epileptic fit but no furor. Every fit that was not followed by a dose of chloral was followed by a fit of mania.

I have now expressed my belief, which may be summed up to be that chloral may produce physical ill health, hypochondriasis and insanity. It may relieve epileptic furor, but cannot cure epilepsy. It may produce sleep in some cases with advantage, but more commonly disadvantageously. It may be used as restraint rather than as treatment in violent cases.

Five Years of Statistics.—By P. Maury Deas, M.B. Lond., Medical Superintendent, Cheshire County Asylum, Macclesfield.*

The object which I propose to myself in this paper is twofold. The first is to lay before you briefly the general statistical results of five years, in regard to those who have been sent for treatment from the districts of Cheshire, to which the asylum is allotted; and the second is to give the results of some further experience, as bearing on the question of "Local Differences in the Distribution of Insanity," of which I adduced some illustrations in my Annual Report for 1873, and which were subsequently published in a separate form in the "Journal of Mental Science" for April, 1875.

In regard to the first division of my subject, that of the general statistics, I wish to explain at the outset that the plan on which I have drawn them up has special reference to the question of the increase of insanity.

In the address which I delivered on the Prevention of Insanity, on assuming the chair as President of the Society in November last, I said, while discussing the great and steady *apparent* increase of insanity, that "there are, at present, I believe, no reliable data for determining the important point whether the *incidence* of insanity is on the increase; that is to say, whether the number of *new* cases arising each year is increasing in a greater ratio than the population, although I think that it would not be difficult to set on foot a statistical enquiry in relation to the classes, at any rate, from which our County Asylums are fed, which would go far to settle the point."

At present there are very great divergencies of opinion in regard to this question of the increase of insanity; and if

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there is not much accurate knowledge on the subject, there are, at all events, strong assertions and dogmatic statements on both sides. It is not, I think, very creditable that such an important question should continue indefinitely to be at the mercy of vague impressions and inferences, and it is time, surely, to attempt to subject these to the test of numerical enquiry.

Imbued with this idea, I would venture, though with considerable diffidence, to suggest a method of drawing up asylum statistics, which, while possessing, in my opinion, other advantages as well, would afford, in time, fairly reliable grounds for answering the question—Is insanity on the increase in any particular district? At present there is a great mass and variety of statistics published every year in regard to insanity and the working of our asylums, but it is very questionable, I think, whether the general interest of these is at all equivalent to the labour bestowed on their preparation, or whether any safe and reliable conclusions could be drawn from them.

The principle which I wish to advocate is that the statistics should embody the history of the cases placed under treatment during a certain given period, so that all the facts and all the conclusions drawn from them may refer to the same set of cases. Each period would then stand by itself, and the results could fairly be compared. At present the total statistics are given for each year, but there is no unity running through them. The admissions, for instance, are those of the year, but the discharges and deaths include those admitted in previous years. In the same way the various statistical information given as to causes, duration of the disease, ages, &c., does not refer throughout to the same individual cases.

If statistics are framed on the principle indicated, and if, at the same time, the particular asylum is so situated as to receive *all* the cases from a certain defined district, we may then, I think, draw fairly reliable inferences from the figures.

The practical method which I would suggest, and which I shall presently illustrate, is that five years should be taken as the unit of statistical enquiry, and that at the end of twelve months after the termination of the five years, stock should be taken, as it were, and the results of treatment, along with other statistical particulars, summed up, as far as the cases admitted during the five years are concerned. Each quinquennial stock-taking would then not only afford grounds of

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comparison much more reliable than are attainable at present, but the actual statistics would possess an interest and value impossible under the existing hap-hazard system.

Five years I have selected as the unit, as being a convenient space of time; on the one hand not too extended, and on the other long enough to do away with some of the sources of fallacy attaching to short periods. The object of allowing twelve months to elapse before the figures are made up is that thereby the results of treatment in regard to recovery can be given almost absolutely, and the death rate will also thus pretty accurately represent the mortality from the direct effects of diseases of the nervous system, as well as from other diseases existing at the time of admission. Of course both these limits of time are open to criticism, and are merely suggested as "convenient."

