the corresponding large class of solid and sensible, if somewhat apathetic, men in the profession who watch the successive crazes of our own day, they would make hardly any show in the public controversy, leaving the novelty to be judged by time.

The practical success of Woodville in procuring an abundance of cowpox matter for trial, and the confirmation, under the eyes of a number of men in London, of the correctness of Jenner's plates and of whatever objective description his text contained, gave a start to the movement which would have else been wanting perhaps for ever. Even the authoritative voice of the Royal Society, in the person of Sir Joseph Banks, the

president, was turned from its former opposition to a more or less cordial assent, after the demonstration of cowpox at the milk-farm in Gray's Inn Lane. Sir Joseph was not the man to oppose anything, if it seemed likely to become a success. Mr. Cline's interest had been enlisted from the first appearance of Jenner in London. Mr. Abernethy had sent for publication some observations on cowpoxed milkers collected by his brother-in-law, the Rev. R. Holt, of Finmere, Oxfordshire, and had so been drawn into the circle of Jenner's supporters. Mr. Francis Knight, a court surgeon of great influence, who was connected with the west country, had heard of cowpox before, and was ready to back up the Gloucestershire project as in private duty bound. Dr. Denman had been button-holed about it, and gave the weight of his name, without showing any intelligent grasp of the problem. Dr. Saunders, senior physician to Guy's Hospital and a leading man at the College of Physicians, had also allowed his name to be used. At Oxford, Sir Christopher Pegge, reader of anatomy and one of the leading physicians, happened to hear of cowpox and horse-grease in company at a farm near Thame, and came forward with his cases as a warm supporter of the movement; although the shifty Jenner was at that very time seeking to escape from the horse-grease doctrine which poor Sir Christopher's cases proved. At Cambridge, Sir Isaac Pennington, regius professor of physic, had made inquiries among the dairy farms in the Cottenham district; he had formed an opinion adverse to the horse-grease part of the hypothesis, and he was understood to be adverse to the whole project, but not publicly so.

A very useful man for Jenner's purpose was Matthew Tierney (afterwards Sir Matthew), who was surgeon to a Gloucestershire regiment of militia, and was acquainted with Jenner at home. "Let Tierney know," Jenner wrote to a common friend, "that my new edition mentioning his name is published." Tierney was shortly after in Edinburgh, and well repaid Jenner for that mention of his name. He succeeded in persuading the great Dr. Gregory, who had hitherto read nothing on the subject,¹ to adopt the new practice, almost as a personal favour, or at all events on the easiest evidential terms. Tierney wrote to Jenner from Edinburgh: "Its being received by the professors here will certainly be a means of spreading it more rapidly, and I flatter myself this is now established." Hardly any assent was more important to secure than that of the great medical school of Edinburgh.

In the army cowpoxing had made way under the patronage of the Duke of York, who saw Jenner upon the subject in London on the 1st of March, 1800. The Duke of Clarence had given him an interview in February. The navy had a very zealous champion of Jennerism in the person of Dr. Trotter, the well-known author of *Medicina Nautica*. Trotter had an imaginative vein in him, and wrote a five-act tragedy in verse, entitled *The Noble Foundling; or, the Hermit of the*

¹ Tierney to Jenner, 21st March, 1800: "Dr. Gregory, the professor of physic here, knew very little about it, and of course did not encourage it. I gave him the sum of my experience, and he now seems to entertain more favourable opinions of it. Indeed, he did me the unwished-for honour of reading my accounts to his class."—Baron, i. 376.

Tweed. Of the progress of cowpoxing he says : " Like the early propagation of Christianity by its Divine Leader, it was first preached to the poor. The children of poor soldiers and poor fishermen first partook of its blessings ; publicans and sinners have since embraced it ; and the purity of its doctrine and practice is making proselytes to the very Land's End in Cornwall." ¹ The first of a number of medals struck in honour of vaccination was one from the naval medical service, led by Trotter, which Jenner had presented to him in February, 1801.

Support of great use as an advertisement, but of no intrinsic authority, came from the men of science. Wollaston wrote to Jenner, in the year 1800 : " You have proved to the satisfaction of every candid person that there is a disease of the very mildest kind communicated by inoculation, which perfectly secures the constitution from the smallpox." ² Blumenbach, the celebrated anatomist of Göttingen, wrote to Jenner that they had elected him into their Royal Academy of Sciences on account of " that immortal work by which you have become one of the greatest benefactors to mankind." ³

Dr. Erasmus Darwin, the famous author of *Zoonomia*, wrote to Jenner on the 24th February, 1802 (a few weeks before his death) : " In a little time it may occur that the christening and vaccination of children may always be performed on the same day." ⁴ Dr. Darwin was something of a humorist, and a little tainted with

¹ *Med. and Phys. Journ.*, iii. 525 (6th May, 1800).

² In *Baron*, i. ³ *Ibid.* ⁴ *Ibid.*, i. 541.

irreligion ; it is just possible that he was poking fun at Jenner.

On the 19th of July, 1800, thirty-six of the leading physicians and surgeons in London issued an advertisement in the *Morning Herald*, in the following terms : "Many unfounded reports having been circulated, which have a tendency to prejudice the mind of the public against the inoculation of the cowpox, we, the undersigned physicians and surgeons, think it our duty to declare our opinion—that those persons who have had the cowpox are perfectly secure from the infection of the smallpox. We also declare that the inoculated cowpox is a much milder and safer disease than the inoculated smallpox." In January, 1801, thirty new signatures were obtained in London for this manifesto by the indefatigable Ring ; and similar declarations were made by the chief medical men in York, Leeds, Chester, Durham, Ipswich, Oxford, and other important centres.

Those who thus came forward to lead public opinion mostly took their cue from Jenner, who, in his *Further Observations*, published in April, 1799, was bold enough to say :—

"In every instance the patient who has felt its influence has completely lost the susceptibility for the variolous contagion ; and as these instances are now become numerous, I conceive that, joined to the observations in the former part of this paper, they sufficiently preclude me from the necessity of entering into controversies with those who have circulated reports adverse to my assertions on no other evidence than what has been casually collected." The *Continuation of Facts and Observations*, published in December, 1799, spoke of the

evidence as then practically complete.¹ In like manner Dr. Huggan, of West Kent, wrote, on 31st December, 1799: "The discussion of the subject will, of course, be considered as closed. This is a circumstance truly honourable to Dr. Jenner. *Exegit monumentum aere perennius.*"

On the 1st of March, 1800, Dr. Denman re-echoed Jenner's own claims as follows: "It appears to me that none of the facts or observations mentioned by Dr. Jenner have been disproved or refuted, and that no new information has been gained on any material point by all that has been written on the subject since the publication of his first treatise." A pamphlet by Mr. Creaser, of Bath, in 1800, speaks of "extensive and impartial trials by men of the first talents, independent of concert or co-operation. The result is that, although the inoculation of the cowpox is one of the boldest and most direct innovations on preceding practice, and as such has had to encounter all the impediments which are usually opposed to novelty by the operations of scepticism, prejudice and interest; yet its asserted and almost unparalleled advantages have been realized in their highest extent by a mass of irresistible evidence. . . . It is extraordinary how exactly Jenner has been confirmed." The confirmation went, indeed, rather too far; for Creaser himself included in it the horse-grease origin of genuine cowpox, which Jenner was at that very time

¹ "I have the pleasure of seeing that the feeble efforts of a few individuals to depreciate the new practice are sinking fast into contempt beneath the immense mass of evidence which has risen up in support of it." (Jenner, *l.c.*)

retiring from. In July, 1800, John Ring, the most active of the cowpox propagandists, writes: "Whatever be the origin of vaccine virus, it must give every friend to the interests of humanity peculiar pleasure to contemplate its end. . . . It may now be considered as completely established." In September he wrote again to say that smallpox inoculation had been discontinued since three months at the Inoculation Hospital. On the 5th December, 1800, Dr. Woodforde, of Castle Cary, Somerset, writes that the Jennerian practice is now fully established, "every generous mind will congratulate himself."

In June, 1800, the editor of the *Medical Journal* announced that "vaccination is nearly established in this island." Simmons, of Manchester, wrote on the 9th December, 1800: "Perhaps no subject ever met with so ample an examination in so short a time. . . . If the testimony of medical men in its favour, more general than ever was published before on any one subject, can be supposed to determine the former [that cowpox wards off smallpox], it must be admitted as proved beyond all controversy."

Clement of Shrewsbury, wrote on the 16th June, 1801: "I have the pleasure to add that the Jennerian inoculation is universally adopted by the medical gentlemen of this town and neighbourhood." Peck, of Higham Ferrers, on 8th June: "It has been left for the present period to glory in *very* important discoveries. Witness the indisputable extermination of that dire scourge of the nation, the smallpox. A Jenner has been ordered to arrest its insatiate rage."

Paterson, of Montrose, on 23rd May, 1801, confesses

that he is "lost at once in admiration and gratitude. No farther testimony is needed."

In sending to the *Medical Journal* a piece of testimony from France, James Moore explains, on 5th February, 1802, that he does not send it as additional proof; for "all opposition to this great discovery seems now to be silenced. Like the doctrine of the Circulation of the Blood by the immortal Harvey, it is already established." Huggan, as we have seen, did not wait so long; more than two years before (31st December, 1799), on the last day of the year in which the practice really began to be tried, he wrote that "the discussion of the subject will, of course, be considered as closed."¹

On the 12th September, 1800, Leese, a London practitioner, writes to the *Medical Journal* that "the general opinion of the most discerning of the profession, as well as of the public, now preponderates in favour of the new disease." On the 17th September, Huggan again writes on alleged failures: "People are weak who believe such an occurrence *probable*, or even possible. Such cases may impose upon the credulous, may perplex the minds of those who still have their doubts, and may afford a malicious and short-lived triumph to the ungenerous part of the profession, but can never influence the liberal and enlightened."

It was in vain for such an outsider as "Candidus," in the *Gentleman's Magazine*, to write (11th July, 1799): "There is much to be done on this subject, Mr. Urban . . . The public mind is by no means satisfied; and indeed it is impossible it should be; for the story

¹ *Med. and Phys. Journ.*, vii. 201.

hitherto has had more of the appearance of a *bottle-conjuror's* history than of a sober philosophical disquisition, and could not fail to excite ridicule."

"Candidus" was a medical man who had retired to the country after a busy practice, and was able to apply that independent scrutiny which members of his profession are then in a far better position to exercise than when they are in the whirl of daily business.

Meanwhile among the public there was the usual willingness to accept professional authority. A doctor in the Midlands wrote¹ that the ordinary class of paying patients "take the opinion of the practitioners they employ, and sometimes commission them to inoculate their children 'with either kind of pock.' The upper classes judge for themselves, and those among them who are philanthropists and converts to the new faith inoculate their own children and those of the poor together." The nobility and gentry of Gloucestershire came forward in 1801, and presented their countryman, Edward Jenner, M.D., F.R.S., with a piece of plate.

On the 17th of March, 1802, Jenner laid before Parliament a petition, asking to be rewarded for his discovery. The prime minister, Addington, took the King's pleasure on it, who strongly recommended it to the consideration of the House of Commons; and a Committee was appointed to consider it, with Admiral Berkeley, one of the members for Gloucestershire, as chairman. This was the first opportunity for a public and impartial scrutiny of Jenner's claims.

The Committee was pledged in its very constitution,

¹ Stokes, of Chesterfield, in *Med. and Phys. Journ.*, v. 17.

and made no such scrutiny as a very simple line of cross-examination would have led to. The Committee were hardly qualified to judge on the merits of the pathological and epidemiological question, and had practically no doubt on the empirical evidence of protection. They called three adverse witnesses, and gave a further show of fairness to their proceedings by hearing a good deal of evidence against the priority of Jenner's claim. It was shown that Farmer Jesty had cowpoxed his wife and children a good many years before Jenner, and there was some evidence that a formal project for cowpoxing on a large scale had been communicated, shortly after that event, in a letter to Sir George Baker, president of the College of Physicians. So far from damaging Jenner's claim, all the evidence of that sort did it good; it served to show that these ideas had been in the air, and that therefore there was some general truth in them. It was an obvious conclusion for the Committee to come to, that Jenner was entitled to the priority inasmuch as he had been the first to come before the profession with his *Inquiry*. The only serious stand was made by Pearson; and that was, of course, not against the truth and value of cowpoxing, but against Jenner's claim to have made it current coin. Woodville, whose practical merits were really greater than those of Pearson, gave Jenner the whole credit, and did not say a word in support of his London colleague. Pearson's attempt to minimize Jenner's merits did not make a favourable impression for himself; while it served, like the evidence of pre-Jennerian cowpoxing, to raise a side issue, and to divert any suspicious feeling that the whole thing was a mistake.

The Committee, naturally, did not formally overlook the great question whether cowpoxing was a preventive of smallpox. They called evidence as to whether cowpoxed persons were incapable of receiving the infection of smallpox, whether cowpox inoculation was preferable on various grounds to variolous inoculation, and whether cowpox could be inoculated without injuring the health. The opposition was represented by Mr. Birch, Dr. Moseley, and Dr. Rowley, all of them men of good position. They were rather easily disposed of by the familiar English device of asking them what was their personal experience of the practice in question. They had, of course, to make the damning admission that their experience of it, as practised by themselves, was *nil*; so that, on the whole, Birch, Moseley, and Rowley were of little account before the Committee. They had a fairer field afterwards as pamphleteers before the public, who were just as willing to hear the dialectical bearings of the question as the House of Commons Committee was unwilling to listen to anything but the voice of "experts" and authorities.

The authoritative opinions which the Committee heard were monotonous in their approval of the new practice.

DR. ASH, a leading fellow of the College of Physicians, had had three of his own children inoculated with it. It is an effectual and permanent security against the smallpox, as sufficiently proved by the immense body of experiments (which the doctor could hardly have read with care, or he would not have spoken so).

SIR EVERARD HOME, F.R.S. (who had advised the Royal Society to reject Jenner's *Inquiry*), said that his own opinion was best stated by his having had one of his own children inoculated with vaccine matter, and he is perfectly satisfied with its security.

DR. WOODVILLE gives the preference to the vaccine over the smallpox inoculation because he finds it equally certain in securing the patient from the smallpox, and because it is without danger or risk of life, and not, like the smallpox, contagious.

SIR GILBERT BLANE was at first prejudiced against cowpoxing, owing to the numerous cases of smallpox eruption conjoined with it at Woodville's hospital. Afterwards he vaccinated one of his children, who went through it perfectly well and has since resisted the variolous infection, which was performed seventeen months after the other. If the vaccine was universally substituted, he thinks the smallpox must in a short time be extinct. The objections to it were grounded on fallacy or misrepresentation.

MR. FRANCIS KNIGHT, Inspector-General of Army Hospitals, had seen some cases of the spurious kind.

MR. JOHN GRIFFITHS, surgeon to the Queen's Household and to St. George's Hospital, had inoculated upwards of fifteen hundred persons with cowpox, not one of whom had any untoward symptoms. He does not speak of the variolous test.

DR. DENMAN believed vaccine to be a perfect preventive of smallpox, if properly conducted.

DR. CROFT had his own children vaccinated, and had uniformly recommended the new protective to his patients. A greater blessing to mankind than any

other discovery ever made in medicine. Would cause the smallpox to be remembered only by name.

DR. NELSON, of the Vaccine Pock Institution, believed that about 700 persons had been inoculated at that Institution, who had all done well, and none had since then taken smallpox either from inoculation or otherwise. The health of sickly children in general much mended by vaccine.

SIR GEORGE BAKER, F.R.S., physician to their Majesties, knew of no instance of cowpox inoculation creating or exciting any constitutional disorder, or of its being fatal.

DR. THORNTON, of the Marylebone Dispensary (author of *Vindiciæ Vaccinæ*), had inoculated with cowpox two children of Lord Somerville's coachman; heard afterwards that they had both taken the smallpox; their cowpox must have been spurious. Dr. Jenner had elucidated the very obscure subject of spurious cowpox.

MR. KEATE, Surgeon-General to the Army, surgeon to the Queen and to the Prince of Wales, gave the new practice his general approval.

DR. LISTER, physician to St. Thomas's Hospital (who had assured Cline as early as July, 1798, that he was sure cowpox was a protective), was now called to explain away a case of failure to protect, which he did very fluently.

MR. CLINE had been convinced from the first, and had recommended cowpox strongly to all his friends, including Sir Walter Farquhar. Cases of failure must have been done with spurious matter.

DR. BRADLEY, physician to the Westminster Hospital (and editor of the *Medical and Physical Journal*,

in which he had pledged himself to the utmost to back up Jenner), believed that cowpox will prevent smallpox to the extent of human life. Thinks that if Dr. Jenner had settled in London and kept cowpox a secret, he might have made ten thousand a year for the first five years, and double that sum afterwards.

SIR WALTER FARQUHAR, M.D., had seen cowpox in one of his grandchildren, who had it very mildly and was protected by it. Thinks it a permanent security. Believes that an income of ten thousand a year was lost to Dr. Jenner by making the secret public.

DR. JAMES SIMS, president of the Medical Society, thinks Dr. Jenner might have become the richest man in these kingdoms by trading on his cowpox secret. The Medical Society of London sent through him a unanimous testimony in favour of cowpoxing.

DR. SAUNDERS, physician to Guy's Hospital, thinks cowpox bids fair to extirpate the poison of smallpox. If Dr. Jenner "had rendered the subject more studiously mysterious, and by that means secured to himself in some degree a monopoly of the practice, instead of acting in the liberal and candid manner he had done, it would have been a source of much greater emolument to him."

DR. LETTSOM, F.R.S., believed that cowpox secured the person from smallpox as much as the inoculation of smallpox does. Two relations of his, variolated in the Suttonian way, had afterwards taken smallpox, and one of them had died. Had attended two other patients in severe smallpox, both of whom had been inoculated with smallpox a year or two before.

DR. FRAMPTON, physician to the London Hospital,

had never found cowpox fail in preventing the attack of smallpox ; had tried it on three of his own children, who had stood the variolous test three several times.

DR. MATTHEW BAILLIE, late physician to St. George's Hospital, had gone to see a few cases of inoculated cowpox, in order to become thoroughly acquainted with the appearance and progress of the cowpox pustule. A patient who has properly undergone the cowpox is perfectly secure from the smallpox. Spurious cowpox was so difficult a question, that Dr. Jenner's knowledge of the genuine sort would have enabled him to make a considerable fortune if he had traded on it. The most important discovery ever made in medicine ; would ultimately banish the smallpox from the class of diseases.

The Committee took their pathology almost exclusively from the Rev. G. C. Jenner, the curate who had tried until fifty times to vaccinate himself, and had produced spurious cowpox only then. They took the history of the rise and progress of the cowpoxing idea from the lips of the great discoverer himself, who omitted the horse-grease part of the comedy. The Committee heard no reference to horse-grease from first to last, nor do they seem to have had the smallest curiosity to know what sort of pox the cowpox really was. They were re-assured over and over again that it was not catching, like smallpox, that it was mild when inoculated on the arm, that no one ever died of it, and that, if it were not spurious, it was a certain preventive of smallpox. If they had read the cases published even by the friends of the practice, such as Ward's cases at Manchester, they would have found that the variolous test had failed in the most obvious way in a good many ; and if they had inquired into the larger number of cases

where the variolous test had apparently proved the prophylactic virtues of cowpox, they would have found that the patients had usually developed about as much of smallpox as the inoculative methods of the time were calculated to produce.

It was unfortunate that the only persons who had a motive for scrutinizing the Jennerian evidence, namely the friends of the old variolous inoculation, had also a motive for not inquiring too closely into the shadowy or formal type of the variolous test. They would probably not have been listened to ; but, as it was, the opportunity was missed of showing how the profession had been deceived, or had deceived themselves, on the grand question of the antagonist character of cowpox. If the variolous test had been shown at that date to be the meaningless thing that it was afterwards admitted to be, even Admiral Berkeley and his fellow committee-men could hardly have reported as they did. It was part of the peculiar irony of the situation that the only opponents of the Jennerian doctrines were precluded, by their own interest in variolation, from attacking these doctrines on the ground of the variolous test. The apparent success of that test was what chiefly gained assent ; it was really the most vulnerable point in the Jennerian theory as stated for public apprehension ; but, to have shown that the trifling effects usually produced by variolation in a cowpoxed subject were neither more nor less than the usual results of Suttonian variolation when there was no question of cowpox at all, would not have served the purpose of the Suttonians, for it would have placed the formal and illusory character of their own prophylaxis in too glaring a light. Moseley himself was impressed

by the evidence of the variolous test ; he admitted that cowpox might hinder the development of variola for a time, perhaps for two or three years. Birch and Rowley, in their evidence, did not adduce any of the numerous cases where experimental variolation following cowpox had produced an average degree of smallpox, but only a few cases where the cowpoxed had taken the smallpox in the natural way by contagion. Thus the body of *experimental* evidence was allowed for the time to pass unchallenged ; and there can be no question that it was upon the experimental evidence that the verdict really turned.

The Committee reported to the House that the claim of Dr. Jenner's petition had been established : "As soon as the New Inoculation becomes universal, it must absolutely extinguish one of the most destructive disorders by which the human race has been visited." On the 2nd of June, 1802, Admiral Berkeley proposed in the House a grant to Jenner of £10,000, to which Sir Henry Mildmay moved an amendment (lost by 56 to 59) to make the sum £20,000. The prime minister, Addington, a notorious worshipper of authority, and more ignorant, naturally, of pathology and epidemiology than of most things, gave it as his opinion that cowpoxing was among the greatest, if not the very greatest of discoveries since the creation of man. Mr. Windham, Mr. Wilberforce, and Mr. Grey were all convinced, and spoke handsomely of Jenner. The substantive motion was put to the vote and carried unanimously : "That it is the opinion of the Committee that a sum not exceeding £10,000 be granted to his Majesty to be paid as a remuneration to Dr. Edward Jenner for promulgating the discovery of the

Vaccine Inoculation, by which mode that dreadful malady the small pox was prevented." ¹

The *Annual Register* ² remarks that the public were highly gratified by this munificence. Admiral Berkeley's Committee, it seems, had left no means untried to procure cases hostile to the efficacy of this noble invention, but in every instance the result was highly satisfactory. At the same time the gallant Admiral is commended as having been from the first "the friend and patron of Dr. Jenner," and as having "brought his discovery forward to notice through the medium of his high rank and great connections, and pressed it upon the attention of the nation by procuring the unanimous approbation of parliament to the discoverer." Just so; it is a useful thing to have an aristocratic friend who is strong enough to procure the unanimous approbation of parliament. Only, a less *naïve* chronicler would not have put the matter quite so plainly. Mr. Bankes, member for Corfe Castle, who had sat on the Committee, said in the debate that, although he considered the discovery a useful one, yet he looked upon the report of the Committee with some degree of jealousy. The members of it appeared to him "in the light of nominees on a committee to try the merits of a contested election, as being the friends of the petitioner." ³ Bankes, having been a member of it himself, was in a good position for forming an opinion. It is on record that Jenner fell into so despondent a mood while the evidence was being taken that he

¹ *European Mag.*, xlii. 137.

² For 1802, p. 182.

³ *Morning Herald*, 3rd June, 1802.

actually talked of abandoning his claim, and was prevented from doing so by the assurance of Admiral Berkeley that it would all come right.

This judgment of the House of Commons, based upon that of the medical leaders, gave a great support to the doctrine and practice of cowpoxing both at home and abroad, a support which proved invaluable when the epidemic of smallpox returned in 1804-5, and exhibited the protective in its true light to the eyes of those who were most immediately concerned with the practical results. Meanwhile we have to see how the Jennerian novelty was received abroad. Foreign opinion was bound to react upon opinion at home and was afterwards publicly appealed to, Wilberforce in particular being impressed by the consensus of all Europe. Germany, Austria, France, and Italy had famous medical schools, as well as academical societies of great authority and renown. The reception which foreign countries gave to the English project for exterminating smallpox deserves as careful an examination as the reception which it met with in the country of its birth.

CHAPTER IX.

THE GERMAN ENDORSEMENT.

THE adoption of cowpoxing by foreign countries has always been considered one of the best arguments for the truth of the doctrine and the value of the practice. To this medical innovation the famous aphorism of St. Augustine has been confidently applied : *Securus judicat orbis terrarum*. The defender of vaccination in the blue-book of 1857 waxes eloquent over "the common convictions of mankind."¹ An English statesman and critical historian, who had a trained eye for fallacies and illusions, Sir George Cornewall Lewis, has adduced vaccination as a striking instance of the beneficent influence of scientific authority upon popular opinion. After a few years, he said, the Jennerian teaching "had been brought to a certain test, and had made its way in all countries."² That it made its way in all countries, and very quickly too, is unquestioned. The point of Sir George Lewis's argument is, that vaccination was brought to a certain test, that it rested on scientific evidence, that it was promulgated by

¹ *Papers on the History and Practice of Vaccination*. Presented to both Houses of Parliament, 1857.

² *Influence of Authority in Matters of Opinion*, 2nd ed., p. 36.

the respectable authority of the medical leaders after they had duly satisfied themselves, and that it was rightly accepted by the people as having been found to be all that it claimed to be. The same philosophical historian, who finds in the early Roman history an unlimited field for scepticism, and an occasion for insisting on a standard of evidence which most persons will think impracticable, finds in the latter-day cowpox legend a happy illustration of the trustworthiness of scientific or medical authority. The argument from common consent has seldom been used with greater effect than in the case of the Jennerian mystification ; the sceptical in everything else are not sceptical here, because vaccination has been established in the very age of science, under the cognisance and approval of the scientific body, and by the almost unanimous voice of the medical profession in all countries. Writers of the Cornewall Lewis school show a confidence in scientific and medical authority which no one can share who has made it his business to study the history of scientific and medical developments. Scientific or medical authority arises under the same mundane influences as all other authority. This is not the place to set forth the full psychological grounds for rating scientific authority in doctrinal matters at no higher value than any other kind of authority. We are here concerned with the scrutiny of a small fragment of established medical doctrine. When the result has been made clear, those who will may point the moral for themselves.

Jenner's first formal scientific recognition abroad was his election, in the autumn of 1801, into the Royal Academy of Sciences of Göttingen. Blumenbach, the

illustrious anatomist, announced the fact to Jenner on the 12th September, and took occasion on his own part to compliment him on "that immortal work by which you have become one of the greatest benefactors of mankind." Merely on the face of it this recognition was of great value. Göttingen had maintained a high standard for science and scholarship ever since the Georgia Augusta University had been called into existence in that third-rate country town, in 1734, by the magic wand of George II. The greatest care had been taken of its academical repute; the choice of professors continued for many years to be a matter of the most anxious thought to the Elector's ministers. The consequence was that an immense concourse of students of all the faculties flocked to the homely little place. The professors were alive to every movement in the learned and scientific world; the academical voice of Göttingen was authoritative in no ordinary degree.

There were also special reasons why the deliverance of the Academy of Sciences upon the new Jennerian project should carry great weight. Hanover had taken the lead in Germany in trying the new method of inoculation, just as it had been the centre from which the original smallpox inoculation had spread over Germany a generation or two before. Ballhorn, a rising young physician of the capital, translated the *Inquiry* into German in 1799, and the *Further Observations*, along with Woodville's *Reports*, the year after. In February, 1801, he published a treatise in French, in conjunction with Stromeyer, a court surgeon, on the results of their own experience of cowpoxing up to that date.¹ In Göttingen

¹ *Traité de l'Inoculation*. Leipsic, 1801.

itself the practice had been tried in 1800 by Osiander, professor of midwifery, and by Arnemann and Wardenburg, the directors of the surgical clinics. In the summer of 1801, Osiander published a disquisition on Cowpox,¹ including an account of the Göttingen practice and a minute record of his own cases. "Perhaps never before," he says, with reference to that locality, "has a method of the kind been so widely diffused in so astonishingly short a time, or adopted with so great zeal and unselfishness by medical practitioners, who saw before them a certain prospect of diminished incomes in taking these measures to ward off the smallpox."

