An Address

ON

WET WINDS AND EARLY PHTHISIS.

DELIVERED TO THE SOUTH-WESTERN BRANCH OF
THE BRITISH MEDICAL ASSOCIATION

BY

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THE subject which I have chosen for my address is one which I hope will be considered appropriate. It has been closely associated with the south-west; for its initial observation was made in Devonshire, and its subsequent development owes much to the able and generous help of Devon colleagues. At the present moment fresh figures compel fresh insistence upon it. Its importance now spreads far beyond the limits of its native county. I have tried in every way in this investigation to avoid mistake, and, above all, to escape the pitfall of bias in my own mind. What is more difficult to eliminate is bias in the minds of others.

To convey to you the full significance of the tables I am going to place before you, illustrating the influence of rain-bearing winds on the fate of early cases of phthisis, it is necessary very briefly to review the work which has led up to them.

The Initial Observation.

Twenty-five years ago, in the autumn of 1899, I compared the phthisis death rates of the rural sanitary districts of Devon, in the hope of discovering the best site for a phthisis sanatorium, and found, to my great astonishment, that the localities then accounted ideal, sheltered from north and east winds but open to the south and west, were precisely those in which the mortality from phthisis was highest. I tried to explain this distribution of the death rate by reference to general sanitation, to geology, and to rainfall, and failed. Nothing except exposure to west and south-west winds seemed to offer a satisfactory explanation. But this explanation was so contrary to the time-honoured teaching of the day that nothing but a searching test could have convinced me of its validity.

The First Test.

This test was as follows. I chose four separate rural sanitary districts in different parts of the county, with an aggregate population of over 85,000 persons, and examined the phthisis death rates in their parishes (the small areas of parishes allowing of fair comparison of relative shelter and exposure), and in every one of these districts the correspondence between relative exposure to west and south-west winds and relatively high phthisis mortality was found strikingly apparent. These figures were read to a meeting of the Royal Medical and Chirurgical Society in 1900.

The Second Test.

After it had been urged (rightly) that female death rates would furnish safer conclusions than mixed death rates, and (wrongly) that wind from any quarter might be equally hurtful, I took for investigation Okehampton Rural District, in the centre of Devon, presenting specially marked contrasts of exposure and shelter in respect of all winds except the south (which practically does not reach it), and constructed from it, not merely tables, but, to make the test severer, maps, both of the female parish phthisis death rates and of the relative parish exposures in respect of south-west, west, north-west, north, north-east, east, and south-east winds. In this undertaking I had the invaluable help of my friend Dr. E. H. Young, the medical officer of health for the district. The maps display even more strikingly the relation between these winds and the phthisis death rates; the correspondence is remarkable. In this investigation there came out a definite though slighter

effect of north-west wind upon the death rates. Therefore, in all subsequent work, this, the third chief rain-bearing wind of Devon, has also been taken into account.

The Third Test.

It was now objected (unreasonably, in view of other accepted work) that ten years was too short a period on which to found conclusions. So another specially suitable area, as regards contrasts of shelter and exposure, consisting of twenty-three parishes of the Exmoor part of Barnstaple Rural District, was examined over a period of forty-five years. In this work I have to thank my friend Dr. J. R. Harper, the district medical officer of health, for his invaluable help. Here once more the same law came plainly into view. Not only was the effect of exposure to north-west, west, and south-west winds upon female phthisis mortality evident for the whole period of forty-five years, but it was evident for every one of the nine successive five-yearly periods except the first. Further, during the twenty years 1885 to 1904, in which Koch's discovery of the tubercle bacillus must be held to have made the diagnosis more certain, the contrast in the death rates between exposure and shelter became more marked; and in the decade 1890 to 1899 the death rates presented a still more remarkable contrast. Finally, again to test even more severely the correspondence, maps were made showing the female phthisis death rates between 1890 and 1899, and the relative parish exposures to south-west, west, and north-west winds. Again the correspondence was very striking.

