CASE XIII.—W. H. (A 416 S), aged 76, has catheterized himself three times in each twenty-four hours for five years, very little urine passing naturally. The urine is clear, and the prostate is large and round. Lithotripsy was performed twice—April, 1897, and November, 1904. The first stone was pure uric acid, the second had a thick coating of phosphates; for five or six weeks previously he was advised to employ a catheter to withdraw 4 oz. of urine, which he did in a most careless manner without being taught. Cystitis naturally resulted. After the removal of the stone and proper instrumentation, he rapidly recovered and remains well.

CASE XIV.—G. C. (A 308 S), aged 78, has for fourteen years used catheter once in each twenty-four hours, urinating naturally three to four times; the prostate has not been examined recently. For many years he has had attacks of retention relieved by the use of the catheter. Urinary troubles invariably followed (cystitis, haemorrhage, and grave constitutional disturbance). Fourteen years ago he commenced habitual catheterization, and two years later adopted strict asepsis and has had no complication since. He was an old patient of the late Sir Henry Thompson.

C.—Those who have to Rely on the Catheter alone.

CASE XV.—F. S. P. (B 380 S), aged 63, has used catheter for seven years four times in each twenty-four hours. The urine, none of which passed naturally, is nearly clear, and the prostate was large and full. In November, 1902, he was in a very serious condition, with frequent rigors and a temperature varying from 100° to 104°. His tongue was dry and coated. Incessant distressing and ineffectual calls to pass urine with much pain. Instruments had been used recently for purposes of examination. There were 60 oz. of residual urine, which was thick and foul. Prostatectomy was urged upon him as absolutely essential; but he preferred to try treatment, with the result that he is now quite well, bicycling, walking, etc.

CASE XVI.—Dr. L. (C 312 S), aged 72. Catheter has been used three to four times each twenty-four hours for two years. No urine passed naturally; it is clear; the prostate large and soft. He adopted strict aseptic methods from the commencement and has never had any trouble.

CASE XVII.—G. D. L. (B 351), aged 73, for seven years has used catheter three times in every twenty-four hours. Urine clear but none passed naturally; the prostate is firm and extending upwards. He has boiled his catheter from the first. The prostatic urethra is long and offers some resistance to the catheter. At first he used it twice, but recently three times, a day, and has lost the power of micturition entirely. He is in excellent health and full of activity.

CASE XVIII.—G. W. P. (C 31), aged 77. Catheter used for seven years three times in every twenty-four hours; urine clear, none passed naturally; the prostate is large and round. He was properly taught to use the instrument by Dr. Beaumont of Norwood. At first he employed it twice, but recently, as the prostatic resistance increased, three times a day. He has sugar and albumen in the urine, and, with the exception of slight haemorrhage on two or three occasions, has remained well.

CASE XIX.—Dr. D. (B 170 S), aged 86, for nine years has used catheter three to four times in every twenty-four hours. Urine slightly cloudy; none passed naturally. Fourteen days before I saw him he had used a catheter night and morning; this produced greatly increased desire to micturate, pain, difficulty with the instrument, and slight fever. The urine was thick. Under proper aseptic treatment he gradually improved, and now, notwithstanding his age, is very well.

CASE XX.—A. M. (A 85 S), aged 96, has used catheter for forty years four times in every twenty-four hours. Urine nearly clear, though none passed naturally. An old patient of the late Sir Henry Thompson. Previously to 1902 he was much troubled with stricture and could only pass a No. 5 catheter to withdraw the urine, which he used five times in twenty-four hours. He had many troublesome attacks of orchitis, fever, urethral discharge, and other complications which frequently confined him to bed. In the latter part of that year he had an attendant to prepare his instruments by boiling, and he has had no complications since; even the stricture has yielded, and he can now employ a No. 7 with comfort. He is in wonderful health considering his advanced age.

The question, "When should prostatectomy be advised or undertaken?" has still to be considered. I am entirely opposed to the practice of operating as soon as the gland is found to be abnormal, and before any marked symptoms have arisen. The enlargement has been detected very probably by rectal or cystoscopic examination, and it is contended that its immediate removal, while the patient is in good health, will prevent troubles hereafter; this deduction appears to be based on insufficient evidence, for a large number of men go to the grave in advanced years with an enlarged prostate, which has never given any trouble, nor has its existence ever been diagnosed.

