

# The Devonshire Colic

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THE Devonshire Colic was a disease of the cider counties of England. It occurred relatively infrequently in Hereford, Gloucester, Worcester, Somerset, and Cornwall, and acquired endemic proportions only in Devon itself. The first written reference to the Colic is found in William Musgrave's treatise on gout (*Dissertatio de arthritide symptomatica*) published in 1703.

Musgrave was well known both as physician and antiquarian. He was born in Nettlecombe in Somerset in 1685 and educated at New College, Oxford, where he graduated Bachelor of Civil Laws in June 1682. He then moved to London and was elected a Fellow of the Royal Society in 1684, becoming Secretary of the Society in the following year. In 1685 he moved back to Oxford and was awarded his M.B. that same year. He proceeded M.D. in 1689 and was elected a Fellow of the Royal College of Physicians in 1692. For a few years Musgrave practised in Oxford, then in 1691 he settled in Exeter and practised there with great success until his death thirty years later.

In his treatise on the gout Musgrave was concerned, amongst other things, to show how colic could precipitate attacks of gout. Since there were many varieties of colic, he began by describing them, including in the catalogue one which

prevails with the inhabitants of Devonshire, arising from the rough and acid cyder, drunk in too great quantities; . . . it prevails only among those who have accustomed themselves to that drink; and they are affected more or less, in proportion to the quantities they make use of: so that in those seasons when we

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have plenty of cyder, the disease is frequent and endemic. On the other hand, in those years when we have not plenty of apples, it is seldom we meet with this complaint.

Those [with the disease] are very often afflicted with the colic about the age of twenty-eight or thirty-two; and, when it is obstinate and fixed, the gout is not infrequently produced from the colic, with a pain of the joints, in the extremities, swelling and other appearances usually observed in the regular and genuine gout (pp. 8-9).<sup>1</sup>

Musgrave gave no other symptoms of the Devonshire Colic but he did give a complete description of what he called bilious colic. The patient was 'a lady . . . of forty-five, with an obstinate bilious colic, whose concomitant symptoms were, a nauseous taste in the mouth, difficulty in making water, frequent vomiting of bile; few motions, high red urine, a pallid complexion and a small degree of fever. She also had a weakness of the feet which was later accompanied by the true gouty heat [pp. 11-12].'

Although Musgrave did not consider this as Devonshire Colic, there seems little doubt that he is describing a case of lead poisoning. The red colour of the urine is interesting as it may have been caused by the excretion of large amounts of porphyrins, a common manifestation of lead poisoning.

He failed to mention the sometimes fatal termination of the Devonshire Colic in paralysis or encephalitis. He was well aware that this could happen in the *colica Pictonum*, however, since he described a patient with this complaint who developed paralysis in his limbs and became bedridden for the rest of his life. The *colica Pictonum* was well known to be caused by lead, but despite the similarities in the two conditions, Musgrave was unable to attribute the single cause to them both.

There are several possible reasons why he gave no clinical account of the progress of the Colic. Musgrave's prime concern was to demonstrate that the various forms of colic, as he classified them, could lead to gout, and he may well have felt this was as far as he need go. Also he may not have seen many cases of paralysis arising from the Colic for there was no county hospital at Exeter at the time he wrote his treatise; the Devon and Exeter Hospital, later the Royal Devon and Exeter Hospital, founded 27 August 1741, did not begin admitting patients until January of the following year.

Again, Musgrave may have been reporting a relatively new phenome-

1. The translations of Musgrave's tract are those contained in Hardy's tract (J. Hardy, *A candid examination of what has been advanced on the colic of Poitou and Devonshire, with remarks on the most probable and experiments intended to ascertain the true cause of the gout* [London, 1778]).

non, for although it has been suggested that the Colic had been endemic for centuries before his time,<sup>2</sup> it was probably of more recent origin. There is certainly evidence that cider was drunk in Devon from very early times (it was being sold in Exminster for ½d a gallon in 1286<sup>3</sup>), but the orchards were neglected for several centuries thereafter. They were probably revived in the seventeenth century since Westcote, writing in 1630,<sup>4</sup> was able to refer to the great enlargement of Devon orchards 'of late years' more especially for the making of cider, which he described as 'a drink both pleasant and *healthye* [my italics], much desired of seamen for long voyages, more fitted to make beverage than beere, and much cheaper and easier to be had than wyne.'<sup>5</sup>

In Queen Anne's reign extensive planting of orchards took place. The standard of cider making was high, and the best cider fetched high prices and was widely known. It was at the beginning of the eighteenth century that its consumption began to increase considerably and probably only at this time did the Devonshire Colic begin seriously to manifest itself and come to the attention of the physician.

Musgrave never returned to the subject of the Colic; he died in 1721. The next writer to devote attention to the disease was John Huxham, born in 1692, the son of a Totnes butcher. Orphaned at an early age, he was brought up by his guardian, a Nonconformist minister. In 1715 he entered the University of Leyden, studying under the great Boerhaave. He was not able to stay there the requisite three years because of lack of funds, and so he moved to Rheims and took his M.D. there without residence in 1717.<sup>6</sup> Returning to England, he first took a house in Totnes, then moved to Plymouth where he stayed for the rest of his life. Huxham had a great regard for Musgrave, whom he almost certainly knew, and referred to him as 'our very learned Musgrave, who was an honour to our Profession whilst he lived.'

Of Huxham's achievements there is no doubt. He published frequently (often in Latin) and in due course gained an international reputation. He held that no theory was valid unless it rested on experiment and consequently he was no blind devotee of the opinions and theories of his age but

2. W. R. Bett, 'The endemial colic of Devonshire,' *Monthly Bull. (Brewing Ind.)*, 1959, 30, 90, 95.

3. W. H. R. Curtler, *A short history of English agriculture* (Oxford, 1909), p. 37.

4. T. Westcote, *A view of Devonshire in MDCXXX with a pedigree of most of its gentry*, Rev. George Oliver and Pitman Jones, eds. (Exeter, 1845), p. 56.

5. Probably to allay scurvy.

6. F. Willcocks ('Notes on the Devonshire colic and its connection with cider,' *Trans. Dev. Ass.*, 1885, 17, 324-334) says that Huxham received his M.D. from Leyden in 1715, but the evidence seems against him.

dared<sup>7</sup> to look into doctrines and observe for himself, as free from prejudice as<sup>8</sup> most can expect to be. The humoral doctrine of Hippocrates is evident in his writings, however, especially when he considered the causes of the Devonshire Colic. The influence of Hippocrates in his work is not surprising since Huxham had written 'a person cannot be a good Physician without consulting that great oracle of Physic.'

Huxham's essay on the Colic was published in Latin in 1739 and again in 1752. An English translation was published in 1759,<sup>7</sup> and it is from this that the following extracts are taken. The essay describes a particularly severe outbreak of the disease which occurred in 1724. The fact that Huxham felt it worth describing may be another indication that—in the endemic form—it was a relatively new disease. The outbreak began in August and spread over the county, particularly amongst those 'not very elegant and careful in their diet.'

Unlike Musgrave, Huxham gives a very full account of the symptoms of the disease written in the forceful language of the day.

[It] began its Attack by an excessively tormenting Pain in the Stomach, and epigastric Region, with an unequal, weak Pulse, and coldish Sweats; the Tongue in the mean Time was coated with a greenish or brown Mucus, and the Breath was most offensive. An enormous Vomiting soon followed, for the most Part of exceeding green Bile, sometimes black, with a great Quantity of Phlegm excessively acid and very tough; nay the foul Matter brought up was oftentimes so very acrid, that, by excoriating the Throat and Oesophagus, it was tinged with Blood, and created a Difficulty and Pain in Swallowing. Things continued in this State for a Day or two, the Belly became extreamly bound, neither answering to the most drastic Purges, or sharpest Clysters, the latter coming off without Wind or Stool, the former being soon vomited.

The vomiting abating somewhat, the Pain descended, and most grievously tortured the umbilical Region, and Small of the Back, so that you would have thought the Patient actually laboured under a nephritic Paroxysm, and the rather as a Suppression of Urine now also comes on, and yet a perpetual Desire of that Discharge is urgent; . . . (pp. 5-6).

In addition to this, the abdomen was hard and tense and stools were passed only seldom. The urine was red in colour. Later in the disease the terrible Gripping and Pain on the Belly might have ceased a little . . . [but] a most excruciating Pain now seized the whole Spine of the Back, most violent between

7. J. Huxham, *A small treatise on the Devonshire colic which was very epidemic in the year 1724*, translated from the Latin original and contained in *The works of John Huxham* (London, 1788), 1. The extracts which follow in the text are from this translation.

the Shoulder-Blades; thence soon affecting the Arms it fixed chiefly in the Articulations, and altogether destroyed the Motion of the Hands—nor were the Legs and Thighs much less tormented, for a very sharp Pain affected them . . . and yet there was seldom any Redness, or Swelling of the Parts (pp. 7–8).

In some patients the pains ceased, only to return a few days later after drinking beer or cider. Other patients had convulsions and a few died from what Huxham called epileptic fits when they had had the symptoms for a long time. However, he considered the mortality rate low, considering the great number of people who laboured under the disease. The epidemic lasted from the autumn of 1724 through to the following spring and disappeared completely in the summer of 1725.

