

although it might be one which I should not myself have originally selected, I should not change it if the patient were doing well.

In regard to the other question—namely, at what period of convalescence it may be safe to pass from the diet of fever to a more ordinary diet, I believe the best rule is that which is laid down by most authorities in the present day, and which I always practise—namely, to make no change whatever in the food until the temperature has never risen above the normal for ten days. I have never seen any harm done by keeping a patient a few days longer on the ordinary liquid diet, especially if its quantity has been increased, and if it is not necessary why should any risk be run at all, especially when it is remembered how serious a relapse may be? During the first day or two the risk of a relapse being produced by a change of diet is considerable, and it becomes less and less with every day of the post-febrile stage; it is less at the eighth day, and still less at the tenth.

As a rule I think it may be safely said that if the patient passes the tenth day the risk of relapse is extremely small. This, however, is only a general rule, and there are exceptions to it; thus I have lately seen a case in which the relapse occurred on the fifteenth day.

I do not think this question should be treated as an open one, as if it had not been seriously considered hitherto, for the current opinion is really the outcome of a multitude of observations conducted without bias over a series of years, and all the more trustworthy because no attempt is made to prove it by figures. So far as my own personal experience is concerned, I am led to endorse the current opinion with emphasis. I cannot but believe that if change of diet from liquid to solid food in the early days of convalescence, and still more before the fever has ended and convalescence commenced, become the routine practice, some of the individuals upon whom the experiment is tried will suffer, while in the end the conclusion will be that the current opinion is correct.

THE STIMULATING TREATMENT OF PNEUMONIA.

In the *London Lancet* for April 4th, 1896, was an interesting paper by Dr. Squire, strongly advocating the stimulating treatment of pneumonia, in which a hospital case was cited in illustration of its efficacy. The patient was "so far gone" that the physician in attendance gave him up as in a hopeless condition. The interne and the nurse, however, determined he should not die if they could help it, and, accordingly, plied him with brandy, as much as they could get down. The result was that he took in twenty-four hours thirty-two ounces of brandy, with decided benefit, and following up the treatment the man got well. A good abstract of the paper is published in the *Therapeutic Gazette* of August 15th.

This case brings very forcibly to my mind one under my care in the Massachusetts General Hospital some years since. The patient was a respectable young woman, eighteen years old, who at the time of her admission was suffering from double pneumonia, the lower half of both lungs being solidified. She was, of course, in a very critical condition