Prostatectomy should not be undertaken unless the symptoms present are definitely ascertained to arise from that organ, and from nothing else. Urinary symptoms are very frequently attributed to the gland when in fact they come from quite a different cause. For instance—and this, perhaps, is the most common mistake—a patient

complains of occasional retention, haemorrhage, pain, etc.; a rectal examination is made; the prostate is found to be enlarged and is straightway accused, without further search, of being the cause of the trouble. What is the result? During the operation for removing the offending member a stone is found in the bladder, which could easily have been removed by lithotripsy and the patient restored to health in a week, whereas he is now condemned to pass through an unnecessarily severe ordeal. In skilful hands the *x* rays will nearly always eliminate such errors, even when the sound has failed to reveal the true cause.

Prostatectomy should be deferred until after treatment by catheter has been tried. The patient may recover his power, or he may decide, when he knows exactly what to expect, to continue treatment. No harm can come of a delay of a month or more, and in septic cases much good will result, for the patient will improve and be in a better condition to bear the operation. Afterwards, if the catheter fails or is found too irksome, recourse can be had to operation.

When the patient is comparatively young, in good health, and has many years of life before him, operation is very rightly advocated as soon as it is proved that catheter treatment will not cure. The same may be said of those who, owing to their position in life, calling, or lack of funds, are unable to carry out the few simple details necessary in sterile catheterization. Prostatectomy undoubtedly is the best treatment when the prostate, by its mere size, has largely encroached upon the bladder space, and there is no residuum. The urine is clear and free from all signs of sepsis, yet the patient has constant and urgent calls to pass water both by day and night. Finally, the operation may be necessary in certain cases where the prostate is constantly bleeding and filling the bladder with clot which interferes with instrumentation and endangers the patient's life.

REFERENCE.

¹ *The Development and Anatomy of the Prostate Gland*

HOME CONDITIONS AND EYESIGHT.

By KARL PEARSON, F.R.S.,

FROM THE GALTON EUGENICS LABORATORY, UNIVERSITY OF LONDON.

It may not be without interest, in view of recent criticisms of a memoir by Miss Barrington and myself,* to publish some further determinations of the relation of home conditions to sight.

Before doing so it will be well to point out one or two important considerations which I think have considerable bearing on the manner in which statistics ought to be collected, having regard to the medical inspection of school children, which is now becoming universal.

The first point I would insist upon is that careful distinction must be made between (a) home environment and (b) school environment.

Our paper dealt chiefly with the influence of home environment on both refraction and acuity of vision. Now, as far as I am aware, the only material hitherto available for testing the influence of the actual home conditions on the presence of eye disease, on the goodness or badness of vision, or on shortsightedness, is that provided by the report of the Edinburgh Charity Organization Committee, where we have for the first time the sociological report on the home conditions placed alongside the ophthalmological report on the child's eyes, and accompanied in each case by the age of the child. I do not see how it is possible without such information to draw conclusions as to whether the home conditions do or do not affect sight. I am quite prepared to be shown that the conditions in Edinburgh are exceptional, but the proof can only be given when the children who are reported on elsewhere are followed up into their homes, and the state of these is recorded, as in the Edinburgh investigation.

Further, the Edinburgh material was of special value, because there is not the same extraordinary mixture of racial types in that city which is to be found in Glasgow or London. We are told, for example, that Russian Jews

* *Eugenics Laboratory Memoirs. V, A First Study of Vision and of the Relative Influence of Heredity and Environment on Sight.* Dulau and Co. 1909.

have in London a very high percentage of eye defect. Any one who has studied the copious statistics of Randal must be convinced that the degree and extent of myopia is markedly a racial character.* Those who have investigated any local class in this country know that in anthropometric characters it is usually significantly differentiated from any other local class. The population of our suburbs is usually more sedentary than the population in the working class districts of the town itself and less mixed.

