

our practice too much, but that is equally true whether they appear to support or to oppose any special line of treatment. One of Dr. Barrs' chief objections to the present mode of treatment is that the explanations given are to him unsatisfactory. They are equally unsatisfactory to me, and I believe them to be incorrect; but still, a wrong explanation does not affect the value of a fact.

I am confirmed by my own observation in the opinion generally held that too early and incautious a change of diet from liquid to solid is attended with a considerable risk of relapse. I have seen relapse follow so often that I cannot question the fact; and the relapses have occurred not only in cases in which solid food has been given somewhat prematurely, but even in cases where the change of diet has been from one kind of liquid food to another. Thus I have seen it follow, and I believe result from, the taking of orange-juice or grape-juice without the pips, just as I have seen it follow a plum bun or piece of Christmas pudding smuggled in. The risk that premature change of diet introduces is of a relapse—that is, of a recrudescence of the disease—with all its consequences, whether it be perforation, hæmorrhage, or what not. There is no evidence to show that the food itself induces perforation or any other morbid process except the relapse, and then all the accidents may occur which are met with in the original attack.

If it be a fact, as I believe, that a too early change in diet may lead to relapse, and the explanations suggested are unsatisfactory or wrong, what explanation is there? We cannot answer this question until we know why it is that any fever ever comes to an end spontaneously. We can at the present time only speculate as to the answer. As far as we can judge, the spontaneous limitation of these infectious fevers seems to be dependent upon very subtle changes produced in the body of a chemical or physical character by which either the germs are prevented from developing, or the poisonous products they produce neutralized. If it depends upon the production of antitoxins, bodies of very complicated chemical composition, it is quite a conceivable possibility that very slight chemical changes may so far modify them as to lead to their complete transformation or neutralization, or actually to stop their production, in which case the original toxins would produce their symptoms all afresh, and a relapse would occur.

There is some evidence to be produced in favor of these chemical changes to which I am referring. Some years ago, in the course of some observations upon the excretion of urea, I noticed the remarkable influence which an abrupt change of diet has upon the amount of urea passed. For instance, in a patient who had been placed upon a fixed diet for some time and was passing an average percentage of about 1.5 of urea the diet was changed, as, for instance, by the administration of a single egg, or something of that kind. The percentage ran up at once to 3.5, or even 4, and remained at that height for some little time. This was quite independent of the new article of food given, for if the same diet was continued the percentage of urea fell in the course of time to what it was before, namely, 1.5; or, if the offending article was withheld, so that the diet continued what it was before, the percentage still remained high for