Here, then, was the experience at their own door for enabling the Göttingen Academy of Sciences to form a correct judgment upon the doctrines and pretensions of Jenner. We shall see how Jenner gained their suffrages. He had sent to Blumenbach, professor of Anatomy, and the greatest personage in the medical faculty, a copy of his collected cowpox essays by the hands of an English student, accompanied, it would seem, by a copy of or a reference to his Cuckoo paper in the *Philosophical Transactions*, and a reference to another paper which he had on hand for the Royal Society on the Migration of Birds. These credentials, together with common report, appear to have satisfied Blumenbach, who proposed him for election at a meeting of the Academy. Osiander, Arnemann, and Wardenburg were the members to whom their colleagues would look more especially for guidance in a matter which

¹ *Ausführliche Abhandlung über die Kuhpocken.* Göttingen, 1801.

most of them knew nothing of, and their testimony may be judged of by the fact that Jenner was elected by acclamation. Let us now see what native experience this authoritative endorsement had behind it. The revelations here are more curious than anything that we have seen concerning the reception of Jennerism in the country of its birth.

The most obvious thing in Osiander's book is his child-like readiness to accept every statement, conclusion, and promise of Jenner without scrutiny. He believes in the immunity of the cowpoxed from smallpox as absolutely as if vaccination had been practised for a century and had proved an unqualified success. He adopts the apologetic argument about "spurious cowpox" without the smallest hesitation, reproducing the horse-grease doctrine in a mechanical way, as if he hardly saw its bearing. He has no suspicion of the unwarranted liberty that Jenner had furtively taken in his title *Variolæ Vaccinæ*; for he gives *Kuhblattern* and *Blattern der Kühe* (smallpox of the cow) among the synonyms of *Kuhpocken*. He is especially indignant with Dr. Johann Valentin Müller, of Frankfurt-on-Main, who had issued a pamphlet to the laity, calling upon them to reject cowpoxing as an untrustworthy protective, inasmuch as cowpox had no connexion whatsoever with smallpox. It would never do, says Osiander, to reason in that theoretical way, and to reject the plain teaching of facts and experiments. Had it not been shown by hundreds of experiments, both in England and elsewhere, that the cowpoxed could not take smallpox? After this bold appeal to experiment, we turn, naturally, with some interest to the minute account of nine cases of his own,

in September and October, 1800, and in February, 1801; but in not one of these do we read of the variolous test having been applied. Perhaps he trusted to Ballhorn and Stromeyer to have applied it. Let us see, then, how much scrutiny these medical men applied to this new kind of protective pox which had come to them from over the sea.

In April, 1800, Stromeyer wrote to a London correspondent¹ that Ballhorn and himself had applied the variolous test in only one of their vaccinated patients that year, and that the variolation in that case had produced the local pustule. One naturally supposes that they had fully satisfied themselves in their practice of the year before, and that they saw no use in trying the variolous test any longer. As a matter of fact, Ballhorn and Stromeyer, the pioneers of cowpoxing in Germany, tried the variolous test just five times; thrice in 1799, and twice in 1800. "We repeat here," they say in the French treatise of 1801, "our most solemn assurance that none of these variolous inoculations had the smallest effect." But let us look at the facts with our own eyes.

Of the three earlier cases we have only scanty particulars:² one was vaccinated on 17th June, 1799, and tested with smallpox on 14th August; another was also vaccinated in June, and tested on the 22nd of September; the third was vaccinated on the 28th of May, 1799, and tested in January, 1800. The smallpox in all three was "gänzlich unwirksam" (quite without effect). But, of the preceding vaccinations of these same children, we also read that "there were almost always obdurate and callous ulcers left behind on the arms"; so that in at least the two cases tested within a few

¹ *Med. and Phys. Journ.*, iii.

² *Hufeland's Journal*, x. pt. 3, p. 106.

weeks of being cowpoxed, there was a simple explanation of the smallpox aborting.

Of the two cases tested among the vaccinations of 1800, one was cowpoxed on the 3rd of February, suffered in due course from a general vaccinal eruption, was tested by the inoculation of smallpox on the 19th of March, and developed a correct variolous pustule, which went through the full stages, and was still covered by a crust on the twelfth day; its vaccinal eruption would itself have sufficed to check whatever general eruption the mode of inoculation of smallpox matter (by lancet-puncture) was likely to have produced on a fair field. The other child, tested in 1800, had been vaccinated the year before (20th June, 1799); a sister, whose vaccination in June, 1799, had failed, while her brother's had succeeded, was also tested with smallpox as a controlling experiment. The two children had the smallpox matter inserted on the 24th April, at an incision on the right arm of each, and again on the 25th by means of threads soaked in the matter and introduced into a small blister which had been raised on the left arm of each for the purpose. In neither child did the incision-spot produce a pustule; in both the blister-inoculation ran almost the same active course, and had become a crust on the ninth day. The chief difference was that the sister, whose vaccination had failed, developed close to the blister a single smallpox pimple or pustule on the tenth day, which died away in less than forty-eight hours.

There does not seem to be much to choose between the result of the test in the vaccinated brother and the unvaccinated sister; but the authors solemnly concluded that the variolous test came to nothing in the brother, thanks to his cowpox, whereas the sister's inoculation had really given her the smallpox, although "*extrêmement bénigne et légère.*" Down to the end of the year 1800, Drs. Ballhorn and Stromeyer had vaccinated five hundred children with their own hands, and in just five of these had they experimentally asked the great question by means of the variolous test—with what result or under what circumstances we have now seen.

However, the great question was getting itself asked in those months without any experiment, and was getting itself answered without any ambiguity. Smallpox was prevalent with varying intensity among the children in various towns and villages of Hanover and Brunswick, and in Bremen, Hamburg, and other parts of North Germany. The disease rose to epidemic intensity at one place after another, and the alarm that it caused made the people more inclined to submit their children to the new inoculation. Ballhorn¹ assures us that many allowed it to be tried at first merely as a harmless thing (which, however, it was not, as his own experience of slow ulcers showed), and with no great belief in it; but that the subsequent outbreak of epidemic smallpox made them take to it more seriously. Lentin, another Hanover physician, wrote to Hufeland² on the 27th July, 1800, that they were awaiting the rise of smallpox to the height of an epidemic, so that the efficacy of cowpox inoculation might be tested. At that date, he says, they knew of no authentic case where a vaccinated child had caught smallpox, no matter how much it had been "exposed"; but he gives immediately after a case in Hanover, vouched for by Drs. Mühry and Lodemann, in which a child had been vaccinated to protect it from the smallpox then in the house, and had taken the latter disease a fortnight after its vaccination.

Ballhorn adduces the following as examples of cowpoxed children successfully tested by exposing them to smallpox: In the winter of 1799-1800 there was

¹ *Hufeland's Journal*, x. pt. 3, p. 106.

² *Ibid.*, x. pt. 2, p. 185.

a great mortality from smallpox among infants and children at Langenhagen; he proceeded thither and vaccinated three children, none of whom caught the epidemic disease, although they were in the midst of it. Now, who were these children? One was the child of Herr von Stapper, another of Pastor Holicker, the third of Lieutenant Dreschler—just the class of persons who might be expected to have good houses and to keep their children out of harm's way. By the month of February, 1801, when Ballhorn published his book on cowpox, he had to admit a certain number of cases in Hanover, in which the epidemic contagion had taken hold in spite of their recent cowpoxing; but these cases, he says, had been vaccinated either imperfectly or with "spurious" matter. No details being given, it is impossible to follow him here.

Let us take, rather, a remarkable series of events at the small Brunswick town of Oebisfelde in the summer and autumn of 1801, or at the very time when the Göttingen Academy of Sciences was honouring Jenner for his immortal discovery. Professor Wardenburg, of Göttingen, was one of those who reported the occurrences;¹ Professor Lichtenstein,² of Helmstädt, was another; and Dr. Mühlenheim³ was a third. They all agree about the facts, and no one else ever questioned them. In June, 1801, matter was taken from a child's vaccine vesicle and inoculated on several more, and so on through four successive generations, until forty-nine children had been successfully vaccinated. From the

¹ *Hufeland's Journal*, xiv. pt. 1 (1802), p. 87.

² *Ibid.*, p. 107.

³ *Ibid.*, p. 117.

description given, the vesicles must have been large and tumid, the areola of full extent, and the constitutional disturbance considerable; the crusts fell off usually about the end of the third week; the lymph was of the "clearest and freshest" kind, and was taken from arm to arm. Of these forty-nine vaccinated infants, no fewer than forty-five took smallpox in the ordinary way during the months of August, September, and October, five of them having taken it while the cowpox was on them and the other forty at a longer interval.

Whoever is curious to see how far a German medical professor could go in the way of sophistical excuses when he once began, should read the paper by Wardenburg in *Hufeland's Journal*. It appears that the first child in the series, who furnished the vaccine for the other forty-eight, had no *Blatternanlage*, or disposition for smallpox; he had been inoculated with variolous matter before, and had not taken; he had been exposed to contagion, and had not taken; he was, in short, an incorrigible child so far as smallpox was concerned. Was it surprising that cowpox matter from such a child's vesicle (however correct the vesicle might look) should fail in antagonizing smallpox? The matter was *false* in its source, and, for all its fine appearance, it was false in its transmissions through each of the four sets of children. "From such a source matter would have been falsified even if it had involved a million infants," and not these unfortunate forty-five only. This was the Göttingen development of the great doctrine of Spurious Lymph. Wardenburg thus solemnly adjures a colleague, supposed to be confronted with a case of smallpox after cowpox: "Hast du nicht in diesem Falle vielleicht

falsche Schutzblattern erzeugt?" (Hast thou not perhaps raised spurious protective pox in this case?). Because, if he had, it was no wonder the protection had failed. A singular anticipation of Wardenburg's development of the plea of spuriousness, out of the rather unpromising fact that the vaccine lymph had come through a child who could not be made to take smallpox, is furnished by De Carro,¹ the pioneer of cowpoxing in Vienna. He vaccinated a count who had long ago gone through the smallpox; a good cowpox vesicle arose, from which source twenty-one others were successfully vaccinated, at Geneva, by Dr. Pelchier, who had been in Vienna and had admired the perfect vesicles of De Carro's case. But, under these peculiar circumstances, the cowpox failed to protect them from the inoculated smallpox some months after; they all took smallpox, though mildly; and then it was remembered that their common vaccinifer, then aged forty, had suffered from smallpox when he was a child of five. His *Blatternanlage* had, in fact, been exhausted; whereas, in Wardenburg's case at Oebisfelde, the *Blatternanlage* had been wanting from the first.

Returning to the practical lessons of the Oebisfelde collapse, Wardenburg asks, Shall we, therefore, now abandon cowpox inoculation? and answers with emphasis, *Certainly not!* By the time he wrote of these events, but not before the catastrophe itself, he and his Göttingen colleagues had pledged their academical credit to Jenner. They had gone rather too far to turn back, but they could at least put on the whole armour of apologetics.

¹ *Hufeland's Journal*, x. pt. 4 (1800), p. 129.

I shall give one other North German instance of the gross and palpable failure of cowpoxing in the very first year or years of its trial. In 1801-2 the epidemic smallpox was in Bremen, where Dr. G. H. Jawandt vaccinated sixty-two children.¹ He was forced to conclude that cowpox did not ward off the contagion of smallpox, unless there had been sufficient erysipelatous redness and induration round the vesicle, unless the whole system had been affected, and unless the fever had been present of a remittent type. These, of course, are rather hard conditions, not often satisfied. He gives cases of his own, where smallpox had followed what we should esteem a good, fair, average vaccination. In one of these, a child aged five, the vesicle ran the regular course, there was fever on the ninth day, and areolar redness on the ninth and tenth; three weeks after, on visiting the tenement to vaccinate others, he found this little girl running about with a full crop of smallpox pustules on her. This "deceptive case," he says, is accounted for by the fact that the erysipelatous areola was not of the right sort; there was not enough induration of the tissues beneath. Unless we attend to these little things, a good cause will be injured. The paper has a postscript to say that, since writing the above, several children have caught smallpox who are said to have had complete (?) cowpox; but all these children had been vaccinated by *surgeons*. This should be a lesson to us not to allow vaccination to be practised as a mere handicraft. Dr. Jawandt himself belonged to a higher grade of the profession, and was jealous for the

¹ *Hufeland's Journal*, xiv. pt. 1, p. 66.

honour of his caste ; but, unhappily, the smallpox had paid no more respect to vaccination when it was imparted by his own skilful hand than when it was done by a common Chirurgus.¹

The experimental variolous test at Bremen was no more satisfactory than the epidemic test. The chief vaccinator there, Dr. Albers, had cowpoxed four hundred children, and tested "several" of them, five or six months after, with smallpox : "The only effect was a rather severe inflammation of the inoculated spot, which, however, gradually began to decline on the days when the eruption should have appeared"²—a rather general statement, but one that might easily cover an average amount of variolation as induced by the mild methods then in vogue (see chapter vi.).

Before leaving this part of Germany, we may glance at the reception of cowpox in Denmark. The Commission (Winslow, Callisen, and others) made no variolous tests ; but they issued a very strong report, in which we read : "From the experiments of other nations, particularly the English, there are reasons to hope that the contagion of the natural smallpox throughout futurity can be entirely annihilated by the vaccine."³

¹ An English lady, Miss Bayley, of Hope, near Manchester, put all these learned Germans to the blush. She had vaccinated 2,600 with her own hand, up to November, 1805, and offered a crown each to as many of them as could afterwards show that they had taken smallpox. Only one little boy came to claim the premium ; but opposite to his name in her books Miss Bayley found a remark indicating, she afterwards thought, a suspicion that there was something wrong.

² *Medicinisch-Chirurgische Zeitung* (Salzburg), 1801, iii. p. 448.

³ Report of 19th December, 1801, in Baron, i. 475.

While these experiences were being gained in Hanover, the new practice was being tried in Prussia. At Berlin the Jennerian project had from the first the advantage, or disadvantage, of a certain amount of Court patronage. In December, 1799, Jenner had been requested to send matter for the vaccination of the Princess Louisa,¹ and through that Court channel Privy Councillor Dr. Heim had come to know Jenner's writings and to make trial of cowpox.² He vaccinated several children, and found the course of the disease to be very much as Jenner had described and figured it. He tested one of his cases, a girl of eight, with inoculated smallpox four weeks after vaccination, and found her protected; he tested also an unvaccinated brother of the former, and found him not protected; the sister slept in the same bed with the child suffering from inoculated smallpox, but did not catch the complaint: "so that I must conclude that having had the cowpox is a protection against the infection of smallpox." He had heard when a boy from his father, who kept some cows, that the milkmaids were subject to a pox from milking, but he had been told "nichts weiteres," *i.e.* there was no tale about their protection from smallpox.

Hufeland, professor of medicine at Berlin (having been called from Jena in 1799), was really enthusiastic for Jennerism,³ although he made believe to hesitate a

¹ Baron, i.

² *Hufeland's Journal*, x. pt. 2, p. 187.

³ In his first notice of it (*l.c.* x. pt. 2, p. 189) he argues that, if vaccination were universally enforced all over the world for a single year, smallpox must of necessity become extinct. It was a mathe-

litte at first. As an editor, he was so far open on the question that he admitted into his *Journal* a hostile paper of 109 pages by Professor Marcus Herz, "one of the foremost of our philosophical physicians," as he said in an editorial note to the paper. One of Herz's more practical points was that very few cases had been subjected to the variolous test, and that of these few some had yielded an equivocal answer;¹ to which Dr. Michaelis, garrison surgeon at Harburg, replied in a paper of 74 pages that Herz ought to read the report of the Vaccine Pock Institution of London, showing that 2,110 variolous tests had been applied in 4,000 vaccinations, and that not one had failed. And besides, he asks, have there not been many cases tested in Germany? Herz ought therefore to withdraw his words about very few having been subjected to the test.²

In 1801 Hufeland issued, in his *Journal*, an Appeal to the Medical Profession throughout Germany to send in their experiences of cowpoxing. The great experiment, he wrote, is gradually approaching a conclusion very favourable to the business and to the wellbeing of mankind. Thousands of instances proclaim aloud the usefulness of the discovery. But let us have the truth; failures are as important to know as successes. Indeed, we have sufficient of successes already. To investigate the circumstances in which cowpox has failed to protect will be the best means, in fact the only means, to silence

matical demonstration; there would be no more of the virus left on the earth, and it would not arise *de novo*.

¹ *Hufeland's Journal*, xii. pt. 1, p. 1.

Ibid., xii. pt. 4, p. 1.

the rumours floating about of the failures and injurious effects of vaccine at one place or another. To this appeal, he says, in a later number, he had received a good many replies, of which he did not publish any considerable number. The most important are those relating to the failures at Oebisfelde and Bremen already noticed. The balance of evidence, he says, is very much in favour of Jenner's claims—as if such a question could be settled by setting off so many failures against so many apparent successes.

An official circular, drawn up by the Ober-Collegium for Medical Affairs and signed by the minister Graf von der Schulenburg, was issued on 11th July, 1801. It is addressed to the medical profession in Prussia, and calls for an impartial scrutiny of the evidence relating to cowpox. It suggests that the new practice should not be hastily condemned: good things like antimony, Jesuit's bark, and variolous inoculation (which is now rewarded with State premiums) had been mistakenly opposed at first. But the experience of several years would be needed; enthusiasm for the practice should be kept within bounds. Only the qualified profession should make the trial, and send their results to the sanitary boards of their respective provinces, according to a blank form subjoined. The differences between genuine and spurious cowpox are then briefly set forth for the guidance of those about to make this national trial.

The king, Frederick William III., was at this time interested in the question, but by no means persuaded. Hofrath Dr. Schulz, body physician to Prince Ferdinand, having written for leave to vaccinate the children in the

garrison at Potsdam, the king replied, from Charlottenburg, 27th June, 1801, that he considered the evidence hitherto to be ambiguous, and that it would require several years' experience (the same phrase that was afterwards used in the official circular of 11th July) in order to form a safe judgment. So long as there was uncertainty, he could show no public favour to the Jennerian method. No leave was needed for the children at Potsdam ; it was the personal privilege of any one to get vaccinated if he pleased.¹ In the same impartial tone the king wrote,² on the 22nd of August, to Dr. Aronson to acknowledge a copy of his essay (with the motto *Errare humanum est*) replying to the objections of Hofrath Professor Herz and Dr. J. Valentin Müller.

Opinion within the profession was much divided in Berlin and feeling ran high. In the *Hamburger Correspondent* (No. 170, 1801) a "Citizen of Berlin" wrote to challenge the statement made in a Bremen essay, that "since the introduction of the new practice, 50,000 had been vaccinated, without a single case of injury done or of protection failing." As evidence of failure he gives particulars of a number of cases known to himself in Berlin. A long detailed reply was drawn up to this by eleven Berlin practitioners concerned in the cases, which were all satisfactorily excused on one plea or another.³ The cause of cowpox took much benefit from the alleged attempt of a certain Dr. Wolff, in Berlin, an adherent of the old inoculation, to pass off smallpox virus, it was said, for cowpox, the child's parents, who were people

¹ *Medicin.-Chirurg. Zeitung*, 1801, iii. 158.

² *Ibid.*, 1802, i. 112.

³ *Ibid.*, 1802, i. 138.

of position, having expressed a wish to have the latter used. The child at once took smallpox and died; but Wolff protested that it was vaccine which he had used, and not variolous matter.

Early in the year 1802, the king so far departed from his neutral attitude as to get himself and his youngest child vaccinated. Hufeland announced the "good news" in his *Journal*,¹ and added that the infection had been communicated with the happiest success, as he could personally testify. A few months later, on the 7th of June, the returns in response to the official circular of 11th July, 1801, were abstracted and commented on in a report signed by the president, the *decanus*, and councillors of the Ober-Collegium for Medical and Sanitary Affairs.² The return had been made by seventy-one physicians in civil practice and thirty-six regimental physicians, and it related to 7,445 vaccinations. In a large number of these, "attempts of every kind" had been made to test the efficacy of cowpox, not only by inoculation of smallpox, but also by exposing the vaccinated to the contagion in various ways. Four medical men, whose names are given, had especially distinguished themselves over these tests; but the particular results are published from only one of the four, Dr. Küster, of Conitz. He had made sixty vaccinations, and had variolated every one of them *eight to ten days after*; not one of the sixty "took," the inoculated spot having shown redness and inflammation to the third, fourth, or fifth day only. Not more than four cases had occurred

¹ *Hufeland's Journal*, xiv. pt. 1, p. 65.

² *Ibid.*, xiv. pt. 1, p. 130.

in which evidence of protection was wanting, and these raised the question of the genuineness of the lymph. The Ober-Collegium ends its report with an eloquent recommendation of the Jennerian method; at the least, it would protect from smallpox as well as inoculation with the "natural" disease, and it was free from the objections to the latter.

This report was made public on the 7th of June, and on the same day a Royal Proclamation was issued, recommending the general adoption of the Jennerian practice throughout the Prussian dominions.¹ It had not taken so many years' experience to settle the question as the king's letter of 27th June and the circular of the 11th July, 1801, had indicated. If there was a judicial temper in some quarters, there was enthusiasm in others.

The course of events in Silesia affords a curious illustration of the hesitancy for a time on the part of the Prussian king and his councillors. On the 1st of July 1801, a proclamation² was issued from the Royal Prussian Kriegs- und Domainen-Kammer at Breslau, advising all parents throughout Silesia to have their children vaccinated, and strongly inculcating on all physicians and surgeons in the province the duty of furthering the vaccine inoculation by every means in their power. On the 24th of the same month, the Kammer at Breslau issued another proclamation amending, or rather rescinding the former,

Hufelana's Journal, 1802, pt. 3, p. 108.

Medicin.-Chirurg. Zeitung. Salzburg, 1801, iii. 159.

which, it is expressly said, had been issued at the special instigation of the Collegium Medicum of Breslau. A closer inquiry had meanwhile shown that the vaccine inoculation was "not yet commendable by the Government as a means of checking the natural smallpox." The former proclamation should be therefore amended as follows: "That the cowpox inoculation must still be regarded as a not infallible protection against smallpox." The incident means officially, perhaps, nothing more than that Breslau had to wait until Berlin should have decided; but it is clear that there was enthusiasm for the new practice in influential professional circles in Silesia. We are enabled to look a little behind the scenes here.

The leader of the movement in Breslau was a certain Dr. Friese, who had translated Woodville's *Reports* and Aikin's *Concise View*, and had taken much pains to circulate De Carro's Vienna treatise. He was joined in the practical work of vaccinating more especially by seven others in the city, some of them men of position in official, civil and military circles. From the 23rd December, 1800, to the 25th of June, 1801, these eight had vaccinated 509 children, of whom a list was published,¹ with the name and profession or occupation of the father in each case. Most of them were the children of well-to-do people. Friese says that these all escaped the smallpox that was then epidemic, although some of them were exposed; he gives two or three trivial instances of exposure to the contagion, and one or two cases where variolation was done more as an additional

¹ Friese, *Kuhpocken-Impfung in Schlesien*. Breslau, 1801.

protection than as a test, with the curious result in one case that the old vaccine sores on each arm were inflamed anew on the thirteenth day of the variolation. There is ample evidence that the people in better circumstances were willing to try the new method ; but there is nothing in the writings of Friese to show that he or they had an intelligent acquaintance with the radical differences between it and the old. There was some opposition in Breslau, which found expression in a tract, *Something more about the Cowpox* ; but Friese summarily disposes of it by the usual cry of "merely theoretical."

Another supporter of cowpoxing in Silesia was Dr. Struve, of Görlitz, the author of several popular works on the health of children, which were all translated into English. Struve makes a great point of having been converted from opposition to the new inoculation by the evidence. The reviewer of his *Introduction to Vaccine Inoculation*, in the weekly journal of the German profession published at Salzburg, says that Struve's own experiences are nothing exceptional, but that, taken along with his variolous tests, they serve to establish the advantages of the great discovery. But Struve distinctly admits the fewness of the variolous tests among his two hundred vaccinations ; if he had tested them all, he says, it would have been but a small addition to the thousands of proofs already given. He has the vaguest notion of what cowpox is, thinks it is smallpox of the cow, and puts down the pustular eruptions, which some of the children caught, as being the proper eruption of cowpox, whereas it is beyond doubt that they were an attack of the contagious smallpox then raging in Görlitz and the country around.

He gives only five variolous tests in his two hundred cases;¹ namely, cases 21, 22, and 23, vaccinated on 7th February and variolated on 17th March; case 79, vaccinated on 1st March and variolated in April; and case 167, vaccinated on 23rd April and tested in August. The three on 17th March had been vaccinated with tenth-day lymph from two children who had ulcers on their arms for a number of weeks after; whether the stock from their vesicles produced the same effects we are not told, but as it was taken at the tenth day, it would probably reproduce its ulcerous properties; so that we should be really dealing with the variolous test as tried upon three children with suppuration going on near to where the smallpox matter was inserted. Of the fourth case we simply read that the variolous test was without effect. But the remaining case, No. 167, which was tested four months after vaccination, is given with particulars. It appears that no cowpox vesicle was produced at all, although there was some "local and general action." Dr. Struve therefore doubted, as he well might, whether the vaccinal effect had been produced on the constitution, and on that account he variolated the child. As that inoculation produced no smallpox, he felt sure that the cowpox protection had really been imparted. Of course, if the child had been successfully variolated, or had caught the epidemic smallpox, it would have been said that the vaccination had failed,—as indeed it had failed, if the presence of a vaccine vesicle be essential to success.

Throughout these dreary records we constantly meet with palpable fallacies of that kind, such as no one would dare to present to an assembly of ordinarily intelligent laymen.²

In the important city of Frankfurt-am-Main, the

¹ *Anleitung zur Kenntniss und Impfung der Kuhpocken.* Breslau, 1802.

² In Dr. Struve's country of Lusatia, the popular feeling against vaccination is now very strong, according to the Vienna *Fremdenblatt*; which adds the following anecdote: A schoolmaster having asked, "Why was Moses hidden by his mother?" a small pupil replied, "Because his mother did not want him to be vaccinated."

Jennerian inoculation was taken up by no less a person than the anatomist and surgeon von Sömmerring, in conjunction with Dr. Lehr. I advert to it because it was specially the variolous test that Sömmerring directed his attention to.¹ He set to work with all the precision which we might expect from one so thoroughly practised in the most rigid methods of descriptive anatomy.

Fourteen vaccinated children were brought together to one place, and all inoculated on the same day with smallpox before witnesses. The smallpox matter was taken fresh from a child's pustules at the third day of their suppurative stage, and was inserted by lancet-puncture. The children were kept under observation, and inspected from time to time by impartial witnesses. By the second or third day inflammation had arisen at the punctures in them all, and a papular elevation could be felt; on the fourth day all the papules had a zone of redness about half an inch around, and a little yellow fluid at their summits; on the fifth and sixth days eleven of the fourteen cases showed the papules become pustules, larger or smaller, filled with yellow matter, the remaining three cases having aborted from the papular stage; on the seventh day the redness began to decline and the pustules to wither; and on the eighth day the redness had disappeared, and the pustules become covered with yellowish-brown semi-transparent crusts. No eruption followed.

This is one of the best-recorded variolous tests in the whole literature of vaccination. I have taken it from the account in the Salzburg journal, which omits to say how soon after vaccination the test was applied; and I have been unable to supply the omission by reference to Sömmerring's original paper; but the practice of the

¹ Summary of *Prüfung der Schutz-oder Kuhblattern durch Gegenimpfung mit Kinderblattern*. Von Hofrath Sömmerring und Dr. Lehr (Frankfurt-am-Main, 1801, pp. 38), in *Med.-Chirurg. Zeitung* for 23rd July, 1801.

time was to apply the test very soon after (in the sixty cases of the test, which were the only ones adduced in the Prussian official report upon vaccination, it was applied on the eighth or tenth day); and it seems probable that a set of children had been vaccinated together and kept together until the test was undergone. The absence of the general eruption was therefore no marvel; and in eleven of the fourteen the local infection was complete. Not only so, but it was such a degree of inoculated smallpox as would have been pronounced a satisfactory protection, if the variolation had not been to test the antagonising power of cowpox, but had been an end in itself. The same medical journal which reproduces that test with approval had asked, only two years before (23rd May, 1799), when cowpox was a new thing: "Is it worth while adopting this novelty, seeing that the usual method of inoculation [with smallpox] is for the most part undergone so happily that the children hardly appear to be ill at all?"