I know, as a matter of fact, that the cephalic index of school children varies very sensibly from one district of London to a second. On this ground alone it is not possible, without control measurements, to assert that, because the percentage of eye defect varies from Whitechapel to Hampstead, the result is due to home environment. It may be so, but a mere statement of percentages in different districts without (a) age distributions, (b) racial proportions, and (c) percentages of defective parents in each district, will not convince those who desire logical statistical proof before forming any conclusion. It is well known that the defective parents also gravitate to the worst districts, and we may expect the defective children there also. For these reasons the work at Edinburgh was especially valuable. It gave age distribution, it dealt with a racially fairly homogeneous material, and it followed up the children into their homes and told us something, if not all we might desire to know, about their parents. I see no other way in which a real solution could be obtained for London. An individual school or two must have all—not only the defective—children examined, and the eyesight report must be accompanied by a sociological report.

A report such as that of Dr. A. Hugh Thompson on the London school children, in which the numbers at each age of both the normal and the defective children are not given, cannot help the statistician in the least to arrive at definite results. Nor, unless the children are followed into their own homes, is it even possible to say how far bad food, bad air, or parental neglect may account for the presence of any disease. There is always the diathesis as a contributory source, and the fact that all the children in the same family may not be attacked, shows that the variability within the family also plays its part.

Again my critics will, I hope, pardon me if I say that I am not convinced when I am asked whether this or that "is not a fact." For example, that it is a fact that in the better homes the children are more studious, or that the more respectable parents keep their children at home. Either may be the truth, but what we want are actual numerical measures of the effect of these supposed causes in invalidating the apparently small influence of the environmental as compared with the hereditary factor.

What we want are more data before we conclude that results are paradoxes because of such or such an explanation being "a fact" or "a matter of common experience."

Let us attempt to get any information we can on the problem of whether the better class parents keep their children at home, and so their offspring are more studious and suffer more from myopia.† Now, I know of no statistics which at all touch this point of the studious character of the offspring of better class parents, but I suppose the argument to be that the children are kept more at close work and that this damages their sight. Now, it would be possible to compare the sight of children of the same age who came to school at different ages originally, and thus find out whether those who began to read earlier have markedly worse sight. It would be very desirable to collect statistics *de novo* on this point. The only material that I know of bearing on the subject is that used by Miss Barrington and myself, and provided by Cohn, who gives the correlation tables of sight and years of school life, and sight and age. We have shown that the relationship is more intimate between sight and age than between sight and years of school life, and that, considering the high correlation of years of school life and years of age, the latter is most probably only a derived result of the former. Now, we have recently reworked Cohn's data and discovered the correlation between the ages at which the child began to read and the degree of

myopia at a constant age. The partial correlation co-efficient is:

0.04, if age and length of school life have a correlation of 0.8.
0.13, if age and length of school life have a correlation of 0.9.

Now it will be, perhaps, said that the latter is a quite sensible correlation, but compare it with the heredity correlation of 0.40 to 0.60, and we see that environment is quite overmastered by heredity. Furthermore, the correlation is the wrong way, the later the child went to school, the worse at a given age is the myopia. I have little doubt that the correlation in this case is sensibly zero, or the age at starting reading has little influence on the result. Cohn's statistics are, of course, not final, but they are very suggestive, because they have hitherto been supposed to prove the exact reverse of what is really extractable from them.

I now turn to the question of the effect of good or bad home environment on disease of the eye. I will consider first corneal nebula and illustrate by some of the percentages:

Feature of the Home Estimated.	Boys.		Girls.	
	Good.	Bad.	Good.	Bad.
Economic condition	3.6	2.3	3.9	2.2
Overcrowding	2.5	3.3	2.5	3.4
Physical state of parents	3.8	2.5	3.1	2.8
Moral state of parents	3.5	2.5	2.5	3.5
Means	3.35	2.65	3.00	3.00

Now it is extraordinarily difficult to lay any stress on results whose differences are of this order. A bad moral state of parents would be interpreted to mean less corneal nebula for the boys and more for the girls, the percentages being just reversed. Good economic conditions in both boys and girls would mean 1.5 per cent. more of the disease, and so would the good physical condition of the parents. In such circumstances what stress can be laid on the 0.8 per cent. difference between crowded and non-crowded homes? The only safe conclusion to be drawn is that in Edinburgh at least there is no marked relationship between corneal nebula and bad home environment. The curious point is that if we take all diseases of the eye and eyelid, we appear to get a slightly more definite, but still a quite negligible relation between disease and home environment. A difficulty is whether to include strabismus or not; the correlations have been worked out by Miss E. M. Elderton, of the Eugenics Laboratory, including and excluding "squint." In each a positive correlation means that there is more eye disease with the worse home condition.

