

TYPHOID FEVER IN PALESTINE.

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(*Jerusalem.*)

No information from official or non-official sources is available on the prevalence of enteric fevers in Palestine in pre-war days. Since the establishment of the British Administration (1919) the Annual Reports of the Government Department of Health invariably emphasise the high incidence of enteric fevers. Several papers dealing with the epidemiology of the disease appeared in the Hebrew periodicals *Briuth Haam* and *Harefuah* and in 1926 an elaborate Report was submitted to the Department of Health by a Committee of Enquiry appointed by the Department to consider and advise on the situation as regards enteric fever in the country. The data thus accumulated, although very valuable for the local health authorities, did not reveal any peculiar features of more than local interest. Their publication abroad seemed, therefore, superfluous.

Kligler (1927), taking his data in part from the above sources, published in this *Journal* a paper on the epidemiology of typhoid fever, which contained a severe criticism of the Committee's Report and of a Hebrew paper by Felix (1926) on this subject. Kligler claims that his conclusions on the epidemiology of human typhoid confirm the results obtained by Topley, Greenwood, Webster and their co-workers in mouse typhoid in a mouse population. Moreover, he considers his evidence to be of sufficient weight to justify the suggestion of substantial changes in the almost universally accepted measures of control.

Although the ordinary reader of Kligler's paper might be justified in assuming the reliability of his figures and conclusions, one familiar with the local conditions has no difficulty in recognising their unreliability. To prevent the foreign reader from being misled the following observations are published.

THE RAW STATISTICAL DATA.

(*Tables I and II of Kligler's paper.*)

Kligler justly concludes from the higher fatality rate of typhoid among non-Jews than among Jews in Palestine, that notification amongst the former is incomplete and that, therefore, data derived from them are useless for epidemiological purposes. It is true, he should have taken into consideration the fact that the non-Jewish part of the population is less adequately provided with medical assistance—this is well known in Palestine and has been explicitly emphasised in the Committee's Report.

With regard to the notification of cases among the Jewish population, Kligler is of the opinion that it is complete, but of this he furnishes no proof.

He holds this view although the contrary was shown by *me* (1926) in the course of an investigation carried out in Telaviv on behalf of the Department of Health. It is true that fewer cases remain unnotified among Jews than among Arabs, but still notification is sufficiently incomplete to invalidate the official figures and their use for the exact determination of incidence and mortality rates amongst the various racial sections and age groups of the population.

Kligler explains the higher typhoid fatality amongst the Jews in Telaviv as compared with that in other places in Palestine by their under-nourishment, that is to say, by a lower resistance. He gives, however, no evidence for the truth of this statement.

As I have shown (1926) the higher typhoid fatality in Telaviv is due to the great shortage of hospital accommodation—in 1926 typhoid patients were admitted to the hospital there after long delays and then only the more severe cases were selected. The extent to which Telaviv suffered from a shortage of hospital accommodation as compared with Jerusalem may be illustrated by the following figures:

Hospital beds per 1000 of population in 1926.

Jerusalem town	10.0
Telaviv town	3.6
Jerusalem town and district	7.0
Telaviv town and district	2.0

As I have shown in my article in *Briith Haam*, in Telaviv during 1925 and 1926, it was practically only the typhoid cases treated in hospital which were notified to the authorities. Cases taken to hospital in their third or fourth week had generally not up to then been notified. Those cases which ran their course at home, were only in exceptional circumstances reported to the authorities. All this has been established in the investigation mentioned above, which Kligler ignores. He insists on the contrary that hospitalisation is much greater in Palestine than elsewhere and that the campaign against typhoid will not be furthered by increasing the hospital accommodation.

The fatality rate of 7.8 per cent. in Telaviv in 1926 is that of the notified cases; what it was for all cases is quite unknown. As the analysis of the conditions in the various quarters of the town showed, the real fatality rate must be much lower (Peller). Support for this conclusion is also obtained from my report on the epidemic in Emek Israel, where in May 1926 a typhoid epidemic ran its course among roughly 5000 Jewish inhabitants; 187 cases occurred of which only 5 died. This was the only epidemic in Palestine which, thanks to special circumstances, was so dealt with, that no case—not even the least severe—remained unnotified, and all cases, except 5, were treated in hospital. It is indeed the only epidemic in Palestine in which exact epidemiological observations, examinations of carriers and convalescents—segregated for weeks and months in a specially provided building—were carried out. The

mortality in this epidemic, which lasted for $2\frac{1}{2}$ months, did not reach 3 per cent. Although this epidemic falls within the period dealt with by Kligler, he makes no mention of it.

