Hospitalism and zymotic diseases : as more especially illustrated by puerperal fever, or metria a paper read in the hall of the College of Physicians, on the 13th of March and 10th of April, 1869, before the Obstetrical Society of Dublin also, a reply to the criticisms of seventeen physicians upon this paper delivered on the 10th of July, 1869 / by Evory Kennedy.

HOSPITALISM

AND

ZYMOTIC DISEASES

EVORY KENNEDY, M.D.

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AS MORE ESPECIALLY ILLUSTRATED BY

PUERPERAL FEVER, OR METRIA.

A PAPER READ

IN THE HALL OF THE COLLEGE OF PHYSICIANS,

On the 13*th of March and* 10*th of April*, 1869,

BEFORE THE

OBSTETRICAL SOCIETY OF DUBLIN;

ALSO,

A REPLY TO THE CRITICISMS OF SEVENTEEN PHYSICIANS

UPON THIS PAPER,

Delivered on the 10*th of July*, 1869.

BY

EVORY KENNEDY, M.D. E.&T.C.D. Hon.;

F.K.Q.C.P.;

PAST PRESIDENT OF THE COLLEGE OF PHYSICIANS; PAST PRESIDENT OF THE OBSTETRICAL SOCIETY; EX-MASTER OF THE DUBLIN LYING-IN HOSPITAL; HON. MEMBER OF THE HUNTERIAN SOCIETY OF EDINBURGH; HON. MEMBER OF THE MEDICAL SOCIETY OF HAMBURGH; FORMERLY V. P. OF SOC. D'AFRIQUE DE PARIS; V. P. OF MED. CHIR. SOC. DUBLIN; ETC. ETC.

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1869.

TO

THE RIGHT HON. W. E. GLADSTONE, D.C.L., M.P.,

FIRST LORD OF THE TREASURY,

ETC., ETC.

SIR,

The purport of the following pages is to prevent the holocaust which is unconsciously offered to a spirit and an interest known as "Hospitalism."

To such an attempt, prejudice and a variety of motives which I need not analyse, oppose obstacles so formidable that, in the words of the great poet (with whom your profound Commentaries have familiarized so many of us), the exclamation—"Τίς Θεῶν"—naturally presents itself.

Happily, an appeal to the occupants of the Twenty Chairs, or a selection from the Theotechny is, in this emergency, uncalled for.