Huxham was a believer in the aphorism of Hippocrates that 'He who knows the nature of the disease knows the method of cure,' and so the greater part of his essay is given to an investigation of the cause of the disease. After this had been established, he proposed the cure, which consisted mostly of emetics and purges.

Huxham thought that the Colic owed its origin to the great glut of apples that year, 'greater by much than was known in the Memory of Man.' The farmers threw vast quantities of their surplus fruit to the pigs, many of which died as a result, and, faced with the seemingly impossible task of disposing of the enormous quantities of cider which had been made—there were apparently not enough vessels in the county to contain it all—either drank vast quantities of it themselves, or hired others to do it for them. As soon as the casks were empty, fresh cider filled them again. The poorer people fed almost entirely on apples and drank cider to the exclusion of anything else. In other years there had been large apple crops, particularly 1722, 1728, 1730, and 1734, and the Colic had been similarly prevalent but not to the same degree as in the year described by Huxham.

Although his description of the symptoms of the Colic was an almost classic account of lead poisoning, Huxham did not consider this as a possible cause of the disease. Instead, he thought that the disease arose as a result of drinking the cider before it was properly fermented. If anyone drinks the crude juice of the apple, he wrote,

it necessarily ferments vehemently in his Stomach and Intestines, and hence it distends them greatly with Wind, and racks and gripes them. . . . Hence arise Vomiting, Cholera, Diarrhoea and Dysentery, from an Excess in the use of it: For this Mucus of the Guts being greatly deterged, which should defend the Tunica nervosa of the Intestines, it becomes sensible, and impatient as it were of the very least Acrimony, much more of the gross acid Salts with which such

Fruits abound: and being on every Side irritated by such Stimuli, this very tender Membrane is grievously affected. . . .

Hence a most horrible Pain is excited, and thence Inflammations of the Intestines, which are soon succeeded by a Gangrene, or a Rupture of the Coats, unless a speedy Remedy prevents it (pp. 19–21).

The speedy remedy consisted of violent purges and emetics. If neither of these followed the immoderate drinking of the new cider, however, worse awaited the hapless victim.

For by long and frequent Drinking of it [the cider] such a quantity of crude, gross Tartar is thrown into the Blood that it thence becomes very acrid, and not only the Blood, but, from that impure Source, all the Humours thence secreted. So that instead of a very soft, lubricating Mucus, . . . we have, as it were, a sharp coagulated Matter; whence arises a great Pain of the Joints and Impotence of their Motion—Instead of an exceeding soft Lymph to moisten the Nerves, a corrosive Ichor, and hence epileptical Attacks (p. 22).

At length even the bile was affected and stagnated in the ducts of the liver to prevent circulation through that organ. Because of the lack of bile, the intestines discharged their contents slowly, and the belly became bound. Huxham, like a good follower of Hippocrates, devoted much space to a consideration of the effects of the bile tartar derived from the cider and to the various kinds of bile found in the disease, for he was convinced that the effects on the bile were the most important contributory factors in the disease. It is a pity that a man who claimed that all theories must rest on sound experimental evidence should have put forward such an unsubstantiated thesis. None of his ideas regarding the cause of the Colic was subject to experiment, but, nevertheless, his theories were widely accepted as being the most likely explanation of its cause, even as late as 1778.<sup>8</sup>

Huxham, like Musgrave before him, wrote nothing further on the Colic, although he was involved, at second hand, in the controversy which arose following the publication of Sir George Baker's essay on the disease in 1767, the year before Huxham's death.

#### PART II. SIR GEORGE BAKER

The name of Sir George Baker (Fig. 1) is inextricably linked with the Devonshire Colic. In most of the standard histories of medicine in which

8. Hardy (n. 1), p. 18.

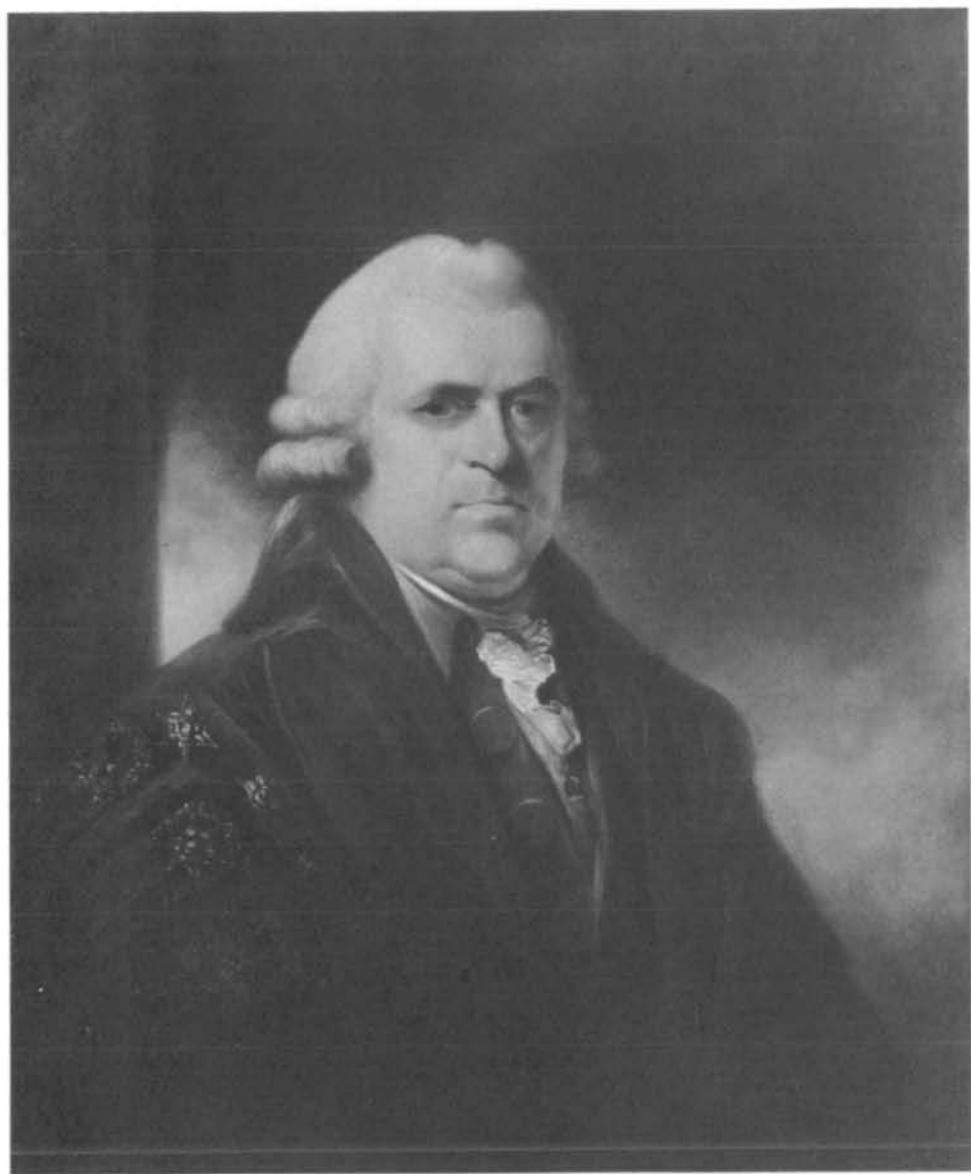


FIG. 1. Sir George Baker. (By permission of the Treasurer of the Royal College of Physicians of London.)

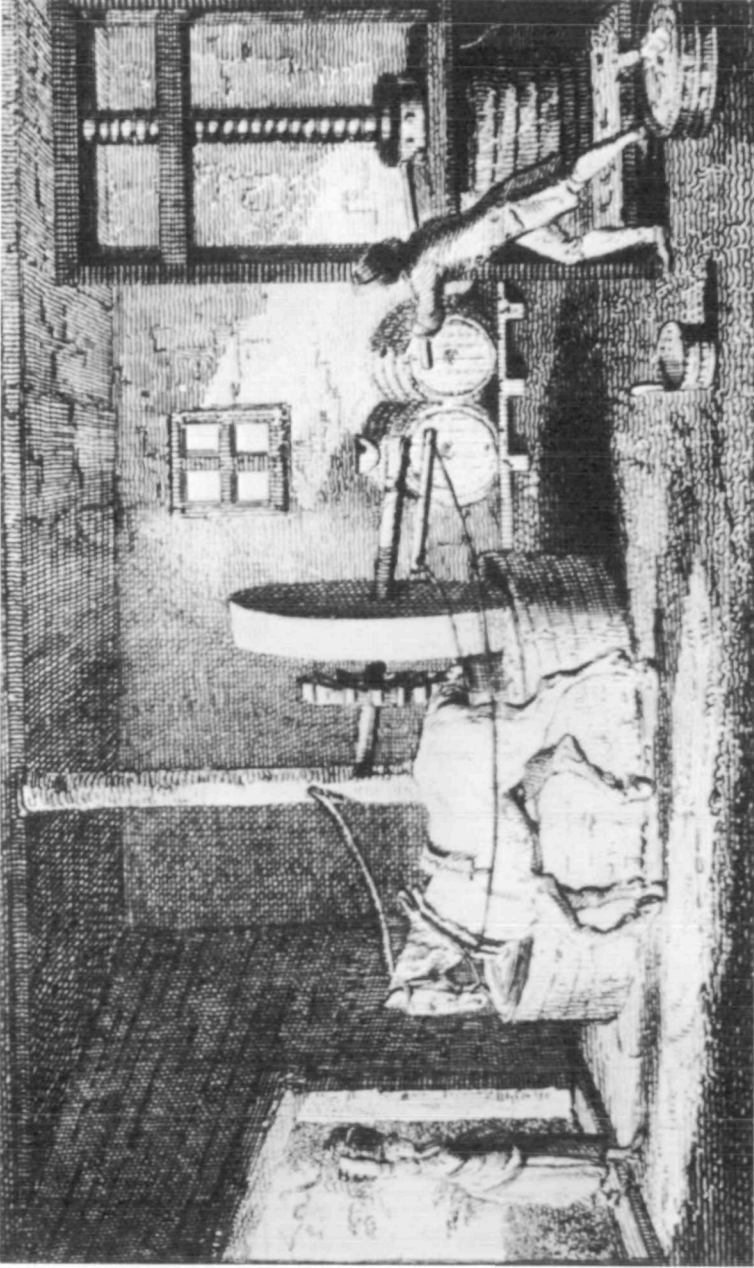


FIG. 2. Eighteenth Century Cider-making Apparatus.



the Colic is mentioned, it is Baker's name which goes with it as being the man who elicited its cause and brought about its decline. Baker was a man of tremendous intellectual stature and ability.