Another point I would just mention: At the end of every five years there would, of course, be a certain balance remaining—a residuum—which would represent the amount of the tendency of chronic cases to accumulate in the asylum; and the natural check to this tendency, viz., the mortality among this residuum, during the next quinquennial period, would be the truest test of the *general* sanitary condition of the population. This death-rate could be kept distinct from the death-rate among the cases admitted during each five years, and by this means there is no doubt that a much truer standard of comparison would be obtained in regard to the mortality among the insane in different districts, and also in regard to the sanitary condition of different asylums.

In turning now to the statistics of five years in this asylum, treated in this way, I may say that we are very favourably situated here, inasmuch as all the pauper insane of one division of Cheshire, comprising a small number of large unions, are sent to us; and also from the asylum accommodation being ample, no cases are sent elsewhere for treatment, as is so frequently the case in other districts. In fact, there is but one source of fallacy; and that is the cases who may be detained for treatment in the workhouses, without ever being sent to the asylum at all.

If we are favourably situated in regard to this enquiry, as far as its completeness and accuracy are concerned, we are by no means so in regard to the preparing of the statistics, owing to the very large number of cases which we have from other counties, which renders the work of isolating those of our own county one of considerable time and trouble.

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The period embraced is from 1872 to 1876 inclusive.

The total number of admissions in these five years was 505, viz., 274 men and 231 women.

The first step necessary is to deduct from these the cases re-admitted during the period, amounting to 30 in all; 19 of the instances referring to men, and 11 to women.

These 30 include each *instance* of re-admission; the individuals being only 23. This matter of the re-admissions or relapses, is a very important one; want of due regard to it may cause quite an erroneous estimate of the actual *production* of insanity going on in a district. Seventeen men and six women were admitted twice during the five years; one man and one woman three times, and one woman no less than four times.

The net number of individuals under treatment is thus reduced to 475—255 men and 220 women.

But even this does not fairly represent the production of insanity in the district. To get at the number of strictly new cases, those must further be deducted who are reported to have had "previous attacks," in addition to those actually admitted more than once in the five years.

This number amounts to 55—18 men and 37 women.

Thus, out of 475 patients, it may be said that 78 had had more than one attack, or one-sixth of the whole number.

The actual number of entirely new cases is thus brought down to 421—237 men and 183 women. A noticeable point here is the large preponderance of the men over the women, the former exceeding the latter by almost 30 per cent.

There is also rather a singular point in regard to the relapsing cases. It is usually recognised that women are more liable to relapses, or to have subsequent attacks, than men. Now of the 23 patients who relapsed during the five years, 15 were men and only eight women. But, on the other hand, of the 55 patients who had had attacks at some former period, 18 were men and 37 women.

Taking both together, 33 men and 45 women had shown a tendency to relapse, which is in accordance with ordinary experience.

Perhaps an explanation of the apparent anomaly, if more than accidental, may be found in the fact that the majority of the relapsing cases among the men were due to drink: these recovering once, were admitted again, labouring under general paralysis or permanent insanity. In women, on the other hand, although there is a greater tendency to relapse, the attacks being more due to functional or moral causes, the

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prospect of recovery is greater, even after two or more attacks; and the attacks themselves may be separated by considerable intervals of time.

Before tracing how these 475 cases became disposed of in the course of six years, or by the end of twelve months after the last case was received, a few statistics may be given as to the types of insanity from which they suffered, and the causes of the disease, so far as they could be ascertained. In regard to the nature of the cases, 159, or exactly one-third, suffered from forms of insanity which practically must be regarded as forbidding all hope of recovery from the outset. The following table classifies these cases under four heads:—

	M.	F.	T.
General paralysis	50	6	56
Epileptic insanity	28	23	51
Insanity, associated with other organic brain diseases, or senile decay ...	22	13	35
Congenital insanity (idiocy or imbecility) ...	15	2	17
	115	44	159

The most striking point in these figures is the great excess of hopeless forms of insanity among the men, as compared with the women, caused largely, but not entirely, by the very large amount of general paralysis among our male patients, amounting to no less than one-fifth of the whole number admitted. For better comparison the above figures are thrown, in the following tables, into percentages on the total number of men and women respectively:—

	M.	F.
General paralysis...	20	3
Epileptic insanity...	11	10
Insanity associated with other organic brain diseases or senile decay ...	9	5
Congenital insanity	6	1
	46	19

To the above may be added cases of ordinary insanity, but in whom the disease was in such a chronic state on admission as to preclude all hope of recovery. These amounted to 30—10 men and 20 women.