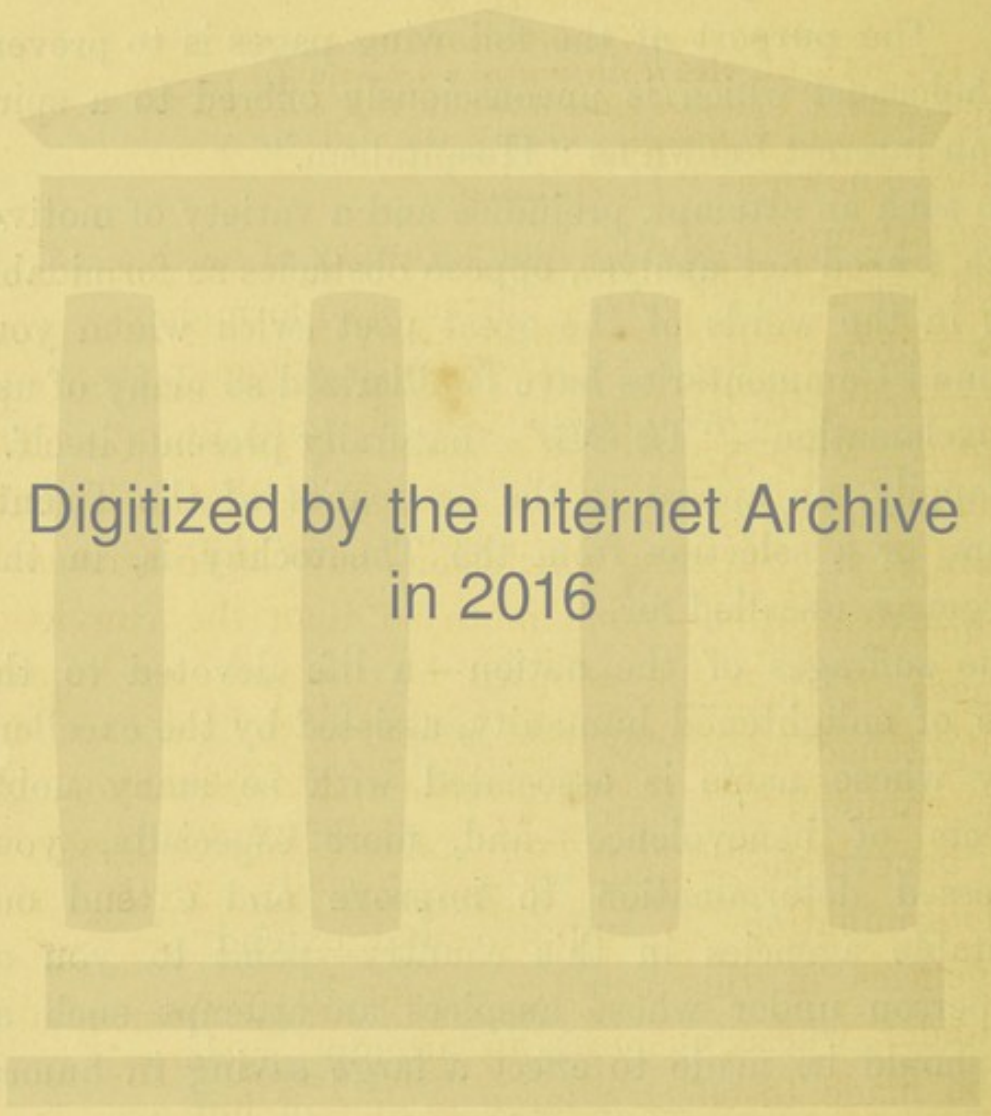
The suffrages of the nation—a life devoted to the cause of enlightened humanity, assisted by the excellent Lady whose name is associated with so many noble projects of benevolence—and, more especially, your expressed determination to improve and extend our charitable agencies in this country—point to you as the person under whose auspices an attempt such as this should be made to effect a large saving in human life.

Should I be fortunate enough to secure your approbation for this Paper, the justice and propriety of its objects will be established, despite the defects of its execution.

Your obedient servant,

EVORY KENNEDY.

MERRION-STREET, DUBLIN.



INTRODUCTION.

"I have come to believe that the hospital system is an intermediary stage of civilization. At the end of a life spent in hospital work to this conclusion I have always come—that the poor are better relieved in their own home."—*Florence Nightingale.*

It is a humiliating admission for humanitarians that misdirected benevolence is, in many of its results, near akin to malevolence. The subject of zymotic,[a] or miasmatic diseases, and the means available for their prevention and remedy, is one of universal interest at the present day. Startling conclusions have been arrived at in its investigation—none more so than the conviction that the very means adopted by us and our humane forefathers for lessening and relieving suffering and disease have conduced to its increase and confirmation, and thus absolutely caused a large unnecessary or avoidable mortality—nay, has even developed diseases previously unheard of.

To prove that this is the case, and yet not irremediably so, is the object of the following pages. The motto selected above shows that the grievances resulting from Hospitalism are pressing so heavily on the minds of the calmest and soundest thinkers, that the alternative of their annihilation would be preferable to the existence of hospitals as at present. I am not one of those who

[a] Names derived from Ζυμοω, to ferment, and μιαινω, to pollute. They include epidemic, endemic, and contagious diseases.

think thus. I believe that, properly conducted and improved, they may be of infinite value, and quite compatible with the saving of human life, and the humane intentions of their founders. The attempt was made, in the first instance, through my fellow governors of our great institutions to modify these so as to meet the urgency of the case, as guided by our advancing knowledge. But this having failed, it was determined, as the most legitimate mode of accomplishing the required improvements, to challenge a full discussion of the question on the part of the medical profession.