Feature of the Home Estimated.	Boys.		Girls.	
	With Squint.	Without.	With Squint.	Without.
Economic conditions	0.10	0.14	-0.03	0.02
Overcrowding	0.05	0.08	0.04	0.12
Physical state of parents	-0.02	-0.02	-0.10	-0.15
Moral state of parents	0.04	0.03	-0.01	0.01

Now, it is quite impossible to lay any stress on these results. Six out of the 16 show less disease with worse home conditions, and if we have positive correlations of 0.10 and 0.14, we have negatives of -0.10 and -0.15. Further, no correlation of this kind lying under about 0.25 can be really considered to have definite importance. How very different they are from the values running from 0.4 to 0.6 found in our memoir for the inheritance factor!

It is worth while putting on record here the quantitative contributions of refraction and diseases of the eye in producing defective vision. The refraction results are taken

* The Jews, like the Germans, are largely brachycephalic, and the increasing brachycephaly of the town populations is a point not without suggestiveness for changes in eyesight.

† The reader of our memoir will remember that our conclusion was, not that the better homes produced more myopia, but that there was no marked relationship between bad homes and defective sight.

‡ This category should a priori be considered the most important one in this matter. It marks essentially neglect of the children. Where the home is practically a brothel or the parents habitual drunkards there can be no proper cleanliness or care of offspring.

from our memoir; the disease results have been recently worked out by Miss Elderton.

Correlation of Keeness of Vision with Refraction and with Disease.

	Keeness of Vision and Refraction. (Contingency.)	Keeness of Vision and Disease.	
		Squint Included.	Without Squint.
Boys ...	0.58	0.36	0.21
Girls ...	0.54	0.50	0.32

It will be seen that, if we exclude strabismus, the diseases of eye and eyelid do not produce half the effect on vision that refraction does.

Now I do not intend to argue for a moment that the Edinburgh data provide the final word in these matters, but they form the first solid attempt to provide information as to the influence of home environment. Neither the school medical officer nor, as a rule, the teacher can follow the children into their homes. All that has hitherto been done has been more or less plausible guesswork as to the relation of home environment to the health and intelligence of the child. I shall not be surprised to find, when further data are available, that the nation has for years been putting its money on "Environment" when "Heredity" wins in a canter. To say this is not to discourage all attempts to better defective sight in the schools or to check the intensification of myopia. But it would be foolish—in face of our increasing degeneracy—to neglect the possibility that only other methods can in the long run hinder the spread of defects. Better qualities, such as keen and strong vision, are no longer an absolute requisite for survival, and defectiveness, instead of meeting the stern judgement of Nature, is by governmental and charitable agencies supported to multiply its kind. The curative art in its tenderness for the individual may be disastrous for the race, unless it realizes fully the relative biological importance of heredity and environment.

MEDICINE OR SURGERY IN EARLY CASES OF CANCER.

By SKENE KEITH, M.B., F.R.C.S. EDIN.,
LONDON.

At the present time surgery claims the treatment of all early cases of cancer, and allows an experiment only when it has failed. I hope to show a fair case for medical treatment on an early case, after extensive experience gained by experimenting on hopeless ones has apparently rendered this permissible. I do not for one moment suggest a prolonged course of treatment in any suitable case for operation, but one limited to a few weeks only. When a case presents itself where the diagnosis lies between cancer and syphilis, it is considered good practice to clear up the diagnosis by giving antisyphilitic treatment—in other words, to experiment to a certain extent; and this brings me to the point I wish to make, why, as surgery is not altogether satisfactory, not experiment for two or perhaps even three weeks. This is, of course, allowable only after encouraging results have been obtained in hopeless cases. If this length of time would make any appreciable difference to the prospects of permanent recovery, if operation had eventually to be had recourse to, it would be a very serious matter to suggest such delay; but if the disease is growing so rapidly as to show much difference in that time, the case would be one of those in which return is certain to be rapid.