To him [wrote MacMichael in a much-quoted passage] the whole medical world looked up in respect, and in the treatment of any disease in the least degree unusual, if it was desired to know all that had ever been said or written on the subject, from the most remote antiquity, down to the case in question, a consultation was proposed with Sir George Baker. From *his* erudition everything was expected.<sup>9</sup>

Baker was himself a Devonian, the son of the vicar of Modbury. He was born in 1722 and educated at Eton and King's College, Cambridge. He gained his M.D. in 1756 and the following year he was elected a Fellow of the Royal College of Physicians of London. He began to practise medicine in Stamford in Lincolnshire but in 1761 he moved to London, finding provincial life too limiting for a man of his talents. Once in London he quickly rose to the forefront of his profession. He bestrode the medical world of his day like a Colossus: in an age of great men he was second to none. He held at one time or another all the highest offices open to him. He was successively physician to the Queen's household, physician in ordinary to the Queen, and then physician in ordinary to George III. 'No man [wrote Munk] ever followed the career of physic and the elegant paths of the Greek and Roman muses with more success. As a scholar he had few equals and no superior.'<sup>10</sup>

Baker's reputation today rests mainly on his work in connection with the Colic, the results of which were published in 1767; indeed it has been suggested that but for this piece of work, he would have passed into history as no more than an accomplished physician who enjoyed a more than usually large share of the public favour.<sup>11</sup> In 1767 Baker read not only his essay on the Colic before the College of Physicians, but three other papers also,<sup>12</sup> all relating to aspects of lead poisoning, and all four are masterpieces in their own right.

His observations on the Colic were published in the form of a pamphlet

9. M. MacMichael, *The gold-headed cane* (London, 1884), p. 116.

10. W. Munk, *The roll of the Royal College of Physicians of London* (London, 1878), 4 vols., II, 213.

11. A. Chaplin, *Medicine in England during the reign of George III* (London, 1919), p. 43.

12. G. Baker, 'An examination of several means, by which the poison of lead may be supposed frequently to gain admittance into the human body, unobserved, and unsuspected.' *Med. Trans. Coll. Phys., Lond.*, 1768, 1, 257-318. 'An attempt towards an historical account of that species of spasmodic colic, distinguished by the name of the colic of Poitou,' *Ibid.*, 1768, 1, 319-363, 'An examination of the several causes, to which the colic of Poitou has been attributed,' *Ibid.*, 1768, 1, 364-406.

in 1767 to give the Devonians the earliest possible notice of their danger so that they might take the appropriate steps to eliminate the hazard to their health which Baker had uncovered. In the following year the essay was printed in a slightly amended and lengthened form in the *Medical Transactions of the College of Physicians*.<sup>13</sup> Both versions are quoted from in the text that follows.

In both essays Baker observed that in the past much time and effort had been bestowed on the investigation of remote and obscure causes, whilst others, more immediate and obvious, had frequently been overlooked and disregarded. Fortunately, thought Baker, this was a failure mainly of the bygone age. 'We have now learned not to indulge ourselves in visionary speculation, but to attend closely to nature and make experiment the great basis of our reasoning.' Physicians of Baker's day were becoming much more interested in the causes of disease and not just in disease itself, more especially they were interested in causes that might lead to the cure of a disease or, better still, to the prevention of the disease.

The basis of Baker's essays was to show the cause of the Devonshire Colic and thus bring it to an end. 'I flatter myself [he wrote] I shall be doing an especial service to the inhabitants of my native county, if, by giving them notice of a mischief of which they are not aware, I may induce them to avoid it, and may at once promote the health and prosperity of my countrymen [*Trans.*, pp. 177-178].'

He began with a survey of the writings of Musgrave and Huxham and a discussion of Huxham's theories concerning the cause of the disease. Baker confesses, however,

I have for some time conceived doubts concerning the solidity of this doctrine. When I have considered that there is not the least analogy between the juice of apples and the poison of lead, and that this colic of Devonshire is precisely the same disease, which is the specific effect of all saturnine preparations; it has not seemed to me at all probable that two causes, bearing so little relation to one another, should make such similar impressions on the human body (*Essay*, pp. 13-14).

Baker dismissed Huxham's theory that the acid in the apple juice brought about the disease by pointing out that although people in the neighbouring counties of Worcester, Gloucester, and Hereford drank a weak acid cider, they did not succumb to the disease nearly as often as did the Devonians. And in the West Indies, although acid punch was partaken

13. G. Baker, *An essay concerning the cause of the endemial colic of Devonshire* (London, 1767), reprinted with some additions in *Med. Trans. Coll. Phys., Lond.*, 1768, 1, 175-256.

of freely, the colic had almost disappeared since the use of leaden worms had been prohibited in the distillation of rum.

Baker's great familiarity with early writers had no doubt helped him to formulate his theory that the Devonians contracted lead poisoning through their cider. There had been many instances in history of wine makers, or wine merchants, adulterating wine with lead, either accidentally or deliberately, to improve its quality. The most famous incident was that which produced the colic of Poitou described by Citois in his classic work published in 1617.<sup>14</sup>

Summing up all his evidence Baker said of the Devonshire Colic,

It therefore seems not to have been without sufficient foundation, that I have for some time suspected, that the cause of this colic is not to be sought for in the pure Cyder, but in some, either fraudulent, or accidental adulteration. [And] lead being certainly of such a nature, as to be abundantly answerable for all the ill effects, complained of from the cyder, my thoughts naturally carried to the search of it; and well might I expect to find it, in some way or other combined with that liquor (*Trans.*, p. 188).

At the time Baker was writing, the disease was very common in Devon, especially in those parts where the most cider was drunk. Baker had been in correspondence with John Andrew of the Devon and Exeter Hospital and had received from him an account of the number of patients admitted to the hospital with the Colic since September 1762.

Sept. 1762	to	Sept. 1763	72
Sept. 1763	to	Sept. 1764	75
Sept. 1764	to	Lady-Day, 1766	86
Lady-Day, 1766	to	July 6, 1767	<u>52</u>
			285

Of this total, 209 were cured, although Andrew added that the disease often recurred. At the time of admission to hospital most of the early symptoms of the disease had disappeared, and the patients presented with a paralytic weakness in the arms.

It is not possible to check Andrew's figures as detailed records of the diagnosis and treatment of the patients admitted to the Devon and Exeter Hospital were not kept until July 1768, although no doubt Andrew, who was appointed to the Hospital at its foundation and remained there until his death in 1772, would have kept some records of his own. In any event, the num-

14. Baker himself gave a good account of the colic of Poitou (n. 12). He was in no doubt that it owed its origins to the same cause as the colic of Devonshire.

ber of patients who were admitted after 1767 suffering either from colic or paralysis is far fewer than quoted by Andrew.<sup>15</sup> This would seem to indicate that the disease became less prevalent after Baker published his findings, but this may not have been the case.<sup>16</sup>

Some of Andrew's patients, especially those in whom the disease proved obstinate, were sent to take the waters at Bath which were considered to be a most effective remedy for the complaint. Eighty patients were sent there in 1766, of whom forty were sent away cured and thirty-six greatly improved. The number of patients with cider colic was considerable. Rice Charleton, physician at the Bath hospital from 1757 to 1781, published a tract in 1770<sup>17</sup> giving the number of patients admitted with palsies from cider or bilious colic from May 1751 to May 1764 as 237. Of this total, nine died and only five were discharged no better than before. Charleton's patients came from Devon, Somerset, Gloucester, and Cornwall and during the thirteen years reviewed by him, only one patient was sent from Herefordshire and none from Worcestershire. This is not to say that the disease did not occur in those counties; it may only reflect the difficulties patients experienced in reaching the hospital in view of the appalling state of the roads in those days. There is an interesting handwritten note in the copy of Charleton's tract held by the University of Birmingham Medical School Library which indicates that the illness was known in Worcester. The writer, commenting on Charleton's observations says, 'Yet Worcestershire is undoubtedly at some periods affected by the Cyder Colic. I have often been called to patients affected by it.'

