

CHAPTER V.

THE INFECTIVITY OF INFLUENZA.

Few living epidemiologists doubt that influenza is an infectious disease. (a) The occurrence of secondary cases after the introduction of a sick person into a previously untainted community ; (b) the non-random distribution of cases in houses, seen in the excess of multiple cases, the small average interval in time between successive cases in the same house ; and (c) a host of clinical observations, although severally not logical proofs of contagion have a cumulatively convincing effect.

Yet the most distinguished epidemiologists of an earlier generation categorically declined to recognise influenza as an infectious disease and held it to be an outstanding example of an epidemic disease covered by the ancient definitions. It will be recalled that the ancient masters of our art unanimously characterised an *epidemic* disease as one generated by the action of the atmosphere upon men's bodies. Such was the view of Hippocrates, and in his commentary upon the Hippocratic writings, Galen rationalised the conception. In the introduction to his comments on Hippocrates' books of Epidemics, Galen observed :—

" It does not often happen that a disease takes hold upon a city, a nation, or an army, through their food supply nor even by virtue of community of occupation or labour. But if we are immoderately heated or chilled, dried, or moistened, by the all-embracing air, the symmetry of our bodies—which is health—is confused and destroyed. The air is all around us, and we breathe it. To no other influences are we all simultaneously subject and throughout the whole day. It cannot be, but that the properties of living bodies are influenced and modified by atmospheric change."

It is implicit in this argument that an epidemic (the remark just quoted applies exclusively to epidemics) does really occur as an explosive phenomenon, differences of diet, housing and occupation being without influence. When these conditions *are* fulfilled we can hardly deny the cogency of the argument. But in fact they have never yet been fulfilled by any epidemic disease. Chroniclers spoke of plague in just the terms more recently applied to influenza, uniformly exaggerating both the local explosiveness and the velocity of diffusion. Plague has been said to outstrip in its progress the swiftest horse, even although the writer recorded facts which proved that the extension was really quite gradual. The final magnitude

of the calamity has led to foreshortening. In plague the error has been easily recognised, since the warning epizootic has been observed, or, where no record of an epizootic exists, bills of mortality record the scattered deaths (as in the great plague of London) which happened sometime before the storm gathered head. The relatively low fatality of influenza deprives us of such warning while the increased velocity of human travel is but slowly realised in epidemiological studies. Thus we can understand why influenza lingered in the ancient class as a cataclysmal or telluric phenomenon. We need scarcely hesitate now to rank influenza amongst the other infectious or contagious diseases such as measles, scarlet fever, whooping-cough and small-pox.

Accepting this general doctrine, we are called upon to inquire whether personal infectivity varies sensibly through the clinical stages of the disease, to learn when the sick man is most dangerous to his associates. We must also learn whether the infective matter is capable of conveyance through articles soiled by the discharges of the patient or by objects with which he has been in contact. Precise answers to such questions could only be furnished by controlled experiments and to plan and conduct such experiments in the midst of an epidemic have proved impossible. We, therefore, reluctantly fall back upon the essentially imperfect test of personal opinion and common fame.

The Ministry circulated a questionnaire to the medical personnel of the home command and among the matters upon which an expression of opinion was invited were (1) Evidence as to the stage when influenza was most infectious and the duration of infectivity. Ninety-three replies were made to this question and 72 or 77.4 per cent asserted that the early stage of the disease was the most infectious; 12 thought the most infectious period to be during the pyrexia, 7 during the early catarrhal stage; these two sets might safely be included amongst the supporters of early infectivity and if they are so included, only two of the 93 informants denied that the most infectious stage was early, alleging it to be seven days from the onset, but without assigning any reason. Of the evidence in support of early infectiousness we may cite such instances as the following:—One officer remarks that 100 cases were admitted to hospital on the second or third day of illness, but no secondary ward infections occurred, although in the camp of 613 men which gave origin to the cases 413 contracted the disease. Another officer notes that a patient while in billet infected house mates, but after removal to hospital infected none.