The total number of cases, therefore, who admitted of treatment with a view to recovery is thus reduced to 286—130 men and 156 women; or, in the case of the men, about

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50 per cent.; in that of the women about 70 per cent. of the total number.

In regard to the important question of the probable causes of the malady, this is often a very difficult matter to ascertain with anything like certainty. Often the needful information is wanting, it may be from ignorance, or it may be intentionally suppressed. On the other hand, trivial and insufficient circumstances are not unfrequently alleged as the cause, or a fact is stated as the cause, which is really a symptom of the disease. Then again, more than one genuine cause may have co-operated in producing the attack. All this renders great caution necessary in drawing conclusions from statistics under this head. Still, with care, the potential cause can be assigned with tolerable accuracy in a very considerable number of cases; and results are thereby evolved possessing considerable general interest.

I have been able to assign causes, with a fair amount of accuracy, in 368 cases—195 men and 173 women.

The following Table exhibits the causes, arranged in certain natural groups:—

	M.	F.
Moral causes, including business anxiety and overwork	47	41
Intemperance...	46	19
Impaired health, or accidental injury ...	27	34
Puerperal and other conditions arising from disordered female health ...	—	29
Epilepsy	28	23
Other diseases of the brain and old age ...	22	13
Congenital defect, or hereditary predisposition	25	14

It will be observed that about one-fourth of the cases were due to moral causes, and three-fourths to physical, or constitutional causes.

The proportion due to the former class of agency is about equal in the two sexes; but among the physical causes it will be seen that intemperance was the cause in 46 men, but only in 19 women, while, on the other hand, impaired health, including disorders peculiar to their sex, was the cause in 63 women; but only 27 cases among the men appeared to be due to affections of the general health.

And now to follow out the history of the cases admitted during the five years.

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The following statement shows how they had been disposed of by Dec. 31st, 1877:—

	M.	F.	T.
Cases admitted	255	220	475
Discharged			
Recovered	98	119	217
But deduct for relapses	16	9	25
	82	110	192
Not recovered...	15	9	24
Died	93	47	140
Total removals	190	166	356
Balance remaining	65	54	119

The general result is that three-fourths of those admitted were removed by discharge or death, and one-fourth remained as a residuum. Of the 119 remaining 70 belonged from the outset to the incurable class, from some of the causes mentioned before; in the others amounting to 50—30 men and 20 women, the disease had developed into chronic, intractable forms of insanity. In only three did there still remain some faint hope of recovery.

I stated before that the numbers practically admitting of curative treatment were 130 men and 156 women. Out of these, therefore, about 23 per cent. of the men and 12 per cent. of the women resisted treatment, and passed into a chronic, incurable state. Of the remaining 100 men and 136 women, 82 men and 110 women actually recovered, the remainder having died or been discharged unrecovered.

The percentage of recoveries on the total number of cases is 40, that for the men being 32, for the women, 50, showing a great preponderance in favour of the women; but if we calculate the rate, after deducting the cases incurable from organic disease, &c., the percentage in the sexes is much more equal, being 63 for the men and 70 for the women.

The much less favourable nature of the types of insanity among the men than among the women is shown still more strikingly by the relative death-rate. Of the 255 men admitted during the five years, 93 had died by the end of 1877, while only 47 had died out of the 220 women; the rates per cent. being 36 and 21 respectively.

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The causes of death may be classified as under:—

	M.	F.	T.
Cerebral affections	66	20	86
Respiratory diseases	5	3	8
Cardiac „	4	3	7
Phthisis pulmonalis	8	8	16
Other diseases, including senile decay	10	13	23

Upwards of 60 per cent. of the mortality is thus seen to have been due to cerebral affections. The great difference, too, between the death-rates of the men and women is seen to be due solely to the fact that 46 more men than women died from brain disease, which, singularly enough, is precisely the excess of the male over the female deaths, the number from ordinary causes being exactly the same in the two sexes, and amounting to about 11 per cent. on the total number of cases.