However, Baker's contention that the Colic *was* almost exclusive to Devonshire was supported by the evidence of John Wall the Worcester physician,<sup>18</sup> given in a letter to Baker. He was only one of the many West Country physicians to whom Baker wrote in the course of his researches. The bulk of this correspondence has been lost, but Wall's letter was published by Baker in his essay and subsequently republished with annotations by his son Martin.<sup>19</sup>

Wall told Baker: 'The Counties of Hereford, Gloucester and Worcester, are not, so far as I know, subject to the Colic of Poitou, or any other en-

15. These figures are given in the Appendix.

16. Hardy (n. 1), p. 3.

17. R. Charleton, *An inquiry into the efficacy of warm bathing in palsies* (Oxford, 1770), p. 34.

18. John Wall (1708-76) was famous for founding the Worcester Royal Infirmary and also the Worcester Porcelain Company.

19. J. Wall, *Medical tracts of the late John Wall M.D. of Worcester, collected and republished by Martin Wall* (Oxford, 1780), p. 304.

demic illness, unless it be the Rheumatis, which I think the Inhabitants of Herefordshire are more liable to, than those of some other neighbouring Counties.'

Having satisfied himself that the Devonshire Colic was a manifestation of lead poisoning, Baker was left with the problem of deciding how the lead came into the Devonshire cider and yet not in that produced in the neighbouring counties. He was willing to believe that, although some farmers put a lead weight into casks of weak cider to improve it, this habit was far from common and was performed without the knowledge of the ill-effects caused by their actions. It *was* common, however, for the dealers to rack a poor cider into a leaden cistern, and the use of white lead in correcting acidity was also well known amongst farmers and merchants alike.

The main source of contamination, however, was to be found in the machinery used in the manufacture of the Devon cider. There were two processes involved in preparing cider from the apples. In the first, the apples were ground in a pound, sometimes after they had stood a while to bruise and begun to rot. The crushed apples were allowed to stand a day or two, then, mixed with straw, they were put through the press and the apple juice, known as must, was collected into wooden casks to ferment.

The pound was generally made from two or more pieces of moor-stone cramped together with iron. A trough was cut in the stone in which the apples were crushed by a vertical granite disc operated by horse power. If the stones did not meet properly, the gaps in the trough were filled with lead. As the apples were ground they would immediately come into contact with the lead.

The presses were constructed of wood and operated by hand, but they too, could present a hazard. Of them Baker wrote:

It is common, in several parts of the county, either to line the cyder-presses entirely with lead, in order to prevent their leaking; or to make a border of Lead quite round the press, in order to receive the juice of the apples, and to convey it into a vessel, made of wood or stone placed underneath. And in many other places, where these methods are not used, it is common to nail sheet over any cracks or joints in the presses; and likewise to convey the juice of the apples from the presses in lead pipes (*Essay*, pp. 31-32).

(The general appearance of the presses and pounds is shown in Fig. 2.)

In Hereford, Gloucester, and Worcester, the construction of the pounds was, in general, similar to those used in Devon, but no lead was placed where it might come into contact with the apples or the expressed juice.

John Wall told Baker that he had recently had two or three patients in

the Infirmary with the Colic due to their having drunk cider made in a press covered with lead.

But this fact of a Cyder-Press covered with Lead is a singular, and perhaps the only, Instance of this Kind in this Part of England. It happened in a Part of the County . . . where very few apples grow; and the Bed of the Press being therefore cracked by Disuse, the Sagacity of the Farmer contrived this Covering to prevent the Loss of his Liquor. In general the Cyder-drinkers with us are healthy and robust; but for the most part lean.

Baker had established to his own satisfaction the probable cause of the Colic and the reason for its localization. He now had to put his hypothesis to the test, for, as he wrote elsewhere: 'He is an enemy to improvement and is no Philosopher, who fastidiously, and upon mere speculation, rejects what he has not brought to the test of experiment. One solid practical observation is of more real value, than all the unsupported theories, which the wit of Man ever invented.'<sup>20</sup>

In October 1766 he was at Exeter and obtained some must from the cider press at Alphington which was entirely covered with lead. He examined this for its lead content, taking a further sample and some cider made the previous year back with him to London in a stone vessel. These he proposed to examine with the help of William Saunders, 'being unwilling to come to any decisive conclusion, solely on the authority of my own trials; more especially as I had been under the influence of a preconceived opinion.'

Saunders was at the time only twenty-three and it was typical of Baker, who was well known for his generosity to the new members of his profession, to choose a young man just starting his career to help him. Saunders had been born in Banff in 1743 and educated at Edinburgh where he was the pupil and friend of William Cullen. He graduated M.D. in 1765 and on arriving in London proceeded to lecture on chemistry and pharmacy at Covent Garden. His *Syllabus of Lectures* was published in 1766 and this may have commended him to Baker. He evidently held views which were sufficiently similar to those of Baker to be attractive to the latter.

Experiments of whatever kind are of themselves useful [wrote Saunders] for altho' they may bring no other advantage, yet they free the mind from prejudice; they illucidate what would otherwise be obscure; they distinguish doubt from certainty; they restrain the licentiousness of fiction; they give entertainment from

20. G. Baker, *An inquiry into the merits of a method of innoculating the small-pox, which is now practised in several counties of England* (London, 1766), p. 4.

the variety of phenomena they exhibit; they excite the mind to farther tryals; and last of all, they extend the limits of science.<sup>21</sup>

Altogether Baker and Saunders performed six experiments on the cider, although in the essay only the first five were reported. The account of the experiments was slightly altered in the *Medical Transactions of the College of Physicians*. As reproduced here, the first two are as they appeared in the essay and the fifth and sixth are from the *Medical Transactions*.

#### *Experiment I*

A small quantity of Devonshire cyder being exposed upon clean paper to the fumes of the volatile tincture of sulphur, became immediately of a darkish colour, approaching to black. And we could only imitate this colour by exposing a dilute solution of *saccharum Saturni* to the same fumes. A small quantity of cyder, made in the county of Hereford, exposed in like manner to the same fumes, exhibited no such appearance, until a few drops of a solution of *saccharum Saturni* were added to it.

#### *Experiment II*

A small quantity of *Hepar sulphuris* (prepared by digesting together in a sand-heat one ounce of orpiment, and two ounces of quick-lime, with twelve ounces of water, in a closed vessel) being added to some Devonshire cyder, in a few minutes occasioned a darkish colour in the body of the liquor, approaching to black; and the whole became very opake. No such change was produced in the cyder of the county of Hereford, until a few drops of a solution of *saccharum Saturni* were added; when the same appearance, which was produced in the Devonshire cyder, was perceived (*Essay*, pp. 44-46).

The third experiment was essentially similar to the second, showing lead in the Devonshire cider and none in that from Hereford. In the fourth experiment some of the must from the Alphington press was examined. This 'treated in the same manner with the cyder, produced precipitates of a deeper black colour. This sufficiently shows, that the solution of Lead in the Must was stronger than that in the cyder.'

The last two experiments attempted to extract lead from the cider in its metallic form.

#### *Experiment V*

In order to put the matter entirely out of doubt, an extract was made from 18 common quarts of Devonshire cyder, of the preceding year, (first strained

21. W. Saunders, in the preface of *A new and easy method of giving mercury, to those affected with the venereal disease*, translated from the Latin of J. J. Menck (London, 1767), p. vii.

through a linen cloth) which had been in my cellar more than three months, but had been only a fortnight in bottles. This extract, being assayed with the black flux, a quantity of lead, weighing four grains and an half, was found at the bottom of the crucible.

#### *Experiment VI*

Two pounds of pearl-ashes were dissolved in water; and the solution, having been filtered, was added to three gallons of Devonshire cyder, drawn from a cask. The liquor became turbid, showing marks of a precipitation. It was then filtered; and the filtering papers, together with the powder, which was left on them, was burnt in a brass mortar, and afterwards burnt in a crucible, being stirred with an iron spatula in order to promote the union of the particles. The contents of the crucible were then exposed on a sheet of clean paper, and were carefully examined. Some very small globules were here found; which, being melted in a clean crucible, were reduced into one mass, which was evidently malleable lead. The quantity was little more than one grain (*Trans.*, pp. 227-229).

The first four experiments were repeated on several other samples of both Hereford and Devon cider taken both from the cask and the bottle. In only four cases were results obtained different from those described above: three bottles of Devon cider gave negative results and one bottle of Hereford cider gave a positive result.

The results of his experiments convinced Baker that there could be no doubt that the Devonshire cider was heavily contaminated with lead, in contrast to that produced in the neighbouring county of Hereford. Having proved the validity of his hypothesis, Baker felt sure that it would become widely accepted, although he was well aware that it might cause some ill-feeling amongst the cider manufacturers, but he did not feel that anyone would doubt that it was lead which caused the disease.

Only one question now remained for him to answer. How was it that all people who drank the cider were not equally affected by the disease; and why did some who drank heavily of the cider not become affected at all? Baker admitted that the problem presented difficulties and he felt that it could only be explained in terms of 'that remarkable idiosyncrasie, in which there is so remarkable a difference among men.' It is well known nowadays that lead can have widely differing effects on different individuals, some appearing to be unduly sensitive and some resistant to it. Baker was probably the first person to intimate that this was so.