Sömmerring, however, was satisfied, and the doctrine of protection was established in Frankfurt. Certain cases of alleged smallpox after vaccination were hunted up by Dr. Ehrmann, a rather violent opponent, although a man of position; but these, or some of them, were accounted for by Sömmerring and Lehr. Two satirical pieces were published in the city, making out that the new inoculation was being taken up by enterprising young doctors, in order to introduce themselves into private practice, or to find a means of supplanting their old-established but less go-ahead rivals.¹ The same motive

¹ *Med.-Chirurg. Zeitung*, 1801, ii. 399.

has operated so frequently in the adoption of new medical fashions that it is quite credible it should have played a part in determining the reception of cowpoxing and the particular hands into which the new practice should fall. There are other evidences from Germany that vaccination was in request among the well-to-do public. Stromeyer, for example, wrote to a London correspondent from Hanover on the 14th of March, 1800, that, at that early date, most of the Hanoverian physicians "exclaim against the vaccine inoculation, asking, Are people thus secured against the smallpox for their whole life-time? Nevertheless, I have the satisfaction to see a partiality for it displayed by the greater part of the public."¹ But in February, 1801, he is able to say that the bulk of the profession in Hanover, including all the leading men, were now in favour of the Jennerian novelty;² from which we may infer that they had found it advisable to supply that sort of article for which their clients showed a partiality.

It is clear from the bulky handbooks of the new practice that quickly began to appear in Germany,³ that the period of scrutiny was soon over. Professor Nolde, of Rostock, had indeed the temerity to say that a much more deliberate and protracted inquiry was needed, the evidence being insufficient; but his reviewer in the chief critical organ of the profession tells him that the evidence was quite sufficient, and indeed conclusive in favour of protection as asserted by Jenner.⁴

¹ *Med. and Phys. Journ.*, iii. 474.

² *Traité de l'Inoculation*. Leipsic, 1801.

³ By Buchholz, 1801 (pp. 542), and by the elder Hecker (pp. 248), Erfurt, 1802.

⁴ Hufeland's *Bibliothek*, 1802.

In other parts of Germany the new practice was adopted with even less of scrutiny and discrimination than in Hanover and Prussia. The apostles of cowpox in Hesse Darmstadt were a certain Professor Hessert, and a Captain Pilger, who ended by formally adopting the veterinary profession. They started in 1801 a journal for all matters relating to vaccination,¹ which admitted some hostile papers. A critic of it in Hufeland's *Bibliothek* expresses the hope, in 1804, that a journal of that kind will soon be superfluous. Down to June, 1801, they had performed three thousand vaccinations in Hesse Darmstadt, amidst opposition or indifference on the part of "so-called learned physicians," but with a kind of patronage or approval from the sovereign as early as November, 1799. Hesse Cassel gave origin to another cowpox periodical,² edited by Dr. Hunold, of Cassel. At Erfurt, the new practice was taken up by Hecker the elder, professor of surgery, who twice published upon it. The old smallpox inoculation, he says, did not make so much progress in the eighty years since its introduction as the Jennerian inoculation with "smallpox of the cow" had done in two or three years.³

Of the practice at Leipzig, Stuttgart, and other places there are some extant memorials, but they furnish no evidence so good as that already given for Hanover, Frankfurt, and other cities. At Meissen, near Dresden, the failure of the vaccinations done by Dr. Weigel,⁴ to

¹ *Archiv für Kuhpocken-Impfung*. Giessen.

² *Annalen der Kuhpocken-Impfung zur Verbannung der Blattern*. Fürth. Part I., 1801.

³ Extracts in *Med.-Chirurg. Zeitung*, 1802, i. 274.

⁴ *Ibid.*, p. 282.

protect from the smallpox then epidemic, was peculiarly obvious, and was admitted, but was at the same time excused with a *naïveté* which disarms criticism. These cases of failure were cases of spurious vaccinations, "die freilich nicht vor Kinderblattern schützen." The *freilich* is inimitable, and cannot be translated. Despite his unfavourable experience of the epidemic, Dr. Weigel got favourable results by the experimental method. He tried thirteen of his 121 cases with variolous inoculation, and found them protected.

There are hardly any details in the medical journals of the time to show what scrutiny the Jennerian doctrine met with in Bavaria before it was accepted; the reception of it would seem to have been on faith alone. On the 16th of August, 1801, a proclamation¹ was issued, in which the Commission for Public Health at Munich, on the initiative of his Serene Highness, urged all medical practitioners in town and country to apply themselves to the great work with true patriotic zeal. The new method had already been tried, it seems, with the best effects; and parents were demanding it. The voice of experience was waxing ever louder for the good cause of cowpox inoculation. Children during an epidemic should not be chosen for trial of its protective virtues. It was necessary to distinguish carefully between true and spurious vaccine.

At Regensburg an impulse was given to the Jennerian practice by the patronage of the palace, which made up in part for the absence of "a good theory of the

¹ *Med.-Chirurg. Zeitung*, 1801, iii. 411.

antagonism between cowpox and smallpox, two unlike diseases," to quote Schäffer, the Regensburg vaccinator.¹ In the country near Erlangen, the cowpox was discovered, and was found to be a spontaneous affection unconnected with horse-grease; its independent origin on the cow's teats gives occasion for the remark, that it is well known that Jenner's opinion of the origin of the malady has been long overthrown. The actual finding and identification of cowpox at Erlangen served in a vague and unreasoned way to strengthen the belief in Jenner.² In Holstein, not only had the cowpox been found, but, it was alleged, also the country legend of its protecting from smallpox.³

In Vienna the movement was vigorously started by two men, De Carro and Careno, one if not both of them graduates of Edinburgh, who must be classed with the pushing practitioners spoken of in the Frankfurt satires on cowpoxing, the men who are on the outlook to float themselves into reputation and practice on the wave of some new fashion. Careno had published some ten years before a popular catechism of inoculation, which had reached a third edition. Of his enterprise in the new business the following is an example:—

It happened that Dr. Schulz, of Berlin, body physician to Prince Ferdinand, had sent to the Czar of Russia a copy of an essay that he had published on the cow-

¹ *Beitrag zu einer Theorie der Englischen Pocken-Impfung*. Regensburg, 1801.

² Lavater, "Ueber die Milchblattern," a lecture at Zurich, 1st December, 1800.

³ Hufeland's *Bibliothek*, 1801.

pox. He received in reply a letter from the Czar,¹ stating that the Russian trials of cowpox up to that time had not turned out quite so well as was to be desired, and that any physician who could and would practice the protective inoculation with success might reckon on the applause of the public as well as the favour of the Czar. Upon that hint, Dr. Schulz repaired to St. Petersburg, and came back loaded with honours, and enriched with two thousand gold ducats given him by the Czar. Meanwhile Careno had heard of the Czar's invitation to any physician who could make the cowpox charm effective, and had sent his writings also. He did not make such good business out of it as Schulz, but he received a letter from the Czar, thanking him for his books, and the gift of a ring set with brilliants.

The practical trial of cowpox at Vienna was one of the earliest made abroad. The criticism, also, of Jenner's *Inquiry* was begun earlier, and was done far better in the weekly medical journal published at Salzburg² than in any other journal, English or foreign. Perhaps Ingenhousz had a hand in this. A reviewer of the *Inquiry*, 14th January, 1799, remarks upon the fact that cows' smallpox (Kuhblattern) is claimed in Jenner's title-page as a new disease ("discovered" is Jenner's word): the fact that it was only the name *variola vaccinae* which was new had escaped the reviewer. He points out that only three of Jenner's vaccinations had been tested with smallpox, and that three was too small a number.

¹ *Med.-Chirurg. Zeitung*, 1802, i. 31.

² *Medicinish-Chirurgische Zeitung*.

The hopes built on such reasoning might be illusory ; there were analogous cases, he says, under the old practice, in which children supposed to have been successfully variolated had yet caught smallpox in an epidemic. He counsels deliberate inquiry and close scrutiny : " that will bring more credit to us Germans than if we join the English forthwith in beating the big drum." In the number for 24th October, another pen reviews Woodville's *Reports*, a book which was unquestionably a more workmanlike performance than anything of Jenner's. The reviewer of Woodville thinks that readers of the book can hardly fail to carry away the impression that cowpoxing was destined to replace smallpox inoculation ; he discovers, also, that there is some sort of help in it to the doctrine of animal poisons, and an important contribution to pathogenesis. In the very same number, a less friendly hand reviews Pearson's *Inquiry*, and concludes that much experience is still needed before we can accept the prophylaxis of cowpox as correct in principle, and give the old variolous inoculation its dismissal. The next number contains a guarded review of Jenner's *Further Observations*, in which it is remarked with obvious irony that " all his experiences have so fully convinced him of the truth of his original positions [including horse-grease], that he holds it superfluous to return one syllable of answer to those who are of a different way of thinking."

The first reference to the practical trial of cowpox at Vienna is on the 23rd of May, 1799, when " C." writes to give an account of experiments by Dr. F. and Dr. De C. The writer doubts whether the new protective is really milder than the variolous inoculation as then

practised, and whether it really does protect. De Carro himself did, in fact, see enough of cowpox ulceration on the arms to have made him doubt the mildness of the new protective, if not to have shown him what kind of pox it really was. He saw, also, enough of failure to protect from smallpox to have satisfied him that the one kind of pox was altogether irrelevant to the other. The greatest breakdown with De Carro's lymph, comparable to the fiasco of Oebisfelde, was near Geneva, the lymph being spurious because it had passed through a vaccinator who had had an attack of smallpox five-and-thirty years before.¹ He had other experiences of which we do not know the details, but only the conclusions. He discovered that the cowpox which forms a large scab, remaining until the 29th day, is spurious ; this kind does *not* protect from smallpox. He allowed himself to make as many spurious varieties as he pleased.

There were two formal variolous testings of cowpox on a considerable scale in Vienna. One was made in the presence of a good many witnesses on the 14th of July, 1801, by Drs. Portenschlag and Helm (under the instigation of De Carro), in the garden of Count Schönborn, upon twenty-one children who had been cowpoxed (all but one) in March, April, or May preceding. The progress of the inoculation is not described ; what was done was to bring the children back for inspection on the 23rd July, or tenth day, and on the 29th July, or sixteenth day, those on the latter date being perhaps

¹ "Höchst merkwürdige Erfahrung über die Entkrafung des Kuhpockengifte durch die vorhergegangene Menschenpockenkrankheit." By Dr. De Carro. *Hufeland's Journal*, x. pt. 4. p. 129.

some who did not present themselves on the earlier. When the children were seen nine or fifteen days after their variolation, none of them were labouring under smallpox eruption, and only three of the twenty-one had traces of local suppuration, the inoculated spots having "completely dried up" in the remaining eighteen.¹ That, of course, is peculiarly disappointing as a record of what really happened. Was there not as much effect of the variolation in each case as the practice of the time was calculated to produce? Did not the same medical journal which records these experiments say on the 23rd of May, 1799, that variolation "is mostly so happily undergone that the children scarcely appear to be ill"?

The other formal trial at Vienna was made on behalf of the Government medical department at the Allgemeine Krankenhaus on the 12th of November, 1801, upon fourteen children who had been vaccinated all together on the 1st of September. The matter for their variolation was taken from the pustules of a child in the natural smallpox. They remained a fortnight in the hospital, and were daily inspected by the Director himself; "but in not one of them did the slightest trace of *smallpox infection* declare itself." This means that there was, at all events, no general eruption, although there may have been, and almost certainly was, the local pustule. Hofrath Dr. Frank reported the result of this trial² to the Government, which, in March following (1802), issued

¹ *Med.-Chirurg. Zeitung*, 1801, iii. 237.

² *Med.-Chirurg. Zeitung*, 1802, i. 159; also a report of the same by Careno to the French vaccine commission, *ibid.*, p. 227.

a proclamation recommending the general use of vaccine to ward off smallpox. "The prejudices which had at first opposed it," says the biographer Baron,¹ "were thus effectually overthrown, and a series of regulations were established which soon rendered it general in Vienna; and in no long time smallpox was almost banished from that capital."

From other parts of the Austrian empire we receive no evidence of scrutiny or scepticism. From Prague the first report² is that Dr. ô. Keilly had vaccinated twenty persons by the month of June, 1801, and had publicly declared that he would answer for it that every one vaccinated by him (ô. Keilly) would never be attacked by smallpox.

The enthusiasm for the new kind of protective is well shown in the projects that were at once started for extending it to other diseases besides smallpox. De Carro found evidence at Constantinople that cowpox was an antidote also to the plague; six thousand had been cowpoxed in that city and not one of them had taken the plague; there were villages near the capital where true cowpox of the teats occurred, and it was the unanimous testimony of the residents that neither plague nor smallpox ever entered them.³ Struve believed that vac-

¹ *L. c.*, i. 525.

² *Med.-Chirurg. Zeitung*, June, 1801.

³ *Journal de Méd.*, vii. 355; Jenner (in Baron, ii. 13) did not like this extension of the area of cowpox prophylaxis: "I will just drop a hint—the vaccine disease, in my opinion, is not a preventive of the smallpox, but the smallpox itself. . . . Now, if it should ever be discovered that the plague is a variety of some milder disease," etc.

cination moderated the severity of scarlet fever, if it did not prevent the attack; and Careno found reason to adopt the same opinion. Various sanguine expectations of the same kind were floating about also in England, but the only practice that was seriously tried was to vaccinate puppies against the distemper. It need hardly be said that the cowpoxing turned out to be purely irrelevant.¹ But there was one significant attempt to extend the area of vaccine usefulness which demands a more particular notice.

If cowpox could ward off human smallpox, it would have been a very strange thing if it did not also serve to protect the sheep from the variolous contagion to which they were peculiarly exposed in some parts of the Continent. The sheep-pox is a true smallpox of sheep; it is *variola ovina*, properly so called in respect of its being a highly contagious pustular skin-complaint, indistinguishable in almost any point from the smallpox of man. The cowpox was no sooner given out as a means of anticipating the natural or epidemic incidence of human smallpox than it was tried for the protection of the flock-master from his heavy periodical losses. Viborg, the Copenhagen veterinary authority, was very busy in those years with all questions relating to smallpox or other poxes of animals, and from him I take the following:²—

¹ Jenner vaccinated the king's staghounds in June, 1801 (Baron, i. 444). Eight years after, he published in the *Med.-Chirurg. Trans.* (vol. i.) a paper on the dog-distemper, of no value clinically or pathologically, and omitting all reference to vaccination as a protective.

² Abstract in *Med. and Phys. Journ.*, 1802, viii. p. 271.

“It is known, from the observations of French physicians, that cowpox defends the sheep against the sheep-pox infection, in the same manner as it secures men from the smallpox; which seems evidently to prove the identity of the cowpox and the sheep-pox.” Viborg ought to have known that the identity of cowpox and sheep-pox was not to be proved either by the verbal jingle of the names or by a piece of speculative reasoning. Viborg, like all veterinarians, doubtless prided himself upon being a practical man; but his manner of proving the identity of cowpox and sheep pox might have been learned of the Schoolmen. It is clear that he accepts the French doctrine of the prophylaxis of cowpox against *variola ovina*; and why should he not, if cowpox wards off *variola humana*? As a matter of fact, the vaccine inoculation does not ward off the smallpox of sheep, although it “takes” in them just as in man. It has turned out a commercial failure; and, as flock-masters are in a position to take a thoroughly business-like view of the matter, they have not scrupled to abandon the practice. The evidence of its failure will be referred to in the chapter on vaccination in Italy.

CHAPTER X.

RECEPTION OF COWPOX IN FRANCE.

FOR reasons both inherent in the national character and depending on the circumstances of the time, the reception of cowpox inoculation in France could not have been a mere echo of the verdict of superior persons in England or of professors in Germany. There is something in the best French writings on the new inoculation, whether in favour of it or adverse to it, which makes them at once more readable and more worthy of serious attention. The verdict of France having been just as decidedly favourable as that of England and of Germany, it becomes a matter of fresh interest to understand how this great nation, still breathing a spirit of scrutiny and rationalism, should have been hoodwinked into adopting a medical dogma which had as little scientific basis in the pages of Jenner as it had in the foolish heads of some Gloucestershire old women.

It is in the reception of Jenner's project by the French that we see most clearly the insidious working of his disingenuous title-page. The French knew nothing of cowpox at home, or at least had no corresponding word in their language; from the very first they took Jenner's trumped-up name of *variolæ vaccinæ* in good

faith, and constantly spoke of cowpox as *petite vérole des vaches*, or smallpox of the cow, until the ingenious abbreviation of "vaccine" came into general currency. Thus, the translation of Woodville's *Reports* by Aubert in 1800, before the practice had begun in France, bore the title *Cowpox, ou la Petite Vérole des Vaches, substituée à la petite vérole*. Even the most acute of all the critics that vaccination called forth, in France or elsewhere, in those years, Dr. Jean Verdier, did not quite fathom the enormity of Jenner's great title-page trick. Verdier, a man of varied fortunes, who had made his mark five-and-thirty years before by his medico-legal treatises, published in 1801 a sixteen-page pamphlet on vaccination,¹ which the wearied traveller through dusty files of journals and essays comes upon as an oasis in the desert.

One sentence of it only concerns us at present: "The country people in England, as well as the doctors, have represented the vaccine disease as being the smallpox itself. That is a good thing for inspiring confidence; but, unfortunately, the two diseases have nothing in common, and so the ground of protection falls through (*et voilà le fondement du preservatif écroulé*)." But, to do the farmers and milkers in the dairy districts the justice that is due to them, they never represented cowpox to be anything but the pox-sores which they knew by painful experience. There was, indeed, the foolish legend, more at home in the empty heads of idle gossips than among those who knew by experience what the cowpox was, that the affected milkers carried a charm against the

¹ *Tableaux analytiques et critiques de la Vaccine et de la Vaccination*. Paris, An ix. Germinal.

smallpox, just as the plant hound's-tongue gave to those who carried it on their persons a protection against a mad dog ; but there is not a particle of evidence that the rustic ideas about cowpox identified it in its nature with smallpox. On the contrary, the milkers would associate it then, as they are said to do now, in some vague way with the "bad disorder," and be disposed to be somewhat shame-faced over it. It was Jenner, and Jenner alone, who called the cowpox "smallpox of the cow," having insidiously placed the name in Latin on the forefront of his *Inquiry*, and then carefully abstained in the preface and text from ever once saying that he had given the disease of the cows and milkers a new name, or why he had given it a new name. Even so trenchant a critic as Verdier was hardly prepared to find that an Englishman, whose designation of F.R.S. proclaimed him to be of academical rank, should be wanting in the rudiments of common candour. The trick of the title-page had relatively more effect in France than elsewhere ; it implanted an illusory idea as to the nature of cowpox, which at once found expression in the French name, and became the more fixed in the minds of the French profession of medicine by reason of their having few or none of those first-hand experiences of the shocking nature of cowpox in the cow which the English were not wanting in.

There was another reason why inoculation with smallpox of the cow should prepossess the French mind favourably. The original inoculation with human smallpox had been thoroughly discredited in France on account of its palpable disadvantages, and had fallen into almost total neglect. It began to revive somewhat in the years immediately preceding the appearance of Jenner on the

scene ; but even Goetz, the Paris variolator of most repute about 1798, averaged hardly more than a hundred cases in the year ; and in the public services inoculation was not practised except among the pupils of the *École Militaire*, "where Gatti did not, indeed, have brilliant success."¹ The rival plan of checking the heavy mortality from smallpox among infants and children, by rigorous isolation of the sick, which appears to have been taken up by Juncker and others in Germany, and found an English advocate in Haygarth, was in those years seriously entertained in France. But the *petite vérole des vaches* was admittedly free from the great objection to inoculated *petite vérole* itself, however unaccountable the non-contagiousness of the former might be. The new inoculation had therefore a clear field in France ; it seemed to promise all the easiness of the old inoculation without the drawback of contagiousness, while, on the other hand, the arduous nature of the isolation-plan, although never realized, was so clearly foreseen as to make any safe alternative welcome.

While the Paris physicians were thus favourably disposed towards the Jennerian inoculation, they had no intention of formally accepting it and recommending it without rigid scrutiny. A public subscription was raised, and a vaccine station opened in the month Floreal, an viii. (1800), with the following objects : "to repeat the experiences of the English ; to seek for new experience ; to add to the number of variolous tests ; to investigate the truth of all the rumours current as to

¹ Salmade, *La Pratique de l'Inoculation*. Paris, An vii. (1798), p. 6.

the alleged disastrous effects of vaccine." These inquiries were conducted by a Comité Central de Vaccine, composed of twelve medical men of character and repute, with Thouret as chairman. Among the other members were Guillotin, who had played a part in the Revolution, Leroux, professor at the École de Médecine (one of the editors of the *Journal de Médecine*, which began to appear in 1801, and became the organ of the vaccinists), and Salmade, who had published a treatise advocating the re-introduction of the smallpox inoculation in the very year (1798) in which Jenner's *Inquiry* appeared. The Comité Central were three years in issuing their final report (1803), which was a bulky, prolix document that few were likely to read. But they published a good many interim reports in the journals,¹ which practically committed them to the new practice from the outset.

On 28 Vendemiaire, an ix., they published a few variolous tests² which, as we shall see, were ambiguous if not altogether irrelevant; and although they professed to be "far from regarding the evidence as sufficient," yet they had observed a protective action of vaccine in those whom they had "re-inoculated" with smallpox. A few months later (29 Pluviose, an ix.) they announced that grave mistakes had been made in vaccinating; there was a kind of vaccine, non-protective from smallpox, which is known as spurious vaccine (*fausse vaccine*).³ On 21 Germinal, the protective power of vaccine, "if not demonstrated, is on

¹ *Moniteur, Journal de Paris, and Journal de Médecine.*

² *Journ. de Méd.*, i. (1801), p. 254.

³ *Ibid.*, ii. 27.

the point of being so.”¹ On 3 Prairial, they return to the subject of spurious vaccine: “The Committee, in several of the notes which it has published, has been careful to premise that, under certain circumstances, the vaccine inoculation may not follow its regular course in certain subjects, and may give rise to a false vaccine which affords no protection from small-pox.” They then refer to the notorious cases near Geneva, and to “cases that have occurred quite recently under our own eyes in one of the villages near Paris.”²

The strongest report is that of 30 Brumaire, an x.³ A long and uniform experience had convinced the Committee that the dangers of vaccine were few. But it remained to assure themselves of its protective power, and more particularly to find out if the protection lasted more than a year. Accordingly they invited a large number of representative physicians and surgeons in Paris to witness the variolous test at four sittings upon 102 infants, some of whom had been vaccinated a year before and a few of them eighteen months before. The results are certified by all the invited witnesses; who included eight members of the Institute, fourteen physicians of the *ci-devant* Faculty of Medicine, six professors of the *École de Médecine*, five members of the Army Board of Health; four members of the *Société de l'École de Médecine* (Bichat, Dupuytren, Auvity, and Alibert), and thirteen others. This, of course, was a great demonstration; but it only amounted to an attestation that inoculated smallpox produced no effect

¹ *Journ. de Méd.*, ii. 162.

² *Ibid.*, ii. 307.

³ *Ibid.*, iii. 303.

in most of the infants, and merely the local pustule in the rest. The Committee, however, in their next report, concluded that the results of the trial on the 102 infants ought to dissipate all uncertainty as to the *duration* of the vaccine protective power.

Before remarking on the nature of the evidence which, in all good faith, served to convince the Comité Central, we may notice the criticisms that reached them from without, at successive stages of their inquiry. Their most trenchant critic was Verdier, who appeared only once in the field; the other considerable antagonist was Dr. Joseph Vaume, a retired surgeon-major, who issued three pamphlets.¹ The Committee replied to Vaume's several objections in the newspapers of the day, making him speak, as Vaume complained, in language of their own choosing; his own rejoinders were refused admission by editors, and at the end of his third pamphlet he explains that, "whether they answer me or whether they keep silence, this is the last time that I address the public on these chimæras. I have brought the dangers of them under notice; my task is fulfilled."

Vaume's objections were partly of the dialectical sort, which the Committee had, of course no patience with, and made no answer to, and partly founded on the results of vaccination as observed by himself. He produced affidavits of several disasters and deaths from vaccine in Paris, which the Committee met by denials

¹ (1) *Reflexions sur la nouvelle Methode d'inoculer la petite Vérole avec le Virus des Vaches*, Paris, An viii.; (2) *Les Dangers de la Vaccine*, An ix. Germinal; (3) *Nouvelles Preuves des Dangers de la Vaccine*, An ix. Prairial.

or explanations. He scrutinized the variolous tests, pointing out that the very infant whose vaccine vesicles were selected from among those of thirty others for the purpose of being sketched and engraved as typical had well-marked variolous pustules, and fever on the eighth day, when it was tested three months after. He insisted that no conclusion could be drawn from the negative results of a variolous test unless a sufficient interval, a year at least, had elapsed before trying it; and he asked leave to apply the variolous test himself.

It would take many years of common experience, he said, to prove the alleged protective power of cowpox. As to the plea of spurious vaccine, politeness hardly allowed him to speak plainly: "Those who do not know your morality," he writes to the Committee, "might believe that this spurious vaccine is nothing but a subterfuge. I am far from entertaining that idea of the respectable members who compose the committee."¹ It was a matter of surprise to him that, in an hour when miracles were discredited in France, they should be taking seriously this miraculous virtue of the cows in a single district of England to preserve the whole human race from one of the greatest of its scourges. Do not forget, he exclaims, that this pretended specific has taken its rise in a country which has been fertile in fantastic projects. Medical men in England have a leaning towards charlatanism and system-making; they have already led us astray with their project of rejuvenescence by transfusion of blood, with their nitric acid and muriatic acid as infallible remedies for syphilis.

¹ *Les Dangers, etc.*, p. 35.

To-day it is a disease of their cows that they would inoculate upon us.

Dr. Vaume delivered his mind, and retired from the unequal contest. Dr. Verdiér's arrows of criticism were not less ineffectual against the stolid appeals to experiment and future experience. The reception accorded to Jenner, he begins, had been as brilliant as that given to the most celebrated innovators; still, his friends complain bitterly of being contradicted, and they denounce every opponent as an enemy of the truth. It was every one's duty to be on guard against enthusiasm and authority touching a matter which had to conform entirely to the general principles of medicine, or to the same reasoned experience upon which those principles rested. The vaccinists appeal to experience, and set aside all objections founded on the unlikeness of cowpox to smallpox. We are to be made invulnerable by vaccine as Achilles was made invulnerable by being bathed in the waters of the Styx. The prophylaxis by cowpox is a contradiction of the received doctrine of protection by inoculating the smallpox. It is in vain to appeal to experience against established principles; for true principles are the result of the experience of all ages, and become the touchstone of each successive empirical innovation.

You have hastily taken this on trust, he continues, from the English, who are more eager for medical novelties than any other nation; their reports are defective, unfaithful, often disfigured, and so drawn up as to serve only the glory of vaccine. Jenner's doctrine is "un système romanesque," which the natural course of things has already disavowed in its most considerable part [horse-grease]; he deals merely in conjectures,

most of which are refuted by his own data, although he erects them into indisputable axioms. In one place he depicts cowpox as a very grave malady, and in another place tells us that it hardly deserves to be reckoned a disease at all. Everywhere there is inexactitude, vagueness, and palpable contradiction. To prove protection, cases are adduced by the thousand, but few details are ever given. We have more assurances than observations. The variolous tests are not reported with sufficient detail, and what little is said about them indicates a heedlessness which is not compatible with the scrupulous exactitude of true observers. All failures are ascribed to spuriousness of the vaccine, although it had come from the same source as matter counted genuine. If smallpox befalls the vaccinated, the germs of it had been received before. If a fever follows the variolous test, it is not the fever of smallpox, but a fever of irritation. The after-effects of the cowpox virus could not be learned by keeping the children under observation only a few days.

If it be a virus, as you say, then it must change the whole mass of the humours. It is an unprecedented piece of foolhardiness not to follow up its after-effects; we know that it invades the body by the lymphatics, and that it has no grand depuratory crisis for its elimination; it may linger long; and what slow effects, what ravages, may it not produce with the lapse of time? It may lead to a degradation of the national temperament, just as a general inoculation with syphilitic virus would do. He does not question the merits of the Comité Central; but they are mistaken in seeking merely to be propagandists; they ought also to verify. Every case should be

done under their own eyes, and every one should be tested afterwards by able variolators. A complete record should be given of all the after-effects of vaccine, cutaneous, lymphatic and other, and of all cases where smallpox has followed vaccination. Lastly, there should be public conferences, where the new project might be discussed with as little of jealousy as of enthusiasm.

