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[Lind J \(1763\)](#). Two papers on fevers and infection. London: D Wilson.

**Title pages**

T W O  
P A P E R S  
O N  
F E V E R S  
A N D  
I N F E C T I O N.

Which were read before the  
Philosophical and Medical Society,  
In E D I N B U R G H.

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By J A M E S L I N D, M. D.  
Physician to the King's Hospital at Haslar, near Portsmouth;  
And Fellow of the Royal College of Physicians in Edinburgh.

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L O N D O N:  
Printed for D. WILSON, at Plato's Head, in the Strand.  
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Key passages

Lind (1763), p1-2.

T W O P A P E R S  
O N  
F E V E R S and I N F E C T I O N.

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P A P E R I.

*Haslar Hospital, near Portsmouth, 5 June, 1761.*

**O**F all the various diseases which afflict mankind, few are more fatal, none more frequent, than the fever. A Dissertation on this malady, but more particularly on the means by which it is propagated, and conveyed from causes hidden, and too frequently unsuspected, I propose as the subject of the following sheets.

A very extensive practice in fevers during three years, in one of the first hospitals in Europe, qualifies me, in some measure, for making researches into the dark and abstruse subject of *infection*. The reading of what has hitherto been published, has induced me to write my own sentiments upon it; and chiefly with an intention, to select from a *Chaos* of contradictory precepts, the most proper, simple, and effectual means of protecting particular persons, as well as guarding the community in general, from this contagion. But as a good foundation is necessary to a solid structure, so it is requisite that I begin with historical facts, which your excellent plan admits as the only basis of all physical disquisitions.

the method prescribed has had better effects, than any manner of treatment recommended by the authors I have read on this subject, or any that I have had opportunities of trying myself. And in relapses, where I have had the most frequent cases for a fair trial, as also in the nurses, and other servants of the house, I have been fully satisfied of its beneficial effects.

Most of the men sent from the *Garland*, and some other infected ships, were bled before they came into the hospital; but this operation is always dangerous, in proportion to the virulence of the taint. Fevers highly malignant will not bear bleeding; and though this operation be not so dangerous, nay sometimes necessary, in slight infections, where there is a fixed topical pain, with a full or hard pulse, yet it is to be remembered, that, after this evacuation, less benefit is afterwards to be expected from the vomit.

I cannot here omit further observing,— That much has been said against the promiscuous use of blisters, and their early application in fevers. In fevers truly inflammatory, and some other cases, I have known bad effects proceed from them. — But, what I have often reflected upon with astonishment, and has surprised the gentlemen who attended me, is,

that, very seldom in those ship-fevers, there was felt such a state of pulse, as forbid their use. What some mechanical writers in physic have advanced, concerning the theory of blistering applications, or their acting, as *stimuli*, and so always raising and encreasing the fever, is not applicable to the most of those fevers of which we treat, where, according to the nurses phrase, the patient generally received a *cool* from the blister.

When the infection by these means hath been removed, in 24 or 36 hours after the operation of the blister, the intestinal canal should a second time be gently cleansed, by giving rhubarb, with a small quantity of *vitriolated* tartar.

These observations claim the more attention, as not being a few remarks made in private, or on any one particular fever, which might prove an exception to a general established principle in practice \*. They are the result of an attention to some thousand patients, whose cases are still preserved in the hospital.

But as the best proof of the efficacy of any method, is the success with which it is attended, I here give you an account of the mortality amongst the nurses, servants, and all other persons in the hospital (exclusive of the patients), from *June 1758.* to *January 1760.*

In the first six months only one nurse died, of whose case I kept no particular account. In the year 1759. there died two of the labourers, and two nurses. With regard to the two former, how they were at first treated I know not. One of them, upon being taken ill, went to his own house, and there died of a fever. And the other I did not see till the eleventh day, when I observed him to be covered with spots, even to the end of his fingers; he had been a very irregular, drunken man. Of the two nurses, the first was an old woman, who seemed to die purely of a decay of nature, without any just suspicion of another cause. The second died in the following manner, of an infection.

Lind (1763), p78-80.

We would, indeed, advise all those, who intend to pursue the practice of physic, for the benefit of mankind, to peruse the histories of such observations as have been made on the fevers of different countries, as also the ancient relations of this distemper, together with the many excellent accounts of it published in our own times. For to the publication of such writings, all improvements in the art of healing are entirely owing, notwithstanding the contempt of the ignorant, the envy of the malevolent, and the censure

of defects, to which authors are exposed.

But knowledge in physic can never be attained otherwise, than by a series of observations; for which we must add to our own experience, that of men who lived before us, or who practise in different places, carefully separating experimental truths from *hypotheses*. By the help of this acquired knowledge of what from age to age, or in different countries, has, by the inspection of nature and diseases, been discovered or improved, the mind becomes stored with experimental truths and observations. And a physician, thus qualified, will be enabled to take a comprehensive and instructive view of the operations of nature, and of diseases under various shapes, and as influenced by the climate, seasons, weather, constitution, and medicines.