The case was closed. With inexorable logic Baker had shown that the symptoms of the Colic were none other than those of lead poisoning; he



had shown that the Devonians exposed themselves to a peculiar lead hazard through the construction of their cider pounds and presses and finally he had clearly demonstrated lead in the Devonshire cider. And so he concluded his essay:

May I not presume to hope that the present discovery of a poison, which has for many years exerted its virulent effects of the inhabitants of Devonshire, incorporated with their daily liquor, unobserved and unsuspected, may be esteemed by those, who have power, and who have opportunities to remove the source of so much mischief, to be an object of their most serious attention? I have long lamented that a County, which is distinguished by some peculiar blessings, should likewise be distinguished by a peculiar calamity, as it were in consequence of its fertility. The subject therefore having appeared to me important, I have spared no pains in this investigation; and I am insured of my reward in the consciousness of having endeavoured to preserve my countrymen and fellow-creatures from one of the most dreadful disorders, incident to the human body (*Essay*, pp. 67-68).

#### PART III. OPPOSITION TO THE LEADEN HYPOTHESIS

'When I first undertook the unpopular task of exposing what appeared to me a most dangerous provincial error, I was well aware that much cavil and contradiction was to be expected.' In this Sir George Baker was not disappointed. The publication of his essay produced a storm of protest from the West Country and the argument which followed was, according to Martin Wall, 'combated with a Degree of Warmth, almost unaccountable to those, who deeming themselves uninterested in the Dispute, do not consider that the Opponents believed the Honour of their County depended on the Confutation of Sir George Baker's arguments.'<sup>22</sup>

It is not difficult to see why the farmers were so antagonistic towards Baker's views. In the early part of the eighteenth century the distillation of grain and the encouragement of cheap gin had decreased the demand for cider, and although after 1750 cider exports from Devon increased again, the cider tax imposed by Grenville's government in 1763 to help recover the cost of the Seven Years War dealt a serious blow to the cider trade. Many farmers grubbed up their orchards, and a large number of tracts were prepared against the bill, written by West Countrymen to point out the iniquitousness of the measure and the hardships likely to be caused by it. The bill was repealed in 1766 by Lord Rockingham's government, al-

22. Wall (n. 19), p. 304.

though it was not so much to ease the suffering of the West Country as to appease Pitt. But, whatever the reason, the repeal of the hated tax brought great rejoicing.

Thus, when Baker published what seemed to be a direct attack on Devon cider, the Devonians were in fighting mood. Thomas Alcock, already something of a popular hero on account of the tract he had prepared against the cider tax, once more felt constrained to put pen to paper, and he was supported by Francis Geach, a Plymouth surgeon. A third author, writing under the pseudonym of Danmoniensis, completed the triumvirate who entered the lists against Baker.<sup>23</sup>

Alcock was born at Runcorn in Cheshire in 1709. He lived for most of his life in Plymouth where he held the living of St. Budeaux, remaining Vicar for sixty-five years before returning once more to Runcorn where he died in 1798.

Francis Geach was the senior of the two surgeons appointed to the Royal Navy Hospital which had been completed at Stonehouse in 1763. He was elected a Fellow of the Royal Society in May 1767 and received his M.D. from Marischal College, Aberdeen, in 1782. He died in 1798 aged seventy-four.

Geach and Alcock issued a joint pamphlet in 1767 attacking Baker's leaden hypothesis. Geach's part of the work is unconvincing and shows signs of having been hastily written. It is probable that he undertook the writing of it at the instigation of the elderly Huxham with whom he was friendly and he merely reiterates the theories Huxham had put forward thirty years before.

Geach insisted that the Colic could not be due to lead in the presses since ladies and little children contracted the disease despite the fact that they drank little cider. Instead it was caused by 'the intemperate use of crude fruit and its juice not well fermented, and partly also to some peculiar disposition of the atmosphere. Warm autumns which ripen the fruits and render their juices more elaborate, are less productive of this disorder than cold and inclement seasons (p. 4).'

It was the difference in the seasons which accounted for the fact that the Colic was more prevalent in one year than another, for, if lead caused the

23. T. Alcock, *Cursory remarks on reading Dr. Baker's essay on the cause of the endemial colic of Devonshire* (London, 1767) and F. Geach, *Some observations on Dr. Baker's essay on the endemial colic of Devonshire* (London, 1767). These pamphlets were published together in one volume. Danmoniensis, *An answer to Dr. Baker's essay concerning the cause of the endemial colic of Devonshire, wherein the cyder of that county is exculpated from the accusation brought against it by that gentleman* (Exeter, 1767). These tracts are widely quoted from in the text that follows.

disease, then it would be expected that there would be no such variation, as the degree of contamination by the lead would presumably be constant.

Presenting his evidence against Baker's contention that the cider became impregnated with lead from the pounds and presses, Geach said first that *all* the apples were crushed in apparatus free from lead but then admitted that lead was sometimes used in the interstices of the mill stone but seldom in the groove where the apples were ground. Even so, he concluded, 'it is doubtful if one pound in fifty uses lead.'

And just in case this seemed an insufficient rebuttal to Baker's point, Geach put forward some evidence to show that even if lead *was* used in the cider-making apparatus, it occasioned no harm by citing the experience of a farmer of his acquaintance named Ward. This farmer had a mill in which lead was incorporated. The lead had fallen off at sometime and been crushed under the grinder and a great deal of cider had been made (and presumably impregnated with lead) before the accident had been discovered. This cider, however, produced no untoward results when drunk, indeed two of Ward's labourers drank twenty-two *quarts* of it in one day between them and suffered no ill-effects at all!

Having established, as he thought, that there was no lead in the apparatus, or if there was, it was harmless, Geach attacked the results of Baker's experiments on the grounds that his samples were not representative. The press at Alphington was claimed to have been unique amongst cider presses in having sheet lead nailed over it, and so to take must from this press as being a typical sample was likely to lead to results which were not applicable to Devonshire cider as a whole. Moreover, even the results themselves were suspect, for, said Geach, cider 'will become black by standing a short time exposed to the air, in a glass or silver vessel.' Therefore, the blackness Baker found on applying his test solution did not necessarily 'shew evident signs of lead,' but was merely the outcome of the cider having stood for a time in glass. The fact that Baker did not find the black precipitate in the cider until the test solution was administered to it, and that no such precipitate was found in cider which had stood as long but had not been tested, did not deter Geach from his argument.

Danmoniensis also disputed Baker's experimental findings by stating that when a few drops of a volatile alkali are introduced into cider which had been sulphured, a black precipitate is formed similar to the one Baker supposed to be caused by the presence of lead. In Devon, cider was commonly 'smeeched' by enclosing a lighted rag dipped in brimstone in a partly filled cask of cider. The rag was shut in the bung and did not touch

the cider, but when it was burnt out, the cask was well shaken and filled. It was the contention of Danmoniensis that the precipitate Baker found was caused by sulphur present in the cider; the Hereford cider on which Baker obtained negative results he assumed not to have been smeeched. The fact that the test solution itself contained sulphur and would therefore precipitate in both solutions was overlooked.

But in the event that Baker *was* demonstrating the presence of lead by his experiments on the cider, Geach could account for this also. It was the result of the fifth experiment on which Geach supposed the proof that the cider contained lead depended (although Baker himself does not seem to have given this experiment especial weight, except as a final proof) and to refute the validity of the result Geach introduced extracts from two letters written to Baker by Samuel More, 'an eminent chymist, in Jermyn Street.'<sup>24</sup>

More wrote to Baker saying that Saunders had visited him in October 1766 bringing with him some lead which he had extracted from the Devonshire cider. It was More's contention that this lead was either 'only the remains of some shot carelessly left in the bottles after washing; a thing not at all uncommon, though certainly, on all occasions, to be cautiously avoided [or the cider] was impregnated with lead, from long standing of the shot in the bottle, (which I am strongly inclined was the case).'<sup>25</sup>

From either cause, concluded More, 'such a particle of lead, as found in the assay, may be readily accounted for.'

To satisfy himself of the truth of this supposition, More conducted an experiment on some Devonshire cider which had been brought to London in a cask and had never been bottled. Although no lead could be detected in the original cider, when it had stood for five days in a glass containing two small pieces of lead, More's tests showed 'a convincing proof that some of the lead was dissolved in it, and a strong argument for laying aside the use of lead shot in cleaning glass bottles.'

Knowing of Huxham's interest in Baker's essay, More sent him copies of the letters, and these Huxham gave to Geach to include in his tract if he so wished.

24. Samuel More became Secretary of the Society for the Encouragement of Arts, Manufactures and Commerce (the forerunner of the Society of Arts) in 1798. He died in 1799, aged 74, 'truly lamented by every person who had the pleasure of knowing him.' (*Gentleman's Mag.*, 1799, 69, 909.)

25. Both Geach and Alcock have been credited with first suggesting that the lead Baker discovered in the cider resulted from shot left in the bottles and not contamination by the pounds. The author of Geach's obituary in the *Gentleman's Magazine* of 1798 says that Baker ascribed the colic to lead from the pounds, 'Mr. Geach to shot left in the bottles' (*Gentleman's Mag.*, 1798, 68, 259). Alcock is said to have refuted Baker's hypothesis by proving shot was left in the bottles (T. H. Williams and H. J. Johns, *Picturesque excursions in Devon and Cornwall* [London, 1801], p. 33).