The most obnoxious part of Verrier's criticism was his appeal to scientific method. He entitled his pamphlet *Tableaux Analytiques et Critiques*, and boldly asserted that the whole movement in favour of cow-poxing had been characterized by disregard of the analytic method of Bacon, Locke, and Condillac. There had been numerous instances before that, he said, of credulity being encouraged by medical men of the first rank. Enthusiasm could always be got up for some doctrine and practice which promised great benefits with little trouble, which called for no reflection, and secretly fostered the blind workings of cupidity. On the other hand, let any one announce the most valuable discovery, based upon natural laws, but flouting men's prejudices and demanding close study to apprehend it, as well as much work and expense to give effect to it—such an one will be met by contradiction, calumny, and persecution.

This line of remark, which all who know the history of medicine will recognise to be sufficiently just, called forth a reply from Mareschew, a physician of the Montpellier school, which the Comité Central thought so well of as to publish.¹ The Montpellier doctor, who

¹ *Journ. de Méd.*, ii. (1801) p. 340.

had some reason for asserting the philosophical character of medical writings in so far as his own school was concerned, challenged the accusation of Verdier, that the method of Bacon, Locke, and Condillac had been neglected by the advocates of vaccine. That is to accuse, he replies, those who are really the disciples of these great men, the professors of clinical medicine in the schools of Vienna, London, Paris and Montpellier, of having all at once forgotten or neglected a method which has been long familiar to them. Jenner himself had followed the analytic method, he had taken up the question from every point of view, he had done all that the most severe analyst should do.

This, of course, is the mythical Jenner so often held up to our admiration in this country. Verdier, who seems to have read Jenner's writings at first hand, had found out the crudities, contradictions, and absurdities with which they abound.

But the Montpellier defender had clearly not given himself so much trouble; in the enthusiasm of the hour, he had taken without scrutiny the romantic story which Jenner in 1801 had given in his "concise history" of the origin of cowpoxing, as if it had been historical truth. That brief narrative of years of thought and toil, of difficulties encountered and manfully surmounted, is the grand source of all the nonsense which men, known for their ability, rectitude, and even erudition, have written about Jenner's "caution, accuracy, fairness and modesty."¹ It is impossible for any one of average intelligence or

¹ Choulant, "Ed. Jenner," in *Zeitgenossen*. Leipzig, 1829.
pt. vii. p. 20.

acuteness to study the *Inquiry* and the *Further Observations*, and apply to them such terms as these.

Vaume and Verdier can hardly have made much impression upon public and professional opinion in Paris; they had to depend upon their pamphlets, whereas the supporters of Jenner had the press, lay and medical, open to them.¹ Moreover, Vaume and Verdier, although not perhaps variolators themselves, had a certain tenderness for what they considered the legitimate kind of protective inoculation; and, like the anti-vaccinists in England, Vaume, at least, did not see, or shrank from dwelling upon, the radical fallacy of the variolous test. It was the variolous test that appealed most strongly to the imaginations of all, and that gained for vaccination an assent which was given quite honestly according to the stating of the case, or according as the premisses were apprehended. In two former chapters I have pointed out that variolation in those years had come to be the mere shadow of its old self, and that the operation when resorted to for testing the protective power of cowpox often gave a result which would have been thought satisfactory if variolation had been the end itself and not the test of a rival protective. It was the irony of the situation that the most resolute opponents of vaccination were precluded by their own commitments from attacking it on its most specious and at the same time its weakest side. We have now to make this clear with special reference to the acceptance of cowpoxing in France.

One of the Comité Central de Vaccine, whose name is appended to all the interim reports, was Salmade, who

¹ Vaume, *Nouvelles Preuves*. An ix.

had published his practical treatise on Variolous Inoculation¹ only two years before he joined the Vaccine Committee. It was he who applied the variolous test in some, if not in all or most, of the cases that were publicly tried. The mode of variolation described in his book is the English or Suttonian method of the period; he names two French inoculators who had lately gone to England to learn how to inoculate large numbers at once, with a view to the revival of the practice in France. He does not quite adopt the arm-to-arm plan of variolating which had been used by Gatti, was the farthest development of the Suttonian imposture, and was the plan that Jenner insidiously recommended to his readers when the variolous test of cowpox was to be tried. He says there are physicians who "think they have observed that when the variolous pus for inoculation is taken always from inoculated arms through a succession of cases, the smallpox becomes at length weakened to the point of nullity, so that the later inoculations produce no effect."² The grand success of Sutton, he tells us, was ascribed by Chandler to the fact that he used the crude moisture from a case of inoculated smallpox at a stage prior to the eruptive fever, and therefore from the local pustule—the very thing which Jenner himself did in his testing experiment for cowpox, and advised all others to do. Salmade knew well the significance of that practice; for himself, however, he gives it as "more prudent" to take the matter for inoculation from a case of the natural smallpox, of the discrete or mild type: probably "more prudent" because the other mode

¹ *La Pratique de l'Inoculation*. Paris, An vii. (1798). ² *L.c.*, p. 15.

might produce nothing at all. He is, however, quite Suttonian in spirit: "The best, the most fortunate smallpox is that in which there are few pustules, or even none."¹ That was the teaching enforced by Goetz, the best reputed Paris variolator of his time. "A grand principle," he says elsewhere, "is that the presence of pustules (*boutons*) is not necessary to the manifestation of smallpox. The appearance of the fever after inoculation is the one essential thing so as to be certain that this operation has had the effect of communicating the smallpox." Only the most minute quantity of variolous virus was required—not more than the point of a lancet would take up.²

One precaution dwelt upon by Salmade in his directions for inoculation (1798) was singularly ignored when the variolation was done as a test, at least in the earlier of the Paris trials. It is a precaution that was originally stated by Heberden, in the following words quoted with approval by Woodville³ in 1796, but disregarded by him also in his tests of the validity of cowpox at the Inoculation Hospital in 1799: "It seems a reasonable practice to take some care, at the time of his receiving the infection of the smallpocks, that the person should be as free as may be from any other distemper; lest nature should be hindered in producing, maturing, or rightly discharging them."

Salmade's version of the law thus stated by Heberden is as follows:⁴ "It sometimes happens that the patient,

¹ *L.c.*, p. 55.

² *L.c.*, p. 59.

³ *History of the Inoculation of the Smallpox in Great Britain*. Lond., 1796, p. 327.

⁴ *L.c.*, p. 157. 1798 (before he knew of cowpox).

at the moment when he is inoculated, finds himself attacked by some principle of disease alien to the smallpox ; if that morbid principle should be in greater abundance than the variolous virus, or more disposed than it to coction, nature will occupy herself in the first instance with the malady pre-existing at the time of inoculation ; the effect of the latter operation is accordingly suspended until after the termination of the first ailment, and the smallpox does not declare itself until later." More probably the smallpox will not declare itself at all as the sequel of an inoculated virus, but will abort there and then with the drying up of the local pustule.

Now the cowpox, on the showing of the vaccinists themselves, was such a pre-occupying disease ; it was a considerable lesion of the skin at the very spot where the variolous virus was to be applied in the way of a test, it was an affection of the absorbent glands, and a brief constitutional disturbance. What are we to say, then, of that German, specially commended for his zeal in the testing business, and alone quoted by the Berlin Ober-Collegium, who variolated sixty persons from the eighth to the tenth day after they were vaccinated ? Moreover, in these early days, in Paris as well as elsewhere, the vaccine sores were apt to keep active under the scab beyond the average period. Let us now take the particular evidence of Dr. Voisin,¹ who introduced vaccination at Versailles, and is a better than average instance of the scientific qualities of the first vaccinists.

Dr. Voisin is very severe on mere *a priori* objections ; they have long since been banished, he says, from medi-

¹ *Memoire sur la Vaccine.* Versailles, An ix.

cine ; it is by facts alone, by observations and experiments, that we can either establish or overthrow the utility of vaccination as a substitute for variolous inoculation. He had himself practised the latter mode for fifteen years, and would welcome something in place of it. He had done 218 vaccinations. The crusts often remained on the arms until the 30th, 40th, and even 45th day (which means suppuration underneath). His variolous tests were confined to seven children in the Hospice Civil. They were done in the presence of witnesses, some time within the first four months of his vaccine practice, but how long after vaccination in each case we are not told, although we are told much else. The variolous matter was taken from a case of natural smallpox in full suppuration, and was inserted by lancet-puncture at a spot (on the thigh or other arm) remote from the vaccine. Inspected on the ninth day, four of the seven had the spots dried up and all traces gone, two had variolous pustules, and one had a red spot without elevation : on the eleventh day, one of the pustular cases had progressed to farther suppuration and to efflorescence around ; on the thirteenth day, the suppuration had dried up.

Such is Dr. Voisin's experimental record. His experience, on the other hand, gave him twelve cases of natural epidemic smallpox among his vaccinated children, but these were all concurrent with and not subsequent to the vaccine. In others who had eruptions subsequent to vaccine, the eruptions were more "like those known commonly as *petite vérole volante*." Three of his vaccinations turned out to be spurious, but why spurious he does not say. One cannot help thinking

that a little less of vapouring about experiment and experience, and a little more scrutiny of the premisses of the whole matter, and of the several notions and propositions contained within them, would have made him a more competent judge.

The variolous test was applied in Paris by Dr. Colon, with a very neat result ; but for some reason the Société de Médecine, before whom his report was read, declined to publish it.¹ Dr. Colon was really the pioneer of vaccination in Paris, and not different from other vaccinators, except that he made no pretensions to be a disinterested friend of mankind, but a man of business ; he was constantly denounced, however, by the Comité Central, and by the academical physicians generally, as a charlatan.

He tested forty-nine children with matter from a child in the tenth day of a copious smallpox eruption. Forty-seven of these had been vaccinated successfully at odd times during the previous twelve months, one had been three times vaccinated without effect, and one had never been vaccinated. The children were to be visited in the succeeding days, and notes made on a uniform plan by physicians told off for that duty in the several districts of the city ; the physicians re-assembled at Dr. Colon's on 30 Thermidor, when the following results were disclosed :²—

Forty-three had either no action at the inoculated spots, or had then no traces of such action, or had no traces left except dry crusts more or less ready to fall ;

¹ F. Colon, M.D., *Observations critiques sur le Rapport du Comité central de Vaccine*. Paris, An xi. (1803).

² *Precis des Contre-Epreuves Varioliques*. Paris, An ix. (1801).

Two had still redness at the inoculated spots ;

Two had not only the crust of the primary variolous pustule remaining, but also one or two pustules on the skin around ;

One (thrice vaccinated, but each time without effect) had a few pustules on the variolated arm as well as on the body generally ;

One (never vaccinated) had an ordinary smallpox eruption of the discrete type.

This is all to the glory of cowpox in a fine *crescendo*. But if (changing the figure) we shuffle the forty-nine cards for ourselves, we shall find that a certain proportion had effects of variolation, and a certain proportion had none ; if we had the dates of vaccination, we should perhaps be able to explain why some of the variolations aborted. The forty-three who are meant to weigh so heavily in the scale are conveniently lumped together as if they all had the same import ; but, as an unknown proportion of them had, even at the date of inspection, evidence of recent variolous action, and another unknown proportion at the same date had actually the smallpox crust still adhering, the import was clearly various, and one part of it counterbalanced the other. It is idle to say that the local pustule meant nothing without the eruptive fever ; the state of the variolated children is not certified until a period when their pustules were scabbed, and who knows whether there had been the constitutional disturbance or not ? To have admitted the fever premonitory of the general eruption, even if no such eruption had followed, would have been fatal to the point at issue in the variolous test ; for according to the French variolators of the time, as their practice is expounded in the treatise of Salmade (1798), the fever alone was sufficient indication that the inoculated variolous virus had "held." If the same degree of fever

(of course with the local pustule) had followed the variolous virus when used as a test, it would have been a clear inference that, other things being equal, the antecedent cowpoxing had not prevented the variolous virus from "holding" to the same extent.

The Comité Central itself, for all its horror of the charlatanism of Dr. Colon, gradually drifted into a way of withholding relevant particulars, of lumping together the several pieces of evidence, and of declining to canvass the data up and down so as to get at the truth of them—just as Colon himself might have done. They report their first variolous tests on 28 Vendemiaire, an ix. :—

They were done in three groups : four children on 3 Fructidor, an viii., three months after the vaccinations ; eleven on a later date, two months after vaccination ; and four on another day, also "about" two months after vaccination. The last four all had the correct variolous pustule, matter from which produced the ordinary smallpox of inoculation ; the eleven had none of them anything to show for their variolation ; and of the first group of four, only one, the child Blondeau (whose vaccine vesicles had been so fine that they were selected for making a picture of), had the variolous pustule and the eruptive fever.¹

That is the rather meagre or summarý account as given by the Comité Central itself. The cases were known, however, to Dr. Vaume, who gives a version of them somewhat less favourable to the test, which it would be tedious to reproduce.²

In the great variolous test³ of the Comité Central upon

¹ *Journ. de Méd.*, i. 254.

² See *Les Dangers de la Vaccine*.

³ *Journ. de Méd.*, iii. 303.

one hundred and two vaccinated children, which was certified by so many of the day's distinguished names and was perhaps the greatest testimonial that the cow-poxing enterprise called forth, the most essential fact in each case, namely the date of vaccination, is systematically omitted. The test took place at four sittings of the Comité Central and their numerous distinguished assessors at the École de Médecine, on 23 and 30 Vendemiaire, an ix., and on 7 and 19 Brumaire, an x., an additional sitting having been held on 30 Brumaire to observe the result in the children variolated on the 19th.

The first portion of the test (23 Vendemiaire) was on 37 children, with matter fresh from a smallpox patient inserted at not less than three punctures in each child; they were all brought up on that day week, when the punctures were found in twenty-four to be effaced (*éteintes*), whereas in the remaining thirteen they had developed into pustules which had all dried up by 6 Brumaire, without fever, as they were told, and without general eruption following. The sitting on 30 Vendemiaire was utilized for inoculating twenty more, of whom nineteen bore no traces of the variolation a week after, the other one having the local pustule. Of twenty-five variolated on 7 Brumaire, only two had some traces of action at the spot. Of twenty on 19 Brumaire, again only two had anything to show twelve days after.

This great public test had an immensely reassuring effect. The experimental test was thought to be the right thing, and perfectly valid; and what could be more satisfactory than the answer that it gave? It was conveniently forgotten that Salmade, the inoculator for the Comité, when he was still practising inoculation for its own sake, had desired nothing more than the local pustule, and a degree of fever which would require

some watchfulness to detect in one case, and no great inattention to miss in another. The same quondam variolator's other principle, that the action of variolous matter might be easily interfered with or postponed (or rendered abortive) by the pre-existence of another morbid process in the body, was also left out of sight, together with the dates of vaccination, by which alone we could have told whether such morbid process had been operative. A third point not dwelt upon by the Comité, and probably unknown to their distinguished assessors, was that some of those one hundred and two children were stock experimentees, having been tried with variolous matter in vain on former occasions. Insusceptible subjects were never wanting in the history of variolation; they were apt to be numerous among the scrofulous inmates of orphanages, who were often used for the test. It was easy for a good many insusceptible children to accumulate for the purpose of the variolous test and its repetition, by the almost unperceived operation of a principle of selection.

The gross experiences of everyday life were held of little account beside these niceties of experimentation. There had been smallpox here and there in Paris among the vaccinated; there had been a more general outbreak of it among the "spuriously" vaccinated in a village near Paris;¹ there had been a similar fiasco in a commune near Brussels² (where the matter used was, oddly enough, also spurious, but whether spurious in the

¹ *Journ. de Méd.*; ii. 307.

² *Rapport sur la Vaccine par les Commissaires de la Soc. de Méd. de Bruxelles.* 15 Thermidor, An ix., p. 7.

same way we know not); there had been deaths from smallpox among a number of Odier's very first vaccinated cases at Thonon, near Geneva, in the then department of Lac Lemman; there had been distressing cases of the same in the practice of Dufresne at Toux, near Bonneville, in the department of Mont Blanc; and there is mention¹ also of deaths under the same circumstances in the department of Mont Blanc, on the authority of Dr. Villars, the celebrated Alpine naturalist and geologist of Grenoble, which may or may not have been Dufresne's Toux cases over again. These all, and many more unrecorded, yielded to the plea of "spurious," which, as I have said before, was a mere cry, and had about as much rational value as a street cry of "mad dog" would have. As a sample I shall give the Toux incident.

Dr. Dufresne,² physician of that place, being resolved to give the new protective inoculation a trial, received vaccine on a thread from Dr. Coindet, one of the Geneva vaccinators, with which he raised a successful vesicle, thereafter vaccinating from arm to arm. He vaccinated a number of children, including his own child and the child of General Herbin. Some time after, smallpox broke out, and most of the vaccinated children were attacked, Dr. Dufresne's child and General Herbin's both dying of it. The doctor and the general concluded, not unnaturally, that vaccination did not protect from

¹ J. M. Reynald, M.D., *Réflexions sur la Vaccine*. Albi. An ix.

² His narrative is printed in the *Rapport sur la Vaccine*, by the Commission of the Soc. de Méd. de Lyon. Lyon, An. ix.

smallpox ; and that was, perhaps, the less reasoned impression left upon the parents in humbler life whose vaccinated infants had succumbed in the same epidemic. Dr. Dufresne wrote a letter detailing the facts to the Vaccination Committee then engaged upon inquiries at Lyons. These eminent Lyons doctors thought that their Toux colleague had been hasty in his conclusions : “la douleur paternelle excuse la précipitation d’un pareil jugement.” Accordingly they wrote to Dufresne for further particulars. Had not the vaccine become spurious in some way? perhaps, in his arm-to-arm practice, he had passed it through the body of a child which had had *smallpox before*? was he sure that the vaccine vesicles were correct to look at? To these questions Dr. Dufresne returned no answer, feeling, perhaps, too sore all over to enter upon metaphysical subtleties of that kind. Odier, of Geneva, the great promoter of vaccination in Switzerland, was accordingly appealed to ; he confirmed the fact that “most of Dufresne’s vaccinated subjects had taken smallpox subsequently, and that several had died” ; but, from what the father of one of the children had told him, he thought it “far from certain that they did not all have spurious vaccine ;”—with which very thin whitewashing the incident was covered up for the time, and, of course, very soon forgotten.

The plea of “douleur paternelle,” to excuse the very exceptional conclusion come to by Dr. Dufresne, was re-echoed in Berlin about the same time, by way of accounting for the hostile attitude of Dr. Wolfram, a regimental physician in the Prussian army, who had at an early date taken a profound interest in the Jennerian

project. Being anxious to get the very best matter for the vaccination of his own little girl, he had written to Jenner, but received no reply. He then got matter from Stromeyer, of Hanover, which did not "take"; and in the end he was supplied by Heine, of Berlin, with vaccine which produced vesicles on his child's arm as described by him in full detail. The child caught smallpox of a bad type in the epidemic some time after, and died on the 13th of March, 1801.¹

The Lyons Commission on Vaccine,² which was confronted with the facts of the Toux disaster, held its head as high, scientifically speaking, as any of the persons, or associations of persons, who undertook to give an opinion on the merits of Jenner's project. They intended to go below the surface, so as to get at the real truth; they would avoid enthusiasm on the one hand, and carping detraction on the other. If there had been discoveries in the past kept back unjustly, many more had been "rushed" with foolish enthusiasm; and they, the Lyons physicians, were going to commit neither the one mistake nor the other. Let us see, then, how they justified these brave words.

Their report contains a table of the one hundred and fifty-seven persons vaccinated by, or under the observation of, the Commission, with certain particulars for each case. Forty of these were children in the Hospice des Vieillards et Orphelins de Lyon, where cases of smallpox were occurring about the same time. Only two (or three) of the successfully vaccinated, who were thus ex-

¹ *Medicinisch-Chirurgische Zeitung*, iv. 111, 1801.

² *Rapport sur la Vaccine*. Lyon, An ix. (1801).

posed, caught smallpox; and in them the eruption appeared on the tenth day from vaccination, so that it was within the recognised limits of concurrent infection. Nearly all the cases of vaccination in the hospice, or out of it, are briefly given as "regular"; but we learn from the text that there were a number of cases of bad arms (ulcers discharging ichor, and with livid edges, most of which healed without treatment, while the more inveterate yielded to the action of "l'eau phagedénique"), and some cases of spurious vaccination, the latter, oddly enough, corresponding to cases (in the city) in which smallpox was "understood," or was "suspected" to have occurred subsequently. There were, indeed, two species of vaccine, a true and a false, "which latter is not protective against smallpox."

At Lyons, the great variolous test was applied just twelve times, among the forty children vaccinated at the Hospice; the Commission say that they might have done it on all the forty, but they were keeping some of them to try it later.

They were well satisfied with the result of the test in those twelve; "none has developed the smallpox; in some the punctures have had a red blush round them, or have become an elevated point, which has promptly subsided." In the table we find the particulars of the twelve tests which were thus reassuringly summarised. Each of the twelve is entered in identical terms in one of the columns as "variolé sans succès." In none is the date given of the vaccination, or the interval between that and the variolation; but it is evident from the context that the latter followed quickly on the former. From other columns of the table we gather that three of the twelve selected for testing had required to be inoculated a second or a third time with cowpox before they "took," whereas they would seem to have had only one (perhaps perfunctory) chance with smallpox. Three more of them were

artificially variolated *after* their attack of natural smallpox in the hospice (an absurdity for which Woodville was responsible in the first instance); and one had ulcers of the vaccinated spots until the thirty-second day. For the remaining five no particulars are given.

If these were the data and conclusions respectively in the great medical school of Lyons, it is not to be expected, nor is it the case, that the records of the trial of cowpox at Rheims, Poitiers, Lille, Rouen, and other towns in France, will show to better advantage under critical examination.¹ I shall give details for only one other centre in France, the city of Amiens, where there was a pretentious Jury of Health, dating from the revolutionary period, and eager to try all innovations offered for the benefit of mankind. The Marquis Cornwallis being then (1802) at Amiens, as British plenipotentiary to the Congress, the Jury of Health took occasion to present him with an address.²

The address assures Lord Cornwallis that the jury are constantly occupied with whatever relates to the preservation of man. Vaccination has justly called forth their particular attention. In the course of a year a great variety of experiments have been made here upon six hundred persons. The vaccine is now proved to be a protective against smallpox; this can no longer be doubted. England has the honour of this discovery. The friends of science never interrupt their fraternal intercourse, although their governments may be wield-

¹ On the report of the Comité Central, that vaccination had all the merits of variolation and none of its demerits, the Minister of the Interior issued, on 6 Floréal, An xi. (1803), a circular to the Prefects of Departments, advising the general adoption of the new protective.—*Journ. de Méd.*, vi. 481.

² *Med. and Phys. Journ.*, vii. 201.

ing the thunder of war. We have repeated the experiments of the immortal Jenner, and we have found them correct. None of our experiments were more decisive than those which we shall relate to your excellency. We relate them not so much for our gratification, as for the benefit of medicine and of humanity ; and in laying them before you, we wish that the glory which has been acquired may be transmitted to the discoverer. On the 25th of last Germinal, three infants (ye gods! *three* infants) at the Hospital St. Charles, named Duneuf Germain, Fracaster, and Pisson, who had before been vaccinated with success, were now inoculated with small-pox matter. This produced no effect. The triumph of vaccine was proclaimed. To meet objections which had been raised, the Jury of Health at Amiens inoculated the three children again after six months, on 25th Vendemiaire, and again they failed to take small-pox. After this, who shall dare to assert that the vaccine is not a preservative against the smallpox. Accept, my lord, our homage, and this account of the last experiments we have made, as an offering which we have the honour to present to you. We have already declared that the French physicians have never ceased to consider themselves as brothers to your physicians ; and when you have finished your important labours at Amiens, the two nations will love each other reciprocally, and France and England, glorying in their valour, united by mutual esteem, shall command repose to the rest of the world.

Alas ! the enthusiasm of these rhetorical doctors for the extermination of smallpox was just as vain as their enthusiasm for the cessation of war.

CHAPTER XI.

THE JENNER OF ITALY.

THE story of the introduction of cowpox inoculation into Italy is so full of significance that it deserves to be told, even at the risk of extending this history to an excessive length. Dr. Luigi Sacco, "the most extensive vaccinator in the world," and *emulo del Britanno Jenner* (as he is described on his monument in the Ospedale Maggiore of Milan), was one of those enterprising young practitioners, rather common in all countries, who promptly seized upon the novelty as a handy means to reach fame and fortune. He was one-and-thirty when he suddenly emerged into notoriety in Milan as a vaccinator. His career from the year of his graduation (variously given as 1792 and 1795) until his appearance in Milan in 1801 with a stock of vaccine lymph in his possession is wrapt in obscurity. He had already lived in Milan for a time, and had been awarded a medal by the Patriotic Society of that city for a paper on a "New Way of Preserving Insects," which is rather paradoxically introduced with the motto from Cicero that "honest occupations are to be preferred to useless and base leisure."¹

¹ The paper is printed in Amoretti's *Opuscoli Sceleti sulle Scienze, etc.*, xix., 1796.

His biographer¹ says that he "travelled about in Italy in order to learn more, and was always eager to visit America." On one occasion he was actually on the point of embarking for the New World, but was kept back "by the prayers, not to say the command, of a reigning princess." This mysterious intervention was a special providence, for the ship was wrecked. Making some allowance for a mythical halo surrounding his early years, we may take it, at least, that Dr. Sacco had been a rolling stone. Another biographer² locates him for a time at Chambéry, as medical officer to the Hôpital Civil. The medical journal which introduced his first vaccination book to English readers in 1802, spoke of him as "a medical man of great eminence in Italy,"³ which he certainly was not. It was vaccination that made his fortune, and it was he that made the fortune of vaccination on the other side of the Alps.

In the autumn of 1800, "a fortunate combination of circumstances," as he says,⁴ obliged him to take up his residence at Varese, which was his native town (and has now a Via Sacco to commemorate him). Jenner's cow-pox notions had been made known to Italian readers a few months before in the translation of the *Inquiry* by Careno of Vienna; a few vaccinations had also been tried at Genoa in April, 1800, by Dr. Scassi, with lymph

¹ *Vita ed Opere del grande Vaccinatore Italiano, Dottore Luigi Sacco*. By Cav. Dr. Giuseppe Ferrario, Milano, 1858.

² Quoted in Callisen's *Medicinisches Schriftsteller-Lexicon*, 1846.

³ *Medical and Physical Journal*. Feb., 1802.

⁴ *Osservazioni pratiche sull' uso del Vajulo Vaccino, come Preservativo del Vajulo Humano*. Di Luigi Sacco, M.D. New edition. Milano, Anno x. (1801).

sent from the Geneva stock. At Varese, in September of that year, Sacco took occasion to ask questions about cowpox of certain cattle-drovers and dealers whom he encountered on their way home from the fair at Lugano. A dealer of Cremona told him that he had at that moment resting in a neighbouring meadow a drove of forty cows which had come down from the Swiss mountain pastures, and had all been affected successively with "pustules on the ends of their teats." He took the doctor to see them, and pointed out several which had still the crusts upon their teats. Sacco picked off some of the crusts and kept them.

When he remarked that he would prefer the proper fluid matter of the cow's *vajulo* or smallpox, the cattle-dealer offered to take him to another drove belonging to a friend of his, which was also halting at Varese. Two cows in that herd were pointed out, with red spots on the teats and udder, which the animals would hardly allow to be touched. Next morning Sacco found, in one of the two cows, four elevated and tumid pustules, three being on the teats and one on the body of the udder; in the other cow he found six pustules of larger size and surrounded by a zone of redness, only two of which were on the teats. The pustules did not appear to be ripe for yielding matter; and as the drove was going to make another stage that day on the road to Milan, the doctor went the distance with them. On the following morning he found the pustules of a pale red colour, translucent, and with a commencing brown spot in the centre; with the help of the drover, he had no difficulty he says, in taking off matter by soaking a thread in it repeatedly.

There the narrative ends. But it was illustrated, in the second edition of Sacco's ensuing work, if not in the first, by a large plate of a cow's udder bearing ten natural vaccine vesicles, of round shape, on the teats, and two artificially inoculated vesicles, of oval shape, on the body of the udder. This plate was the first ever given of cowpox in the cow, Jenner having given none; it was reproduced in England¹ in 1802, and in France and Germany subsequently. It is not like any original pox of the cow's teats that has ever been described or figured by any one else. The picture appears to have been constructed by drawing a cow's udder, and then filling in a number of vaccine vesicles of the conventional type here and there upon the teats. This plate did duty for forty years; and it must have given great satisfaction to all those, whether in England or abroad, who had heard more than enough of a filthy, ulcerous, corroding disease of the teats requiring to be checked with caustic, and were puzzled to know how such an affection as that could be smallpox of the cow. The charming illusion² of Sacco's neat and clean-looking vesicles on the cow was not disturbed until Ceely's realistic narrative and drawings forty years after; but by that time the fixed idea of "smallpox of the cow" had gained so completely the upper hand in the vaccination doctrine that even Ceely himself disregarded his own revolting experience in the Aylesbury cowhouses, and went off into the

¹ *Medical and Physical Journal*, vol. vii., March, 1802.

