demanded for putting them into execution. Sanatoriums undoubtedly help the non-tuberculous by teaching the tuberculous the social importance of sanitary behaviour, but their influence in lessening the mortality of the already tuberculous is by far the more important. This adverse comment is not a new question. Nor, again, is the infectious nature of tuberculosis any answer to the statistical evidence put forward by myself and others as to the importance of the hereditary factor. It should, however, be noted that Dr. Charles Goring found among criminals for tuberculosis a high relation between father and son, but scarcely any relation between husband and wife, and that I found in a different class two cases of transmission after marriage between father and son that Dr. Goring and myself recognized, and that between husband and wife, to see that infection due to environment is not the only factor. The old view “that consumption could be inherited” was verbally inaccurate, but practically sound in that the constitution liable to tuberculosis is inherited, and, while the tubercle bacillus abounds, the hereditary factor must be, as actual statistics show, of the greatest importance. It is all the more to be regretted that in the recent crusade against consumption it should be thought necessary to assert that:

We may now claim to have obtained certainty that this opinion [the importance of the hereditary factor] is correct, and that the complete silence of the children of consumptive parents is not born with the seeds of disease inherent in others, if, at all, only the greater risks of infection to which they are exposed by their surroundings (Times Leader, September 14th).

To “surroundings” must be added “and their hereditary constitution,” a statement consonant with the existing knowledge, or a basis for practical action with regard to tuberculosis.

In the very interesting correspondence which has appeared in your pages we have had much personal opinion and many impressions. All these are of value, but what we need is rather the presentation of statistical material relating to heredity and relating to mortality. The general conclusions arrived at by the latter are those of J. H. Draper and F. Dr. Pogge-Tomlinson, and both these contributions seem to me to show that the case for sanatoriums as based on lessened mortality is not proven.—I am, etc.

University College, London, W.C. Sept. 20th. K. PEARSON.

Sir,—I have been much interested in reading the correspondence in your columns relative to sanatorium treatment. My interest has been further stimulated by the perusal of a brochure, just published by the Department of Agriculture, Ministry of Agriculture, London, entitled: “A Third Study of the Statistics of Pulmonary Tuberculosis and Sanatorium Treatment,” by Messrs. Elderton and Perry. The general conclusions arrived at would appear to be that cases of pulmonary tuberculosis are as well whether untreated or treated. This is somewhat disappointing to one who is engaged in this kind of work, and, when the unsatisfactory nature of all other kinds of treatment can be so profusely distasteful to those who have had the misfortune to contract this serious disease.

There are, of course, many difficulties in the way of finding out the exact results of sanatorium treatment; but in an inquiry of this kind I think it would be well to give, first of all, a definition of what sanatorium treatment consists in. It is by no means uncommon to find what a few of us regard as the essentials of successful treatment practically omitted from the regimen of many sanatoriums.

I should like to suggest to the authors another inquiry, one in which they might be able to contribute to the chaff; one where it might be discovered which were the harmful and which the useful parts of sanatorium treatment. For all lines of inquiry tendency to expose the errors into which we may have been led by too much enthusiasm would be really useful.

I should like, also, to suggest that, as Dr. Pollock and Dr. Williams were experts in the diagnosis of this condition, that they might be able to supply superior diagnosticians to the ordinary general practitioner of the present day.

Further, there should be no need to go to Dr. Pollock and Dr. Williams for statistics of consumatorium days: most of the tuberculous at present live in presanatorium days, since only an insignificant minority of present-day patients get the opportunity of going to a sanatorium.

No one knows better than a sanatorium doctor that pulmonary tuberculosis may exist for years, and all practitioners of age and experience know that some persons affected with the disease make a good recovery in spite of the adverse conditions. This fact has been of well-known and fully utilized by quacks for many years.

Further, so far as my experience goes, and I have resided in five different sanatoriums, the patients are “carefully selected,” the ship being, with a few notable exceptions, that we seldom get a case unless it is going wrong, cases that are apparently getting well not being sent to a sanatorium, though they offer for a short time tolerant and frequent improve-

ment, when they have relapsed.

I see that some allowance is made for the presumed time the patient was ill before being admitted to the sanatorium, but I do not think this matter, which is all-important, is quite realized by the compiler of those statistics. I have as present in the sanatorium a man whose first symptoms appeared seventeen years since; it should surprise no one to find the patient cannot restore to this man his breathing apparatus.

A short time ago I discharged on the same day four men—none of whom had suffered from the disease for nine years, two for 8, six for 7, and one for 18 months. None of these men had been admitted to the sanatorium, and the week following I discharged a woman who had been ill for five years before admission. Some of these had during the periods mentioned enjoyed a fair measure of health, and all have supplied excellent material for presanatorium statistics of longevity. Unfortunately it appears more than probable that they will all die without applying the sanatorium statistics of non-longevity, for they were all with one exception third stages, and although much benefited it was impossible to supply them with new lungs.

