for going his round, and very frequently, owing to some cause or other, it is considerably past that hour before he makes his appearance, and as the demonstration commences at nine o'clock, it is evident that the time is very limited for the pupil to make his observations on the numerous and interesting cases that are in the wards.

Perhaps, Mr. Editor, you or some of your numerous readers will be kind enough to inform me what reason is assigned for Dr. Latham's paying his visit at so early an hour; for I am confident the majority of his pupils would prefer his coming in the middle of the day, in the manner of the other physicians and surgeons. I am, Sir, your obedient servant,

A MEDICAL PUPIL.

November, 1829.

CONFINEMENT OF SUPPOSED LUNATICS.

To R. BROWNE, Esq. one of the Metropolitan Commissioners of Lunacy, &c.

SIR,—In your letter to the editor of *The Times* of the 11th instant, you state that the law relative to the insane is as follows:—"That no person or persons represented or alleged to be insane, shall be put under any restraint by any individual (*not a relative, or a committee*), without a written order from an authorised person, and the previous personal and separate examination of 'two' medical practitioners of the state of mind of the patient."

I beg most respectfully to ask, if by the above law, a *relative* can put a person under restraint who is supposed to be insane, *without* the previous and separate examination of two medical practitioners, or without any examination by competent persons, of the state of mind of the patient?

If it be so, I conceive an individual, even in the present reformed state of the law, might be confined in the house of a malignant and rapacious relative, although in a perfectly sound state of mind.

I also beg leave to ask, to what extent the present act would affect a medical practitioner, in the event of his giving a *false* certificate, or one without previous examination of the patient. I should consider the heinousness of such an offence to be so great, that nothing short of transportation for life, or hanging, would be commensurate to the crime. The favour of an answer through the medium of *THE LANCET*, would oblige, Sir, your obedient servant,

AN INQUIRER.

ON THE PULMONARY CREPITOUS RATTLE.

By JOHN WHITE, Esq., M.R.C.S.
St. Neots.

AN important advantage attending the publication of *THE LANCET* is, that by its means, new opinions may be made known to every member of the medical profession. If the opinions be erroneous, they may be confuted by the very same means to which their publicity is owing. This latter circumstance will render unnecessary any apology from me, for disputing the correctness of the explanation of the crepitous rattle which is given by Laennec.

The description of the crepitous rattle cannot be given better than in the words of its illustrious discoverer, as translated by Dr. Forbes. I will add, too, his explanation of the cause:—"The moist crepitous rattle has evidently its site in the substance of the lungs. It resembles the sound produced by the crepitation of salts in a vessel exposed to a gentle heat, or that produced by blowing into a dried bladder, or it is still more like that emitted by the healthy lungs, when distended by air and compressed in the hand, only stronger. Besides the sound of crepitation, a sensation of humidity in the part is clearly conveyed. We feel that the pulmonary cells contain a watery fluid as well as air, and that the intermixture of the two fluids produces bubbles of extreme minuteness." Of this supposed presence of the two fluids, I doubt the correctness; and even if they were proved to be present in the pulmonary cells, I do not think their intermixture in such very minute quantities as must necessarily be contained in each cell, would produce the sound known as the crepitous rattle.

The crepitous rattle is invariably present in the first stage of pneumonia, and is supposed to indicate inflammation of the air-cells, attended with a secretion of a watery fluid. In every inflammation of a mucous membrane which comes under the cognizance of the senses, it is evident, that in the first stage of the inflammation, as suppression of the natural secretion occurs, and a consequent preternatural degree of dryness of the inflamed membrane. In proof of this circumstance, I need hardly mention the sensation of dryness and grittiness which is experienced in inflammation of the conjunctiva, or the still more common occurrence of the dryness felt in the commencement of a coryza or catarrh. But although these effects of inflammation of a mucous membrane are so constantly presented to our observation, it is supposed that the air-cells, whose lining membrane is mucous, no sooner become inflamed, than an increased secretion of fluid takes place from their surface.