These two last series of comparisons—namely, those in the rural districts of Barnstaple and Okehampton—have been the subject of the first independent inquiry into the correctness of my figures. Dr. John Brownlee, in his able report to the Medical Research Council on the epidemiology of phthisis in Great Britain and Ireland (Part 3, 1920), re-examined the death rates in the parishes of these two districts, over a different series of years from mine, using a different method, and concluded that "it therefore appears probable that in these districts exposure to the south-west winds is associated quite definitely with an increased susceptibility to phthisis"; adding, "I think it must be admitted that Dr. Gordon has made out a very good case for further extensive detailed inquiry, and that he must be congratulated on the work which he has done." I need scarcely say that I only quote these remarks in full in order to make clear the extent of Dr. Brownlee's agreement. The subject is far too grave for egotistical triviality. It is noteworthy that, till a few weeks ago, this has been the only serious attempt made to verify my work.

The Fourth Test.

One of the objections raised to my conclusions (if, indeed, erroneous explanation can be counted as objection) was that these winds only acted indirectly by compelling the closure of all doors and windows against them! As if all doors and windows were only on the western sides of the houses! So another investigation was devised. If it could be shown, in a town, that the streets whose roadways were swept by these winds suffered most from phthisis, that would be some evidence that the effect is a direct one on the people living in those streets, since they must be regarded as the chief users of them, and windows and doors need not be generally closed against wind which only blows past them. A report issued by the sanitary authority of Exeter afforded the opportunity of investigating this point.

It was then found that not only had the streets of Exeter, whose roadways were swept by the westerly winds, about twice the female phthisis death rate of those whose roadways were sheltered, but that, when the streets on different soils were separately considered, the same contrast came out, the ratio being rather less than 2 to 1 on clay, rather more than 2 to 1 on sandstone, and more than 3 to 1 on gravel. Further, when the streets were divided into four classes according to their differing rateable house values, in every class a ratio of about 2 to 1 was maintained. Lastly, when specially exposed roadways were compared with specially sheltered roadways (differences of soil being eliminated by only considering streets on clay, those being the most populous), the ratio became nearly 4 to 1!

A World-wide Investigation.

These four tests convinced me that, in Devonshire, populations exposed to prevalent strong rain-bearing winds suffer more from phthisis than populations sheltered from them. The next step was to see if the same law could be traced elsewhere. Now here it is necessary to say that the vast bulk of the available material abroad was not sufficiently reliable to support positive conclusions. But my search through many parts of the world was rather for *contradiction* than for confirmation of my other results. Such contradiction might have been very useful, I thought, in indicating fallacies undiscovered in Devonshire. *But there were actually no contradictions.* From every available part of the world confirmation came. As an interesting item I may mention that from the Sandwich Islands, which Hann says afford the finest example of a contrast between a wet and windy side and a dry and wind-sheltered side, Dr. Grace wrote to me that, in respect of phthisis, "the fact that the windward side is the bad side is well recognized here."

Bovine Tuberculosis.

But the effect of these winds on tubercle is not confined to human phthisis. In 1910 Professor Delépine of Manchester read a valuable paper on the distribution of bovine tuberculosis amongst the farms supplying Manchester with milk: 1,524 farms, scattered over 5,000 square miles, were investigated and mapped, and these maps made possible an inquiry into the effect of westerly winds on bovine tubercle. Exactly the same relation became obvious, and when only evidently fully exposed farms were compared with evidently fully sheltered farms, the exposed were found to have about four times as much tuberculosis amongst the cattle as the sheltered.

So much for the influence of these winds on the incidence of phthisis. Now for their effect upon the fate of those who have contracted the disease.

EFFECT ON PHTHISIS PATIENTS.