Both Saunders and Baker gave a refutation to More's statements. Saunders<sup>26</sup> noted that Geach had suppressed that part of More's letter in which he wrote that he thought that the tests were made on the same cider as used in Experiment v, although Baker had informed More that it was clearly printed in the essay that the tests were carried out on several specimens of cider, all with consistent results. More was also told that both bottled cider and cider from the wood had been used. It was obvious to Saunders that this part of the letter had been suppressed deliberately so as not to weaken Geach's case. Saunders also noted that when he told More of the enquiry upon which he was engaged, More 'used many arguments, to dissuade me from persuing such an enquiry; and refused either to encourage, or even be witness to any experiment, which might end in discoveries in any way disagreeable to his very good friends in the County of Devon.'

More's letters were published by Geach before the second version of Baker's essay was printed in the *Medical Transactions of the College of Physicians*, and Baker was able to add a footnote to the experiment.<sup>27</sup>

As a doubt has arisen concerning the validity of this fifth experiment, I shall lay before the reader every circumstance relating to it. Two small granules of lead having been observed in a former extract, which was made from eighteen bottles of Devonshire cyder, it was immediately suspected, that these granules could be no other than *shot*, used for the purpose of cleaning bottles, and left in them by accident. This extract was therefore thrown away; and the cyder, used in the fifth experiment, was strained through a cloth, in order to prevent a possibility of any shot being found in the extract. Dr. Saunders informed me that, in the cloth through which the liquor had passed, there were two or three such granules of lead. Therefore in order to determine whether or not the cyder, which had been the subject of our experiments, owed its saturnine impregnation to this accidental cause, the tests were applied, in the manner before-mentioned, to some of the same cyder, taken from a bottle, which, we were sure, had not contained any *shot*; for the bottle was very carefully examined, after having been broken, in the presence of several gentlemen. The change of colour, on the application of the tests, was precisely the same, as in the former experiments.

However, in order to obviate all cavil, another experiment was made. The first part of this sixth experiment, namely what relates to the precipitation and filtration, was performed intirely under the direction and management of Mr. Hewson. The latter part was performed by Dr. Saunders, in the presence of Mr. Hewson, and of Dr. Rushton.

26. W. Saunders, *An answer to the observations of Mr. Geach, and to the cursory remarks of Mr. Alcock, on Dr. Baker's essay on the endemial colic of Devonshire* (London, 1767).

27. Baker (n. 13), p. 227.

The evidence suggests, therefore, that Baker was well aware of the presence of shot in the bottles before More pointed it out to him, and assays were clearly performed on cider uncontaminated by shot. The results in both essays are from these repeated experiments. Baker's experiments on cider from the wood leave no doubt that this contained lead from sources other than shot.

Danmoniensis had read the pamphlet by Geach and Alcock and he had an alternative explanation to account for the presence of lead in the cider. During plentiful years, apples were left in heaps out of doors and the farmers would keep birds off the heaps by shooting at them. The apples would inevitably come to contain a proportion of shot and Danmoniensis suggested that perhaps the cider on which Saunders' assays had been performed 'might be made from the Apples off which the Blackbirds had been killed.'

Alcock's part of the joint tract contained many points similar to those raised by Geach, although it was better constructed than Geach's. Alcock did not hide his reason for entering into debate with Baker.

I am afraid the principal point advanced in this essay will tend . . . to injure . . . our property; will either frighten the cities of London and Westminster, and other distant towns and places, as well as many of our own people, from drinking any cyder at all; or send them to Hereford and its adjacent counties, for all the liquor of this kind, which they may chuse to purchase. It seems as necessary, therefore, for the good of our West Country Plantations, to get this doctrine speedily refuted, as it was to get the cyder act repealed (p. 42).

Danmoniensis was also remarkably candid in declaring his interest in the matter: '. . . if the people in London believe that our Cyder has lead in it, they will be afraid to drink it. This Opinion will prevail according to the Esteem which Dr. Baker has acquired; let the Farmer, who offers his Cyder for sale, say what he can to the Contrary.'

Alcock found it particularly hard to believe that the Devonians, alone of the cider-makers, were not apprised of the hazards of using lead in their machines as Baker suggested they were. In fact, he doubted if the use of lead was as widespread as Baker said it was; the first lead-lined press he had heard of was at Alphington. And even where lead was used, the cider would hardly be in contact with the lead long enough for it to dissolve any of the metal. Besides, if the cider was constantly dissolving the lead at the rate indicated by Baker, why then did the lead never need replacing? Alcock's own troughs had lead in them and although it came into contact with the apple juice, the lead was worn off only by the friction of the wheel

and not dissolved. Alcock neglects to say how he determined this, and all three of Baker's opponents were unaware of the minuteness of the amount of lead which would be required to make the cider toxic and, similarly, they did not know that lead is a cumulative poison.

Alcock did not discount the possible dangers of lead, but he rejected it as the sole, or even the principal, cause of the Colic, instead he took refuge, as Geach did, in the acid theory of Huxham. The Devonian apples were more acid than those of neighbouring counties, he said, since the trees were planted closer together than in Hereford, Gloucester, and Worcestershire. Thus, much Devon fruit, 'not having equal advantages of sun and wind must be green and crude and the juice austere and sharp.' Also, Devon had an unusually damp climate owing to its geographical situation. The moist air 'stops up the pores of the body, checks perspiration; and throws the matter, that ought to pass through the strainers of the skin, inwardly on the bowels; and in conjunction with other causes, may produce an endemial colic [p. 41].'

The answer to Geach and Alcock was contained in a pamphlet by Saunders which was in the form of a letter addressed to Baker.<sup>28</sup> Baker himself did not issue another major work on the subject, although a Postscript was added to the essay as published in the *Transactions of the College of Physicians* and a short Appendix was also published in the same journal.<sup>29</sup>

Saunders evidently did not take the opposition seriously, nor indeed did the author who reviewed their tract in the *Monthly Review*. 'We take no notice of the vague observations and hypothetical reasonings which are scattered through this pamphlet. They are of no weight with respect to the point in question.'<sup>30</sup>

'This confederacy of authors,' Saunders informed Baker, 'comprises Mr. Geach, a surgeon, residing in the neighbourhood of Dr. Huxham and Mr. Alcock, a Reverend Ecclesiastic, whose studies, as it may be presumed, have had but a small connection with medical subjects.' Geach's part of the tract is referred to rather contemptuously as 'a dull, uninteresting, unin-

28. Saunders (n. 26). Some sources (W. Munk [n. 10] p. 399, S. Wilks and G. T. Bettany, *A biographical history of Guy's hospital* [London, 1892] p. 110) state that Saunders wrote two pamphlets, one in 1767 and one in 1768. It is almost certain that he wrote only the one referred to here. The *Monthly Review* reviewed all the pamphlets written during the course of the controversy in a number of articles and only the tract under discussion here was mentioned as coming from Saunders. A correspondent in the *Gentleman's Magazine*, writing under the name Devo (*Gentleman's Mag.*, 1798, 68, 305-306) states that he has all the pamphlets relating to the Colic and specifically refers to one pamphlet by Saunders.

29. G. Baker, 'An appendix to Dr. Baker's inquiry concerning the cause of the endemial colic of Devonshire,' *Med. Trans. Coll. Phys., Lond.*, 1768, 1, 460-469.

30. *Monthly Rev.*, 1768, 30, 18-19.

structive repetition of a vain unsubstantial theory, together with an addition of much false reasoning, from his own private stock, with an abundance of misrepresentation.' Geach relied so entirely on the authority of Huxham that he allowed it to interfere with his critical faculties. 'I have seldom met,' said Saunders, 'with observations more uncandid, or arguments less conclusive, than what here occur.'

Having disposed of Geach's arguments in general, Saunders proceeded to dissect each point in particular which he did with evident scorn, lacing his criticism with much wit. He then passed on to Alcock's remarks, 'rather more sensibly written than the observations of Mr. Geach.' He added little new in this part of his reply, and after a point-by-point refutation, he concluded: 'We pay adulation to no *ipse dixit*; nor do we allow that the authority of any man whatsoever, can stamp infallibility on opinions which are not established by experiment.'

Saunders' pamphlet, however, was not the end of the matter, for both Geach and Alcock retaliated with tracts answering it.<sup>31</sup> Geach's second pamphlet was a considerable improvement on his first. He was angry with Saunders' attack upon him, and the tone of his pamphlet was noted by a contemporary reviewer: '. . . we must observe that both Dr. Saunders in his answer, and Mr. Geach have a great deal of the acid. . . .'<sup>32</sup>

In his reply to Saunders, Geach abandoned his position that there were few, if any, lead pounds and presses in the county, and his intention was, instead, to show the innocuousness of such machines. To do this he included in his text the results of some assays made on cider taken from lead presses—those in Alphington and Sharpham—by William Cookworthy, a druggist in Plymouth, but best remembered as being the founder of the British porcelain industry.

In his experiments for Geach, Cookworthy prepared an extract from five gallons of cider and was unable to produce from it 'the least particle of lead.'