When an experiment has been carried on, it has always been difficult to say if there is any real improvement. All who have much experience with malignant disease know that a certain amount of temporary improvement can be gained in many different ways. If one can show that some definite change is caused by cancer, and that this change can be arrested by treatment, it seems to be reasonably probable that a right line for farther investigation has been arrived at. I believe that it is possible, perhaps probable, that the change which takes place in the blood in all cases of cancer when at all advanced may prove the

guide which is wanted. The changes are not confined to one element; the red corpuscles are diminished in number except when there is pressure leading to starvation, and are altered in appearance, being weak and more or less colourless; the haemoglobin is diminished; the white cells are increased in number, and are altered relatively, the polymorphonuclear cells being increased at the expense of the lymphocytes.

If a careful blood examination were made before every operation for cancer it might, and probably would, show that the cases in which there was no return were those in which the blood was normal or nearly normal, or where it quickly returned to a healthy condition after operation. As examination of the blood after operations shows in some cases a gradual return to a normal condition of the white cells, it is apparent that once the local disease is removed, the general condition it had caused may right itself. It is therefore reasonable to advise after all operations for cancer the treatment my brother and I recommend, or any other which has the effect of materially improving the condition of the blood, even when the actual disease is present. There is not sufficient information to enable one to say how quickly the blood should become normal after the whole of a cancerous growth has been removed; but as an illustration of what may happen I give the figures of a case in which the patient became, exceptionally quickly, well and strong after two abdominal operations.

The history was that a colotomy had been performed, followed by the excision of about 9 in. of the transverse colon. My brother saw the patient for the first time on November 4th, 1908, seventeen days after the second operation. He made a blood examination, and found 4,400,000 red and 30,000 white corpuscles per cubic millimetre. As no blood count had been previously made, we do not know if there had been any improvement in the seventeen days. There were 78.1 per cent. of polymorphonuclears and 21.2 per cent. lymphocytes. Treatment was begun at once. On November 25th the white cells numbered 15,000, and there were 68.8 per cent. of polymorphonuclears, showing that while the percentage was within the normal limit, the number of polymorphonuclears in the total quantity of blood was roughly half as many as there should be. On March 7th, 1909, the number was 8,000 and the percentage 66, the total number of the polymorphonuclears being thus normal. The red cells had been in excess since the end of November, the count always being over five millions. On May 4th the number was 9,000 and the percentage 67, again within the normal limit. The treatment consisted in fifteen injections, each containing soamin 1 grain, green citrate of iron and ammonium 10 grains, and saturated solution of sodium cinnamate 20 minims. The treatment was finished by the end of December.

Examinations made at intervals after operations might show that the disease was returning before local evidence could be made out. This would not of necessity upset the local theory of the disease, as there might be return internally.

If we take the condition of the blood as a starting point for treatment, we see at once that no one remedy is likely to bring it back to normal, and we must give remedies which will increase the number of red cells, and the amount of haemoglobin, while the white cells are reduced, and the relative numbers of the polymorphonuclears and lymphocytes are put right.

My brother and I have now been working for six years with practically the same remedies. They are arsenic, iron, and sodium cinnamate, all old-fashioned cancer remedies. The preparations we have found best are soamin, green citrate of iron and ammonium, and a saturated solution of sodium cinnamate. Soamin is sodium para-aminophenyl arsenate, and contains 22.8 per cent. of arsenic. Whether these remedies should be given together or separately we are not certain.

Beginning any new line of treatment, one must naturally limit oneself to hopeless cases, to those in which the death sentence has been passed. This is only fair, but after six years' experience with such cases and with the knowledge gained by their study, and more especially when treatment can be placed on a scientific basis, and when certain failure can be seen within a very short time by its want of effect on the blood, surely the time has come when one cannot be accused of great rashness when one advises treatment which is capable of correcting the condition of the blood caused by a cancerous growth, in an early case, and provided always that treatment will not complicate operation should it be necessary later. I was exceptionally fortunate in meeting with an early case lately