to return again to the back, should it be necessary, which, however, it seldom is.

There is another form of acute thoracic inflammation in which blisters of a smaller size are extremely useful; the local inflammations which often supervene in a tuberculised lung, around a cavern, or softened tubercles. If the patient in such a case is bled from the arm, or even leeches are applied to any extent, his strength is diminished by the loss of the vital fluid, and his few remaining days on earth proportionately diminished. One or two moderate-sized blisters over the region affected will generally suffice to subdue the inflammation, and that without much weakening the unfortunate patient, and thus precipitating his end.

I may resume the opinions which I have thus briefly expressed in the following manner:—

First. Both in non-inflammatory and in inflammatory diseases blisters do not give rise to as much general excitement as is generally supposed. Secondly. In the treatment of acute inflammatory diseases, especially those of the organs contained in the thoracic cavity, large blisters may be applied with perfect safety and great benefit, and are even more useful than in the chronic stages of those diseases. Thirdly. Their use is attended with but little pain, and never increases the inflammation, however near to it the blister may be applied. These assertions, as also those which I have made with respect to the influence of blisters over the urinary system, will be found to hold good in the very great majority of cases, but certainly not in all. No rule is without an exception, especially in therapeutics, and some few cases may present themselves in which counter-irritation may produce great general irritation, and perhaps do harm. I am, however, convinced that such cases are *excessively* rare. I must, also, in justice, state that although in severe acute thoracic disease the pain and annoyance of a large blister is but little felt, during the convalescence the patient often suffers considerable inconvenience from it. This, however, in my eyes, is but a trifling objection to the use of so powerful and so efficacious a therapeutic agent.

Cambridge-square, Hyde-park,
August, 1844.

ON THE NATURE OF NEURALGIA AND TIC DOULOUREUX.

By EDWARD BINNS, M.D., Hounslow.

IN THE LANCET of the 17th of August, 1844, there is a letter from Dr. Skae, of Edinburgh, claiming the anatomical theory of pain in neuralgia. This gentleman refers to a paper, by himself, in a northern periodical for 1840, in support of his claim. Other practitioners have also claimed this theory, among whom, if I understand Dr. Skae rightly, are Drs. Black and Wallis. Dr. S. says, "Dr. Black proceeds on the principle that the affected nerves, being contained in rigid canals, must be subjected to injurious pressure whenever their accompanying vessels are unusually distended with blood." And, again, according to Dr. Wallis, "upon this pressure depends the neuralgic paroxysm." (LANCET, August 17th, 1844.)

Now, I do not lay claim to this "discovery," or "theory," but I will relate what occurred to myself in 1828, when I was examined for my degree in Edinburgh. About the conclusion of the severe examination by each professor of the faculty individually, an examination that I am quite certain he who has once undergone would never voluntarily undergo a second time, for, independent of the knowledge required in botany, anatomy, medicine, materia medica, therapeutics, &c., the questions and responses were then put and made in Latin, and when I had begun to congratulate myself that all was going on smoothly, Dr. Monro, professor of anatomy, still alive, commenced his inquisition. Among many dissimilar questions, one of which was, which were perforations and which were not in the diaphragm, he asked me to define neuralgia. I did so in the usual way. The remedies for the alleviation or cure of the disease were next required, and, after enumerating opium, arsenic,

quinine (then a novelty), and what not, I added *cold*. This was very nearly fatal to me. The professor actually groaned and turned himself abruptly on the chair, resting his face on his right hand, as if the *idea* of my remedy had given him *tic douloureux*. As may be conceived, I was not a little alarmed. I had been under the fire of the faculty for more than an hour and a half, and was faint, it being an extremely warm day in June, nevertheless I timidly repeated that cold was a remedy. The professor seemed roused by the repetition, and asked me the question a third time. I replied again in the affirmative. He then inquired an explanation of the *modus operandi*? I immediately asked leave to give my reasons in English; to this request he kindly consented. I then said, that the object in applying cold was to repress or moderate congestion (upon this doctrine I do *now* insist, as I am only detailing what occurred, and Dr. Monro may probably remember the circumstance), because I imagined there was congestion of the blood-vessels surrounding the neurilema, which caused pressure on the nerve in its adit and exit from the osseous canal; or, in other words, I gave the same explanation, or theory of the pain, claimed by Drs. Black, Wallis, and Skae, which I am ready to believe is original to all, since it is so obvious in most cases. I believe this theory of the pain in neuralgia is founded on sound pathological principles. I have known the citrate of iron aggravate the pain most sensibly, while venesection, when all other remedies have failed, has produced decided good effects. One case I have met with recently,—it was that of a joiner and cabinet-maker, of the name of Leslie, who lives in my immediate vicinity. He had been suffering for many years under neuralgia in its most aggravated form; none of the usual remedies afforded any relief. He was bled at Christmas, *ad deliquium*, since which period his attacks have been less frequent and less severe. After venesection he was treated with arsenic (Fowler's solution) and citrate of iron. He is now, comparatively speaking, free from neuralgic attacks, but the disease, I regret to say, is not radically extirpated.

August, 1844.

MIDWIVES' MIDWIFERY.

By ROBERT BARNES, M.B., M.R.C.S.

I FORWARD to THE LANCET the following specimen of midwives' midwifery, not that I imagine such to be of rare occurrence, or hitherto unnoticed, but because that it is but discharging a duty to society to set on record proofs of the lamentable extent to which life and limb are brought into jeopardy by the culpable rashness of ignorant persons. It will be conceded that this duty is the more imperative at this particular time, when it is in contemplation to abandon a large proportion of the population of these realms into the hands of charlatans and impostors.

I was called up at a quarter past eleven, p.m., yesterday to go to a midwife engaged at a labour in the immediate neighbourhood of my residence. I found the patient tossing about on the floor in a state of great excitement, afraid that all was not going on well, and calling aloud for the use of instruments, having been in labour since early in the morning, although the midwife had "worked like a horse." On a rapid examination I found the head low down in the pelvis; there being time, I consequently lifted the patient on to the bed, in which proceeding I was resolutely opposed by the midwife, who loudly exclaimed against any interference with "her case." She boasted that the "hand had been down, and that she had turned the child." On the strength of this feat, and of having been twenty-six years a "*medical woman*," she was determined not to yield, although the friends had sent for me. I, however, insisted on making a more accurate examination. I then found the head on the perineum, a large mass of the umbilical cord protruding, and the expelling pains strong; there was, as might be expected, no pulsation in the cord. Having ascertained the state of affairs, I asked the "midwife" her opinion as to the nature of the case. "The hand had come down, and she had turned the child," was the re-