This was done by reading the following paper in the Hall of the College of Physicians, before the Society that had made the illustrative diseases their special study. The subjects mooted were fully discussed by seventeen Physicians throughout nine nights, when the reply, which follows, was delivered. The whole discussion has been published, and the public can now judge for themselves wherein lies truth.

The following pages contain, in addition to my original paper and reply, an Appendix, with a Curvilinear Diagram by Dr. Grimshaw, as well as additional matter bearing on the enquiry.

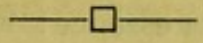
HOSPITALISM

AND

ZYMOTIC DISEASES,

AS MORE ESPECIALLY ILLUSTRATED BY

PUERPERAL FEVER, OR METRIA.



In presuming to draw your attention to Zymotic Diseases, and more especially Puerperal Fever (a disease with which you are all so familiar, and one with which my opportunities, I regret to say, have rendered me but too conversant), I shall not weary you with unprofitable details, but at once investigate, with your assistance, their true nature, their characteristic features, and the laws which regulate their production. The tenor and purport of my observations shall be directed principally to prevention; whilst I hope some hints may be thrown out in the course of our investigation that may also assist us in treatment.

In order to simplify our subject we shall begin by accepting your acquiescence in the broad principle, that puerperal fever is "par excellence" a zymotic disease.

As a motive for grappling with this class of diseases in the manner I propose to deal with puerperal fever, let us estimate the proportion of general zymotic mortality on the basis of the tables furnished by London and the other great cities of England. We may state it to be between one-fourth and one-fifth of the gross mortality; further, it may be asserted, for argument sake, that zymotic diseases result, in nine out of ten cases, from preventable causes. It follows then that by the prevention of these causes the mortality may be reduced one-fourth, minus one-tenth. It would be quite possible to offer a proximate calculation of the diminution of morbility under such a reduction of mortality; but the misery, suffering, distress, and poverty to be prevented by such a consumma-

tion would be beyond human calculation. The condition precedent to such results is the acquisition of an accurate knowledge of the principles that regulate the development and spread of each disease of the zymotic type; or, at least, of those laws that regulate their primary occurrence and subsequent growth. Whether the difficulties that invest the detection and analysis of the subtle poisonous miasm, and which have hitherto baffled[a] the efforts of physicians for centuries, are to continue unsolved, is a question within the womb of time. But although the miasm or germ has hitherto escaped our detection, as recognized by its sensible qualities, its existence as an entity or such primary principle is admitted universally.

The laws that regulate its development and spread are within the scope of our observation, and we know and can handle certain morbid solids and fluids in which the poison, at least, may be said to possess its habitat, if not its essence: witness the lymph of cow-pock, the pus of small-pox and syphilis, and the cutaneous powder of scarlatina, &c.

In the first place, are we to ascribe the different zymotic poisons to a common principle, modified by a variation in natural or physical circumstances or conditions? The fact that diseases of the zymotic character prevail so frequently at the same time, would appear to support this opinion. Their being traceable to the same sources would further tend to corroborate it, and although they assume very distinctive characters in their development, yet the same observation holds in the varieties observable amongst diseases which belong unmistakably to the same genus, showing that *they*, at least, have been due to a common origin. With the occurrence of metria in hospital after other zymotic diseases have, either accidentally or endemically, shown themselves, all hospital physicians are familiar, so much so, that they look with the greatest apprehension to the result when either typhus, scarlatina, or erysipelas occurs in their hospitals and maternities.

I cannot illustrate this necessity or the conclusions I have long

[a] The latest claim to the detection of the morbific principle is that of Dr. Harris, of New York, who states that he has detected in cattle infected with the prevailing plague a living organism or spongiole of definite form and properties, which multiplies in the body of the infected animal. Whilst scientific incredulity is to be deprecated in investigating so obscure a subject, quite as much as accepting hastily any doctrine or theory because it is new, I confess that looking for an escape out of one difficulty by accepting another and greater difficulty, requires at least deliberation and "confirmation strong" before adoption. The difficulties attending the elucidation of the zymotic molecule or germ can scarcely, in this view, be said to be simplified by presupposing the occurrence of so questionable a postulate as spontaneous generation, be it animal or vegetable.

arrived at, better than by transcribing here a question put to me by a Medical Commission, who were engaged in inquiring into the state of the hospitals and sick poor in the year 1840, with my answer:—

Query—"Does puerperal fever appear to have prevailed more extensively when any general epidemic has prevailed in Dublin?"