Even in countries with old established Health Organisations, it is extremely difficult to draw epidemiological conclusions from official figures alone; but in a country which, like Palestine, has a rapidly changing Jewish population and a varying degree of excellence in hospital service, it is almost impossible to make use of such data.

The Typhoid Committee made use of such official figures but was fully aware that they could not serve as a basis for accurate epidemiological research. The Committee's Report (1926) stated (p. 1) that the available statistical material both from official and non-official sources, could not be regarded as complete, and this fact "has compelled the reservation of opinion in questions regarding which decisive opinion appeared desirable." It further stated (p. 3): "In making comparison of incidence between different districts of the country or sections of the population, the Committee is sensible that this can only be made directly by means of calculated rates per thousand or other unit of the population under consideration. It is regrettable that the absence of reliable population figures for the areas or groups concerned, prevents this statistical method being adopted. It is not possible accurately to gauge the distribution of immigration to the various towns and districts and, on this account, it is no longer possible to produce estimates of local populations. The Committee desires here to note the great advantage which should accrue to the Government in statistical inquiries of every kind from a census taken in 1927 at the end of the quinquennial period from the date of the last enumeration of the population." As no census has been taken since 1922, it can easily be realised that the difficulties encountered by the Committee in 1926 had certainly not become less at the time of the publication of Kligler's paper, in 1927.

It seems to me that from the above the reader may judge of the reliability or otherwise of the raw statistical data on which Kligler's study is based. The following observations will serve as an illustration of the faulty methods applied by him in the analysis of these raw data.

KLIGLER'S CONCLUSIONS AND A CRITICISM OF THE METHODS BY WHICH THEY WERE FORMED.

The Typhoid Committee (1926) had previously come to the following conclusions: the incidence of the disease is that of endemic typhoid; infection by contact from cases and carriers is the important factor, assisted by late notification, delayed hospitalisation, over-crowding and insanitary housing conditions; immigration into the country and incidence of typhoid are not related directly as cause and effect; the unequal age incidence in places with a predominant immigrant population (Telaviv and Haifa) as compared with that in places with a mainly indigenous population (Jerusalem and Saffad),

is explained by the well-known fact that in endemic areas a high incidence occurs in the earlier years of life, ages 5–15 years, the adult residents in such areas having presumably either suffered from the disease or in other ways developed a relative immunity.

In contra-distinction to these conclusions of the Committee and also those of Felix (1926), Kligler is of the opinion that immigration is directly responsible for the epidemicity of the disease in Palestine. He supports this opinion by data arranged in a series of tables whose analysis, however, discloses many inexactitudes. Some samples of these are given here, together with the most obvious objections.

1. *Table V of Kligler's paper—purporting to establish the fact that there is no striking difference in the age distribution of typhoid in an indigenous population (e.g. Jerusalem) and in an immigrant population (e.g. Telaviv):*

(a) Kligler includes the non-Jews of Jerusalem in his Table V, although on p. 15 of his paper he declares quite rightly that the notification data for non-Jews are “useless for the purpose of a statistical epidemiologic analysis.” His Tables I and II show that the age incidence as officially reported differs markedly for the Jews and non-Jews of Jerusalem. Two explanations are possible: in these two groups either the conditions of infection or the proportion of notified and concealed cases are different. If Kligler had confined himself to a comparison of the Jewish cases in Jerusalem with those of the entirely Jewish town Telaviv, he would have arrived at the same conclusions as the Typhoid Committee.

(b) To arrive at a ratio between case percentage and population percentage in various age groups Kligler compares the typhoid statistics of 1924–6 with the census figures of 1922, although these have changed markedly, especially in the case of the Jews.

(c) In this table Kligler has but four age divisions—which is much too few. Moreover, the age grouping of the census reports for the two towns is not identical. For both these reasons the true differences in age morbidity may be obscured and Table V rendered completely worthless.