It may be noticed, finally, that the inequalities in the recovery and death rates of the two sexes seem to compensate each other, from a numerical point of view; for the proportion which the cases remaining bears to the total number, is precisely the same among the men and the women, namely, 25 per cent.

In concluding this part of my subject, I think I may claim that in the plan of statistics which I have exemplified are embodied the main points of medical and general interest, arranged on a consistent and intelligible basis, and well adapted for purposes of comparison, not only in regard to different institutions and districts, but what is equally important in regard to different periods in the same district. In particular it affords an easy and fairly reliable means of gauging what, if any, increase is taking place in the production of insanity, and how far an apparent increase is due to re-admitted cases, or to the accumulation of a chronic residuum.

The second subject on which I wish to lay before you some statistics is with regard to some remarkable differences which I had been led a few years ago to observe in the amount and types of insanity in different divisions of our district, as illustrated by the cases sent for treatment to the asylum. My object was to correct and check by the results of further experience the conclusions which I then arrived at, and by an analysis of the cases sent from the different unions during the five years already referred to, to see if any further support is given to the theory advanced, that "within very narrow limits, as regards locality, great differences may be observed in the distribution of insanity."

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In the following table the first column shows the total number of cases received from each of our five unions during the years 1872-76 inclusive, and the second table shows the number which would fall to each union if the total number received were divided *pro rata* in proportion to the population:—

	Actual Number Received.	Proportion according to the Population.
Altrincham... ...	... 90 ...	89
Ashton	... 43 ...	84
Congleton	... 65 ...	61
Macclesfield ...	... 137 ...	103
Stockport	... 134 ...	131

In three of the unions the actual numbers and the proportional numbers correspond very closely, but in Ashton the actual number is much below the estimated, and in Macclesfield it is very considerably above it. Perhaps it will put this more clearly if it is stated that while in the cases of Altrincham, Congleton, and Stockport the admissions amounted to 18 to each 10,000 of the population, the number from Ashton only represented 9 to each 10,000; while in the case of Macclesfield it amounted to 23. Now, on examining the relative proportion of the sexes among the admissions from the different unions, I find that while in the three first named unions the men and women are pretty evenly divided, the percentage of men being respectively 55, 57, and 45, in the case of Ashton the proportion of men is only 40 per cent.; while in the case of Macclesfield it amounts to 62 per cent. of the admissions.

In regard, now, to the prevailing types of the disease in the different unions, the following table shows in juxtaposition the total numbers, the number of cases depending on organic cerebral disease, including general paralysis, epilepsy, softening of the brain, &c., and the percentages of the latter on the total number:—

	Total Number.	Number of Organic Cases.	Percentage of Ditto.
Altrincham ...	... 90 ...	25 ...	28
Ashton ...	... 43 ...	5 ...	12
Congleton ...	... 65 ...	17 ...	26
Macclesfield ...	... 137 ...	66 ...	47
Stockport ...	... 134 ...	30 ...	22

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This brings out the striking fact that while among the Ashton cases only 12 per cent. were of the unfavourable type indicated, no less than 47 per cent., or nearly one-half of the Macclesfield cases, were affected with organic disease of the brain. As I have indicated before, much the larger proportion of these cases occurred amongst the men; but it may be remarked that the proportion among the women from the Macclesfield district was also much higher than in the other districts.

I may further add in regard to the special disease, general paralysis, that the cases sent from the Macclesfield Union exceeded in number those sent from all the other unions put together, there having been 30 such from Macclesfield and 27 from the other four unions. As to cases of softening of the brain and other allied affections, there were 20 such from Macclesfield and 16 from all the other unions. On the other hand, the union which appears at the opposite end of the scale is only credited with one case of general paralysis and one depending upon softening of the brain, a state of matters equally happy and rare. In regard to epilepsy, the same great disparity does not appear to exist; the 51 cases of insanity depending upon this disease being pretty evenly distributed amongst the different unions.