² In a note to p. 42 of his *Osservazioni*, he admits that he had never found in cows the phagedenic ulcerations which Jenner had spoken of. He had not found them because he had not looked for them, being a mere tyro in the matter when he wrote as above.

pleasant by-paths of an experiment to inoculate human smallpox upon a semi-exposed mucous membrane of the heifer, thereby persuading himself that *that* also was cowpox. He was not even undeceived when his assistant, having accidentally pricked his hand with a lancet covered with pus warm from the heifer's pustule, developed in due course an ordinary smallpox pustule at the spot, and an ordinary smallpox eruption on his face and elsewhere a few days after.

Sacco's account of how he found original cowpox at Varese is so circumstantially conceived that its omissions call for remark. Was it with the crusts from the first drove, or with the thread soaked in matter from the two cows in the second, that he made his vaccinations? The existence of cowpox among the forty cows which had come down into Lombardy from the high Alpine pastures at the end of summer is intelligible enough. It was just when a cow was taken to market, being driven or kept standing with her udder full, that the pimples, cracks, or other common ailments would arise, out of which cowpox ulcers might be induced through the rough manipulation of the teats by the milkers, and might be conveyed by them to other cows. The market-cow sort was admitted by Jenner to be a common type of the spontaneous cowpox; only he laid it down quite clearly in the *Inquiry*, but also quite arbitrarily to serve a disingenuous purpose, that it was at the same time spurious. The crusts which Sacco took from the teats of these cows at Varese would doubtless have furnished the cowpox virus for inoculation; indeed, the crusts of the sore teats were the only form in which Ceely could ever get original vaccine virus, notwithstanding all his

careful search for fluid matter from unbroken vesicles in several outbreaks during a number of years at the dairy-farms near Aylesbury.¹

Sacco does not say that the drove of forty cows were in milk ; but it is not easy to understand their all having had cowpox unless they were, or had lately been so. He is throughout under the influence of the idea that cowpox is smallpox of the cow, and he notes no fact that is at variance with that idea. He argues against the possibility of cowpox arising from those simple or physiological causes which were commonly assigned by the dairy folk and the veterinarians. Cows with distended udders are to be found, he says, everywhere; and yet cowpox is rare. Again, he argues, women may have distended breasts from refusing to suckle their infants, and although eruptions sometimes befall them in consequence, producing thick crusts and serious inconveniences, yet there has never been a single instance of contagion from the disease of their nipples so produced. Nothing can show better than that illustration how entirely Sacco missed the point which other observers have emphasized in cowpox: namely, that it is the rough handling of the chapped, or pimply, or otherwise sore teats by the milkers twice or thrice a day, and the necessary aggravation of any soreness which the ever-renewed irritation entails, that causes the infective and communicable properties of cowpox to arise. No one who knew the common experience of cowpox at dairy

¹ The same difficulty was noted as early as March, 1802, in the outbreak of cowpox at Thorpland, near Downham Market, Norfolk. See *Med. and Phys. Journ.*, vii. 541.

farms ever alleged that it was contagious, as smallpox is; yet Sacco, being dominated by the idea of *vajulo vaccino*, and having had no previous practice in dealing with the "pathology" of infective diseases, repeatedly speaks of it as contagious (pp. 38 and 56); and only at the end of his essay, when he is treating of the inoculated vaccine, does he draw the stock distinction between it and inoculated smallpox, with the intention of getting the usual vote of confidence in the former because it is non-contagious.

His pathology of cowpox and smallpox is almost advanced enough for an "expert" of the most recent type. Both diseases, he says, are exanthems; and there is a theory that exanthems are caused by *worms* insinuating themselves under the skin, and there developing. Itch and other contagious diseases are thought to be owing to worms; and why not smallpox? But he had as yet got no microscope powerful enough to give positive results in that highly promising field of inquiry. With his theory of cowpox being due to worms, Sacco had little need to consider the common sense of its origin; that was an aspect of the matter which any ordinarily reflective and humane person could deal with, whereas it is given to but few to discover the more minute forms of worms with a high-powered microscope.

Before we proceed to his practical work as a vaccinator, it will be convenient to deal at this point with Sacco's other services to the theory of cowpox as a disease and as a protective. The first thing that we have to notice is his astonishing fertility in devising experiments. Knowing nothing and caring nothing about the vulgar circumstances amidst which cowpox arises in various countries, or about the true significance

of its characters as milkers experienced them, he instituted a series of experiments with vaccine lymph which were of so sporting a kind that he could not have failed, had he lived now, to gain the approval of the medico-scientific leaders, even if his adherence to the worm-pathology had not assured him of that before. He vaccinated seven dogs, and, on applying the variolous test to six of them, found that they were protected; he would have tested the seventh also, only it had to go with its master on a journey. One of the cowpoxed dogs became rabid, and bit a number of persons, none of whom took hydrophobia. He communicated cowpox also to the ox, the calf, the sheep, and the pig. Except in the case of the sheep, of which more in the sequel, the experiments appear to be meaningless. But these domesticated mammals were a mere fraction of all the animals he experimented upon. He inoculated cowpox also on wolves, bears, apes, cats, mice, rabbits, hares, and squirrels; also upon hens among the avian class, upon snakes, lizards, and frogs in the reptilian and batrachian classes, and upon certain unnamed fishes. Experiments upon the various classes of the invertebrata are unfortunately lacking. The results were mostly too indeterminate for him to record in detail; but he mentions that the cowpoxing of the hen succeeded.¹

Whatever may have been the scientific truths which the experiments on these several species of animals were calculated to draw forth, the experimental cowpoxing of the sheep had a real practical or economic interest. The sheep in Italy, especially the flocks of merinos,

¹ *Trattato di Vaccinazione.* Milano, 1809, p. 178.

were from time to time decimated by a smallpox (*variola ovina*) which was the same in all respects as the smallpox of man. The district of Padua had suffered much from that scourge among the sheep in 1797, and there were isolated occurrences of it in subsequent years. In the course of his vaccination journeys, Sacco found it in 1804 among sheep near Capua, and again in October, 1806, near Montemiscoso. On the latter occasion he cowpoxed several sheep; they developed vaccine vesicles, and resisted the variolous test which was tried upon them soon after, nor did they catch the contagion from the tainted part of the flock. After that triumphant test, Sacco persuaded several extensive flockmasters to have their sheep (especially merinos) cowpoxed, the consequence being that the smallpox was driven from among them. What really came to pass was one of those periodic lulls which occur in all epidemic or epizootic contagious diseases. Whenever the time came, smallpox of the sheep raged as before; cowpox had absolutely no relation to it, or relevancy in the matter, being another sort of pox altogether.

The protective power of cowpox against the smallpox of sheep is a delusion which has been confessed with brutal frankness by those whose pockets are concerned. It took some time to arrive at the truth of the case; but as soon as the truth was apprehended, the sensible, practical step of ceasing to vaccinate for sheep-pox was taken, regardless of what might happen to the professional credit of those who had warranted it. The following is the authoritative summary by Dr. William Budd, in 1863¹ :—

¹ *Variola Ovina*, Address in Medicine at Bristol Meeting of

“Against ovine smallpox, vaccination offers no specific protection at all. It has been proved by experiments on an enormous scale, performed under every condition to ensure accuracy, that vaccinated sheep, when afterwards exposed to the infection of *clavelée*, take the disease in large proportion in the natural way; and that when inoculated with it, they not only incur the usual consequences, but suffer quite as severely as unvaccinated sheep.”

This is all the more remarkable, that sheep, when vaccinated, develop the same vaccine vesicle as man does, and that lymph taken from the vaccine vesicle of a sheep produces the correct vesicle in man. Dr. Budd adds that this correct vesicle in man, raised by cowpox lymph from the sheep, protects the human being from smallpox, although its original, and exact counterpart, in the sheep, gave no protection from ovine smallpox. Men are not like sheep in that respect. Sir James Paget has said: ¹ “Jenner had to fight his fight for the benefit of men's lives against a vehement opposition; to that for the benefit of cattle, which are human property, there is no such opposition. It is truly a fact that we may well remember; though it is not a novelty to many in our profession, who have frequent opportunities for seeing how much more valuable a man feels his own property to be than his neighbour's health. . . . Property and healthy life may soon be regarded as more nearly equivalent than they have been hitherto.”

British Medical Association, 1863.—*Brit. Med. Journ.*, Aug. 8th, p. 147.

¹ Speech in proposing a vote of thanks to M. Pasteur at the International Medical Congress, London, 1881. *Transactions*, i. 90

The marvellous power of names, as contrasted with realities, over men's thoughts and actions is shown by the Italian dealings with sheep-pox in another way. It occurred to some, in the first flush of Jenner's inoculation, that as cowpox prevented smallpox, sheep-pox might do so also. *Variola humana*, *variola vaccina*, *variola ovina*—these were three equivalent forms; and why should not the sheep-pox serve as well as the cowpox to ward off the smallpox? Accordingly, when Sacco, in 1804, obtained variolous lymph from infected sheep at Capua, he gave it to Dr. Legni in the remote Sicilian province of Cattolica to try as a substitute for vaccine in the prevention of smallpox. It was not until four years after (29th June, 1808) that Dr. Legni sent to Sacco an account of his experiment: he had inoculated the *variola ovina* upon several children and found that it produced effects very like those of vaccine; he used it continuously in his practice for two or three years, and had inoculated three hundred with it; about the time of his using it, an epidemic of smallpox broke out—he had been diffusing ovine smallpox all the time—but all those inoculated with the variolous virus of the sheep passed through it unscathed.¹

The last of Sacco's various services to the theory of cowpoxing is his enthusiastic adoption, in 1802 or 1803, of Jenner's doctrine of the horse-grease origin of genuine cowpox. The cowpox at Varese had clearly nothing to do with horse-grease, and Sacco, in his first book, criticised Jenner's facts and reasoning thereon with some severity; he remarked that Jenner had nothing better

¹ Sacco's *Trattato di Vaccinazione*. Milano, 1809, p. 146.

than conjecture to base his theory on. At the same time he had caught up the clap-trap talk about "genuine" and "spurious," although he does not seem to have apprehended Jenner's motive in making the spontaneous cowpox a spurious sort. Being a keen experimenter, he had not been long settled at Milan when he went back to the horse-grease question, and in course of time satisfied himself that Jenner's doctrine was correct, Jenner himself having meanwhile quietly dropped it, except in his private correspondence.¹ Sacco obtained some matter from the ulcerous sores on a horse's hocks (he gives a startling picture of huge, excavated horse-sores in his *Trattato* of 1809), and therewith inoculated several children at the Foundling Hospital of Milan. He found that the effects were very like those of cowpox virus (as we know, in fact, that they always are); and, on trying the children with the variolous test, he found that they were protected just as if they had been cowpoxed.

Accordingly, in a letter to Jenner, dated the 25th March, 1803, he admitted that it was quite certain the grease causes the vaccine, and he suggested that one might by-and-by change the latter name into *equine*.²

¹ In a letter to De Carro (28th March, 1803) Jenner says:—"I am confident that had not the opponents, in this country, to my ideas of the origin of the disease been so absurdly clamorous, particularly the *par nobile fratrum* [Pearson and Woodville], the Asiatics" would now be enjoying, etc. De Carro replied, on 22nd April, "P——'s conduct borders on insanity."

² "J'ai déjà inoculé plusieurs des ces individus avec la petite vérole, mais sans aucun effet. C'est donc bien sûr et consenté que *le grease est cause de la vaccine*, et on pouvait bientôt changer

He sent his horse-grease matter to De Carro, in Vienna, who used it freely and gave of it to others. In a letter of 1804,¹ De Carro signs himself "vaccinator et equinator"; and many years after he wrote as follows: "The matter in use at Vienna from 1799 to 1825 was partly British vaccine, and partly originated from the grease of a horse at Milan, without the intervention of the cow. The effect was so similar in every respect that they were soon mixed; that is to say, after several generations, and, in the hands of innumerable practitioners, it was impossible to distinguish what was vaccine and what was equine."²

This, then, was the adventurous person who introduced vaccination into the Cisalpine Republic, and on whose sole credit, apart from foreign testimony, it was adopted by the State. Having vaccinated twenty-six persons (including himself) at Varese, in October and November, 1800, with matter from the Swiss cows, and tried the variolous test at once upon six of them, he removed to Milan, and performed his first vaccination there on the 8th of December. He lost no time in publishing his book,³ in which a great point was made of his stock of virus having come from an indigenous Lombard source, and of the mildness of the same as compared with Jenner's cowpox. He was hailed as the Jenner of

cette dénomination en *equine*, ou en ce que vous croyez mieux."—Baron, i. 251.

¹ Letter to Ring, in *Med. and Phys. Journ.*, Nov., 1804, p. 463.

² Cited by Copland in the article "Vaccination," in his *Dictionary of Practical Medicine*.

³ *Osservazioni pratiche sulla Vajulo Vaccino*. Milano, 1801.

Lombardy, and in a few months was appointed Director of Vaccination for the whole Cisalpine Republic. Writing to Jenner on the 16th of October, 1801, he says that he had performed more than eight thousand vaccinations with his own hand.

At that stage of his work he sent some of his Lombard cowpox matter to Woodville, in London, who was "so fortunate as to produce the genuine cowpox" with it; some of it, used by Ring, "has produced the genuine pustule and is now being used widely." It was spontaneous cowpox, however, if any cowpox ever was so; and Jenner's original teaching, as well as his later teaching, when it suited him (*e.g.*, Letter to Dunning, 2nd April, 1804), was that the "spontaneous cowpox was no preventive." For most persons it did not matter at all how the genuine and the spurious cowpoxes were respectively defined; a spurious variety was wanted along with a genuine merely for an apologetic purpose, and the more elastic the terms were, the easier the apology for failure or disaster.

Sacco's enormous number of vaccinations in the first few months amounted to a real propaganda. The introduction of cowpoxing into Italy was a sudden dash on the part of a hitherto unknown person with talents suited to the business, who saw his opportunity and was prompt to seize it. Two or three months before he found the cowpox at Varese, a number of the Milanese doctors had indeed published on the 22nd of June, 1800, a testimonial¹ in which they affirmed, without any experience of their own, the four stock propositions, that

¹ Printed in Sacco's *Osservazioni pratiche*, 1801.

cowpox prevented smallpox, that it was not contagious, that it produced no eruptions, and that it was attended with no risk. This was merely copied from the English, and it is not easy to see why the Milan doctors should have put their names to it. It may be inferred from a remark by Buniva, of Turin, who writes on vaccination in Italy in 1801 without mentioning Sacco's name,¹ that there were some, at least, who hesitated about Jenner's novelty; and there were probably more who hesitated about Sacco. If they had read the English history of vaccination with a moderate degree of attention, they would have detected the following passage in their own Sacco's *Osservazioni* of 1801 to be the romancings of an extremely untrustworthy person: "But this discovery, so fortunate for the human race, shared the fate of other grand and useful discoveries by encountering much opposition at its first outset. The basest envy let loose all its virulence against the discoverer on his appearance in London, but its attacks only made him redouble his diligence to bring his discovery to perfection. For a moment he retired from his enemies, to confound them on his return with the victorious arms of multiplied observations and the most decisive experiments. At a distance from the clamours of a populous city, in the retirement of Gloucestershire, where cowpox is almost endemic, Jenner had an opportunity of continuing his experiments in the fullest tranquility."² Of all the rhetorical nonsense written about Jenner, that is the

¹ *Calendario Georgico della Società agraria Subalpina*. Torino, 1802, p. 23.

² Translated in *Med. and Phys. Journ.*, vii. (1802), 169.

most nonsensical; and the man who could write so, purely out of his head, might well have been looked upon with distrust by the responsible leaders of medicine in Italy.

But opportunities soon arose which excited popular enthusiasm for Sacco. There had been a complete cessation of smallpox epidemics in Italy (excepting Sicily) since 1796, after an unusually severe prevalence of them all over the country. At length a small and mild outbreak occurred at Giussano e Sesto, at the lower end of the Lago Maggiore; thither Sacco repaired as a deliverer, "suffocated" the epidemic, and established the "first triumph of vaccine."¹ It was after this that the Republic appointed him Director. Another isolated outbreak occurred at Bologna the same year, which he dealt with in like manner, and had a gold medal given to him by the grateful citizens, of which he reproduced two cuts afterwards in one of his books: it bears on one side his effigy, and on the other side the inscription *Æmulo Jenneri amici Bonnonenses*. In the spring of 1802 there was a rather more severe outbreak in the province of Brescia, in which many died. The Government, careful of the lives of the citizens, "cast a beseeching look upon him," and he hastened to the rescue. The plague was stayed (by vaccinating 13,000 in a population of 300,000 or 400,000), and the deliverer again received a gold medal, whereon Sacco is represented in the act of extracting lymph from a cow's teat.²

¹ *Trattato di Vaccinazione*, p. 14.

² On the evening of the day when I had written the above passage I took up the *Lancet* of that date (7th July, 1888, p. 32), and read, in an editorial note on "Smallpox in Milan," as follows:—

After these popular successes, the medical leaders could no longer afford to be sceptical or indifferent. Accordingly it was arranged that their scruples should be satisfied in due academical form, and "a solemn experimental test" was announced to be held at the Orfanotrofio della Stella, in Milan, on the 31st of August, 1802. It took place in the presence of "many of the authorities of the Republic, the professors of the faculty, and other learned persons."¹ Sacco opened the proceedings with an eloquent speech. He then introduced a child at the eighth day of a copious natural smallpox eruption, and invited the assembled professors to satisfy themselves that it was really smallpox. Sixty-three children or adults, mostly inmates of the orphanage, who had been vaccinated at various dates since June of the year before, were then called in one by one, and inoculated with smallpox from the child in attendance. The assembly then adjourned until that day fortnight. Those who came back on the 14th of September to hear the result were informed by Sacco that the variolous inoculation had not in general produced any effect, only a few having had some local trouble. But two unvaccinated persons, who had been taken into the experiment

"Smallpox and typhoid are never wholly absent from the Milanese population ; the former especially having periods of recrudescence, sometimes so sudden and so pernicious as to amount to positive 'explosions.' One of these has declared itself within the last week, and, as usual, there is a whipping-up of the people to the vaccinating stations, in the vain hope that such spasmodic and unsystematic precautions can stay the disease." Shade of Sacco !

¹ "Contra-prova della Vaccinazione : " the official report, printed in vol. xxii. (p. 121) of Amoretti's *Opuscoli Sceleti sulle Scienze*. Milano, 1803.

as shocking examples, were pronounced to have been "completely infected by the variolous inoculation," one of them, an adult, having had four pustules on his arm, which were dried up at the eighth day, and the other, a child of two, three pustules on the left arm, two on the hand, two on the shoulder, three on the right arm, and one on the forehead.

Sacco's credit was now completely established in the best circles. That year he was admitted to the fellowship of the Milan Academy, and appointed *medico primario* to the Ospedale Maggiore, as a recognition of his vaccinating zeal.

The next epidemic of smallpox was at Florence, in 1805; and in November and December of that year Sacco held another "solemn experimental test" for the satisfaction of the chiefs of the Florentine Royal Medical and Chirurgical College. Eight children just vaccinated by Sacco on the 8th, 16th, and 24th October (one of them being the vaccinifer whom he had brought from Bologna), together with four old vaccinated cases of 1801 and 1803, were brought together on the 24th November, 1805, and inoculated with smallpox from a confluent case at the ninth day, in the presence of official delegates and other representative medical men.¹ Three physicians who had been taking a lead in the movement were delegated to watch the children meanwhile; and the whole were ordered to present themselves again that day fortnight, the 8th of December. Nineteen medical practitioners certified on that day that, according to

¹ *Rapporto delle Vaccinazioni fatte in Firenze dal Dott. Luigi Sacco.* Firenze, 1806.

what was reported to them and what they had seen, none of the twelve children variolated after cowpox had been attacked with smallpox, nor had any shown constitutional symptoms, and that no effects had followed, except some slight irritation at the place of insertion. Therefore they concluded that vaccination prevented smallpox. Another testimonial, signed on behalf of the Royal Medical and Chirurgical College by four of its members deputed, speaks of vaccinations done by Sacco at the Spedale degl' Innocenti on the 13th, 17th, and 21st of November, and of the experimental variolous tests on the children, on the 24th of the same month. It need hardly be pointed out that the children in orphanages were just the subjects who would have their lymphatic glands stirred, by the absorption of the cowpox virus, into plastic activity (which might go on to scrofula), and that the absorbent glands would be so far deprived of their function as to fail in taking up and transmitting another virus introduced under the skin of the same region a few weeks or even days after.

This formal scientific proof at the end of an epidemic in Florence, in 1805, reminds one of hanging a man first and trying him afterwards. Sacco had been for more than four years Director of Vaccination to the whole Cisalpine Republic. He had visited all parts of Italy in his mission as a cowpoxer (or horse-greaser) from the Lago Maggiore to the farthest district of Sicily, and had inoculated some hundreds of thousands with his own hand. In a letter to Jenner, dated Trieste, the 5th January, 1808, he says :¹ "During eight years I reckon

¹ Baron, ii. 112.

more than 600,000 vaccinated by my own hand." In his quarto treatise,¹ published more than twelve months later, the number has decreased to 500,000, so that we may take Sacco's figures as not intended to be accurate to a hundred thousand or so. In 1806, vaccination was publicly enforced, by various indirect means, almost as much as it has ever been enforced in Italy.

It was not until a good many years after that the protective was put to a real test, on the revival of the small-pox epidemics after a rather longer interval than usual, which had been more than adequately filled by typhus.² Then the objections to vaccination began to find utterance, and were answered in the dexterous apologetic manner which we know so well. Sacco appeared at the Vienna meeting of the German Association of Naturalists and Physicians, on 26th September, 1832, and delivered a Latin oration on the need for compulsory vaccination all over the world, in which he said that all the objections that can be brought against vaccine yield to reason and experience (*rationi cedunt atque experientiæ*), or, in other words, they yield to professional apologetics. On that occasion the famous apology for cowpox was brought forward by Sacco, that, if it did not prevent smallpox, it reduced its attack to a mild type. This was promptly challenged by Schönlein (the future leader of German medicine and a man of deep learning, who had made epidemics his favourite study), on the ground that there had been just as large a proportion of mild smallpox cases before the vaccination era as there ever was after it.

¹ *Trattato di Vaccinazione*, p. 18.

² See Corradi, *Annali delle Epidemie occorse in Italia*.

By a singular fate, those very districts of Northern Italy which Sacco provided with an indigenous kind of vaccine, milder than the English stock, were the first in Europe to be afflicted by epidemics of so-called vaccinal syphilis involving the infancy of whole communes at once. In my former book on the *Natural History of Cowpox and Vaccinal Syphilis*, I have entered into the evidence concerning these and other epidemics of the kind, and have stated the conclusion, which has not been as yet impugned, that the so-called syphilitic properties of the vaccine were not a contamination of it by another virus, but a revival, through carelessness as to over-ripeness, etc., of those inherent properties of cowpox to which it owed its original colloquial name of a pox.

While Sacco was the great apostle of cowpoxing in Italy, and for some years almost the only vaccinator in certain provinces, the new practice took an independent start in Piedmont, in association with the Geneva vaccinists, and was carried on by a number of the ordinary medical practitioners.¹ Professor Buniva, of Turin, a leader in all matters concerning natural and medical science in their relations to the domestic animals and to agriculture, presented a report in 1803, on the protective value of cowpox, which I have been unable to see. There was also an unimportant variolous test by Moreschi at Venice, on 26th August, 1801.²

The English emissaries of Jenner had also a hand in introducing cowpox into Sicily and Southern Italy. Marshall, the Eastington practitioner, whose round per-

¹ Buniva, *Calendario Georgico*, l.c.

² Sacco, *Osservazioni pratiche*, 1801, p. 219.

centages are referred to on p. 129, was allowed by the Admiralty to go out in the *Endymion* in July, 1800, on a mission of his own to vaccinate among soldiers and sailors in the Mediterranean. In the course of the year 1801 he came to Palermo, and was hailed as a deliverer by the enlightened monarch Ferdinand IV., and his equally enlightened Court.

“It was not unusual,” Marshall wrote home to Jenner,¹ “to see in the mornings of the public inoculation at the Hospital, a procession of men, women, and children, conducted through the streets by a priest carrying a cross, come to be inoculated. By these popular means it met not with opposition, and the common people expressed themselves certain that it was a blessing sent from Heaven, though discovered by one heretic and practised by another.”

That was the missionary apostolic side of Marshall's cowpoxing zeal; but in private circles at Palermo his fee for vaccination was ten guineas in genteel families, and five guineas in families of the middle class.² Palermo had not seen such another enthusiast since the time when it gave to the world Count Alessandro di Cagliostro, “healer of diseases, abolisher of wrinkles, friend of the poor and impotent, gold-cook, grand cophta, prophet, priest, and thaumaturgic moralist, etc.”

The Italians have never been very critical of the Jennerian legend, or of any part of the same. An English book that they admire greatly, Mr. Smiles' *Self Help*, which has circulated in Italy to the extent

¹ Baron, i. 403.

² *Med. and Phys. Journ.*, vi. 95.

of fifty thousand copies, and appears to have given its name to a wide-spread association, may have had something to do with the more recent developments of Italian enthusiasm for Jenner. The author of *Self Help* gives more than two pages of his crowded space to that worthy; he repeats the Jennerian history in its usual legendary form, with at least one error peculiar to himself. Among other things, he tells us that Jenner's "faith in his discovery was so implicit that he vaccinated his own son on three several occasions."

Now, one of the most remarkable pieces of recent Italian art, which attracted much popular admiration at the Paris Exhibition of 1878, is the group in marble, by Professor Monteverde, of Rome, described in the catalogue as "Edward Jenner che inocula il vaccino al figlio." Jenner did vaccinate his own child, Robert F. Jenner, aged eleven months, on the 12th of April, 1798, after he had vaccinated several; but, as often happened in the first trials, he did not "take."¹ Shortly after, when Jenner was living at Cheltenham, a medical friend came into the house, and, taking the child in his arms, remarked pleasantly that he had just left a family in the smallpox. "Sir," cried Jenner, "you know not what you are doing. That child is not protected." The boy was thereupon inoculated, but not with cowpox; he was inoculated with smallpox.² This visit to Cheltenham seems to have been in the autumn and winter of 1798-99,³ when Jenner had no better stock of vaccine than the matter from the Stonehouse dairy

¹ Jenner's *Inquiry*, p. 40.

² Baron, ii. 44.

³ Baron, i. 303.

which had produced alarming ulcerations both in his own trials of it and in the trials by two of the Stroud surgeons. That was not the sort of "lymph" which Jenner would care to use upon his own son; and it was not until February, 1799, that Woodville provided him once for all with a stock which he could use. But this is how he explains the incident of using smallpox matter on his child. The reason, he says,¹ "for not resuming my operations [with cowpox] at Cheltenham was the supposition that the people assembled at a public watering-place might conceive the disease (then so little known) to be contagious." Accordingly, when his child was suddenly exposed to risk, Jenner saw no alternative but immediate inoculation with smallpox, a disease which "the people assembled at a public watering-place" might not merely conceive to be contagious, but knew very well to be contagious. Indeed, Jenner and his friends were demanding the statutory prohibition of smallpox inoculation on that very ground as early as 1802.

The group by Professor Monteverde might just as well have been called "Jenner Pricking a Child," or "Jenner Inoculating a Child"; but the professor had sought, by the aid of the catalogue, to import an air of heroism and magnanimity into an incident which is in itself vulgar and trivial, and to that end he had used the popular legend of Jenner without critically examining it. The story in marble of "Edward Jenner che inocula il vaccino al figlio," is of a piece with the whole story of vaccination in Italy.

¹ Letter to Baron, 6th November, 1810, in *Life of Jenner*, ii. 48.

CHAPTER XII.

ASSENT TO A MYSTERY.