Answer[a]—"Yes; I have remarked that when continued fever, typhus, or erysipelas were prevalent in the Medical and Surgical Hospitals, puerperal fever appeared to prevail in the Lying-in Hospital, as well as in the city generally. The character of the fever varied also at different times, and occasionally appeared to be influenced in its nature by any prevalent epidemic."

So frequently has metria shown itself afterwards that it is now no longer esteemed an accidental "post hoc," but that they stand in relation of cause and effect. This fact alone goes a considerable way in confirming the idea of a common poisonous principle or miasm. Upon this subject I treated so lately in my paper on Purpuric Puerperal Fever, read before the British Medical Association in 1867, that I shall not dwell upon it further at present.

Should the principle of isomerism, which has of late attracted much notice with our chemists and professors of physical science, come to be established, the difficulties in adopting the idea of a common morbific poison would be lessened. As then the poisonous principle, which we might denominate *zymotocene*, would come to be classed as an original polyatomic molecule or principle, and would fall strictly within Dumas'[b] definition of what he terms *polymorphism*, namely—"one of those variations in the arrangements of integral molecules of a body which influence its physical properties, either temporarily or permanently."

There is nothing at all unreasonable, therefore, in supposing that, as in the case of polymeric or polymorphous hydrocarbons, we may also have the germs of different disease produced by polymeric combinations in the same elements.

To descend, however, from the consideration of these general principles to the special subject with which we are more immediately concerned—puerperal fever. This zymotic disease prevails endemically in crowded hospitals, where it is to be seen in its greatest virulence, and exhibiting its most concentrated fatality; although it is also to be met with in the hovels of the poor and the chambers of the wealthier classes. When epidemic, showing itself generally

[a] See page 124, *infra*; also curvilinear diagram in Appendix E.

[b] Lettre a M. Amphere Ann de Chemie et de Phys. xlviii., p. 208.

in our great maternity hospitals in the first instance, but not confining itself to them: like typhus fever, cholera, scarlatina, and erysipelas, it prevails endemically, and like them it is contagious.

The non-contagious furor having pretty well spent itself, reason resumes her sway, and contagion can *now* be spoken of with calmness, and toleration. Whereas twenty years ago the advocate of contagion was worse than an infidel.

The difficulties that beset medical men in investigating the epidemic and contagious nature of puerperal fever were simply expressed by the late Dr. Collins; when, after detailing its prevalence in the early years of his mastership of the Lying-in Hospital, and the success of the steps taken by him to lessen it, he adds—"The facts here detailed are strongly calculated not only to lead us to suspect, but even prove that this fever derived its origin from some local cause, and not from anything noxious in the atmosphere. To this I should assent," he continues, "had we not proof equally well authenticated, of its prevalence and fatality in the houses of the affluent, as already stated." If the views we have arrived at, and which we now venture to propound, with regard to the true nature of the poisoning in these cases be correct, then the contagious and sporadic nature of puerperal fever will be perfectly reconcilable. Collins's paragraph, above quoted, contains the gist of the puerperal fever difficulty in a nutshell. Its local cause approaches more nearly to a *constant* quantity in the wards of a crowded Lying-in Hospital; whereas it is only an *occasional* quantity in the houses of the affluent; and the only influence exercised in its production by the atmosphere is, that in certain states of the atmosphere, the constant and occasional quantities become *more operative* or *active* in generating or propagating this dreadful malady—a malady thus zymotic in its type and origin, produced by a poison emanating from parturient women; more active in proportion to the concentration of their excretions or exhalations, and consequently in proportion to their number cohabiting in a given number of feet of atmospheric space; but not requiring more than one parturient female to generate it; when the poison she herself has generated may, as in the case of blood-poisoning, be re-absorbed into her own system, and self-contamination then as certainly strike her down as if a crowded ill-ventilated lying-in ward was the generating medium. The most striking parallel that we are acquainted with of the generation and development of diseases under similar conditions are, the gaol fever (a disease now fortunately little met with), erysipelas in surgical hospitals, cholera in our camps and over-crowded human gatherings,

