

TYPHOID FEVER IN PALESTINE.

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IN the December issue of this *Journal* Peller (1928) presented a critical analysis of my paper (1927). His criticism is both general and specific. The general criticism requires no comment on my part. Polemics have no place in English scientific literature. Any contribution which contains an idea and stimulates thought and investigation is, in my opinion, of sufficient interest to warrant publication, provided the data presented are accurate.

Some comment, however brief, is required because the specific remarks create an impression that not only are the deductions wrong, but that the data on which they are based are inaccurate.

(1) The author claims that I ignored his "proof" that the high fatality in Tel Aviv was due to a shortage of beds rather than other causes. He brings as evidence the striking differences in beds per capita in Jerusalem and Tel Aviv. He does not compare fatalities in those two cities. The following figures are, therefore, of interest:

	Week of disease of admission of cases (1925)			
	1st	2nd	3rd	4th
	Percentage of Total			
Jerusalem	53.5	33.0	10.8	1.8
Tel Aviv	38.9	38.9	16.4	3.6
	Fatality in hospital cases (Jewish, 1925)			
	Total cases	Deaths	Fatality	
Jerusalem	188	22	11.7 %	
Tel Aviv	249	21	8.4 %	

Again the average total fatality for Jewish cases for the years 1924-26 inclusive was in Jerusalem—400 cases and 35 deaths (8.8 per cent.) and in Tel Aviv—536 cases and 48 deaths (8.9 per cent.).

He further compares the 1926 fatality in Tel Aviv with that of the Emek, but fails to give the age distribution of the cases and deaths. Again the actual figures are instructive:

Ages	Tel Aviv			Emek		
	Cases	Deaths	Fatality	Cases	Deaths	Fatality
0-19	74	6	8.1	69	1	1.4 %
20-39	130	13	10.0	48	4	8.3 %

In Tel Aviv the ratio of the older to the younger age group is 5 : 3; in the Emek only 2 : 3; the fatality in the older age groups is nearly the same in the two localities.

It is admitted that an unknown number of mild and ambulant cases is not reported, but this is true everywhere and the proportion does not vary much from year to year. It is doubtful whether any Health Department is so organised as to ferret out all mild and suspicious cases and send them to a hospital for observation, as was done in the circumscribed area (the Emek) over a short period of time. The fatality of one year cannot be considered a criterion, not to say a proof.

(2) The author stigmatises particularly Tables V and VI.

(a) Without claiming space for recasting the figures, an inspection of the Jewish cases in Jerusalem and Tel Aviv, shown in Table II of my paper, shows that the number of cases runs parallel with the distribution of population; compare, for example, the cases in age group 20–29 in the two cities. Although no definite census figures of the age distribution of the Jewish population in Jerusalem have been published, it is generally conceded that there is little difference in the age distribution of the Jewish and Arabic population in that city, because the former is as indigenous as the latter.

(b) The census figures are used for age distribution only, and that has changed but little in Jerusalem since the 1922 census. At the time of the census the population of Jerusalem was 62,500. Since then it has risen to 70,000 or 72,000—an increase of 8000 to 10,000. At least 4000 are due to natural increase and the rest to immigrants most of whom consisted of families. Under these circumstances the change in age distribution cannot have been significant. For the purpose of the comparison the actual figures are immaterial so long as the age distribution remains constant. The same is true of the age distribution in Tel Aviv, at least for the year 1926 when immigration had practically ceased.

(3) The objections to Table VI are even less cogent. The only point raised by me was that there was no evidence to show that the Jews were *more* susceptible than the Arabs. The increase in the Jewish population (mentioned in the footnote to Table VI) and the inclusion of paratyphoid cases only emphasise the fact that on the basis of the data the mortality rate from typhoid is higher among the non-Jewish (0.47) than Jewish (0.34) population.

(4) The writer admits that the mortality data give a high correlation, but objects that that is not the case with the morbidity data. It seems to me axiomatic that mortality data are more dependable than morbidity. The real weakness not touched upon by the critic is the short experience. It was because of this that the only claim advanced was that by various comparisons a probability was established of a correlation between epidemicity and immigration. It may be of interest in this connection to quote the critic's statement (1927): "Almost half of the cases reported in Tel Aviv during 1926 occurred among immigrants who entered the country during the year 1925–1926." The ratio of 536 cases reported in the years 1924–26 in Tel Aviv with an average population of 36,000, a large proportion of which was

immigrant, to 400 Jewish cases in Jerusalem with an average Jewish population of 41,000 for these three years is also suggestive.

As to the accuracy of the figures, the critic is referred to the text. There he will find that the population and immigration figures were furnished by the Chief Statistician of the Zionist Executive. The data were given in writing and the letter is on file.

The last statement applies also to the remarks concerning Table IX. This table cannot be compared with Table VI, where for specified reasons the 1922 census figures are used.

In conclusion I would repeat that the paper was written with the full knowledge of the weakness of the statistical material. The data are given in detail for the critical reader to draw his own inferences. They are derived from official records, and hence are absolutely correct so far as they go. The criticisms are well taken when and in so far as they indicate that the data are incomplete and in many respects unsatisfactory. To make statements, however, which are only partially correct is misleading. Such statements as those concerning the validity of the immigration figures and the concluding one concerning the vaccination carried out during 1921-25 are unjustifiable. The author must have known that during the period 1922-25 vaccination was not always strictly enforced, and that at best only one injection was given. Moreover, more people entered during 1925 than during 1922-24 combined, and immigration practically ceased during 1926.

The problem in Palestine is how to check promptly a growing evil. No one is more in favour of sanitation than the present author. But sanitation in Palestine means a transformation of the habits of a primitive Oriental people to correspond with those of the advanced Occidentals. This cannot be brought about by police force, but by education. Hence it appeared necessary to analyse the data and approach the problem from a new angle. No amount of hospitalisation facilities will induce early and prompt reporting of cases or the discovery of the more dangerous mild ambulant cases. According to Newsholme that ideal stage has not even been reached in England. Immediate relief was essential to permit time for instituting the sanitary reforms and for the educational process to become effective. The simplest measure, therefore, and the most economical appeared to be to vaccinate the incoming immigrant population and, in so far as possible, the resident population as well. These measures have been adopted by the Government. The results thus far (1927 and 1928) have been most encouraging. Time will test the full efficacy of these measures.

REFERENCES.

- KLIGLER, I. J. (1927). *J. of Hygiene*, 27, 14.
PELLER, S. (1927). *Briyth Haam*, 1, 33.
— (1928). *J. of Hygiene*, 28, 318.

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