WHOEVER has had opportunity to look into any of the larger and more inspiring problems of pathology, such as cancer or tubercle, or into those great epidemiological themes, reaching out to ethnology on the one hand and to ethics on the other, such as yellow fever or even smallpox itself, will be sure to feel, all the time he is dealing with vaccination, that he has got hold of an exceedingly unworthy subject. One naturally seeks, therefore, to dignify it by whatever associations may be grouped around it. Its acceptance as if by the general assent of mankind is one of the considerations that redeem vaccination from the reproach of paltriness: the famous plea of *securus judicat orbis terrarum* has been put forward for it, if not in words yet in effect, by philosophical historians like Sir G. C. Lewis, as well as by medical apologists. Again, when one discovers that it was urged upon Catholic and Protestant parents in homilies given to them at the baptism of their infants,¹ recommended by

¹ Sacco, *Trattato*, 1809. Moseley's *Commentaries on the Lues Bovilla*, 2nd ed. p. 51. De Carro to Jenner, 14th February, 1801. Baron, i. 339.

sermons from Anglican and Lutheran pulpits,¹ and by a ukase of the Czar to the clergy of the Greek Church,² we seem to be dealing with something of the nature of *confiteor unum baptisma*. And if anything were wanting to dignify vaccination in its psychology, if not in its objective characters, we find it in the circumstance that the general assent to it was admittedly the assent to a mystery. *Credo quia impossibile* was as truly the personal action of men's minds towards the mysterious efficacy of vaccine as it has been towards the arcana of the faith.

I am here concerned with the vaccine dogma as showing forth assent to a mystery; and my first duty is to bring forward evidence of the fact. Dunning, a devoted Jennerian who had some pretensions to scholarship, drew up quite early in the day a Latin definition of vaccine inoculation which begins, *Morbus vicarius, potiusve processus succedaneus, mirifico variolam certe præveniendi, immo (quod veresimilius sit) penitus abolendi, fungens munere*:—a vicarious disease fulfilling the marvellous office of preventing smallpox, etc.³ When Woodville went over to Paris in 1800, and first demonstrated the new inoculation there, Dr. Colon wrote:⁴ “Does not this preservative from the usual disease seem, by its beneficent quality, to be a kind of

¹ Sermon at Great St. Mary's, Cambridge, in 1805; Baron, ii. 49. See also Ring's *Treatise on the Cowpox*, 1801-3; *Med. Chirurg. Zeitung*, ii. 399, etc.; Address to Church of Scotland, by the Managers of the Vaccine Institute. Edin., 1803.

² Letter of Crichton to Jenner, 1811, in Baron, ii. 184-6.

³ *Med. and Phys. Journ.*, iv. 146.

⁴ *Ibid.*, iv. Letter of 27th July, 1800.

marvel, when we consider that the trouble it gives rise to is nothing more than the puncture which one makes for the purpose of inoculating, and is exempt from the slightest accident." It is true that Colon was afterwards stigmatized by the more academical advocates of vaccine in Paris as a charlatan ; but that was mainly because he treated vaccination with too little ceremony as a matter of business. De Carro, the leader of the movement in Vienna, asks in his treatise: "How can it be conceived that an effect apparently merely local can guard against such a disease as the smallpox, whose effects on the whole system are known to us all to be so violent? Certainly the fact is very extraordinary ; it is a new mystery added to those which, from the beginning of medical science, have been deplored by its professors."¹ To take one more foreign confession, Sacco brings forward the common apologetic doctrine of the time, that genuine vaccine becomes spurious if a person who had gone through the natural smallpox become the vaccinifer, and adds: "Those who wish to know the reason of everything will want to know the reason of that. We need new observations so as to be able to rend asunder the veil of this medical mystery." And lastly, Jenner himself had struck the keynote of mystery in the opening pages of the *Inquiry*: "But what renders the cowpox so extremely singular is that the person who has been thus affected is for ever after secure from the infection of the smallpox." In support of "so extraordinary a fact," he proceeds to lay before the reader a great number of instances.

¹ Extracts in *Med. and Phys. Journ.*, vii. 187.

The fact was all the more extraordinary, as Jenner's readers quickly perceived, and he himself had pointed out,¹ in that one attack of cowpox did not prevent a second of the same. If cowpox does not protect from itself, they asked, how can it possibly protect from smallpox? This only made the mystery more mysterious. Pearson was so well aware of that intellectual difficulty that he promptly denied,² and continued to deny,³ the possibility of the same person having cowpox twice. In the very first review of Jenner's *Inquiry*, in an English journal,⁴ the author's statement that one attack of cowpox did not preclude a second or a third is mentioned as being "received with general scepticism merely on account of its improbability." Dr. Winterbottom, a physician of foreign experience, was at a loss to understand how an affection could be constitutional, and at the same time obscure in its action, or "without any evident disturbance of the functions."⁵ A Philadelphia physician wrote to a correspondent in England: "I should have inoculated with the matter of kine-pock two years ago, having received an infected thread from Dr. Pearson; but I was deterred at that time by the fact mentioned by Dr. Jenner, of a person

¹ "It is singular to observe that the Cowpox virus, although it renders the constitution insusceptible of the variolous, should nevertheless leave it unchanged with respect to its own action."
—Jenner's *Inquiry*.

² *Inquiry on the History of the Cowpox*, 1798.

³ *Report of the Vaccine Pock Institution*, 1803, p. 49.

⁴ *Med. and Phys. Journ.*, i. 8 (Jan., 1799).

⁵ *Ibid.*, vi. 1801 (7th June). See also Chapman, in Duncan's *Annals*, 1799.

being able to be infected with the kine-pock more than once, though it rendered him for ever secure against taking the smallpox."¹

These intellectual difficulties were soon forgotten. The profession were unwilling to admit that there was any real mystery. They reasoned: We are practical men; it is not our affair to explain how or why cowpox wards off smallpox; but we know from our experiments and our experience that it does so, and that is enough for us; it is only one more empirical truth added to the long series of empiricisms of which the medical art is made up. As my primary object throughout this book has been to show how Jenner got his cowpox doctrine and practice accepted in good faith by the medical profession and the educated laity all over the world, I am not concerned so much with the logic of the case as with its psychology; and I do not here enter upon such matters as the practical man's blameworthiness in declining to scrutinize with the utmost rigour the terms in which a proposition is stated or an experiment conceived, or his laxity in omitting to apply to his proper business that obstetric or Socratic method by which ideas are disentangled and illusions exposed. I take the assent to the vaccine mystery as a historical fact, and I shall now endeavour to show how far it exemplifies the working of the mind on one of those mysteries that are apprehended as if *ubique et ab omnibus*, and how far our modern scientific instance is peculiar in its psychology.

Cardinal Newman, in his *Grammar of Assent*,² dis-

¹ *Med. and Phys. Journ.*, vii. 317.

² Pp. 45-52, 125-140.

cusses the question of belief in a mystery and expounds the law of our minds according to which the assent is given to it. A mystery, he says, is a proposition conveying incompatible notions, or a statement of the inconceivable. We can assent provided we can apprehend ; therefore we can assent to a mystery, for, unless we in some sense apprehend it, we should not recognise it to be a mystery, that is, a statement uniting incompatible notions. But words which make nonsense do not make a mystery,—such words, for example, as Warton's line, "Revolving swans proclaim the welkin near."

When we assent to a mystery as such, or in respect of its mysteriousness, our assent is notional as distinguished from real. Further, even processes of inference can end in a mystery, our notions of things being never simply commensurate with the things themselves, but aspects of them, more or less exact, and sometimes a mistake *ab initio*. The free deductions from one of these aspects necessarily contradict the free deductions from another. After proceeding in our investigation a certain way, suddenly a blank or a maze presents itself before the mental vision, as when the eye is confused by the varying slides of a telescope. When we try to explain that the physical tokens of creative skill need not suggest any want of creative power, we feel we are not masters of our subject. We apprehend sufficiently to be able to assent to these theological truths as mysteries ; did we not apprehend at all, we should be merely asserting.

The exposition goes on—To give a notional assent to a dogma of faith is a theological act ; to give a real

assent to it is an act of religion. The dogma is discerned, rested in and appropriated as a reality by the religious imagination; it is held as a truth by the theological intellect. But there is no line of demarcation between these two modes of assent, the religious and the theological. In the Athanasian creed, the doctrine so drawn out is plainly of a notional character; is it not also capable of being apprehended otherwise than notionally? Is it a theory, undeniable indeed, but addressed to the student, and to no one else; or does it come to the unlearned, the young, the busy, and the afflicted, as a fact which is to arrest them, penetrate them, and to support and animate them in their passage through life; that is, does it admit of being held in the imagination, and being embraced with a real assent? The answer is affirmative.

Now, the author continues, let us observe what is not in that exposition;—there are no scientific terms in it, no terms which do not admit of a plain sense and are used in that sense; they are not abstract terms, but concrete, and adapted to excite images; and these words, thus simple and clear, are embodied in simple, clear, brief, categorical propositions. There is nothing abstruse either in the terms themselves or in their setting. It is plain, of course, even at first sight, that the doctrine is an inscrutable mystery, or has an inscrutable mysteriousness. But the mysteriousness of the doctrine is not, strictly speaking, intrinsic to it, as it is proposed to the religious apprehension, though in matter of fact a devotional mind, on perceiving that mysteriousness, will lovingly appropriate it. Strictly speaking, the dogma, as a complex whole, or as a mystery, is not

the formal object of religious apprehension and assent ; but, as it is, a number of propositions, taken one by one. A real assent to a mystery is not possible, but only a notional ; because, though we can image the separate propositions, we cannot image them all together ; we cannot bring them before us by one act of the mind ; we drop the one while we turn to take up the other. Our devotion is tried by the long list of propositions which theology is obliged to draw up, by the limitations, explanations, definitions, adjustments, balancings, cautions, arbitrary prohibitions, which are imperatively required by the weakness of human thought and the imperfections of human languages. Such exercises of reasoning indeed do but increase and harmonize our notional apprehension of the dogma, but they add little to the luminousness and vital force with which its separate propositions come home to our imagination ; and if they are necessary, as they certainly are, they are necessary not so much for faith as against unbelief.

The author proceeds :—The dogma is not ordinarily spoken of as a mystery, not even in the creeds ; for these are devotional addresses, in which it would be out of place to speak of intellectual difficulties. What is more remarkable is, that a like silence as to the mysteriousness of the doctrine is observed in the successive definitions of the Church concerning it. Thus the great Council of Toledo pursues the scientific ramifications of the doctrine with the exact diligence of theology, at a length four times that of the Athanasian creed ; but we do not find either the word “mystery,” or any suggestion of mysteriousness. The custom is otherwise as regards

catechisms and theological treatises ; in them certainly the mysteriousness of the doctrine is almost uniformly insisted upon. But, however this contrast of usage is to be explained, the creeds are enough to show that the dogma may be taught in its fulness for the purposes of popular faith and devotion without directly insisting on that mysteriousness which is necessarily involved in the combined view of its separate propositions.

The summing up is :—Theology has to do with the dogma as a whole made up of many propositions ; but religion has to do with each of these separate propositions which compose it, and lives and thrives in the contemplation of them. In them it finds the motives for devotion and faithful obedience ; while theology, on the other hand, forms and protects them by virtue of its function of regarding them, not merely one by one, but as a system of truth. And lastly, if the separate articles are so closely connected with vital and personal religion, is there cause to wonder that the creed should proclaim aloud the importance of the dogma being accepted ?

It is the object of the treatise from which the foregoing illustration has been taken (in the original words so far as compatible with condensation), to expound all that is natural to the mind in the way of apprehending, inferring, and assenting ; and the great illustrations which are always in the background of the author's thoughts are taken to be modes of intelligence, imagination, and feeling proper to our nature, exemplifying the working of the mind at its best and under the best guidance. But the author does not omit to remark upon the numerous laxities and aberrations incidental

to our mental constitution: "In this day the subject-matter of thought and belief has so increased upon us, that a far higher mental formation is required than was necessary in times past, and higher than we have actually reached. The whole world is brought to our doors every morning, and our judgment is required upon social concerns, books, persons, parties, creeds, national acts, political principles and measures. We have to form our opinion, make our profession, take our side on a hundred matters on which we have but little right to speak at all. . . . Such are the mistakes about certitude among educated men; and after referring to them, it is scarcely worth while to dwell upon the absurdities and excesses of the rude intellect as seen in the world at large; as if any one could dream of treating as deliberate assents, as assents upon assents, as convictions or certitudes, the prejudices, credulities, infatuations, superstitions, fanaticisms, the whims and fancies, the sudden irrevocable plunges into the unknown, the obstinate determinations,—the offspring, as they are, of ignorance, wilfulness, cupidity, and pride,—which go so far to make up the history of mankind; yet these are often set down as instances of certitude and of its failure."¹

Having stated in the words of its ablest exponent the case for assenting to a mystery as a normal act of the mind, and having shown by the last quotation that the author had not so handled the case out of a spirit of mere optimism, I shall now proceed to inquire how it stands with the assent to the scientific mystery which immediately concerns us—whether that also conforms to

¹ Pp. 234-6.

the conditions of an "indefectible" certitude, or whether it may not perchance be one of those prejudices, credulities, infatuations, superstitions, fanaticisms, whims, fancies, sudden irrevocable plunges into the unknown, and obstinate determinations—the offspring as they are of ignorance, wilfulness, cupidity, and pride—which are so common in the history of mankind.

That the vaccine doctrine of protection is held with a real assent, or religiously, by vast multitudes of men and women is unquestionable; they believe it to be necessary for salvation in one small contingency of human life; and they are so sure of it that they even enforce it, or allow it to be enforced upon recalcitrating minds. Those who thus hold it are assenting only to each of the separate propositions that compose it, and not to the complex whole of the doctrine or to the mystery of it. The four stock component propositions, as laid down by Jenner and maintained by his contemporaries, are stated in concrete terms, and are simple, clear, brief, categorical. They are, that vaccine inoculation prevents smallpox, that it is itself not contagious, that it is unattended by a general eruption like that of smallpox, and that it is free from risk. These are the original component propositions; they have merely become rather less categorical with the lapse of time.

The complex whole of the doctrine, the system of vaccination truth, is a subject for pathology; and it is here that the first difference is seen between the vaccine mystery and that which has been quoted as a great classical example of the terms on which a mystery may be assented to. Has pathology pursued "the scientific ramifications of the doctrine with the exact diligence of

theology"? Have its exercises of reasoning "increased and harmonized our notional apprehension of the dogma," even if they have added little to "the luminousness and vital force with which its separate propositions come home to our imagination"? Has pathology drawn up a long list of "limitations, explanations, definitions, adjustments, balancings, cautions, arbitrary prohibitions"? Does it "form and protect the separate propositions which compose it by virtue of its function of regarding them, not merely one by one, but as a system of truth?"

Pathology has never pursued the scientific ramifications of the vaccine doctrine with exact diligence. Our notional apprehension of the doctrine has not been increased and harmonized by any exercises of reasoning. There is not even a definition of vaccine, in scientific terms, by reference to which the uniformity of the operation can be assured. Let us bring these statements to a test in relation to the most formal, serious, and responsible handling of the vaccine doctrine, the handling of it in Parliament.

The question of giving facilities for vaccination all over England was first brought before the House of Lords in 1840 by the Marquis of Lansdowne, on the occasion of presenting a petition from the Medical Society of London. The common people had grown dissatisfied with vaccine inoculation, the smallpox epidemics having returned, especially in the intervals between periods of typhus; the people had in some places even shown a disposition to go back to the old variolous inoculation. The Medical Society, approaching the House of Lords through Lord Lansdowne, asked that

variolous inoculation should be forbidden, inasmuch as the revival of smallpox was due to it, both directly as a source of contagion, and indirectly as keeping out the true protective vaccine. Among other things, the Society stated that there was "a perfect identity between vaccination and smallpox, although the symptoms were different," this having been proved by the successful inoculation of a heifer with smallpox matter on one of the mucous surfaces.

A bill was accordingly brought in by Lord Ellenborough, providing facilities for vaccination of the poor under the Boards of Guardians, and prohibiting variolous inoculation, except by medical men. The bill, being a private member's, was taken in charge in the House of Commons by Sir James Graham, an ex-minister, and was passed with the important amendment by Mr. Wakley, a medical man, prohibiting variolous inoculation absolutely, under pain of imprisonment. In these debates nothing is more remarkable than the unanimous expression of belief that vaccine prevented smallpox; it was the real or religious assent to the most important of the several propositions of the complex doctrine. The attempt to deal notionally with the doctrine as a whole, by Lord Lansdowne in quoting the Medical Society's statement that there was a perfect identity between vaccination and the smallpox, although the symptoms were different, served to indicate the existence of a mystery, while failing to increase and harmonize our notional apprehension of it. It was an inchoate attempt such as, in the analogous case, even a very early or apostolic writer would have thought inadequate.

The next appearance of vaccination in the legislature

was in 1853, when Lord Lyttelton, as a private member, brought in a bill to make vaccination compulsory. The bill passed through both Houses without opposition, and with hardly any debate except on points of detail. Lord Lyttelton was asked to inform a correspondent in 1869 upon what evidence he had proceeded in framing the first compulsory Vaccination Act, and replied: "The expediency of making vaccination universal I took, as I believed, on common notoriety, and the medical authorities I chiefly consulted were Dr. Seaton and Dr. Marson."¹ In the House of Lords he said, "It is unnecessary to speak of the certainty of vaccination as a preventive of the smallpox, that being a point on which the whole medical profession had arrived at complete unanimity."

The Act of Parliament of 1853 had no section devoted to the "Definition of Terms"; there was no definition of cowpox or genuine vaccine, an omission all the more remarkable that variolous matter was then being used as vaccine, on the pretext that it had "passed through the cow." Although a medical dogma was therein established by the State, the doctrine was not formulated. In the other great instance of dogma established by the State, there was a body of doctrine carefully defined in a series of co-ordinate and interdependent articles: it had been "pursued into its scientific ramifications with the exact diligence of theology." The vaccine doctrine, in the Act of 1853, stood alone, not co-ordinated to any other principle of epidemi-

¹ Letter of Lord Lyttelton to R. B. Gibbs, 28th July, 1869, in *Vaccination Inquirer*, iii. 71.

ology or of pathology; and it was moreover undefined in any terms whatsoever. It was simply a notorious empirical practice that was established under pains and penalties.

Three years after compulsory vaccination became the law of the land, it was thought desirable to meet objections that were now beginning to be heard, by an elaborate blue-book of history, theory and experience, presented to both Houses of Parliament. In that blue-book the old fragment of theory adduced by Lord Lansdowne in 1840, of the identity of cowpox with smallpox, was reproduced with a good deal of formality and authority. After stating that Jenner's *Inquiry* of 1798 had set the popular belief "on a scientific basis," the Report proceeds: "It was not until forty years after that science supplied an authentic interpretation of Jenner's wonderful discovery. . . . These researches [inoculation of smallpox upon a semi-exposed mucous membrane of a heifer] set in a very clear light the meaning of Jenner's practice. A host of theoretical objections to vaccination might have been met, or indeed anticipated, if it could have been affirmed sixty years ago as it can be affirmed now:—This new process of preventing smallpox is really only carrying people through smallpox in a modified form. The vaccinated are safe against smallpox because they, in fact, have had it" (p. xii.).

This was one of those simple, clear, categorical statements belonging rather to the real or religious assent than to the notional; there was nothing here of the "exact diligence" of pathology, pursuing the scientific ramifications of the doctrine; any such attempt to

represent the complex doctrine as a whole would have brought men face to face with the mysteriousness of it, with the juxtaposition of incompatible notions, with an inoculated smallpox which was not smallpox and yet prevented smallpox, as had been said in 1840 in the very hour when they were making the old inoculation a penal offence.

In the progress of medical science the veil of mystery hanging over the vaccine doctrine as by law established has not been lifted. On one occasion we seemed for a moment to catch sight of the firm outlines of a scientific principle, but the vision proved to be an illusion. When the Ministry of the day proposed in 1880 to relax the penal provisions of the compulsory vaccination law so far as to let a recalcitrant parent off with a single fine or imprisonment for each child, instead of fines or imprisonments at intervals of six months, more or less, until the child was fourteen years old, the project was defeated by the strong representations made to the Minister by those deputed from the medical and scientific corporations. One of these deputations was organized by the President of the Royal Society, and consisted of himself and Professor Huxley, the President of the Royal College of Physicians, the President of the Royal College of Surgeons, the President of the General Medical Council, and others. The President of the Royal Society justified his action in the next annual address to the Fellows;¹ the proposed abolition of repeated penalties for non-compliance with the Vaccina-

¹ Presidential Address by W. Spottiswoode, *Proc. Royal Soc.*, 30th Nov., 1880.

tion law appeared to "trench closely upon the application, at least, of a scientific principle." When asked by a correspondent to state what was the scientific principle, the President of the Royal Society replied briefly: "The principle to which I referred was that of vaccination."¹

Within a year of that adumbrating of the scientific principle of vaccination, a step was taken by M. Pasteur, of the Académie des Sciences, to remove the mystery by generalizing the word "vaccine" so as to include a number of "protectives" which had nothing to do with cows or cowpox. At the International Medical Congress held in London in 1881, he said:² "J'ai donné à l'expression de vaccination une extension que la science, je l'espère, consacrerà comme un hommage au mérite et aux immenses services rendus par un des plus grandes hommes de l'Angleterre, votre Jenner." And in another of the General Addresses spoken on the same occasion, under a title which breathes the severe spirit of scientific scrutiny, "Le Scepticisme en Médecine au Temps Passé et au Temps Présent,"³ we read that M. Pasteur, "reprenant et systématisant l'œuvre de votre grand Jenner, arrive par l'atténuation méthodique des virus, à inaugurer la prophylaxie des maladies virulentes, et nous ouvre ainsi des horizons nouveaux et indéfinis." Here, then, we have the scientific principle; it is the methodical attenuation of virus. Let us examine this last word of science upon the empiricism of a former

¹ Letter of W. Spottiswoode to G. S. Gibbs, 1st Feb., 1881, published in *Vaccination Inquirer*, iii. 12.

² Address at St. James's Hall, 8th Aug., 1881. *Trans. Internat. Med. Congress*, i. 85.

³ Dr. Maurice Raynaud, *ibid.*, p. 51.

age, so as to discover whether we are now quite done with the old juxtaposition of incompatible notions.

An English exponent of the modern French principle of "vaccin" states the case thus: "You know that vaccine lymph came originally from a cow or a calf. . . . The vaccine virus is, probably, a mild form of the most virulent smallpox virus. Pasteur would call it an attenuated virus. Now, he has succeeded in this process of attenuation so far as to do for other diseases what Jenner enabled us to do for smallpox. The agent by which the attenuation is effected, Pasteur considers to be the oxygen of the air."¹

So far as concerns the attenuation of the most virulent smallpox virus, that is an old eighteenth-century practice and theory, of which a full account has been given in chapter vi., on "The Variolous Test." It was that attenuated smallpox virus which Jenner used, not as vaccine, but as the test of the power of vaccine against variola. The attenuation was effected by taking the virus from the local pustule of inoculated smallpox instead of from a pustule of the general eruption, and by taking it when it was a serous or ichorous fluid short of the full ripeness of purulent matter. The distinctive character of the cowpox disease itself, as contrasted with the purulent eruption, contagiousness, and fever of smallpox, was a different thing; it had nothing to do with the oxygen of the air, but depended on the

¹ Professor Tyndall, address at Preston, December, 1884. In his introduction to *L. Pasteur: his Life, etc.* (London, 1885), he adds: "He has also weakened it by transmission through various animals. It was this form of attenuation which was brought into play in the case of Jenner." (p. xxxvii.)

much more intricate process of the transmission of a disease from the horse's hocks to become a disease of the cow's teats, and thence an artificial disease of the child's arm.

Thus we enter upon a dense and tangled underwood of historical origins. If it is ever to be cleared, it will need something more of exact diligence than is implied in the invention of phrases like the "methodical attenuation of virus," or the construction of bold figures of speech like "vaccins charbonneux," or "vaccins rabiques." Science can never divest vaccine of its historical associations with a loathsome corroding ulceration of the cow's teats, due to the callous barbarity of ignorant milkers.

The exact diligence of theology, pursuing the scientific ramifications of its mysterious doctrine to four times the length of the Athanasian Creed, must command the respect even of unbelievers, the more so as it is a Church maxim that salvation does not lie in dialectics. But what shall we say of pathology, which has never faced its miraculous doctrine at all; which has not had the candour even to recognise the juxtaposition of incompatible notions; which can show no better front to the world than a thin tissue of rhetoric or metaphor made to do duty as scientific authority; which shelters itself, whenever it can, behind the establishment by law of its own doctrine, deliberately left undefined and unformulated?

CHAPTER XIII.

ESTABLISHMENT AMIDST DISSENT.

THE first antivaccinist, and one of the most resolute, was Dr. Benjamin Moseley, a physician of wit and shrewdness, with a large practice among the upper classes in St. James's. He had practised for a number of years in Jamaica, had done valuable service in the military operations as principal medical officer of the colony, and had published a standard work on Tropical Diseases and the Climate of the West Indies (three editions), as well as a treatise on Coffee (five editions). On his return from Jamaica, he spent several years in visiting the great Continental schools,¹ and on settling in London had been appointed by Secretary Grenville to the coveted office of physician to Chelsea Hospital, which he filled for thirty years "with the greatest *éclat*."²

When Jenner's *Inquiry* was beginning to be talked of in the autumn of 1798, Moseley was on the point of publishing a historical and practical essay on Sugar, together with some West Indian odds and ends, such as an account of the Obi of the negroes, and a narrative

¹ *Gent. Magaz.*, 1790, p. 10.

² Munk's *Roll of the College of Physicians*, 2nd ed., vol. ii. 368.

of the last stand and overthrow of Three-fingered Jack, the famous negro outlaw of Jamaica, whose Obi-bag he had obtained possession of. Along with these miscellanies he introduced a few remarks on Jenner's novel doctrine of cowpox, which appear to have been written in September, 1798. At that time none of the journals of the profession had spoken; but Jenner had been in London all the summer ventilating his project among his friends, and Pearson, a colleague of Moseley's in west-end practice, had been taking it up in the most serious way, and had by his correspondence stimulated curiosity about it, if not even enthusiasm for it.

Moseley's remarks on the latest medical novelty are a curious mixture of jesting and good sense. The appearance of the *Inquiry* is spoken of as a portent in the heavens, the significance of which was not altogether clear: "Some pretend that a restive, greasy-heeled horse will kick down all the old gally-pots of Galen. . . . To preserve, as far as in me lies, the genesis of this desirable, this excelling distemper to posterity, I mention that it is said to originate in what is called the *greasy-heel* distemper in horses. . . . The virtues of this charming distemper are said to be an amulet against the smallpox. . . . In this *cow-mania* it is not enough for reason to concede that the cowpox may lessen, *for a time*, the disposition in the habit to receive the infection of the smallpox; all cutaneous determinations, catarrhal fevers, and every disease of the lymphatics do the same. . . . The smallpox and the cowpox are not analogous, but radically dissimilar. . . . Can any person say what may be the consequences of introducing the *lues bovilla*, a bestial humour,

into the human frame after a long lapse of years? . . . The doctrine of engrafting distempers is not yet comprehended by the wisest men ; and I wish to arrest the hurry of public credulity until the subject has undergone a deep, calm, and dispassionate scrutiny ; and to guard parents against suffering their children becoming victims to experiment."

The effect of this sensible line of remark was somewhat marred by a few pleasantries or extravagances about the human form becoming assimilated to that of an ox ; these conceits had merely been suggested by something from Ovid which had come into his head ; but the Jennerians took them very seriously, and kept quoting them for many years as examples of the nonsensical opposition with which a great discovery had been received.

Moseley seems to have really expected that his criticism would arrest the hurry of credulity. He had made the mistake, however, of forming an *a priori* judgment, and had so put himself out of court in the estimation of all stolid Englishmen. Eight years after, when a good deal of experience had been gained, a writer in the *Edinburgh Review* expressed surprise that Moseley, in 1798, should have declared against cowpoxing "on the basis of theory," although at that time "he had neither read nor seen anything that was not decidedly in its favour."¹ To this Moseley replied : "It must indeed seem supernatural to ignorant people that I should, solely on the ground of analogy and pathology, have produced a publication foretelling all the horrid

¹ *Edinburgh Review*. October, 18c6, p. 42.

events which have since taken place.”¹ He had made up his mind upon a scientific book after reading it between the lines; he had judged it just as if it had been open to scrutiny like a business project, or to criticism like a literary production, putting his foot down and calling out, as if he had been Dr. Johnson, “The thing is a fraud, and there’s an end on’t.” He had treated Jenner’s monstrous grease-of-horse and pox-of-cow amulet with no more scientific forbearance than if it had been the Obi of Three-fingered Jack, which he described in the same volume (the end of a goat’s horn filled with a paste made of the blood of a black cat, human fat, grave dirt, etc.). He took no account of Jenner’s being a fellow of the Royal Society.