and tuberculosis as observed among the poor work people in Paris and other crowded cities, a fact that has given rise to the idea that phthisis should also be classed with zymotic diseases. In Paris, for instance, where this disease prevails so largely among the "ouvriers," it is calculated that 40,000 live huddled together in "chambres garnies," or furnished apartments, containing from eight to ten beds in each. The confining apes in close ill-ventilated menageries generates tuberculosis in these animals, apparently from similar causes; and glanders, a purely zymotic disease, produced by crowding horses together in ill-ventilated stables, furnishes us with a further example. Of the parallel with erysipelas, pyemia, hospital gangrene, and what we might term generally "hospital malaria," we shall have occasion to treat more at large when alluding to traumatic metria.

In distinguishing between puerperal fever or metria and its congeners, it will be unnecessary to my present practical audience to dwell upon afterpains, hysteralgia, or what I used to denominate to my class peritonidynia or gastralgia; but peritonitis and metritis may require a passing notice. Such cases, in the pure form as we see them occur unconnected with deliveries, are occasionally to be met with, and may be known as partaking more of the character of local inflammation without zymotic indications. These are the cases that will bear depletion best, and the use of the lancet is too much neglected in them. Twelve to eighteen ounces of blood extracted from the arm, followed by eighteen or two dozen of leeches, will often relieve and subdue all inflammatory symptoms, and the pulse that was hard, contracted, and compressed, will, as in ordinary peritonitis, rise to a more rounded resistance under this treatment.

The distinction I should draw between this form of disease and true metria, is that in the latter there are the characteristics of blood poisoning, the shrunken features, the depression, the unmistakable expression of countenance that the practical obstetrician cannot be deceived in.

In true puerperal fever of the present day the use of the lancet is rarely admissible; local depletion must be our sheet anchor. But in having recourse to it the secret of success is to reduce the pain by repetition of leeching before reaction has had time to establish itself.

The physician should see his patient every six or eight hours after the first attack; and if he find that his first leeching has not enabled the patient to bear pressure and relieved her from pain, he should at once apply more leeches before reaction is established,

and so on as long as the pulse will indicate the propriety of further abstraction of blood.

Allowing, according to circumstances, a longer interval between each leeching as he finds the necessity become less urgent or the state of the circulation less indicate its propriety. A neglect of attention to this rule has often, to my own knowledge, caused patients to be snatched away by this fatal disease, who, in the hands of less hesitating or more energetic practitioners, might have been fairly reckoned upon as surviving. I know no more harassing trial to the consultant, in these cases, than to have been called into and assisted in subduing, the acute symptoms by prompt and energetic treatment, and then, when matters have improved, to be relieved from attendance under the impression that the danger was over, only to be recalled to see the patient, perhaps, forty-eight hours later, beyond the reach of treatment, from the inflammation having been again allowed to creep on to an impending fatal issue. This has occurred so frequently in my own case, that it left me in doubt whether, for my peace of mind, I should not refuse altogether to see such cases in consultation. Let my younger, and, indeed, my older, hearers be advised by me, whenever they think it necessary to divide the responsibility in such cases, to make the consultant sustain his share of it, until *he* pronounces the case out of danger. This is only fair by the physician in attendance, as well as by consultant, as a contrary practice is sure to recoil upon the ordinary attendant, in case of a casualty. Another practical hint I would wish to impress upon my hearers is, as to the use of mercury in these cases. There is no doubt of its efficacy if you can produce its specific effects; but, in the worst cases, there is not time for this, and indeed the system seems to be insusceptible.