It might be reasonably surmised that the effect of these winds on phthisis patients would not be favourable. All the great phthisis health resorts which have been conspicuously successful have been situated in pronounced shelter from prevalent strong rain-bearing winds; and the comparative failure of English sanatoriums may be explained by the blind placing of them, on German advice, in all sorts of unsuitable conditions, too commonly in westerly wind exposed localities. But surmise is not enough. The matter is too serious.

Duration of Cases.

Dr. Harper's figures afforded me in 1912 the opportunity of investigating the stated "durations" of female cases in shelter and exposure. He was good enough to have the recorded durations attached to his cases, and I divided these into three categories—namely, "long cases," lasting fifteen months or more; "medium cases," lasting between ten and fourteen months; and "short cases," lasting nine months or less. The result was so suggestive that I proceeded at once to treat two other quite different areas similarly. Dr. Young kindly obtained the durations of the cases in the parishes of Okehampton district. Here I classified the durations somewhat differently. Then I examined the durations in the parishes about Budleigh Salterton, and obtained similar results.

All these three areas showed the same effect of the winds. In all three the exposed parishes had a higher proportion of "short cases" and a lower proportion of "long cases" than the sheltered parishes. It might have been objected that recorded durations of cases cannot be fully relied on. But to this it may be replied that it is very unlikely that the errors supposed would by mere chance tell in the same direction in the records from all three areas; also that there is, after all, no real ground for assuming the presence of such errors as would invalidate the results. Now, however, we can proceed to a much more incontrovertible mode of inquiry.

Effects on Early Cases.

Thanks to the recent tuberculosis regulations, it is now easy to mark on the map the exact location of the dwellings of phthisis patients, and to follow their fortunes through a sufficient number of years. For the application of the new method which I am about to describe I am indebted to the great kindness of my friends, Dr. Adkins, medical officer of health for the County of Devon, and the tuberculosis officers for the county.

METHOD OF THE FOLLOWING INQUIRIES.

Let me explain precisely the plan on which we have proceeded. Lists were first drawn up of *early (Grade 1) cases of phthisis which had passed through a sanatorium (in practically all cases Hawkmoor Sanatorium) and had been observed individually for four years afterwards.* These cases have been classified as "male" and "female," "with tubercle bacilli found," and "without tubercle bacilli found"; and at the end of four years as "dead," "arrested," and "living"—that is, not arrested. The exact position of the dwelling in each case during the four years has been marked by a dot on a Bartholomew's reduced survey coloured contour map (the well known touring map), and to each dot has been attached signs showing the classes to which the case belongs, as well as its exposure to or shelter from all winds between N.N.W. and S.S.W. The signs used have been M.=male, F.=female, + = tubercle bacilli present, - = tubercle bacilli not found, D.=dead, A.=arrested, L.=living but not arrested, S.=sheltered from all westerly winds, X=exposed to any or all of these winds.

As regards "shelter" and "exposure," I have found in long practice that 100 ft. of altitude shelters half a mile to the leeward of it well, and a further half-mile fairly. Beyond a mile from the 100 ft. its shelter must be regarded as ceasing, and dwellings situated further off will be exposed. I am sure that the only satisfactory method of assessing "shelter" and "exposure" is by measurement on the map. No other method can be checked by outsiders. No other method is so free from fallacy. It is, moreover, the method which I have hitherto used in my work. Lastly, it makes an inquiry possible which would be utterly impossible to undertake by local investigation of sites.

In dealing with these new figures I think it may be prudent to lay most stress on the female cases, which have formed the basis of all my former work in Devon. For we know that the occupations of the women are more uniform than those of the men; and (what is even more important in this inquiry) these occupations keep the women mostly in the immediate vicinity of their dwellings, whereas those of the men not uncommonly take them into quite different localities.

Grade 1 Sanatorium Cases in Devon.

The first figures which I shall show you are those from East Devon and from North Devon. The assessment of cases in South Devon has presented exceptional difficulty as regards shelter and exposure, and Dr. Ward has preferred to withdraw them for the present. The figures for East Devon were assessed jointly by Dr. Galbraith and myself. Those for North Devon were mostly assessed by Dr. Swan, my only notable interference being to insert two arrests in exposure, which we had overlooked—an interference by no means in the interests of what might be supposed to be my expectations.