One of the chief duties of a sanatorium is to prevent the formation of chronic invalids. A more fruitful inquiry would be to treat and leave untreated alternate cases, and to tabulate the results in tables. I am confident that the initiated will understand at their true value, and which tend to be quoted loosely as facts—we are once more in danger of being where we began before any real effort was made to treat these patients, and the old laissez-

faire method is likely to be continued for some years more.

Fortunately, until the appearance of so much adverse criticism we were tending to get more suitable cases into the sanatorium; but after the publication of the statistics which I have mentioned—which, I am afraid, few except the initiated will understand at their true value, and which tend to be quoted loosely as facts—we are once more in danger of being where we began before any real effort was made to treat these patients, and the old laissez-

faire method is likely to be continued for some years more.

At least the figures, so far as they go, tend to show the necessity of abandoning the position of cheerful optimism at present too often held with regard to early cases; when this position has been abandoned the doleful pessimism of the present surrounding treatment may at last be removed. It should seem self-evident that mild infected and destructive lesion treatment should be commenced at once, and that instead of months the importance of days should be remembered when the question of treatment is being considered.

New Cumnock, Sept. 10th.

Sir,—I have followed the correspondence on the sanatorium question with interest. The object in view in the modern developments of treatment of phthisis is surely to place the patient in such an environment that the anti-

bacterial agencies of the sanatorium can be stimulated to the highest possible degree of activity.

In a paper I read at the recent British Medical Congress, entitled “The Evolution of Racial Types of Europe, its bearing on the Racial Factor in Disease” (not yet published in your report), I pointed out that sanatoriums and “open-

air” treatment of phthisis is at present applied in an empirical manner, and that if the patient’s racial type or racial origin be determined and the conditions for this type or subtype evolved be studied, we should then have something approaching a working principle in the climatic or environment treatment of phthisis.

Dr. Pollock derives his clinic from the “Dolichcephalic Nordic” and “Mediterranean” types, and the “brachycephalic Oriental” and “Mediterranean;” but there is also almost certainly a strain of the brachy-
The treatment of tuberculosis is receiving a rather unusual amount of attention at the present time. May I venture to raise a question as to the soundness, or at least the usefulness, of the autoinoculation theory? For some years sanatorium physicians, and others who have the special care of consumptives, have, I believe, been influenced largely on a system of gradual inoculation or autoinoculation, an inoculation of small doses of the patient's own particular brand of tuberculin; and have rejected, almost contemptuously, the idea of inoculation with the commercially prepared article. Of course, I do not for a moment seek to deny that autoinoculation takes place. It furnishes a satisfactory explanation of the pyrexia met with in fairly advanced cases. But it is the so-called "exercise temperatures" which I venture to call in question. It is claimed that these are due to autoinoculations of tuberculin; that they therefore have a curative effect; and that they thus render any other tuberculin treatment unnecessary.

I know of no reason why the toxin absorbed from a patient's own tuberculous lesion should differ in its action from that in a tuberculin injected by some other authority.

Yet we know that small doses of tuberculin administered hypodermically either (a) produce no appreciable effect on the temperature; or (b) quickly induce immunity to similar doses; or (c) if in relatively large doses, have a rapid cumulative effect and eventuate in a very decided reaction, with ill effects which may last many weeks. Now, "exercise temperatures" do not show any of these phenomena. On the contrary, patients who take their walks of four, six, or eight miles, according to their strength, show day after day much the same exercise temperatures (probably about 37.5° C.). There is no sign of any acquirement of immunity. All one can observe is that as their legs grow stronger these people are able to walk farther. Again, in cases of a slightly more febrile type—people, for instance, who cannot walk a mile without a temperature of around 38° C.—any little excitement or mental effort or even standing for a few minutes will cause a rise of temperature. Is this also to be attributed to an autoinoculation? If so, one asks how is that brought about? and, once more, why does it not produce immunity, and how is it that the temperature rises and falls on these occasions so much more quickly than it would if it were due to a hypodermic dose of any kind of tuberculin?

The explanation I would suggest for all these slight rises of temperature (including, of course, especially the "exercise temperatures") is that they are due to fatigue or nervous action on the patient's part. They are unduly sensitive by the tuberculous toxins which are always circulating in the blood of consumptives. This seems to me to fit all the facts better than the usual hypothesis.

If this be so, then the graduated exercises, the importance of which is so much emphasised by sanatorium physicians, is only of use so far as any regular exercise improves the general health; but as a factor in the specific treatment of phthisis it must be regarded as a broken reed.

At present, indeed, I believe that this autoinoculation theory is standing in the way of progress. For, even if the effect of exercise as regards the production of curative autoinoculations be not so mythical and valueless as I conceive it to be, it must surely be insignificant in comparison with the systematic intelligent use of suitable tuberculin preparations. If any theory or prejudice were allowed to hinder any longer the proper use in this country of those valuable remedies which Koch has placed in our hands—I am, etc.,

E. M.

Sir,—My friend, Dr. W. Collier, whom I erroneously (British Medical Journal, February, 1905) believed to be with me instead of against me re the open-air treatment of phthisis, asks me some questions which I will try to answer in his usual patient.

Does Dr. Collier, or the advocates of this treatment, sleep in the cold, damp air themselves? I ask this...