When peritonitis or metria were to be anticipated, as when the disease was prevalent, after manual interference, in labour, or for the removal of the placenta, my habit was, to commence at once after the labour was completed, with small and frequently-repeated doses of mercury, say a grain or half a grain of grey powder every third or fourth hour, and even applying in more suspicious cases mercurial ointment to the arm-pits and groins. By these means, without disturbing the patient's system to any serious extent, in the course of forty-eight or fifty-six hours a slight mercurialization may be perceptible, often before the time at which the disease could grapple the patient; and then, should metria or inflammatory symptoms show themselves, by pressing mercurials a little more freely the system may speedily be brought under its specific

influence, and the disease be thus easily checked if it had not already been prevented. I have only on one or two occasions seen puerperal fever prove fatal after mercurialization showed itself, and in them the action was incomplete. But, on the other hand, how often have I seen mercury in every dose fail in producing the slightest approach to its specific effects? I fear the answer to this question would exhibit a fearful array of baffled remedial efforts. I may mention, however, as apparently contradictory to this view a case that occurred under my care, in the Lying-in Hospital, of a woman who came in under the influence of mercury, which she stated she had taken only to the extent of three pills, as what she termed "sweetners;" and yet this woman was attacked with metria on the third day, and the attack proved fatal. Whether this was due to the fact that speedy ptyalism is not always a proof of constitutional mercurialization or to the failure of the prophylactic, my hearers may be the best judges.

In dealing with metria the two-handed treatment, that of relieving local inflammation, whilst we support the constitutional powers—the strength and the circulation, by administering food easily and rapidly assimilated into the circulation, are the two chief indications, the latter just as essential as the former; and, in the true *metria*, or well-marked zymotic puerperal fever, by much the most important of the two. It must never be forgotten that the zymotic metria, *especially*, as we see it in hospital, is a *poison fever;* and no matter how severely it sets in with local distress and anorexia, that it is essentially a disease of debility, passing more or less rapidly into a state of collapse; consequently, that however depletory our treatment may be to check or restrain local distress and lesions, we must look steadily forward and provide for the coming issue—collapse and sinking from exhaustion. Our treatment must, therefore, be directed to that, and nutritious broths, jellies, milk, and farinacious food should be as freely given as possible, compatible with their being retained on the stomach. The same observations apply to stimulants, but they require more judgment in their administration, and, as a general rule, must be given as we observe their effects to be satisfactory.

On the whole, however, I have little doubt that they are given too *sparingly* and delayed too *long* in their administration—delayed, in fact, until the patient is absolutely sinking, when the stomach will not retain them and they produce no reaction. There is, however, a form of this disease which I ventured to describe many years ago, in which the symptoms of marked collapse and

exhaustion set in early, accompanied with tympanitis, showing an exaggerated train of symptoms from the very commencement—countenance sunken; pulse small, rapid, and compressible; but this state not always accompanied by a corresponding amount of abdominal pain. I described this form as allied to the class of cases described by Laennec under the term "Factitie Debile." Now, in this case we cannot commence our stimulants too early, and can scarcely administer them too freely. It is incredible the quantity both of stimulants and nutriments that this class of cases will take. It is the form in which turpentine treatment best agrees. It seems to be one in which the intensity of the poisoning and the extent of the inflammatory lesions bear no relative proportion, or rather are often to be found in an inverse ratio, *post mortem* examination showing in them only slight effusion; sometimes merely a gelatinous effusion behind the peritoneum in the cellular tissue; perhaps only a large flabby uterus, with intense tympanitic distension, and often little or no effusion in the abdomen, pleura or other cavities, nor any lesion of veins or uterus.

I recollect a case of this kind attended about three years since in consultation with the late Dr. Hardy and Dr. Mitchell. We left the house on three occasions, scarcely deeming it necessary to make a fresh appointment, the patient's dissolution appeared so imminent; and yet she recovered perfectly and speedily, and with little or no resulting inconvenience, and is now a strong healthy lady. In this case the lady consumed two bottles of wine, a quart of beef-tea, and a pint of brandy in 24 hours.