In order further to illustrate the subject, in the following table are given the results of treatment in the cases belonging to the different unions, in the shape of the percentages of recoveries, deaths, and residuum remaining under treatment:—

	Recovered.	Died.	Remaining.
Altrincham ...	... 44	... 28	... 20
Ashton	... 53	... 16	... 28
Congleton	... 52	... 30	... 12
Macclesfield ...	... 29	... 40	... 25
Stockport	... 39	... 25	... 30

This shows, what would be expected from what has gone before, that a very small percentage of the Macclesfield cases recovered, and a very large percentage died; while in the Ashton cases the highest percentage of recoveries coincided with a very low death-rate. The proportion remaining under treatment is, however, pretty equal, the total removals, though from different causes, balancing each other.

The smallest residuum occurs in the case of Congleton, where a high rate of recovery coincided with a high death-

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rate, a conjunction favourable for the ratepayers if in no other way.

Without wishing to press the matter too strongly, I think I may fairly say that the result of the above analysis of the cases received from the different unions during a period of five years tends to support the thesis with which I started, and to a large extent is in accordance with the conclusions formerly drawn from a less extended period of observation.

Nothing, I think, could well be more striking than the figures in relation to the Macclesfield Union on the one hand and to the Ashton Union on the other. Even if the conditions giving rise to the state of matters indicated in reference to Macclesfield should only be temporary, and pass away again, there can be, I think, little doubt that during the period referred to a wave has at any rate passed over Macclesfield, carrying with it an increased production of insanity, and insanity of a very bad type. Possibly the great depression of trade coming after a period of great prosperity and high wages might tend to produce such an effect. But I ought to say that I have examined very carefully into the admissions from the different unions in regard to the causation of the disease, but I have been quite unable to elucidate any facts throwing light on the matter in hand. In particular, as to the question of intemperance, the number of cases from the Macclesfield Union in which this was assigned as a cause was not excessive, and considerably less, as it happens, than among the Stockport cases. But it must be admitted that the data for enabling one to come to any conclusion on the matter of causation are very imperfect, as may be gathered from the fact that in at least one half of the Macclesfield cases I have been unable to assign any cause at all.

In spite of the difficulties and uncertainties, however, which beset the question of the local varieties and distribution of insanity, I nevertheless think that it is a subject well deserving of more attention than it has yet received, and one a careful study of which might yield important results. It is with this object in view that I have thought it advisable again to refer to the facts bearing on this matter which have so far come under my observation in this district, and I should feel glad if any others with means and opportunity, and where the conditions are favourable, should think it worth while to examine if similar variations do or do not exist in other districts as well.

In conclusion, as lending a certain *à priori* probability to

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the existence of such local variations, I would just allude to the wider geographical differences which are well known to exist in regard to the distribution of insanity or of particular forms of brain disease. One of the most remarkable of these is the undoubted fact that general paralysis is all but unknown in Ireland. It is again more common in the northern and midland districts of England than in the southern, and more frequent in England generally than in Scotland. Similar remarkable differences exist in regard to the insanity associated with epilepsy. For one case of that form of insanity to be seen in a Scotch asylum, there are, I suppose, ten or more to be seen in English asylums; and it is more common in the south of England than in the north—the reverse of what holds as to general paralysis. Analogy, therefore, rather favours the idea that the factors which give rise to insanity may vary greatly, even within narrow limits, as to locality, and that differences may exist in regard to particular forms of the disease in places quite close to each other, but separated, it may be, by endemic differences in reference to the causation of disease, whether in relation to hygienic conditions, prevailing occupations, the social habits of the people, or other circumstances.

On Forced Alimentation. By Frederick Needham, M.D., Medical Superintendent of the Hospital for the Insane, Barnwood, near Gloucester.

No part of the treatment of certain conditions of insanity is more important than that which relates to the administration of food when from any cause there is inability to take it, or persistent refusal of it.

To decide when to feed, and when it is safe to abstain from doing so, one can follow no fixed rule, but must, of course, be determined by the physical state of the patient and by his antecedents; but it may be definitely assumed that if we err in any direction we shall do so with least detriment to our patient's prospects of recovery in the forcible administration of food at a very early period after its first persistent refusal.

The method of feeding, as I need scarcely say, must depend largely upon the cause to which the indisposition to take food appears to be due, no less than upon the possibilities of the manner of feeding itself. From a somewhat lengthened experience in a mixed asylum, I venture to think that per-

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