Reasoning from analogy, is it not much more probable, nay, almost certain, that in the first stage of pneumonia, the air cells are in a preternaturally dry state, dependent upon suppression of secretion? If this be the condition of the membrane, it is obvious that the crepitous rattle does not depend upon the presence of a mucous or sanguineous fluid and air, which is generally supposed to be its cause. I am of opinion, that the crepitous rattle attends a preternatural degree of dryness of the lining of the air-cells; that it depends upon the friction of air against the internal surfaces of the air-cells, and upon the friction of opposite surfaces of the air-cells against each other, occurring during respiration. A sound similar to the pulmonary crepitous rattle, may be produced in the commencement of inflammation of synovial membranes, by gentle friction of opposite surfaces of the membrane against each other. In emphysema of the lungs, a crepitous rattle of a similar character to that of pneumonia is heard, which cannot be supposed to depend upon the presence of any other fluid than air. All these circumstances appear to support strongly the truth of my explanation; yet I must confess it is with diffidence I offer to the profession an explanation of the cause of the phenomenon, not only different to that given by the immortal Laennec, but different to that which has, I believe, been generally received by his followers. If my opinions are erroneous, it will be most satisfactory to me to have them confuted.

The sonorous rattle, it is probable, depends upon the passage of air through a bronchial tube deprived of its natural degree of humidity.

The sound described as resembling the creaking of new leather, is explicable by the condition of the membrane inflamed. It is heard at the commencement of pericarditis, at which time we may suppose, that the secretion from the membrane is suppressed; and that friction of opposite surfaces of the inflamed pericardium produce this peculiar sound.

Nov. 14, 1829.

MEDDLESOME MIDWIFERY.

To the Editor of THE LANCET.

SIR,—I trust you will permit the insertion of the following remarks upon a communication in your Number for October 31st, entitled "Cases Demonstrative of the Efficacy of the Ergot of Rye."

The article in question certainly proves the efficacy of the secale, but it furnishes remarkably instances of rash, officious, and

unnecessary interference with the parturient process. The exceedingly beautiful process of parturition is either not understood by Mr. Girtin, or, if understood, is lost sight of at a moment when it would be well to have it in mind. According to his own showing, the administration of the secale was unnecessary in the nine recited cases.

He seems unmindful of the benefit derived from the first stage of labour, and appears to view any other pains than "propulsive" as unnecessary. The partial subsidence, or total suspension of pains for a short time, seems to furnish a proper case for the ergot. The sharp lancinating pains are useless. The danger of hurrying the fetal head through an unprepared pelvis and an unyielding perineum, is forgotten.

I will briefly allude to each case, and appeal to the experienced accoucheur whether I am correct.

Case I.—"After four hours' pain, the pains subsided;" surely there is nothing very unusual in that—nothing that justified the ergot (except the accoucheur's anxiety to be gone). If the subsidence of pain be a good reason for giving ergot, it would be well to give a dose immediately on entering the lying-in-room, for the sound of the accoucheur's footsteps has generally the effect of putting a stop to the uterine action for a time.

Case II.—In this case, "the pains steadily improved from eight till ten o'clock, when they slackened, and were feeble and ineffective for three hours—in all, eight hours from the commencement. The ergot was given." Why, I cannot divine.

Case III.—Hæmorrhage to a great extent—no mention made of its cause. He must be a very bold, or a very timid practitioner, who dare trust to any medicine, where "a chamber-pot was filled with blood in an instant, and the bleeding continued in a mitigated stream till my arrival." It is right I should add, that "cold applications to the vulva had some effect."

Case IV.—In this case labour could scarcely be said to have commenced—that possible danger was to be apprehended from waiting? A few drops of liq. op. sedativ. would have put a stop to the inefficient pains.

Case V.—"The pains were sharp and lancinating, with much abdominal uneasiness;" in other words, they were dilating pains; that these pains are part of the simple parturient process, never seems to have struck your communicant. The ergot here was uncalled for.

* Drs. Burns and Hamilton allow twelve hours for the first stage.