In describing the varieties assumed by metria, we would denominate one what we should term traumatic metria, or that occurring as the result of injuries, operations, and lesions of any kind during the delivery. In this way, when zymotic metria prevails in hospital or in private, we know that operations or lesions that could at other times be calculated upon, as productive of little or no inconvenience, almost doom our patients either to fatal or alarming illness. The use of the forceps, of the crotchet, rupture of the perineum, abrasions, slight lacerations or injuries in the vagina, nay, even the use of the lancet in venesection, are thus followed by metria, often of the most indomitable character. In fact, the air of the maternity becomes charged with a poison, in many respects similar to what we observe in our surgical and military hospitals. In this way erysipelas, pyemia, arthritis, diffuse inflammation, phagedena, or hospital gangrene, run their course of havoc, exactly as in the operation wards of surgical institutions.

The year 1838 was replete in the Dublin Lying-in Hospital with these traumatic cases. I drew the attention of the British Medical Association to some of their varieties at their meeting in Dublin in 1867; and showed drawings made at the time by Kirwan, Conoly, and myself. I shall here give a case of traumatic puerperal gangrene, combined with cerebro-spinal symptoms and purpuric discolourations that occurred in that epidemic.

Margaret M'Gouran, aged thirty-two, who was doing well for two days after her delivery with the forceps, in consequence of tedious labour, was attacked on the morning of the 2nd of March, 1838, by cerebral symptoms, with rapid pulse and slight abdominal tenderness. Her excitement was so great that she became unmanageable, and was obliged to be restrained in bed. She complained incessantly of pains in her legs. The whole surface of the body, but more especially the face and lower extremities, became livid. Large blue coloured bullæ, filled with black serum, appeared on different parts on the lower extremities. The excitement subsided in a few hours into a state of depression, and she sank at noon on the 3rd, about twenty-eight hours from the commencement of the attack. Only an abdominal examination could be obtained of this case, when a small quantity of red serum was found in the peritoneal cavity, and some sloughing in the vagina. The subcutaneous cellular tissue of the body was the seat of congestive infiltration; this was particularly evident when a section of the adipose wall of the abdomen was made and the left lower extremity was livid.

The black serum brings to recollection my old friend Dr. Alison's case of the black fluid in the blistered vesicles.

Those who have had an opportunity of seeing this class of case will bring to mind that in arthritic metria the joints are generally secondarily affected, and that at intervals of several days, nay often of weeks after the peritonitis or metritis has run its course. It was not so in several of those, which I might term thundering cases of arthritis that occurred in 1838. On the contrary, the joints were the first organs attacked, and the knee, the elbow, the ankle, the wrist, but above all, and the most violent of all the sacro-iliac synchondrosis, assumed in turn the lead, as the structure attacked. In some there occurred erysipelatous inflammation of the buttock. The torture attending these arthritic forms is extreme in all cases, but in that where the synchondrosis was engaged it was absolutely intolerable. The fullest opiates failed to afford relief, and violent as was the outburst, and extensive

as was the structure engaged, although some of them proved rapidly fatal, others yielded to treatment: but only to that of the most prompt and decided kind; namely, the application of superficial caustics and escharotics, but chiefly to the free use of the actual cautery. Those cases in which lesions occurred in labour, as lacerations of the perineum, were attacked with hospital gangrene, when the free use of caustics and mineral acids, with chloride of soda lotion and antiseptic poultices, were useful. Erysipelatous inflammation of the buttock proceeded in more than one case to gangrenous sloughing and in another of this kind, although the ankle and knee were both extensively engaged, the woman recovered.

Bold incisions were made in some of these cases, under the advice of the late able and distinguished surgeon Abraham Colles; but the result was not satisfactory, and uncontrollable hemorrhage even followed their use.

A form of this arthritic inflammation occasionally occurs, however, that is less immediately destructive, and more chronic in its character, in which it assumes many of the characteristics observed in rheumatism, or more akin to and resembling those obstinate affections of the knee and ankle-joints to which young girls, and boys of scrofulous diathesis, are so liable. I have seen the knee, ankle, wrist, and elbow affected with this form. The free use of ice-bags to the joint, followed by, and combined with, the starch bandage, and the scoring the joint with solid nitrate of silver repeatedly, constitutes the best local treatment; whilst the constitutional health should be built up by full diet and tonics, but particularly alternations of steel, and the hydriodates and bromides, &c. By these means the inflammation gradually subsides, absorption occurs, and stiff joints are prevented.