TABLE I.—*East Devon.*
Sanatorium Cases: First Stage: Four Years' Observation.

	Exposed	Sheltered.		Exposed	Sheltered.
Dead:			Mortalities:		
Males ...	9 (7)	0	Males ...	56%	0
Females ...	3 (1)	2 (2)	Females ...	33%	7%
			All cases ...	48%	4%
			All T.B. cases	72%	33%
Arrested:			Arrests:		
Males ...	0	7	Males ...	0	29%
Females ...	3	22 (1)	Females ...	33%	81%
			All cases ...	12%	57%
Living:			All T.B. cases	0	16%
Males ...	7 (2)	17 (2)			
Females ...	3 (1)	3 (1)			

TABLE II.—North Devon.

Sanatorium Cases: First Stage: Four Years' Observation.

	Exposed	Sheltered.		Exposed	Sheltered.
Dead:			Mortalities:		
Males ...	11 (9)	0	Males ...	52%	0
Females ...	4 (2)	0	Females ...	30%	0
			All cases ...	44%	0
Arrested:			All T.B. cases	52%	0
Males ...	3 (2)	7 (2)	Arrests:		
Females ...	3 (2)	3 (1)	Males ...	14%	70%
			Females ...	23%	60%
Living:			All cases ...	17%	66%
Males ...	7 (4)	3 (1)	All T.B. cases	19%	50%
Females ...	6 (2)	2			

INDEPENDENT INQUIRIES.

Grade 1 Sanatorium Cases in Other Counties.

1. *West Shropshire*.—From Shropshire I have just received the results of the first independent inquiry made on these lines. Dr. Watkin, one of the county of Shropshire tuberculosis officers, has most kindly investigated the West Shropshire area, as being the area affording the greatest contrasts in that county between shelter and exposure. It will be seen that his figures (Table III) fully confirm those which we have obtained in this county. Although, as he points out, the numbers of cases are small, I would emphasize the importance of this entirely independent confirmation.

TABLE III.—West Shropshire.

Sanatorium Cases: First Stage: Four Years' Observation.

	Exposed	Sheltered.		Exposed	Sheltered.
Dead:			Mortalities:		
Males ...	9 (7)	1 (1)	Males ...	31%	8%
Females ...	7 (5)	1 (1)	Females ...	29%	14%
			All cases ...	30%	10.5%
Arrested:			All T.B. cases	54%	25%
Males ...	9 (1)	5 (1)	Arrests:		
Females ...	8 (1)	4	Males ...	31%	41%
			Females ...	33%	57%
Living:			All cases ...	32%	47%
Males ...	11 (6)	6 (4)	All T.B. cases	9%	12%
Females ...	9 (2)	2 (1)			

2. *Derbyshire*.—In Derbyshire it has been impossible to carry out the method in its entirety, but the medical superintendent of the county sanatorium has most kindly sent me the results of two years' observation of Grade 1 patients, dividing the county into three districts, distinguished by differences in their rainfall. Now rainfall *per se* has practically no effect on phthisis mortality. And a glance at the maps, which Dr. Barwise, the medical officer of health for Derbyshire, has very kindly sent me, shows that the north-west area is pre-eminently the most exposed to wet winds. The table of mortalities is here again most instructive.

TABLE IV.—County of Derby.

Sanatorium Cases, Male and Female: First Stage: Two Years' Observation. Discharged 1920; Condition 1922.