Moseley did no more than give expression to the first thoughts of a good many people in London when they heard of the cowpoxing. On the 13th of November, 1798, Dr. Pearson, who was prepossessed in its favour, wrote to Jenner :²—

“You cannot imagine how fastidious the people are with regard to this business of the cowpox. One says it is very filthy and nasty to derive it from the sore heel of horses. Another—O my God, we shall introduce the diseases of animals among us, and we have too many already of our own! A third sapient set say it is a strange, odd kind of business, and they know not what to think of it.”

Dr. Moseley was in a good position for giving currency

¹ *An Oliver for a Rowland* (reply to Rev. Rowland Hill). 10th ed., London, 1807, p. 58.

² *Baron*, i. 305.

to these aspects of the new nostrum. By one means or another during the next two years he disposed of two or three editions of the volume of essays which contained his cowpox paper; and some time after he expanded the latter into a considerable volume under the title of a *Treatise on the Lues Bovilla*,¹ illustrated by cases of "bad arms," so as to enforce the luetic nature of the disease. Having a large connexion in literary and political circles, he found many opportunities of exercising his wit at the expense of the Jennerians. Among his patients was Charles James Fox, who was in the way of encountering Jenner at Cheltenham. Mr. Fox, it appears, had been "poisoned" by Moseley against the pleasing doctrine of cowpox which Jenner had invented, and took occasion to quiz the vainglorious discoverer. "Pray, Dr. Jenner," he said, "tell me of this cowpox that we have heard so much about. What is it like?" Jenner answered, in his favourite figure, that it was like "a pearl upon a rose leaf;" whereat the statesman laughed heartily and praised the simile.²

Moseley was almost the only medical man during the first two or three years who came forward publicly as an uncompromising opponent. We read also in a letter³ of Jenner's, dated 15th July, 1800, that "a man of the name of Brown has made a variety of efforts to write it down; but finding himself deserted by every medical man of respectability, he shot himself a few days ago." Two others besides Moseley were called as adverse wit-

¹ 2nd ed., 1805. Munk gives the date of the first edition as 1801, but it is also assigned to 1804.

² Baron, ii. 305.

³ Jenner to Rev. John Clinch, of Newfoundland. Baron, ii. 324.

nesses before Admiral Berkeley's Committee in May, 1802,—John Birch, surgeon to St. Thomas's Hospital, and Dr. Rowley, a voluminous writer of semi-popular books who had a large following among the public. But neither of these wrote against vaccination until a later date ; and their evidence before the Committee, so far as it is reported, was not weighty. The parliamentary vote of ten thousand pounds in 1802 had the effect of stopping all cavilling for a time, so that there came about that appearance of general assent which I have spoken of in a former chapter. Beddoes, at one time an opponent, was the first to write that the vote was too little, and to suggest a national subscription.¹ Cooke, who put on record some very damning evidence at the outset, had previously written to say that he "had opposed the practice with more zeal than prudence."²

Apart from a remonstrance by Cobbett in the *Political Register* in 1803, addressed to Wilberforce, who was moving to get variolous inoculation put down by law and cowpoxing substituted for it, the controversy slumbered until the spring of 1804. The first enthusiasm for the new protective had died away ; Jenner's attempt to establish himself in consulting practice in Hertford Street, Mayfair, had been a disastrous failure, very few seeking to employ him as a vaccinator. Both the profession and the public were in a cooler mood. The more fanatical Jennerians were sanguine, after the vote in Parliament and the enthusiastic testimonies from abroad, that smallpox would soon be exterminated.

¹ *Med. and Phys. Journ.*, viii. 7 (4th June, 1802).

² *Ibid.*, 29th May, 1800.

About the year 1803 they were talking actually of burning down the London Smallpox Hospital, or of selling it for another use.¹ They knew so little of the very rudiments of epidemiology, or had so lost their heads, that they mistook one of the ordinary lulls of epidemic smallpox for its total disappearance before the cowpox protective, which had been applied to a mere handful of the infancy and childhood of the country. But it would be a mistake to ascribe these extravagant enthusiasms to more than the immediate following of Jenner. In the profession at large the craze was over; and the outbreak of a new epidemic of smallpox in 1804 gave an opportunity to the more candid and independent medical men to apply to the evidence that reasonable and common-sense scrutiny of which we find hardly any trace in their first reception of it.

The epidemic of 1804-5 was severely felt both in London and in various parts of the country, including Wales and Scotland. The Smallpox Hospital, happily preserved from demolition, soon filled with patients and continued full for months. The sprinkling of vaccinated children in the population at large were now for the first time (in England, at least) subjected to the real trial of epidemic contagion. The result must have been the same that has often been experienced and accurately recorded on a larger scale in later times; but for that epidemic, there is little record of it left beyond the evidence that widespread doubt and delusion as to the new protective had arisen in the professional mind. The publication of six cases at Portsmouth set the

¹ H. Fraser, *Med. and Phys. Journ.*, 1805, p. 33.

whole controversy once more in a blaze, producing an effect which can only be accounted for by supposing that many more had the same experience, although they said nothing. Those who did send cases to the medical journals in the next few months were two or three well-known Jennerians, who knew how to account for the failures.

The cases read and discussed before the Medical Society of Portsmouth,¹ on 29th March, 1804, were not different from hundreds that had occurred in 1799 and 1800, either in England or on the Continent; only the enthusiasm was now over, and reason once more held sway. Four of the cases were common instances of the variolous test producing the restricted effects of inoculated smallpox, on being applied a year or two after vaccination. These cases had been in Mr. Goldson's possession for two years, and he had actually sent up one of them to Admiral Berkeley's Committee in 1802.

It was in March, 1804, that he was startled into farther reflection and decisive action. He was called to see a child, vaccinated by himself a year or two before, and found it sickening for some kind of eruptive fever. The illness proved to be smallpox, and Goldson at once invited the leading practitioners of the locality, including the surgeons of the Navy at Haslar Hospital, to satisfy themselves by inspection of the child and by inoculating with the matter. A similar case occurred in his practice within a week or two of the other. A very full meeting of the Portsmouth Medical Society was held on the

¹ *Cases of Smallpox subsequent to Vaccination.* By William Goldson, M.R.C.S. Portsea, 1804.

29th March, which was prominently noticed in the local newspaper of the 2nd April, with the further announcement that Goldson would shortly publish the affair. A copy of the newspaper was sent to Jenner, who wrote to Dunning (of Plymouth Dock):¹ "What a set of block-heads! How will our Continental neighbours laugh!"

Goldson advertised his forthcoming pamphlet in the *Medical Journal* (and probably elsewhere) under a title that was then considered alarming, "Cases of Smallpox Subsequent to Vaccination"; Jenner declared that the advertisement was infinitely worse than the book, and called it the "murderous harbinger." The book caused great excitement, and produced an effect ludicrously disproportionate to anything either novel or weighty that it contained. It is hardly surprising that Jenner's first feeling should have been, "What a set of block-heads!" All this pother was about four failures of the variolous test, and about two vaccinated children who had taken the smallpox in the natural way. Why, the old volumes of the *Medical Journal* contained scores of cases of both kinds, while the foreign journals contained accounts of whole epidemics among the vaccinated. But it makes all the difference whether these things happen during the hot fit or during the cold.

Goldson's pamphlet appeared in June, and the *Medical and Physical Journal* published a long abstract and review of it in the number for July. The author's concluding sentence had evidently touched the editor to the quick: "To suffer zeal for the discovery, to shut their eyes to conviction, and, by deeming every failure

¹ Letter of 5th April, 1804, Baron, ii. 337.

spurious, to conceal it, is beneath the dignity of the profession." The tone of the review is most respectful, and gave great offence to Jenner, who wrote to Dunning, on the 22nd of the same month: "I am sorry to say I cannot send you advertisements to the cover of the *Medical Journal*. The review of G.'s book will tell you I have no interest there." The reviewer had said: "The entire pamphlet claims an attentive perusal from all partisans, friends, and well-wishers of Dr. Jenner's discovery. . . . The objections of Mr. Goldson, if valid, would lead to the entire abolition of vaccine inoculation from the human subject. . . . The author, aware of the permanent value of vaccination *immediately from the cow*, makes an exception to this species of cowpox." The italics are in the original, and they must have given Jenner a cold shiver when he saw them.

The controversy was taken up by Jenner's ever-zealous henchman, John Ring, who published an answer¹ in July, dealing primarily with Goldson, and at the same time accusing the *Medical and Physical Journal*, on account of its full analysis and respectful criticism in the July number, of "prostituting its pages for the purposes of a party." Ring's treatment of Goldson, who was highly respected at Portsmouth, and was known in the service for his book on maritime discoveries, produced general indignation throughout the profession; Jenner himself thus wrote of it to Dunning:² "Ring, the moment he read Goldson's book, instantly charged his

¹ *An Answer to Mr. Goldson, proving that Vaccination is a Permanent Security*. London, 1804.

² 23rd Dec., 1804, Baron, ii. 25.

blunderbuss and fired it in the face of the author." The picture would be complete if it showed Jenner sympathetically watching the highway ruffian from behind a hedge. Both Goldson and the *Medical Journal* were coerced. The effect of Ring's bullying becomes abundantly evident in the successive numbers of the journal, which he dominated for some time after so far as concerned the kind of vaccination papers that were suffered to appear in it.¹ The editor deplored, indeed, the roughness with which Goldson had been handled; while Goldson himself, in a second edition, showed a forgiving and meek spirit. "Our readers," the reviewer again wrote, "will perceive with pleasure a prospect of reconciling Mr. Goldson to vaccination."²

The effects among the profession at large, and among the public, were more lasting. Letters appeared in the *Times*, *Morning Chronicle*, *Sun*, and other newspapers, and in magazines. Jenner wrote to Dunning: "Goldson's book has sent many a victim to a premature grave"; and again, "Never mind; you will hear enough of smallpox after cowpox. It must be so. Every bungling vaccinist [no word now of the ladies and the clergymen who had vaccinated their thousands with his cordial approval] who excites a pustule on the arm will swear, like G., it was correct, without knowing the nicety of distinction which every man ought to know before he takes up the vaccine lancet." The plates to

¹ In 1814 Ring complains that the *Med. and Phys. Journ.* was not staunch enough to the Jennerian cause. He afterwards wrote in the *Medical Repository*, which was started in that year.

² *Med. and Phys. Journ.*, xiii. (1805), p. 268.

show these nice distinctions, which were loudly demanded by the profession and were publicly declared to be under hand by an engraver who was called before Admiral Berkeley's Committee to speak to the point, were never published, for the sufficient reason that "spurious" vaccine was anything one pleased. To Dunning, again, he writes: "Vaccination never stood on more lofty ground than at present. I know very well the opinion of the wise and great upon it, and the foolish and the little I don't care a straw for. Why should we fix our eyes on this spot only? Let them range the world over. . . . *There* I have honour, *here* I have none." Truly the eyes of a fool are in the ends of the earth.

Even Dunning himself was shaken in his faith. He had written that the Portsmouth cases had an "ugly look," a very natural view for him to take, considering that his logical statement of the case, when general assent was reached in 1802, had been: "The genuine vaccine lymph does or does not possess an absolute preventive power against variolous contagion. Such power is or is not a law of Nature. The protection, if it affords protection, cannot be casual, it must be regular and determined."¹ Those who wish to understand the mixture of bullying and wheedling which always characterized Jenner's conduct of his business will find a fair sample of it in his letters to Dunning, while the Portsmouth affair was troubling the mind of his faithful correspondent at Plymouth Dock.

The shock to the credit of cowpoxing in 1804 was

¹ *Med. and Phys. Journ.*, vii. (1802), p. 3.

promptly followed up by a whole series of attacks from the side of the old inoculation party, with whom Moseley was now identified. During the next year or two, Birch, Rowley, Squirrell, and Lipscomb published their books and pamphlets; while Moseley brought out a new edition of the *Lues Bovilla*, and a volume of commentaries, to which Sutton and others contributed cases of smallpox occurring after vaccination, either through contagion or by inoculation. Goldson's book was thus the signal for a much more determined opposition than anything that cowpoxing had called forth in the first years of its trial.

Jenner was equal to the occasion. Although his attempt to establish himself in practice in Mayfair had been a failure, yet he was able to say: "I know very well the opinion of the wise and the great upon it;" and to the wise and the great he now turned. One of his patrons was Lady Crewe, who got Lord Henry Petty (afterwards Marquis of Lansdowne) to meet Jenner at her Hampstead villa, in the summer of 1805.¹ The result of this conference was that "his lordship resolved to bring something forward in the ensuing session." Jenner again saw Lord Henry in the early part of 1806, and found that "his ardour in my cause had suffered no abatement." On the 2nd of July, Lord Henry, who had meanwhile become Chancellor of the Exchequer on the death of Pitt, moved an address to the King "that his Royal College of Physicians be requested to inquire into the progress of vaccine inoculation, and to assign the causes of its success having been retarded throughout the United Kingdom." He took occasion at the same

¹ Baron, ii. 55

time to express his strong conviction that the report of the College would be corroborative, which was exceedingly probable, considering how the leaders had nearly all committed themselves by their evidence in 1802.

The appeal to medical authority, as represented in its most dignified form by the College of Physicians, was the turning-point in the vaccination controversy. All that was academical and respectable was henceforth ranged on one side, against all the free-lances, lay or medical, on the other. The columns of the established medical journals were now less open to adverse facts and reasonings. In 1806¹ a new serial was started, called the *Medical Observer; or, London Monthly Compendium of Medical Transactions*, by a society of practical physicians, which became identified with the opposition to cowpoxing, and carried on the contest until 1811, if not longer.² The opposition was naturally most active in the metropolis. "It is about London," Jenner wrote on 21st February, 1806, "that the venom of these deadly serpents chiefly flows."³

The College of Physicians set to work to collect evidence on the benefits of vaccination, calling in the aid of the College of Surgeons, and of the medical corporation in Edinburgh and Dublin. There was a small show

¹ The first number was on "Advertised or Empirical Medicines," 1806; title in Watts' *Bibliography*.

² It is perhaps evidence of its want of repute that no volumes of it have found their way into the library of the Medical and Chirurgical Society, or of the College of Surgeons, or of the British Museum.

³ Letter to Dunning, in *Baron*, ii.

of adverse facts, but these were counterbalanced by the "respectability," as the report has it, of the testimonies in favour of vaccine. Jenner himself appeared before the College committee on the 19th of February, 1807, with a bundle of foreign diplomas and honours, beginning with that of the Göttingen Academy of Sciences, in 1801, which had been granted under the circumstances noticed in chapter ix. The report had to take notice, in common fairness, of the adverse evidence ; and it stated plainly that "the public had been misled" by Jenner's famous doctrine of spurious cowpox in the cow, "as if there were a true and a false cowpox." But they were too late ; the mischief had been done. They forgot that the whole of the early adverse evidence, which ought to have stopped the delusion at the outset, had been overruled and explained away on that very plea, as I have shown in previous chapters. The report concluded that "the security derived from vaccination, if not absolutely perfect, is as nearly so as can perhaps be expected from any human discovery."

This report was issued on the 10th of April, 1807, and was signed by Sir Lucas Pepys, the president of the College. On the 16th of May, Jenner wrote from Bedford Place, London : "I have just received a note from the president, Sir Lucas Pepys, requesting me to vaccinate his little grandson. Two years ago the worthy president would as soon have had the boy's skin touched with the fang of a viper as the vaccine lancet. But this *inter nos*." ¹ As this worthy person did more than any one to get vaccination established, and most of all to

¹ Letter to Dunning, Baron, ii. 357.

get it endowed by the State, it will be necessary to say a few words about him.

Dr. Pepys, having made a success as a young man in fashionable practice at Brighton, and married a lady of title (Countess of Rothes), was called to attend King George III. in his severe illness of 1788 and 1789. For his services on that occasion he was appointed in 1792 physician-in-ordinary to the King, and promised the office of physician-general to the army when that office should fall in, which it did in 1794. In the latter year the army medical board was started, consisting of the surgeon-general, the inspector-general, and Sir Lucas as president. In that capacity he exercised much patronage and authority, having in his gift the appointment of all the physicians to the forces. Sir Lucas made his appointments from the ranks of civil life, without regard to previous service in the army, but with a strict regard to the privileges or monopoly of the Royal College of Physicians. The army medical board, which had already lost the confidence of all who knew anything of medicine and surgery in the field,¹ at length collapsed, on the disgraceful state of sickness among the troops in Walcheren becoming known. Sir Lucas was ordered to proceed to Walcheren, but boldly declined, on the ground that he "was not acquainted with the diseases of soldiers in camp or in quarters." It was difficult to retain his services after that; but a grateful country softened his dismissal by a liberal pension, which he enjoyed to the

¹ See the pamphlets by McGrigor and Jackson, Scottish graduates, on the one side, and Bancroft, a creature of the College, on the other in 1808.

ripe age of eighty-eight. He was a person of great firmness and determination, "somewhat dictatorial in his bearing, and formed to command."¹ He contributed nothing to the literature of his profession, except a preface to a drug-book.

This was the estimable public servant who presided over the deliberations of the College of Physicians when Jenner made his appeal to academical authority. Sir Lucas Pepys may have had his little hesitations about Jenner and his cowpox; but it was another thing altogether when Lord Henry Petty moved the Crown to invite the College over which Pepys presided to deliver judgment. The practical part of the business behind the scenes was still more congenial to Sir Lucas's tastes. Jenner was to have ten thousand pounds additional voted to him (amended in the House of Commons to twenty thousand), and vaccination was to be endowed with an annual vote of at least three thousand pounds, the patronage to be vested in the College of Physicians and (in a minor degree) in the College of Surgeons.

The vote to Jenner was moved by the Chancellor of the Exchequer (Spencer Perceval) on the 29th of July, 1807, and carried as amended to twenty thousand pounds. That was considered enough for one year, more especially as the populace were in a malcontent mood. John Gale Jones, a radical leader, and himself a medical man, "had the impudence," as Jenner wrote,² "to desire a man to call on me in Bedford Place, to say that he, Jones, would advise me immediately to quit London, for there

¹ Munk's *Roll of the College of Physicians*, 2nd ed., ii. 305.

² Letter to Moore, 26th Feb., 1810, in Baron, ii. 367.

was no knowing what an enraged populace might do." The opposition was now at its height outside the academical circle, and was diligently encouraged by the inoculators to serve their own ends.

Sir Lucas Pepys held over until next session the rest of his practical proposals for rescuing vaccination from its difficulties. Meanwhile, he set Mr. George Rose, Treasurer to the Navy, who must have known all about places and patronage,¹ to prepare the way for the scheme of endowing a number of administrative and executive vaccination offices. Rose wrote to Jenner in the winter and asked him to draw up a plan, with an estimate of the annual cost; Jenner in due course sent the plan to London, and followed in person to see matters through. He spent five months in town on the second of two visits for that purpose, and had interviews with Rose and Pepys.² His advice had been politely asked, but it was not followed.

The scheme, as proposed by Rose to the House of Commons on the 9th of June, 1808, was for a National Vaccine Establishment, to be administered by the College of Physicians and the College of Surgeons. The proposal gave rise to a debate, in which the appeal to constituted medical authority carried the day, as it always does, sixty voting for the Establishment and five against.

The most notable speech was made by Sir Francis Burdett, who characterized vaccination as "a failing experiment," and warned the House not to "prop up

¹ See *The Works of Rev. Sydney Smith*, popular ed., pp. 173, 231.

² Baron, ii. 117.

what might prove to be pernicious error.”¹ Cobbett, who must have known something of cowpox in the country, and believed the Jennerian doctrine to be pernicious error, protested strongly, in the *Political Register* of 18th June, against this interference of authority in a matter which ought to be left to the common sense of the country.

This new business, quietly arranged by official persons with that belief in themselves and in each other which their position creates, roused the opponents of cowpoxing to more strenuous efforts. The walls of London were placarded, says Baron, with falsehoods; “and doubtless many a victim perished at the shrine of this Moloch.” The opposition had become so inveterate, and was so inexplicable to that historian on any ground of reason, that he is “compelled to believe there is a principle in our nature which has too strong an affinity for what is untrue.” The columns of the *Independent Whig* contained long letters by anti-vaccinists; a debate on the question was protracted for several nights at the Westminster Forum; and a new

¹ These are the words as given by Baron. In the *Parliamentary Debates*, Sir Francis is reported to have said: “There was some danger that we might be fostering a very fatal mistake. Before tying the House down by a resolution, it would be well to appoint a committee to inquire into the efficacy of vaccination.” Lord Henry Petty commended the proposed establishment for its “investigating” function; it was “highly proper that investigation should be made under the eyes of the public.” Mr. Secretary Canning “could not figure any circumstances whatever that could induce him to follow up the most favourable report of its infallibility, which might be brought forward, with any measure of a compulsory nature.”

journal, called the *Cowpox Chronicle, or Medical Reporter*, was started, and distributed through the post. But the year 1808 was not a favourable time for the instinctive dislikes of the people being skilfully diagnosed and rationally treated ; the more heroic methods of driving discontent below the surface were still in vogue.

Having received the warrant for the National Vaccine Establishment in October, Sir Lucas Pepys set to work to constitute it. There was to be a Vaccine Board of eight, composed of himself as president, with the four censors of the College of Physicians, and the master and two senior wardens of the Corporation of Surgeons, each at an annual salary of a hundred pounds. The ostensible ground of this corporation job was that there was to be instituted "a full and satisfactory investigation of the benefits or dangers of the vaccine practice." Jenner was excluded from an active share in the work for the obvious reason that he was incapable of the judicial temper. However, it was arranged that he should be named director. He fought hard to retain his hold against the usurpation of the two medical corporations, and brought evidence from Paris that the corresponding administration by the Faculty there was unpaid. Sir Lucas Pepys reassured him, "You, sir, are to be whole and sole director. We are to be considered as nothing ; what do *we* know of vaccination?" But, when the working or executive offices came to be filled up, Jenner's nominees were nearly all set aside, and he resigned the office of director. Sir Thomas Bernard wrote to him on 6th March, 1809 : "From some circumstances which came to my knowledge in November, I guessed that the new Board was to be made an

instrument of patronage; I therefore did not argue well of the result.”¹ The best-paid official was called the registrar; and Dr. Hervey, the registrar of the College of Physicians, physician to Guy’s Hospital, was appointed to the office. The Vaccine Board was one of the scandals investigated on the motion of Joseph Hume in 1827, and was farther reduced by the reformed parliament in 1833; the select committee found that the members had attended casually, and had left the work of “investigation” in the hands of the Executive. For the first two years the vaccinations done by it in London were at the rate of two pounds a head. Walker’s institution, supported by voluntary contributions, did most of the vaccinating that was done.

The National Vaccine Establishment, although Jenner was excluded from it, was really the best defence of his “failing experiment” that could have been devised. From the day of its starting, it was never anything but an instrument of thorough-going vaccination apologetics. In 1811 a new epidemic of smallpox brought the question again prominently under public scrutiny, and fashionable society was startled by the case of the Hon. Robert Grosvenor, son of Earl Grosvenor, who acquired confluent smallpox, although he had received the vaccine protection as an infant in 1801 from Jenner’s own hands. As Jenner truly said, this famous case was “a speck, a mere speck on the page which contains the history of vaccine discovery”; but the page was getting a good deal speckled over and obscured, witness the numerous cases published in 1809

¹ Baron, ii. 130.

by Thomas Brown, of Musselburgh.¹ The Vaccine Establishment issued a special report on the Grosvenor case, of a reassuring tenour; the boy would have died outright had he not been vaccinated, despite the best skill of Sir Henry Halford and Sir Walter Farquhar.

The attacks of the anti-vaccinists became so resolute in 1811 that Jenner was seriously urged to institute an action for libel. Among those who rallied to his support was Samuel Taylor Coleridge, who wrote² to him from Hammersmith on 27th September, 1811, that he purposed inserting in the *Courier* a series of papers on the inception of the cowpoxing idea in Jenner's mind and its establishment as a great truth. He added: "The only painful thought that will mingle with the pleasure with which I shall write them is that it should at this day, and in this the native country of the discoverer and the discovery, be even *expedient* to write at all on the subject." He announced also that, after long thinking over it, he had planned to write a poem on Vaccination, as being a subject well suited to exemplify Milton's canon that poetry should be simple, sensuous, and impassioned. It would have been interesting to see wherein a poem on Cowpox would differ from the old prize poem, criticised by Coleridge, which began, "Inoculation, heavenly Maid!" But neither the papers in the *Courier* nor the projected poem were ever published. It was of more use to Jenner to be able to inform the world that he had been chosen, on the 13th May, 1811, a foreign associate

¹ *Inquiry into the Antivariolous Power of Vaccination.* Edinburgh, 1809.

² Baron, ii. 175.

of the Institute of France, on the occasion of the vaccine protective being administered to the King of Rome.

Although vaccination had now a powerful corporation interest behind it, its public credit was much impaired, and it received no very hearty support from the profession outside the circle of officials. Even Pearson, one of its earliest and most enthusiastic votaries, would seem to have lost faith in it, if we may trust a letter of Jenner's (18th November, 1812), in which he speaks of Pearson's "insinuations that vaccination is good for nothing."¹ Woodville, the real author of vaccination practice, had made no public defence of it after the first years, and had subsequently carried on variolous inoculation side by side with it at his hospital; he died on 26th March, 1805, and as he was an honest man, we may say of him that he was taken away from the evil to come.

The old variolous inoculation had revived so much that Lord Boringdon, at the instance of the Vaccine Board, brought a bill into the House of Lords, in 1813, to restrict the practice to secluded areas (the law in Vienna since last century), and to get vaccination substituted for it among the poor. The bill was successfully opposed by Lord Chancellor Eldon, and by Chief Justice Lord Ellenborough,—by the latter on the ground that the common law was able to deal with smallpox inoculation as a nuisance and public danger, and that the bill was narrower in its operation than the common law. Lord Ellenborough took occasion to say that vaccination did not merit the high encomiums

¹ Baron, ii. 383.

passed upon it, and that he did not believe the protection to be lasting, although he believed it was a good thing.¹ This was the blow that Jenner felt most keenly. According to Baron, he was greatly annoyed. In a letter of that year he writes :² "And if the first Lord in Parliament should offer to degrade vaccination by uttering an untruth, as one of these dignified personages lately did," he would still, etc. The biographer couples Ellenborough's qualified approval with the popular prejudices of the day, and observes that the anti-vaccinists must have been proud of the Chief Justice's co-operation.

On the 23rd June, 1814, Lord Boringdon brought in a new bill, with clauses for the compulsory notification of smallpox, and, in effect, for the compulsory vaccination of the poor. He accused Lord Ellenborough of having excited an injurious degree of alarm in the public mind, declared the assertion of mere temporary protection to be erroneous, and an error that they ought to do everything to counteract. The bill went through committee; but, on the report, it was vigorously opposed by Lords Stanhope, Mulgrave, and Redesdale, and was withdrawn. Lord Stanhope ridiculed it, and said that, if passed, it would prove "one of the most troublesome, inconvenient, and mischievous measures ever enacted." Lord Mulgrave said: "If their lordships recollected how many persons of the higher order were reluctant to introduce vaccination into their families, it really must appear to them a harsh and arbitrary

¹ Parliamentary Debates, House of Lords, 30th June, 1814.