(d) Jerusalem has no Jewish “Hinterland,” the hospital at Telaviv, on the other hand, serves a number of populous Jewish colonies, whose age groups percentages may not correspond with those of Telaviv town, used in Table V. This introduces new sources of error of unknown magnitude.

2. *Table VI of Kligler's paper—dealing with racial susceptibility.* The equal susceptibility of Jews and non-Jews is neither proved nor disproved by the figures of this table, which contains the following inexactitudes:

(a) In Jerusalem the Jewish population has since 1922 increased much more than the non-Jewish. To refer the case numbers of 1924, 1925 and 1926 to the population figures of the 1922 census introduces a greater error into conclusions regarding Jews than into those concerning non-Jews.

(b) A comparison of Kligler's figures in this table with the official ones shows that whereas in the case of Jews paratyphoid infection is never taken

into account, in the case of non-Jews, in two out of the three years under consideration, the figures given include both typhoid and paratyphoid fevers.

Further comment upon this table seems unnecessary.

3. *Chart 5—demonstrating according to Kligler a high positive correlation between immigration (of Jews) and typhoid incidence (among Jews and non-Jews) in Palestine.* It is known that observations extending over a period of merely five or six years may indicate correlation between two phenomena without these phenomena standing in actual relationship to one another. Kligler worked out a correlation factor and found it to be + 0.89. This is indeed the correlation factor of the increment of Jews during 1921–6 and the number of deaths from typhoid in the country. He supplies no factor of correlation between increment and number of cases. If this factor is worked out from his figures it results in + 0.338 or 0.384, according to whether cases are compared to immigration figures of the same or of the previous year. Here there can be no question of a “very high” coefficient of correlation. Of course, no importance whatever can be attached to such figures.

4. *Table VIII—considered by Kligler to confirm the importance of immigration.* In this table the Jewish population is divided into “indigenous” and “immigrant” and the morbidity of both groups investigated. The population figures used in this table for the years 1924, 1925 and 1926, are not identical with the official figures of the Department of Health, which Kligler uses in his previous tables. The differences are as follows:

	Jewish population	
	Government figures	Figures in Kligler's Table VIII
1924	94,669	100,300
1925	120,559	127,900
1926	147,000	156,000

Nor do Kligler's figures agree with those of the Statistical Department of the Zionist Executive which are higher than the Government figures.

But apart from this inaccuracy, this table neglects the fact that immigrants live under quite other social conditions than do the indigenous, that immigrant cases are nearly always forced to seek hospital treatment and thus do not escape notification, as so many of the indigenous do, who have homes in which they may be treated.

5. *Table IX—a comparison of typhoid incidence among the Jews of Jerusalem and of Jaffa-Telaviv.* Here again the population figures are inaccurate, or, at any rate, different from those in Table VI. Therefore, the morbidity figures for the Jews of Jerusalem in Table IX differ from those in Table VI, except where wrong arithmetic (faulty division) has influenced the percentage calculation. In addition, it may be pointed out that Table IX, under the heading 1925, furnishes for Jerusalem typhoid cases alone, whereas, for Jaffa-Telaviv paratyphoid cases are included as well.

If this analysis of Kligler's paper is summarised, it is seen that: the raw

statistical data used by him are admittedly unreliable, the incomplete notification of cases not revealing the true incidence of the disease and reliable population figures for the areas and age groups concerned not being available; these raw data are grouped and used in an unsatisfactory manner. It is clear then, that the conclusions arrived at cannot be correct.

One other point is especially noteworthy. The apparent concordance of Kligler's findings with the experimental data furnished by Topley, Greenwood, Webster, and their associates, suggests to the author the vaccination of immigrants as a simple and effective solution of the typhoid problem in Palestine. His paper, however, does not reveal to the foreign reader the fact that inoculation of immigrants was compulsorily carried out by the Government Department of Health from December 1921 till March 1925. This measure was discontinued in March 1925, but re-introduced in February 1926. If reliability of Kligler's figures could be assumed his conclusion No. 4 would prove that morbidity and mortality is higher in the inoculated immigrant group than in the non-inoculated indigenous population. It will be seen that this conclusion would not justify his suggestion that the hospitalisation of cases should be neglected and that efforts should primarily be concentrated on inoculation as the simplest and most effective solution of the problem.

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