District.	Dead.	Arrested.	Living.	(Untraced.)
A. North-west District (40-50 in. rain):				
Tubercle bacilli + ...	1	11	6	(9)
Tubercle bacilli - ...	1	10	3	(7)
B. Middle District (30-40 in. rain):				
Tubercle bacilli + ...	0	3	0	(2)
Tubercle bacilli - ...	0	16	4	(7)
C. South-east District (under 30 in. rain):				
Tubercle bacilli + ...	0	12	2	(10)
Tubercle bacilli - ...	0	40	13	(22)

Mortality of (all) Traced Cases (taking T.B. + and T.B. - together).—District A, 6 per cent.; District B, 0; District C, 0. Mortality of T.B. + cases in District A = 5 per cent.

I hope to receive later on the results from other counties which have also been good enough to undertake similar investigations.

PRINCIPLE OF "APPROXIMATE ISOLATION OF INFLUENCES."

In all further investigation of this subject I am sure it is essential to bear in mind the principle I have ventured to lay down, of the "approximate isolation of influences." Until Koch devised a means for the isolation of micro-organisms no precise work on bacteriology was possible. That was a complete isolation. The isolation of influences in climatology can only be approximate. But the additional precision given to results by such approximate isolation will be instantly realized by anyone who carries it out. By such approximate isolation I mean not merely the happy-go-lucky elimination of this or that influence in the investigation of a third, but the systematic enumeration of all the known influences which seem to affect the field of inquiry besides the influence in question, and their successive elimination from the problem in one way or another, so far as that is possible. The process naturally is neither easy nor short. For illustrations of its use I must refer you to my former publications.

SOME MISAPPREHENSIONS.

There are some misapprehensions still needing notice, because still causing confusion.

First, there are some people who imagine that my work in England concerns only the south-west wind. It concerns all westerly winds, which in England are the prevalent, strong, rain-bearing winds. In other parts of the world other winds are concerned. In Mid-Europe the predominant wet wind is the north-west. In South Europe the south-east has also to be taken into account. In all Europe, however, west and south-west winds must be always considered. In Japan the north-west is the main wet wind. In the tropics there are the Trades, which are easterly. *Everywhere the formula to remember is "prevalent, strong, rain-bearing wind."*

Again, Dr. Haviland's work is still confused with mine. To Haviland is unquestionably due the credit of first perceiving that "wind" affects phthisis. But, so far from being a "follower of Haviland," I was fortunately quite unaware of his work until I found the fact on which I lay such stress. Haviland's statements were quite different from mine, and, had I been familiar with them, it is not unlikely that I should have missed my point, as he did. Naturally, when my own work brought me to hear of his, I acquainted myself with it. The point I here insist on is not the always slightly absurd point of personal priority, but the vitally essential point of *difference*.

CONCLUSION.

We have thus before us, ladies and gentlemen, a mass of diverse evidence, all clearly pointing to one and the same thing—a profound influence exerted upon phthisis by prevalent, strong, rain-bearing winds, affecting both its incidence and its course.

In respect of incidence, no mere climatic factor has so powerful an effect (a fact whose further proofs would take too long to relate this afternoon), and amongst social factors only occupation equals it in importance. We may safely say that the chance of becoming phthisical is *at least twice as great in exposure to these winds as it is in shelter from them*.

As regards course, it may be fairly said that the chance of recovery is *at least twice as great in shelter from these winds as it is in exposure to them*, and that therefore *there is no detail of treatment more vitally important to the patient than the placing of him in effective shelter*.

In closing I would thank most cordially those Devonshire friends, already named, who have done so much, with their encouragement and co-operation, to make this story possible; also outside Devon, Dr. F. J. H. Coutts, senior medical officer to the Ministry of Health; Dr. J. Wheatley, medical officer of health for Shropshire; and Dr. S. Barwise, medical officer of health for Derbyshire, for their valued interest and assistance; also Dr. A. C. Watkin for the great trouble he has taken in West Shropshire to procure me his independent results, and the medical superintendent of the Derbyshire Sanatorium for the figures he has sent me in spite of much pressure of work. To Dr. John Brownlee, Director of Statistics to the National Medical Research Committee, I would also express my deep indebtedness; the subject owes much to his initiative and skill.