² Letter to Moore, 27th Oct., 1813, in Baron, ii. 389.

measure to lay the poor under the necessity of adopting the practice." Lord Redesdale thought that, if vaccination deserved to be established, it would establish itself by its own merits.¹

Another severe epidemic of smallpox in 1817, 1818, and 1819, extending to many places in England and Scotland as well as on the Continent, made the Jennerian cause to look more hopeless than ever. This was the first occasion on which medical opinion abroad showed signs of wavering. In Scotland, according to Dr. John Thomson,² more of the vaccinated than of the unvaccinated were attacked by the epidemic; but that circumstance, unpromising though it looked, was made to serve the glory of vaccination. The epidemic of smallpox had a distinctive type, as epidemics of other diseases besides smallpox are apt to have from time to time; students of Sydenham will find numerous instances of the type being modified from season to season, while Häser's volume on the *History of Epidemic Diseases*,³ abounds in illustrations of that familiar fact in the natural history of disease. The type in the Scotch epidemic of 1818-19 was not new in the history of smallpox; it corresponded closely to the variety mentioned by Adams in 1795 under the name of "pearly" smallpox, and it was by no means unfamiliar in pre-vaccination times. This was the prevailing type of the eruption in the epidemic in Scotland, both among

¹ Parliamentary Debates, House of Lords, 8th July, 1814.

² *Account of the Varioloid Epidemic in Scotland, with Observations on the Identity of Chicken Pox with Modified Smallpox.* Edinburgh, 1820.

³ Vol. iii. of his *Geschichte der Medicin*, 3rd ed. Jena, 1882.

the vaccinated and the unvaccinated. But, whether from forgetfulness of the old types assumed by smallpox, or from inability to reason correctly upon the facts, or from an overmastering desire to find excuses for the cowpox protective, the variety of the disease was now described as a modification due to the previous cowpoxing of the constitution. Cowpox, it was argued, does not indeed prevent smallpox, but it modifies its type; witness this whole epidemic, in which the eruption is less purulent, harder, and more pearly.

This was the real origin of the famous doctrine of smallpox "modified," if not averted, by vaccination, which is a favourite apologetic plea of our own time. Modified smallpox, or varioloid, or "varicella" in the sense of the Vienna school, is merely mild or discrete smallpox, usually of the common pustular type, which was as frequent in the days before cowpoxing was practised as it has ever been since. Vaccination in 1818 stood in great need of some excuse for failure; hence the ingenious doctrinal fiction of "modified" smallpox. Cobbett, in his *Advice to Young Men*, speaking with the freedom of a layman, said of this new development: "Quackery has always a shuffle left. Now that cowpox has been proved to be no guarantee against smallpox, it makes it *milder* when it comes. A pretty shuffle, indeed, this!"

CHAPTER XIV.

COMPULSION.

THE epidemic of 1817-18 marked the moment of greatest hesitation that the profession has ever publicly owned since cowpoxing was first assented to. Baron says that "professional gentlemen of some name took up the opinion of the anti-vaccinists." Perhaps the most pathetic note comes from Jenner's own district. His old friend, Gardner, who had been in his confidence in the early days, wrote to him from Frampton-on-Severn, 21st May, 1817:—

"From some unaccountable causes the fame of vaccination seems to decline in this part of the country; I find my offers of gratuitous service very frequently rejected even by those whose former children have undergone the operation."

The profession seemed inclined for a moment to agree with the common people in suspecting that there was something radically wrong in Jenner's teaching. In July, 1817, a medical journal in London wrote: "However painful, yet it is a duty we owe to the public and the profession, to apprise them that the number of all ranks suffering under smallpox, who have previously undergone vaccination by the most skilful practitioners, is at present surprisingly great. The subject is so

serious, and so deeply involves the dearest interests of humanity, as well as those of the medical character, that we shall not fail in directing our utmost attention to it." ¹

Unhappily the dearest interests of humanity had to give way before the dearest interests of the medical character. The credit of the profession was at stake. A surrender in Jenner's lifetime would have been too humiliating, seeing that Parliament had been induced to vote him £10,000 in 1802, and £20,000 in 1807, upon the warrant of medical evidence. Again, it was unfortunate that there should be five seats at the Vaccine Board, worth a hundred pounds each, for the College of Physicians, and three seats for the College of Surgeons; the president and four censors of the one College, and the president and two senior councillors of the other, would have had to exercise some ingenuity to prevent these plums from dropping into their mouths. The assent of these official personages in succession was assured, in the very terms of the arrangement that Sir Lucas Pepys had made; their assent meant the assent of their respective corporations; ² and the assent of

¹ *London Medical Repository*, July, 1817 (edited by G. M. Burrows and A. Todd Thomson).

² An instance of faltering conformity has been pointed out to me by a literary friend: Dr. John Johnstone, a fellow of the College of Physicians and of the Royal Society, edited, in 1828, the *Works of Samuel Parr, LL.D.*, with a Memoir. In the latter (i. 649) he prints a satirical paper by Mrs. Wynne, Parr's daughter, addressed to the Committee of Vaccination at Warwick, upon the discovery of *ass-pox* in a boy at Westminster School, and on the successful inoculation of *Zebrine* in many more, who had all stood the small-pox test eighteen, twenty, and even forty times. Dr. Johnstone

the two great medical guilds of England meant the assent of the whole English profession.

It was the more unfortunate that these golden chains, slight though they were, should ever have been imposed, because the medical leaders in London had come to form a tolerably accurate personal estimate of Jenner, and might have come in course of time to form an equally accurate estimate of his cowpox doctrine. It was an open secret in the profession that the great discoverer was a disappointing person at close quarters. He was vain, petulant, crafty, and greedy; he had more of grandiloquence and bounce than of solid attainments. In London, at least, his presence was a bore, and his reputation an incubus, which the profession, outside his own small following, would have gladly got rid of. Having come to town for the last time in the spring of 1814, he wrote to Baron:¹ "I am quite sick of the life I lead here;" but he remained for several weeks longer, in order to be presented to the Allied Sovereigns, in the hope that they would, either singly or conjointly, do something for him. The minute record of his interviews with these august persons, which a literary neighbour drew up for him and published during his lifetime,² would of itself enable us to understand why Jenner was held in small esteem in

says the paper had been "falsely attributed to him by the ill-natured sagacity of some persons." He adheres to vaccination in a curiously guarded way, and sincerely hopes that "time will set the lasting stamp of benefit upon the *experiment*, for such it is."

¹ *Life of Jenner*, ii. 206.

² *The Berkeley Manuscripts*, etc. By Rev. T. D. Fosbroke. Lond., 1821, p. 236.

professional circles, even if that were not intelligible on other grounds. It was only after several applications that the University of Oxford gave him the honorary degree of Doctor of Medicine in 1813; while the College of Physicians refused to the last, even when he brought his Oxford diploma with him as a passport, to admit him to its fellowship on the same terms.

But the whole course of events had helped to place this Old Man of the Sea on the back of the profession. First, his Royal Society credentials; then his support by men of credit like Cline, Pearson, and Woodville; then the powerful interest of the county of Gloucester in Parliament, and of the Berkeley family in particular; then the dexterous appeal to the College of Physicians, and to its old love of authority; then the inevitable placemen's job of the Vaccine Board whenever Sir Lucas Pepys had the chance given him. Those who discover in all this the legitimate exercise of professional, or expert, or scientific authority, can only do so by closing their eyes to the purely mundane and sordid side of the history. The medical profession itself, about the year 1818, was not far from handsomely owning that it had made a mistake. But for the establishment and endowment of the Vaccine Board, and the inertia of corporation interests thereby brought to bear, it is highly probable that such an acknowledgment would have been made.

It was unfortunate, also, that no alternative for the management of smallpox epidemics was then in sight, except a return to variolous inoculation. From the first the anti-vaccinist cause had been far too much the cause of those committed to the old inoculation. The more

that cowpoxing was seen to fail and to do harm, the more did variolation revive. There is abundant evidence in those years that variolators were doing a good business, and that the practice had passed more than ever into irregular hands. In the disastrous epidemic among children at Norwich in 1819, which was due to overcrowding of the town by a great influx of families from the country while trade was brisk, the failures of vaccination were so obvious to those directly concerned that the common people insisted on having their children inoculated in the old way to save them from the contagion. At first only a druggist and some old women could be got to do it; but at length "even a few medical men, yielding to the popular clamour, or listening to the entreaties of their patients, took up the variolous lancet." ¹

Another general epidemic came in 1824-25; and the report of the Smallpox Hospital in London emphasized the fact, in which there was nothing unusual, that 147 of the patients had been vaccinated, and that twelve of these had died. Sir Robert Peel was questioned about this in Parliament, and asked the Vaccine Board to inquire into the circumstances. The result of their inquiry, communicated to the Government by Sir Henry Hallford, president of the College of Physicians and of the Board, "was so satisfactory as to leave no cause to doubt that these individuals had not been properly vaccinated." ² The same apologetic rôle was played

¹ Cross, *History of the Variolous Epidemic at Norwich, in 1819*. Lond., 1820, pp. 12, 24.

² Baron, i. 274; *Med. and Phys. Journ.*, May, 1826, p. 436.

by the Vaccine Board from its first establishment until it was superseded by official apologists of a more modern type.

The independent medical criticism became feebler, and at length ceased. Dr. George Gregory, physician to the Smallpox Hospital, was privately known to be a sceptic, and he occasionally gave vent to his distrust of the Jennerian practice. In the same year of Sir Henry Hallford's inquiry (1825), Dr. Robert Ferguson, who afterwards attained the first rank as a London physician, addressed a pamphlet to Sir Henry, proposing to use cowpox and smallpox inoculation conjointly, for the better security of patients. Ferguson does not appear to have taken any public part in the controversy in after years; but the journal which he helped to found, the *London Medical Gazette*, kept its columns open to anti-vaccinist contributors. It is significant, however, that the opposition had either become anonymous, or was wrapped up in allegory. Thus, in 1839, John Robertson, a well-known Manchester practitioner, published in the *Gazette* a satirical piece showing how vaccination had failed in the island of Baratavia, and how the officials had satisfactorily accounted for its failure.¹ Dr. Henry Holland, writing in the same year, could still use the language of critical freedom.² A

¹ *Lond. Med. Gaz.*, Jan., 1839.

² *Medical Notes and Reflections*, Lond., 1839, p. 401, etc. : "The early enthusiasm for the great discovery of Jenner swept doubts away; and they returned only tardily and under the compulsion of facts. . . . Any explanation from the ignorant or imperfect performance of vaccination was found insufficient to meet the number and variety of the proofs."

few months after, an anonymous writer in the *Medical Gazette*, "Scrutator," who is honoured with large type and a prominent position, published a series of letters of a strongly anti-vaccinist tone. "It is not enough for the thinking part of the profession," he wrote, "that *a few who have the management of this branch* should be wedded blindly to a particular belief. However much our wishes may incline us to favour vaccination, we must not be like the advocates of the Old Bailey, determined to bring off our client victorious, whether deserving or not; because truth will have its way at last; and it may be doubted whether the practitioners of the next century will not laugh at the manner in which we have been misled by Dr. Baron."¹ That was among the last anti-vaccinist protests that were allowed to appear in an English medical journal down to quite recent days. Henceforth the dogmatism hardens, and intolerance reaches a height which it had hardly ever before touched, even in the most bigoted period of the Paris Galenists. The anonymous writer was not far wrong when he deferred the general outburst of laughter to the next century.

The very next year (1840), a small circle of medical men, holding office in the Medical Society of London, petitioned Parliament, through Lord Lansdowne, to put down by statute the practice of variolous inoculation and to give State-aided facilities for vaccination. The disastrous epidemic through which the country had just passed in 1838-9 was owing, they alleged, to the neglect of vaccination in the first instance, and to the practice of variolation in the second.

¹ *Lond. Med. Gaz.*, Oct. 19th, 1839, p. 211.

In the debates that followed, the Bishop of London said it was well known that, in agricultural districts of the country, there had not been for many years past the least difficulty in obtaining vaccination gratuitously; but many of the ignorant poor were strongly prejudiced against it, and paid a much greater attention to empirics than to the advice of the clergy.¹ Mr. Wakley, editor of the *Lancet*, said, in the Commons, that "no one could be ignorant that the working classes of the country entertained great prejudices against vaccination." In the usual manner of constructive logic, he was led to blame variolous inoculation for the whole of the 17,000 deaths in one year from smallpox, and gave it as his opinion that the disease would die out altogether if variolation were prevented and vaccination adopted.² The legislation of 1840 has been referred to in the last chapter but one.

The inoculation of smallpox came to an end, vaccination was encouraged in various ways, and in many parts of the country was as generally practised as it has ever been; but the epidemics continued as before. Then, in an evil hour, came the Dr. Sangrado logic, that vaccination had failed as a State remedy because it was not carried out thoroughly. There was also another Sangrado reason in the background. Gil Blas said one evening to Dr. Sangrado: "Sir, I take heaven to witness that I follow your method with the utmost exactness; nevertheless every one of my patients goes to the other world." "My child," answered he, "I have reason

¹ House of Lords, 16th March, 1840.

² House of Commons, 17th June, 1840.

to make the same observation ; and if I were not as sure as I am of the principles on which I proceed, I should think my remedies were pernicious," etc. "Let us change our method," said Gil Blas. "I would willingly make that experiment," replied Dr. Sangrado, "provided it would have no bad consequences ; but I have published a book in which I have extolled the use of frequent bleedings and draughts of warm water ; and wouldst thou have me decry my own work?" Behind all the scientific good faith with which it was recommended, the first Compulsory Vaccination Act, that of 1853, was also an Act for the maintenance of medical authority and for the saving of medical credit.

The Vaccination Extension Bill, as it was called, although its object was to introduce the principle and practice of compulsion, was brought into the House of Lords early in 1853 by Lord Lyttelton as a private member. No speech was made upon it until the motion for going into Committee on the 12th April. Lord Lyttelton then explained that he was acting in this matter upon the advice of certain able and learned persons connected with the Epidemiological Society. The object of the Bill, he said, was to prevent persons from spreading the infection of smallpox to others. The principle had been recognised in the Act of 1840, by which it had been made penal either to inoculate children with smallpox, or so to expose them that they would be infectious ; and Lord Lyttelton was advised that "leaving them unvaccinated did in reality come under the last head."¹

¹ *Parliamentary Debates*, House of Lords, 12th April, 1853.

The able and learned persons who gave Lord Lytton that remarkable advice had come together on the 30th July, 1850, as the Epidemiological Society. They began with a very full programme of subjects that called for investigation; cholera, yellow fever, and other epidemics were mentioned, but, strangely enough and significantly as the event showed, smallpox was not named in the prospectus as a subject for epidemiological study, although vaccination was brought into a subsequent paragraph along with quarantine. Most of the society's schemes of investigation stuck fast at the outset "for want of funds."¹ It started in 1850 with seven committees, each having an important theme entrusted to it; but only one of these, the Vaccination Committee, reported within the first five years, and some of them never reported at all. No general meetings were held until April, 1854. It would be unfair not to make honourable mention of the memoirs communicated to it on various interesting epidemics, especially by medical men on foreign service who had the vast British empire abroad to draw their materials from; but it may be truly said that vaccination was the Epidemiological Society's first love, and that it has become a solace of its later years. The Vaccination Committee was the first by several years to make its report, on the 26th of March, 1853; and that was the brief from which Lord Lytton spoke on the 12th of April. The report was ordered by the House of Commons to be printed on the 3rd of May as a parliamentary paper.²

¹ *Med. Times and Gaz.*, 14th April, 1855.

² *Parliamentary Papers*, vol. ci., 1852-53.

The committee of the Epidemiological Society begin their report by remarking that there can be "no doubt of the authentic and trustworthy character of the information on which our conclusions are based." They then give Conclusion I. :—

"Every case of smallpox is a centre of contagion; and every unvaccinated or imperfectly vaccinated population is a nidus for the disease to settle in and propagate itself.

"To the two latter propositions, which do not admit of being controverted, we call your special attention, for it is on them, we conceive, must be based any enactment for rendering vaccination compulsory. If it admit of doubt, how far it is justifiable in this free country to compel a person to take care of his own life and that of his offspring, it can scarcely be disputed that no one has a right to put in jeopardy the lives of his fellow-subjects" (p. 4).

A nidus for the disease to settle in and propagate itself—that is a phrase which epidemiologists have to use constantly,¹ and for smallpox among other diseases. But the use of nidus in the foregoing constructive sense was new to epidemiology. Lord Lyttelton merely carried the constructive logic a step farther when he said that "leaving children unvaccinated did in reality come under the head of exposing them so as to be infectious."

As the main proposition upon which the epidemiolo-

¹ See Hirsch's *Handbook of Geographical and Historical Pathology*, passim. (English translation by present writer, 3 vols., New Sydenham Society. 1883-86.)

gists rested the case for compulsion did not "admit of being controverted," it did not, of course, stand in need of proof. The committee, however, did not entirely omit to furnish evidence; more particularly, they gave some marvellous instances, which had been brought to their notice, of towers in Siloam falling upon the unvaccinated exclusively. Their special attention to these portentous events, together with their disregard of the totality of factors determining the incidence of small-pox in time and place, serves to mark the early stage that the science of epidemiology had then reached. All the sciences have begun with the marvellous; thus, pathology, which stands nearest to epidemiology in subject-matter, was almost entirely occupied for many years with monstrosities and curiosities.

The inability of these early epidemiologists to deal with the evidence in any other way than begging the question was shown in that part of their report which brought to light the neglect of vaccination in certain localities. There had been a great deal of vaccination in some places since the Act of 1840, and very little of it in others; there had been also a good deal of small-pox in some places, and little or nothing of it in others. Leicester, Loughborough, Derby, Ashford, Taunton, and the like were shown to have been negligent of optional vaccination; now, was it the case that they had paid the penalty by becoming each "a nidus for smallpox to settle in and propagate itself"? The epidemiologists did not allege that they had; and we may safely affirm that so strong a point would not have been passed over except for the reason that it had no existence. They were pleased to say that their nidus-doctrine did not

admit of being controverted. Nidus was a good word ; in English it means a nest, but in Latin it might carry as much constructive meaning as the exigences of the case required.

The only person who showed a knowledge of what a nidus of smallpox really amounted to was Lord Shaftesbury, who remarked, in the debate, that "smallpox was chiefly confined to the lowest class of the population, and he believed that, with improved lodgings, the disease might be all but exterminated." But Lord Shaftesbury had presided at the inauguration of the Epidemiological Society three years before, and he was bound to defer to his expert friends when they solemnly assured him that it was non-vaccination which formed the nidus for smallpox to settle in and propagate itself. I repeat that the programme of the Epidemiological Society did not even mention smallpox among the great epidemic maladies which required to be studied according to the ordinary methods of historical and geographical research, or dealt with according to the ordinary principles of sanitation.

The first Compulsory Vaccination Bill ran through both Houses without opposition. How such an act, without a reasoned motive in its preamble, and without scientific definitions in its clauses, could have got upon the statute-book in the year 1853, must ever remain one of the marvels of our legislative history. It was the secure hour of eminent persons, when even the minor offices of the coalition ministry were filled by past-masters in the art of legislating. The following contemporary estimate of the session will be read now with interest :—

“As the spring advanced, and measure after measure passed successfully, opposition grew weaker and weaker, till at last discussion was almost reduced to the candid statement of objections and suggestion of difficulties. Here is the bright spot of the year 1853; the patriot may dwell on the labours of our Parliament with pleasure, and the future historian may perhaps find occasion to record that about this period the Parliamentary system of Great Britain had reached its highest perfection.”¹

The party of vigilance in the House of Commons awoke from their enchantment next year (1854), when vaccination came up again in connexion with a technical Amendment Bill.² In 1856 another bill, promoted by the Epidemiological Society, of a much more dictatorial kind, was about to be passed as an unopposed measure; but the minister in charge of it was obliged to give a pledge to Mr. Duncombe that it would not be taken after midnight, and it was found to be of such a kind that it was discharged by the general wish of the House when it came into Committee on the 10th July.

Meanwhile there began among the public that modern anti-vaccination movement which has slowly assumed the proportions of a revolt against the compulsory law. In 1854 Mr. John Gibbs published anonymously *Our Medical Liberties*, and followed it up next year with a letter on Compulsory Vaccination addressed to the

¹ “*The Times*” *Annual Summaries*, 1851-1875, p. 21.

² The minority consisted of Mr. Barrow, Mr. Joseph Brotherton, Mr. Thomas Duncombe, Mr. Frewen, Dr. Michell, and Sir George Strickland.

President of the Board of Health, which the House of Commons, on the motion of Mr. Joseph Brotherton, ordered to be printed on the 31st of March, 1856. This led to the medical blue-book of 1857, on *The History and Practice of Vaccination*, in which the position of the epidemiologists in 1853 is somewhat varied: "It was the liberty of omission infanticide which the law took courage to check." Those among the public who had made a study of the history and practice of vaccination were not satisfied with these medical deliverances. If they did not always ask their question "with Olympian politeness," yet they kept asking the question, But *is* it so? Is the unvaccinated residue really a nidus for smallpox to settle in and propagate itself? Is not all this terrific logic about checking the liberty of omission infanticide merely an ingenious superstructure upon a radically unsound basis?

No answer to the question, by those who had the official means of answering it, was ever given or attempted, until the results of the great epidemic of 1870-1872, particularly of the German portion of it, proved once for all that the unvaccinated residue were not what the Epidemiological Society's Committee had said they were; that is to say, they were *not* a nidus for smallpox to settle in and propagate itself, they were not a collection of combustible materials, they did not put the lives of their neighbours in jeopardy. Absence of vaccination did not amount to omission infanticide, so far not so that the German Government in 1874 extended the age for vaccination to two years. Among the records of that epidemic in Germany, one of the greatest in the whole history of European smallpox, (124,948

deaths in Prussia in two years, 1871-72) are the lists kept by the police in chronological order of the persons attacked in each locality. These lists are now known to be much less perfect as regards the facts of vaccination or non-vaccination than was supposed, but even from the partial revelations certain conclusions are at once obvious. It is found that the first unvaccinated person is generally a good long way down the list.¹ It was not among the unvaccinated that the epidemic in each of its several centres took origin and gathered head; the unvaccinated had no more than their share of the epidemic, and not always that.

For Bavaria, whose vaccination arrangements had been held up as a pattern to other States, the facts for the year 1871 were published fourteen years ago by a medical official of the bureau of statistics at Munich.² The cases of smallpox in 1871 were 30,742, of which the vaccinated were 29,429, or 95·7 per cent., and the unvaccinated 1,313, or 4·3 per cent. There were 3,994 deaths among the vaccinated cases, a rate of 13·8 per cent.; among the unvaccinated, there were 790 deaths, a rate of 60·1 per cent. But 743 of the latter were in infants under one year, leaving 47 deaths of the unvaccinated of all other ages. The excessive mortality of infants is, of course, not peculiar to smallpox.

As Moseley said in 1806, it is always possible for the apologists to get up "a squabble about mis-statements."

¹ At Bonn the 42nd, Cologne the 174th, and at Liegnitz the 225th. In a recent official work (Berlin, 1888), it is stated that the lists from Liegnitz are unfortunately without data as to vaccination.

² Majer, *Vierteljahrschrift für gericht. Med.*, xxii. 355.

But it will be difficult to confuse the issues from such broad facts as these. There is, indeed, no longer any attempt to do so, unless it be here and there by an official in the exercise of what he conceives to be his duty.

The vaccination law in England was made more rigorous in 1861, 1867, and 1871, on the Sangrado principle of giving State blood-letting and hot water a fair trial. In 1880 the ministry of the day brought in a Bill to relax the penal clauses; they thought that one fine, distraint or imprisonment would be enough in the case of each child, instead of prosecutions at intervals until the child was fourteen years old. The ministry had to abandon their Bill, owing to the opposition of the medical profession and the Royal Society. Among the petitions presented against the Bill was one from certain members of the British Medical Association, which contained the following clause:

“3. That the outcry against compulsory vaccination is mainly due to certain interested persons, who, by the dissemination of inflammatory literature, and by the propagation of falsehoods and distorted statements, stir up opposition to vaccination on the part of ignorant and thoughtless people.”¹

These accusations are but the angry words of discontented professional opinion, when it finds out that there is a power in the State setting its authority at defiance. The anti-vaccinists are those who have found some motive for scrutinizing the evidence, generally the very human motive of vaccinal injuries or fatalities in their

¹ *British Medical Journal*, 1880, ii. 103.

own families or in those of their neighbours. Whatever their motive, they have scrutinized the evidence to some purpose; they have mastered nearly the whole case; they have knocked the bottom out of a grotesque superstition.¹ The public at large cannot believe that a great profession should have been so perseveringly in the wrong. The present attitude of the public may be said to illustrate the truth of a maxim of Carlyle's: "That no error is fully confuted till we have seen not only *that* it is an error, but *how* it became one." The task which I set before me when I began this book was to explain to myself how the medical profession in various countries could have come to fall under the enchantment of an illusion. I believe that they were misled most of all by the name of "smallpox of the cow," under which the new protective was first brought to their notice. For that grand initial error, blameworthy in its inception, and still more so in the furtive manner of its publication, the sole responsibility rests with Jenner.

The profession as a whole has been committed before now to erroneous doctrines and injurious practices, which have been upheld by its solid authority for generations. Lesage's satire upon blood-letting, in *Gil Blas*, which appeared in 1715, ought of itself to have made that practice ludicrous in the eyes of the world; but blood-letting survived a hundred years after that in all countries; and in the country of Sangrado it survived a hundred and fifty years. The apology for it, or explana-

¹ See *The Story of a Great Delusion*, by William White, Lond., 1885, and the serial volumes of the *Vaccination Inquirer*, from 1879.

tion of its abandonment, which was still being taught in lectures twenty years ago, was that diseases had changed their type from sthenic to asthenic, and that in our asthenic age blood-letting was no longer necessary. It is difficult to conceive what will be the excuse made for a century of cowpoxing; but it cannot be doubted that the practice will appear in as absurd a light to the common sense of the twentieth century as blood-letting now does to us. Vaccination differs, however, from all previous errors of the faculty, in being maintained as the law of the land on the warrant of medical authority. That is the reason why the blow to professional credit can hardly help being severe, and why the efforts to ward it off have been, and will continue to be so ingenious.

The longer the compulsory law is maintained, the more marked will the contrast become between public intelligence and professional dogma. As for the public, they may escape, as soon as they please, from being dragooned by an official authority which is neither very learned nor very liberal. When the deliberate sense of the kingdom is known, as Burke says, "it *must* be prevalent. It would be dreadful indeed, if there were any power in the nation capable of resisting its unanimous desire, or even the desire of any very great and decided majority of the people. The people may be deceived in their choice of an object; but I can scarcely conceive any choice they can make to be so very mischievous as the existence of any human force capable of resisting it."

THE COWPOX LEGEND IN GERMANY.

(Note to p. 21.)

THERE is an authentic record that the protective virtue of cowpox had been talked of in the country near Göttingen previous to 1769. In a paper attributed to Jobst Böse, on "Pestilence of Cattle; and on Passages in Livy," published in the *Allgemeine Unterhaltungen* for 24th May, 1769, p. 305, cowpox is mentioned as an example of a disease which men suffer from as well as animals. "It is true," the writer continues, "that men do not die of it any more than cattle. But sick enough can the people be from it, all the same. In passing, I must mention that the people in this part of the country [Göttingen] who have had cowpox (*Kuhpocken*) flatter themselves entirely that they are secure from all infection of our ordinary smallpox (*Blattern*), as I myself have several times heard from quite reputable persons." This was reprinted in 1802 by Steinbeck in his monthly journal, the *Deutsche Patriot*, January, pp. 43-46; and may be conveniently referred to in K. F. H. Marx's *Göttingen in medicinischer, physischer, etc., Hinsicht*. Gött., 1824, p. 326.

A corresponding legend had been current in Holstein previous to 1791. In that year, Plett, a poor schoolmaster near Kiel, is said to have inoculated children with cowpox. His narrative was not committed to writing until 1815, when it was taken down from his own lips and printed in the *Schleswig-Holstein Provinzial Berichten*, 1815, p. 77 (copied into the *Literatur-zeitung* of Leipzig, 10th June, 1815, p. 1113, and here quoted from Choulant, "Edward Jenner," in *Zeitgenossen*, 1829, Pt. vii., p. 12). Cowpox is throughout written *Kuhblattern* (smallpox of the cow), and not *Kuhpocken*, smallpox being called *Kinderblattern*, *Menschenblattern*, and *natürliche Blattern*. Plett, we are told, "betook himself to the cowhouse, examined the pocks (*Blattern*) on the cows' teats, and when he found a good one, which looked ripe, he cut it open with his penknife, collected the matter on a chip of wood as it ran out, and returned with it to his schoolroom." The mythical element in the narrative, as written down, is obvious; no one has ever got vaccine from the cow by ripping up a pock with a penknife. It does not follow, however, that Plett had not inoculated some kind of fluid from a cow's teat upon the human skin. It is not alleged that his practice found favour.

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