A judicious *synopsis* of the writings on fevers, in a chronological series, would be a valuable book. Treasures of useful knowledge might then be found in one volume, where, disengaged from conjecture, such naked truths should only appear, as are confirmed by observations and facts. We might

there contemplate, under one view, what advances have been made towards a more complete knowledge, and a more successful method of treating this malady, ever since the time of *Hippocrates*. This book would perhaps not comprise an abridgment of the works of above two hundred authors, whose observations would deserve a place in it. And although a very striking contrast would appear in the theoretical opinions of authors, yet a nearer resemblance, than perhaps is expected, might be found in the facts they relate. A better judgment might thence also be formed of the propriety of the peculiar denominations given to different fevers by the antient writers; and of the reasons why, in every age almost, the appellations of fevers have been changed; — whether also fevers are essentially different in different countries, varying likewise their nature at different periods of time. Conclusions drawn from partial facts, and an imperfect experience of things, would there be distinguished from more universal and better established truths. And, perhaps, the influence of a favourite opinion, or of a pre-conceived fancy on the writings of some, even of our best instructors, such as *Sydenham* and *Morton*, would more clearly be perceived.

Principle in practice\*

\* I have often thought, that publishing only one or two singular or particular cases, does more harm than good. If the effects of the medicine, or method prescribed, are said to be salutary, one or two trials of them cannot be thought sufficient to convince us, that nature, and the constitution alone, could not have performed the cures without their assistance. But if the case is published with a view to represent the bad effects of any particular medicine, or of any established method of practice, that may have proved evidently hurtful in this particular instance, what is the just inference from thence? Only this certain truth, that all the maxims of physic are limited, and that there is in it no universal infallible method, or remedy. And for this, there is one at least very obvious reason to be assigned, which is the peculiar *idiosyncrasy*, or constitution of each individual person; in respect of which not only food, but medicines act relatively. ] Hence mercury, bark, opium, and all other remedies, operate differently in various habits. In some, rhubarb will produce intolerable gripes, and to others, a simple dose of manna will become an emetic. Some are surprisngly affected with the smell of a particular flower, and eating the most innocent things hurts others; thus cheese, onions, and shell-fish, will produce sometimes *nausea*, vomiting, and symptoms as if poison had been taken. But why should I seek to multiply proofs of a thing so universally known? The just conclusion then, upon the whole, is this; that good approved methods and medicines are not to be discarded from practice, because, in some few instances, we are told, that they produced unfavourable effects; and in such cases the calumny is often most unjustly heaped on the medicine, where part at least may be due to the temperament or *idiosyncrasy* of the patient, whose particular constitution it is the business of a judicious physician to learn, study, and know, as much as it is to acquire a perfect acquaintance with the *materia medica*; and, in his final result of judgment, to have taken both under due consideration.

Further, not only in hospitals, but in all private families, persons in fevers ought to be kept separate from others, and free from the idle visits of over-officious friends, and of all disturbance and noise. This ought to be done more particularly, with a view to the enjoyment of that tranquil and hopeful state of mind, which must co-operate with the mechanical salutary effects of medicines towards a recovery. I can affirm, that I have known many patients, who may be said properly to have been killed by the fatal shock or surprise given to them by an account of the death of a friend, or of one or more persons, who were afflicted in the same state that they imagined themselves to be in.

Those who neglect the *medicina mentis*, or a due attention to the passions of the mind in sickness, are inattentive to one of the most important aids towards the relief of it. For, if the most wholesome food does not nourish, when the desponding mind is oppressed with grief and fear, neither can it be expected, with a person under the influence of those afflictive passions, that the most appropriated medicines will produce their salutary effects.

It would be altogether foreign from the purport and intention of these papers, to dilate on the speculative conclusions which might be drawn from the observations contained in them; I shall therefore, on that head, pray only for a very short indulgence. And it must first be premised, that the blood taken from persons in a fever, and even from persons in perfect health, though not so frequently, does, after standing in a clean vessel for a short time, commonly separate into three distinct portions; *viz.* the serum, or water of the blood, the red concremented mass,

and a viscid pellicle, termed the *fize*, which spreads itself on the top of the red concretion. Some time ago, when making experiments with the blood taken from persons in the scurvy, I was surpris'd to find it often covered with that fizy crust. This induced me to extend my experiments to large quantities of different blood, which I had opportunities of inspecting at once in so large an hospital. For which purpose I one morning ordered ten patients in the scurvy to be bled, taking two ounces from each. A larger quantity was taken, for its inspection, from two men in health. That day I had occasion to prescribe bleeding to a woman in labour, two hours before her delivery, to a girl of sixteen years of age afflicted with a lunacy proceeding from the *chlorosis*, to three patients in the rheumatism, and to a person labouring under an obstruction of the liver.

From a nice comparison, and an examination of the different blood, I found in all, that the more *fize* there was on the top, and the thicker and more viscid this white pellicle shewed itself, the concretion below it was generally of a more loose coherence. This was not so observable, when only some slight streaks of white appeared on the top. But when much *fize* had separated itself, the red mass became very soft at the bottom of the containing vessel; and less compact in its different parts, in proportion to their distance from the surface, towards which this whitish portion had ascended.