It is difficult to explain away these results of Cookworthy's. They are seldom mentioned in accounts of the Colic, and if they are, they are usually discounted.<sup>33</sup> Yet it is unlikely that Cookworthy's methods were less ac-

31. F. Geach, *A reply to Dr. Saunders' pamphlet relative to the dispute concerning the Devonshire cider* (London, 1768). T. Alcock, *The endemial colic of Devon, not caused by a solution of lead in the cyder. A particular reply is here given to Dr. Saunders' answer to cursory remarks with some further remarks on Dr. Baker's essay on that subject* (Plymouth, 1769). Both tracts are quoted from extensively in the following text.

32. *Monthly Rev.*, 1769, 40, 126-127.

33. In his account of the Colic, R. M. S. McConaghey (*Med. Hist.*, 1967, 11, 345-360), for example, states merely that Cookworthy *professed* to find no lead in the sample.



curate than Saunders', and his integrity is beyond dispute. He was much appreciated by his contemporaries as a man of science and it is not likely that he would deliberately falsify his results for the sake of discrediting Baker's hypothesis. If, in fact, the cider contained no lead, and assuming his tests were sensitive enough to detect it, then either Baker's hypothesis was based on false experimental results, or Cookworthy had not received cider taken from leaden presses, for both men could not be right. It is perhaps possible that Geach was so incensed by Saunders' attack on him that he stooped to misleading Cookworthy as to the origin of the cider, but he would necessarily have had to implicate those whose witnessed letters accompanied the samples. It must be noted that the witnesses were not impartial in the matter. Cockey, who sent one sample, had written to Geach: 'I am afraid, if Doctor Baker's pamphlet should be credited by the purchasers of the Devonshire cider, it would be to the greatest prejudice to the county.'

At this distance, however, it is impossible to draw firm conclusions, but, however obtained, Cookworthy's experiments were a powerful weapon in Geach's armoury.

Geach was not alone in having taken offence at Saunders' pamphlet. Alcock, in his rebuttal of Saunders' points, refers to him as 'a Mercenary in War, committing Outrages on Characters which the Principal himself would have treated with more levity,' and he proposed for Saunders a new job. After noting that it would be difficult to sell any cider on the London market, Alcock says that he will 'appoint Doctor Saunders Assay-Master of our Cyder, with an allowance of all the Lead he can pick out of it for his Salary [p. 19].'

In his long defence of the Devonshire cider—the tract contained 141 pages—Alcock repeated and enlarged upon the arguments he had used in his earlier work, although this time, he too had performed a few experiments.

I put a small Piece of Lead into a Glass of Must, taken immediately from the Pound. And notwithstanding the Lead was here so considerably more in Proportion to the Liquor, than it ever is in any of our Troughs or Presses, yet on the Application of the usual Tests, not the least impregnation of Lead could be discovered in the Space of Three Days: A Space of Time much longer than the Must continues either in the Trough, or in the Press.

Alcock summed up his arguments against Baker's hypothesis thus:

There is very little Lead in the Pounds and in many there is none, the Must

flows down from the Press very quickly and is too slow a Solvvent (if indeed it is a Solvent of crude Lead at all) to be harmful. The Lead in the Troughs appears to suffer no loss of Lead, the Lead not being replaced in 100 years. However I mean not by any Thing here said, to plead for the innocuous Nature of Lead. We cannot be too careful in the Use of that Metal. Cyder fermented, or boiled, or left to stand in Leaden Vessels, would be very hurtful.

Nobody disputed with Dr. Baker the perniciousness of lead, We should take Care to profit by his Advice so far, as never to admit any Lead (or as little as possible) to come into contact with the Fruit, or Juice, in the Implements of Cyder making (p. 81).

An advertisement to this effect would have been very beneficial, but by his essay

a very great and needless Expense may be incurred, by breaking up old Pounds, and erecting new ones; many of our People, who used to drink Cyder, are alarmed, strangers are frightened; et ni Fama mendax, Devonshire Cyder, notwithstanding the late great scarcity, it is become a mere Drug at the London Market, and almost all Orders for that Liquor, are sent to Hereford, as I foresaw and predicted (p. 82).

The opponents of Baker's hypothesis had challenged him on all points. They had begun by attempting to show that few if any of the pounds or presses contained lead, but it is evident that as they researched more deeply they discovered that, in fact, very many of them *did* contain lead. Not to be dissuaded, they next presented evidence that the cider from such leaden pounds did not cause a greater incidence of Colic than that from those without lead, because, as their experiments showed, the liquor from the pounds did not contain any lead, as Baker had suggested. Finally, Alcock attempted to overthrow Baker's explanation for the absence of the Colic in other cider counties. Baker had attributed it to the use of cider-machines free from lead, but if it could be shown that lead *was* used in the neighbouring counties, then the whole of Baker's argument would be open to doubt. A friend had written to Alcock to say that he had learned from a native of Hereford that it was the usual practice in that county to line the presses with lead, and that these presses were particularly common in that part of the county near the borders of the Wye. Alcock himself had found a lead-lined press in Ledbury in Herefordshire and in his travels through Worcestershire he had seen troughs incorporating lead at Malvern and he had learned that it was general practice to use lead in their construction in that county also. It was evident to Alcock that this part of Baker's thesis, like the others, was not founded on fact. He considered that he had shown

there were sufficient lead-lined presses in the other cider counties to show that Baker's explanation concerning the differences in the frequency of the Colic in Devon and the other counties was not valid.

Alcock's tract was not without influence. The *Monthly Review* had noted after the publication of the tracts up to and including Saunders' that '... we have given our readers the present state of the facts with respect to the lead in the Devonshire cyder apparatus, and its effects in producing the colic; and upon the whole, we apprehend, that the balance is considerably in favour of Dr. Baker.'<sup>34</sup>

In reviewing Alcock's pamphlet, however, they changed their opinion. 'Upon the whole we think the accusation which has been brought against the Devonshire cyder, is rather plausible, than supported by the clear authority of facts.'<sup>35</sup>

For his part, however, Baker never considered his hypothesis incorrect. He had not stated that the lead *only* came into the cider from the pounds and presses, but that several causes might result in its contamination, although that from the apparatus was the most potent. There was no doubt in his mind that the Devonshire Colic was no other than lead poisoning and he wrote:

In fact, every step, which I have taken in this Inquiry, has tended to confirm and illustrate my idea, that the malady, in question, owes its origin partly to a variety of accidents, and partly to fraud, causes which will easily be obviated, when once men have divested themselves of those prejudices, by the means of which that plain and direct path, which leads to truth, has been darkened and obviated; and an error, most detrimental to society, has become inveterate (*Trans.*, pp. 468–469).

#### PART IV. FURTHER LEADEN HYPOTHESIS: JAMES HARDY

In most of the accounts given of the Devonshire Colic, the narrative concludes with George Baker, and the implication is that his findings were generally and widely accepted, albeit reluctantly, with the result that the disease rapidly declined. The evidence is, however, that contemporary opinion was by no means unanimous in considering Baker's case proven. A contributor to the *Monthly Review* wrote in 1778:<sup>36</sup>

Readers cannot be unacquainted with the controversy between Sir George Baker

34. *Monthly Rev.*, 1768, 30, 19–20.

35. *Ibid.*, 1769, 41, 363–364.

36. *Ibid.*, 1778, 59, 67–68.

and Dr. Geach and Mr. Alcock. . . . The testimony adduced by Geach and Alcock against the possibility of . . . an impregnation, in many instances where the colic had appeared, seemed rather to give a preponderancy to their side of the question.

Martin Wall, in 1780, wrote on this question that 'the Belief of many unprejudiced Persons was suspended.'<sup>37</sup>

It was in an attempt to settle the issue conclusively that James Hardy, a physician at Barnstable, re-investigated the cause of the Colic and, as a result, put forward another leaden hypothesis in 1778.<sup>38</sup>

He began his enquiry with a general review of all that had gone before, including an account of the recent controversy between Baker and Geach and Alcock. Hardy considered that Baker had totally overthrown Huxham's acid theory and further, he considered that Baker had discovered the 'real general cause of the disease' that is to say, lead poisoning, but that he was wrong in assigning the mode of contamination to the lead used in the pounds and presses. Referring to the arguments used by Geach and Alcock to refute Baker's theory, Hardy writes: 'It must be acknowledged, these two gentlemen . . . proved beyond all contradiction . . . that the general cause of the endemial colic of Devonshire could not arise . . . from any particles of lead conveyed into the cyder from the pounds or presses [p. 33].'

Geach and Alcock may not have shown to everyone's satisfaction that the cider-making machinery was free from lead, but, be that as it may,

. . . the quantity of lead, stated to be found from the results of Sir George Baker's last cited experiment, was so very inconsiderable, that many persons were disposed to doubt, if it could be of any material injury; or, it is was equal to the effects produced, and observed in this colic (p. 34).

The reluctance to accept the harmful effects of lead in what must have seemed such trivial amounts was a fault with all Baker's critics. The amount of lead recovered from the cider in Experiment v is equivalent to 14.25 mgm/litre and in Experiment vi to 4.75 mgm/litre. Whilst these figures can in no way be interpreted as precise, they indicate a degree of contamination which nowadays no one would consider anything but dangerous.<sup>39</sup>

37. Wall (n. 19), p. 305.

38. Hardy (n. 1). The text of this tract is widely quoted in the section below.

39. A case of poisoning from drinking home-made wine contaminated to the extent of 7.5 mgm lead/litre (very nearly the mean of Baker's findings) was reported comparatively recently. (T. R. Whitehead and A. P. Prior, 'Lead poisoning from home-made wine,' *Lancet*, 1960, 2, 1343-1344.)

