

takes one grain of the acetate, in the form of a draught, at bed-time, and this was the commencing dose.

In conclusion, allow me to observe that I think it impossible that half a grain of this preparation can destroy life in an adult, unless there exists some idiosyncrasy of constitution, and am convinced, with Mr. Bree, that the bulk of two grains would prevent the possibility of its being used instead of half a grain.

The manner in which you have noticed the recent case in *THE LANCET* must be highly satisfactory to Mr. Arden and the profession generally; and, taking into consideration the circumstances attending the analysis of the draught intended for the deceased Mrs. Tweed, her friends may rest assured that death is to be attributed to natural causes, and not to the quantity of morphia administered. I remain, Sir, your very obedient servant,

GEORGE REES, Surgeon.

Fenchurch-street, Dec. 4, 1838.

*To the Editor of THE LANCET.*

SIR:—A patient of mine, an elderly lady, who suffers much from gastrodynia, takes every night the *muriate* of morphia in the quantity and form as follows. She has taken it for four years. She began with half a grain:—

R *Morphiæ muriatis*, gr. iss; *Pulv. per conf. arom.*, gr. x.; *Aq. pulegii*, ℥i. M. ft. h.

The same patient also suffers from severe strangury, when she takes, with instant relief, the following draught:—

R *Morphiæ muriat.*, gr. iij; *Mucilag. acaciæ*, 3vj; *Aq. pulegii*, 3vj. M. ft. h.

I have increased the dose to four grains.

You may make what use you please of this information. Yours obediently,

MICHAEL FOSTER,

Surgeon to the Huntingdonshire Infirmary.  
Huntingdon, Dec. 3, 1838.

## DR. THOMSON'S LECTURE

ON

## HYPERTROPHY OF THE HEART.

*To the Editor of THE LANCET.*

SIR:—In *THE LANCET* of last week (Dec. 1), you have published a letter, signed "A PUPIL," which contains a criticism on a report of a clinical lecture of mine, contained in *THE LANCET* of the preceding week. In justice to your reporter, I must remark that his statement does not appear to me to imply that I considered the case of Gardener as *cured*. It was not cured, although it was very greatly relieved, the poor man being

able to ascend the stairs of the hospital, and to take exercise, daily, without increasing either the impulse of the heart, or exciting such a degree of dyspnoea as prevented him, at one time, long after the ascites was cured, from walking the length of the ward without suffering. The case is marked in the hospital case-book, "Discharged relieved," and it never was my intention to affirm that elaterium, administered in any form, or in any combination, is capable of curing diseased structure, such as the *sawing* or *rasping* sound, in this case demonstrated to exist in the valves of the heart. The impulse of the heart was greatly allayed, and even the rasping sound was considerably less audible when Gardener left the hospital than it was long after the ascites was cured. Two patients, with simple hypertrophy, were last session dismissed in a much better condition than Gardener, arising from the beneficial influence of elaterium, administered in the manner adopted in his case. I remain, yours respectfully,

A. T. THOMSON,

University College Hospital,  
Dec. 4, 1838.

## CAUSES OF THE FAILURE OF THE VACCINE VIRUS.

*To the Editor of THE LANCET.*

SIR:—My brother vaccinators, in this neighbourhood, are in great perplexity in consequence of the failure, and daily increasing failure, of the expected effect of the vaccine virus. I suppose the same occurs in other parts of the kingdom. Permit me to offer a word of explanation on this point.

In Dr. Jenner's book, published in 1799, it will be found strictly enjoined that virus be taken on the fifth day, or the sixth, but on no account after the seventh; but what is the practice? I believe no one thinks of taking it before the eighth; at least I know it is the rule with all whose acquaintance I have; and I have been often supplied with it, by distant friends, in every case taken on or after the eighth day; in one instance, on the fourteenth. It may be said there is no vesicle on the fifth day. I know that virus taken late will not often produce a vesicle so early; but I know (and forty years pretty large experience might give weight to the assertion) that matter taken on the fifth day will invariably produce a good-sized vesicle on the fifth day, and that we shall not have to examine many inoculated arms, even when the matter used was as old as the eighth day, before we shall find one having a small quantity indeed, but most active, on the fifth day. But the objection is proved to be false and mischievous by the fact that Dr. Jenner says, take virus on the fifth day. We say there is none.