While we recognise the need of caution in drawing conclusions from expressions of opinion which are necessarily influenced unconsciously by current medical beliefs and sometimes merely rationalise what has been irrationally and

uncritically accepted, it may be remarked that they are concordant with our *a priori* unexpected discovery that domestic overcrowding is not a demonstrably important factor of varying incidence rates, and our surmise that acute and transitory overcrowding in trains, omnibuses, and places of entertainment is more important. This would be so were the very early or prodromal stage much more infectious than the period of prostration, since the later period contributes only to intra-domestic or institutional risks.

Passing to the second point, viz., the respective importance of direct personal and indirect infection, *per fomites*, the question elicited 79 replies. Six offered no opinion, one believed infection to be entirely air-borne, one thought indirect infection the more important, stating that visitors to influenza wards suffered less than those handling soiled linen, five thought both methods of equal efficacy, stress was laid upon the equal incidence of influenza upon nursing and laundry staff, while 66, or 90.4 per cent., of these committing themselves to any answer, believed direct infection the more important; several of this majority relied upon precisely the same kind of evidence as the minority, viz., a comparison of nursing and laundry staff, evidence which, without further information, is unconvincing, and we cannot say that any particular train of circumstances cited by any informant could be deemed crucial. It is, however, proper to remark that nobody was able to bring forward positive testimony that the disease had been transmitted under conditions from which direct personal contagion could be excluded.

Turning to numerical details, viz., as to the range of infection, only 31 answers were received, and so many of these betrayed a confusion between evidence and speculative opinion (one informant, for instance, would put no limit to the range of infection since "micro-organisms are floating in the air and are distributed indiscriminately by the wind"; another categorically demanded close contact as a *causa essentialis*, because "the "B. influenza is kept alive with difficulty away from its "hosts") that we are unable to draw any numerical conclusions as to the range of infective discharge.

Although the subject must be mentioned elsewhere, it is convenient to take note now of the military medical officers' experience relative to overcrowding. No reporter doubted that overcrowding was important, but hardly any evidence of the relative shares of domestic and extra-domestic overcrowding was forthcoming. The only numerical statements have reference to extra domestic overcrowding. Thus it was reported from the Irish Command that a fall in the death-rate occurred at Cork immediately after music halls and theatres were put out of bounds, and that at Londonderry, where the same order was in force, only eight cases occurred amongst 1,770 troops. From Chatham we were told that only 8 per cent, of a brigade of 10,000 men fell ill when the disease was prevalent in the

town places of amusement had been put out of bounds. The average incidence on the district was alleged to be "more than tenfold" as great. These statements do not, we think, carry conviction. We should know whether the closure of places of amusements was effected early or late in the local epidemic, an essential datum not furnished, while the alleged incidence of more than 80 per cent upon the civilian population of Chatham maybe disregarded as altogether incredible and discordant with the direct evidence collected in other places.

The general inferences to be drawn from the evidence submitted are that while influenza is surely contagious from person to person, the infection being perhaps conveyed by the expulsion of fine particles of mucus from the mouth, and nose, no indirect method of dissemination has been proved to possess epidemiological importance. It also appears probable that the patient is most dangerous in the early stages of his illness; it may even be that there is infectivity in the prodromal stage before the patient experiences any physical inconvenience.

Whether the disease is more infectious than measles or smallpox is a question we cannot answer. That it prevails more extensively during a given time proves nothing, since we are dealing with a disease in which the limitation of susceptibility by age and previous attack is greatly less than for measles or (in a well-vaccinated community) smallpox.

The reader may perhaps be surprised that we have been able to add so little to the evidence provided by Dr. Parsons nearly thirty years ago. The reason is that to pass from a qualitative demonstration of the *fact* of infection to a measurement of the *intensity* of infective power is a difficult undertaking. It might, perhaps, be supposed that a comparison between the final proportion of attacked persons to persons exposed to risk in isolated households or communities within which an infected person had been received would give the measure we seek. We might compare such percentages with a similar measurement upon households into which scarlet fever or diphtheria had been imported. Actually the number of such instances available is very small in pandemic times, while the proportion of the exposed to risk partly protected by previous attack is almost never available. In other words, we have not been able to institute comparisons strictly *in pari materia* and, on that account, do not feel justified in attempting to pass beyond the affirmation that influenza is an infectious disease, to the measurement of *how* infectious it is. That the power of transmission from person to person is not constant but a function of the "epidemic constitution," perhaps also of individual physiological variations, follows from the evidence submitted in other chapters of this report.