Hardy presented another piece of evidence to suggest that Baker may not have won contemporary opinion to his side. He writes that as a result of the controversy, 'less regard was paid to his publications than what the great learning and valuable observations with which they abound, most entitled them to.' These publications were the papers on lead poisoning published in 1768 subsequent to the essay on the Colic.<sup>40</sup>

Being convinced that the Devonshire Colic was a manifestation of lead poisoning, but equally sure that it did not arise from contamination by the cider-making apparatus, Hardy looked for another general cause. He found it in the glazed earthen jugs which were universally used by the lower classes in the county. The amount of lead used in the glaze was considerable, one ounce of lead-ore being used in each quart of glaze. Following in Baker's footsteps, Hardy devised a series of experiments to test his theory.

In all, twenty-five experiments were conducted in which Hardy studied the effects of standing and boiling liquids in a series of vessels, glazed and non-glazed earthenware as well as metal. In addition, the effects of adding lead and arsenic to wines was noted.

His technique was to apply a Test (a solution of orpiment and quicklime) to the liquids under inspection and compare the degree of precipitation obtained with that obtained (if any) when the Test was applied to a control solution. The following experiment illustrates his method well.

#### *Experiment I*

A quart of must, fresh from the pound, stood in a glazed earthen vessel, without being agitated, six hours. Upon the application of a few drops of the test to a glass of the must, a reddish cloud was produced. After standing nine hours, the like application produced a deeper cloud. After standing twelve hours, the cloud was yet more deep: and in a little time, the must became opaque. After twenty-four hours, a deep, almost liver-coloured cloud was produced; which, on being stirred with a small piece of wood, instantly occasioned that colour through the whole.

No adulteration whatever was produced by the addition of the like quantity of the test to a glass of the same must, which had been preserved in a bottle (pp. 47-48).

The results of his experiments showed Hardy that both must and cider absorbed lead from glazed earthenware vessels and that the uptake of lead was quicker if the solution was boiled or stirred than if left to stand. Several solutions were boiled in leaden vessels, including grape water, honey water,

<sup>40</sup> Baker (n. 14).

and brine. In all these cases—not surprisingly—very heavy impregnation was noted. Finally, he bought a two-quart earthenware vessel which had been glazed but not fired and, after washing off the glaze, was able to extract rather more than fourteen drams of lead from the glazing solution.

Discussing the results of his experiments, Hardy concluded that ‘the general cause of the endemial colic of Devonshire, is by them clearly demonstrated.’ The universality both of the use of glazed earthenware vessels and the drinking of cider explained why the cider drinkers suffered from the Colic. The results also explained why one cider drinking family might contract the Colic and others not. Those who had the disease used earthenware vessels, those who did not, used vessels of wood, glass, or stoneware. And Hardy could also provide the solution to the vexed problem of why the Colic was more prevalent in Devonshire than in Gloucester, Hereford, and Worcestershire. It was simply because only in Devonshire was the use of glazed earthenware so common. Finally, Hardy could account for the disease occurring more frequently amongst the poor than the rich. The poor drank a rough cider from glazed jugs, the rich a sweet cider which did not take up so much lead from the glaze and they more frequently used glasses to drink from than the poor.

It was a truly comprehensive hypothesis and to test it further Hardy proposed that ‘a certain course of experiments be tried on convicts, under approved limitations, sufficient to demonstrate the truth or falsehood, of what has been here advanced.’

This suggestion was bitterly opposed by an author in the *Monthly Review* who wrote: ‘We would be very unwilling to have the Faculty loaded with the Odium of racking a man with the Colic, or crippling him with the palsy or gout.’<sup>41</sup>

Hardy claimed no new discovery for himself, indeed he was anxious for his tract to be considered as a commentary and illustration of what had been advanced by Baker in his essay. Nevertheless, Hardy’s work is important and deserves a fuller part in the history of the Devonshire Colic than is usually afforded it. He was the first to investigate experimentally the contamination of cider by the lead from glazed vessels and by so doing he drew particular attention to a hazard which is in evidence even today. Cases of lead poisoning resulting from the use of glazed crocks in preparing home-made wine are occasionally reported from this country<sup>42</sup> and in

41. *Monthly Rev.* (n. 36).

42. C. Rickword Lane and A. Lawrence, ‘Home-made wine as a cause of lead poisoning: report of a case,’ *Brit. med. J.*, 1961, 2, 939–940.

some countries, the use of lead-glazed earthenware is common enough to give rise to a serious public health hazard.<sup>43</sup>

Hardy wrote one other tract in which he referred to the Colic.<sup>44</sup> The chief interest of this is in showing that Hardy, almost alone of all the persons concerned in the history of the Colic, had come to have sufficient insight to appreciate the harmful effects of minute doses of lead and to recognise that lead might exert a cumulative, chronic effect on the body, in addition to the more obvious acute effect.

If the 30th part of a grain of that wholesome mineral iron, diffused or dissolved in one pint of water, and taken daily for a few weeks, can produce the most salutatory effects, by its insensible operation on the human body; does it seem absurd or even difficult to believe, that one half of the quantity of a noxious mineral suspended or dissolved in wine, and taken occasionally for a number of years, should ultimately prove injurious? We cannot demonstrate by what means the inconceivably minute portions of the first mentioned mineral bring about their salutatory effects; yet no one doubts the fact because repeated observation has confirmed the truth: why then should we not extend the like mode of reasoning to the action of noxious minerals (pp. 21-22)?

There is some justification for saying that Hardy has not been properly treated when the history of the Devonshire Colic has been considered, and it is fitting that he should have the last words.

We have been theorizing about this disease long enough, we have advanced cold against heat, and heat against cold: acid against alkali, and alkali against acid; intemperance against temperance, and temperance against intemperance; for ages past, and were not one single point near demonstrating the real cause. Let us now quit this pleasing delusive path—this fairy land; let us at length have recourse to experiment, without which, I have authority to say, all theory is idle, however specious it may appear (pp. 181-182).

#### POSTSCRIPT

After the publication of Hardy's second tract, the argument concerning the cause of the Devonshire Colic ceased, and nothing more was written about the disease until much later, when it had become a matter for the historian to discuss. Whether the efforts of Baker, or of Hardy, hastened the decline of the disease is difficult to say. Baker was writing at a time

43. T. Beritić and D. Stahuljak, 'Lead poisoning from lead-glazed pottery,' *Lancet*, 1961, 1, 669.

44. J. Hardy, *An answer to the letter addressed by Francis Riollay Physician of Newbury, to Dr. Hardy on the hints given concerning the origin of the gout, in his publication on the colic of Devonshire* (London, 1780).

when the moor stone pounds were becoming outmoded and giving way to the mechanical iron engine, and it may well be that progress had anticipated him. Hardy's theory, whilst seeming to me compelling, was rejected out of hand by his contemporaries and thus would seem to have had little influence.

But for whatever reason, the Colic declined and after 1785 cases of the Colic were seldom admitted to the Devon and Exeter Hospital (see Appendix). Willcocks<sup>45</sup> wrote that Dr. Christian Budd saw an occasional case of the Colic arising in connection with the drinking of cider from leaden presses when he first began to practise in Devon in the 1840s. Budd is reported to have said that the deleterious effects of lead were then well recognised and lead presses remained only in remote places and amongst small growers. By 1885, when Willcocks was writing, all the leaden presses had gone, and the Colic was practically unheard of.

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#### APPENDIX

In the accompanying table an account is given of the number of patients admitted to the Devon and Exeter Hospital from 1768 to 1798 with Colic and with other diseases, the symptoms of which might possibly be due to lead poisoning, or which might be confused with that disease.

The figures are taken from Volumes IV, V, and VII of the Journal Books of the Devon and Exeter Hospital now in the County Archives in Exeter. The period covered by each volume is as follows:

Volume IV	July 1768–June 1777
Volume V	June 1777–June 1784
Volume VII	October 1791–September 1798

Volume VI is missing, and in the Journal Books before 1768 the patient's diagnosis was not recorded.

The figures show a considerable drop in the numbers of patients admitted with Colic compared with those supplied to Baker by Andrew. After 1784 the disease seems to have almost vanished.

45. Willcocks (n. 6).



	<i>Rheumatism</i>	<i>Paralysis</i>	<i>Palsy</i>	<i>Colic</i>
July-Dec. 1768	23	5	3	6
1769	54	21	16	4
1770	70	15	6	11
1771	81	18	12	8
1772	76	13	12	8
1773	78	6	12	5
1774	94	14	7	6
1775	104	17	4	2
1776	90	11	9	7
1777	73	20	13	7
1778	95	8	12	9
1779	100	4	27	13
1780	72	10	16	10
1781	80	9	10	11
1782	90	12	10	13
1783	99	7	11	3
1784	84	8	13	9
Jan.-June 1785	52	3	9	1
Oct.-Dec. 1791	30	5	3	0
1792	130	14	12	0
1793	94	22	1	2
1794	102	21	1	3
1795	100	23	1	0
1796	87	23	1	0
1797	98	13	1	0
Jan.-Sept. 1798	55	6	0	0