The last variation of type, observed as a genus, of the general family of metria, to which I shall direct your attention, is what I designated in a paper read at the meeting of the British Medical Association, "Puerperal Purpuric Fever." I described it as a disease of peculiar malignity, that appeared in an endemic attacking the patients in the Lying-in Hospital, in December, 1837, then under my care, and which carried off thirty-five patients between that month and the month of April, 1838.

In a certain proportion of those attacked, symptoms exhibited themselves unlike what had hitherto been observed in metria. Those symptoms corresponded in their characteristics with what Dr. Marston clearly described in his two classes of the disease so

unfortunately denominated the "Black Death," and which attracted so much attention two years ago in this country,—Firstly, those expressive, as he justly terms it, of profound blood poisoning; secondly, those of cerebro-spinal irritation.

Owing to some mismanagement, the coloured illustrations that were to have appeared with this paper, have been mislaid, in their transmission from Edinburgh to London, and their publication with it has been prevented.

PUERPERAL FEVER, WITH RAPID COLLAPSE, PRECEDED BY CEREBRO-SPINAL IRRITATION, 1838.

Case I.—Anne Whelan had been drinking a large quantity of spirits before admission. In the progress of her labour she had convulsive twitchings of the arms, and tremulous motions of the hands, and a slight convulsive paroxysm. This woman made no complaint in the course of the day after her delivery. She appeared to sleep through the night, but was found, at the next morning visit, in a state of collapse, with her abdomen tympanic. Stimulants, externally and internally, were freely administered, and she died in six hours.

The only morbid appearance revealed, on a *post mortem* examination, was a small quantity of serum, tinged with blood, in the peritoneal cavity.

Case II.—Mary Sheridan had rigors on 17th January, 1838. Pulse 120. No local pain—(three days delivered). On the 20th, at two o'clock in the morning, had an attack; pronounced hysterical, with marked globus; crying and laughing. At eight o'clock she fell into a state of stupor, with stertor, and remained comatose until four p.m., when she expired.

Post mortem appearances.—The omentum and peritoneum opaque and covered with lymph; a pint of milky serum in peritoneal cavity; lungs coated with plastic lymph, congested and crepitating. Both pleural cavities filled with flaky lymph adhesions; transparent serum in pericardium; right auricle filled with fibrine; blood fluid; uterus soft and easily torn; ovaries embedded in lymph; subarachnoid effusion and effusion into ventricles.

The dissection here fully evidences the existence of puerperal fever. Now, this fact becomes the more important when we draw the analogy we attempt between the recent typhus and the puerperal

epidemic of 1838. Sheridan's case is one of several that have occurred to me in puerperal fever, in which the hysteric complication furnished an unfavourable prognosis. The same fact has been dwelt upon by Cheyne and Graves, and more recently by Hudson, when hysteria occurs in fever.

The next case we shall give is one in which head symptoms were also very markedly observed to complicate the fever, which ran to a more chronic form, with consecutive arthritis.

CEREBRO-SPINAL PUERPERAL FEVER, FOLLOWED BY ARTHRITIS.

Mary Lynch, delivered on the 30th January, 1838, was attacked by rigor on the 3rd February, with febrile symptoms; much flushing; pulse 30; some abdominal pain.

On the 4th delirium set in, with great restlessness, anxiety, and spasmodic respiration, headache, and rapidity and indistinctness of articulation.

5th.—Delirium continues, with headache, more distressing in occipital region. She struggles to get out of bed. Tongue streaked red; peculiar exhalation from tongue and mouth; sickness.

6th.—Violent and incoherent muttering, but answers rationally when addressed; pulse 136.

7th.—Raving and starting; a quantity of vapour issuing from the mouth; less complaint of head, but complains of abdominal pain, and of pain in left arm; pulse 144.

8th.—Slept well; no delirium; asks for drink; pulse 136; elbow-joint engaged; tender, swollen, hot, and red; stick nitrate of silver freely applied to it.

9th.—Delirium returned in the night; stupor and muttering; elbow more swollen; left knee and bursa engaged; vesical retention.

On the 10th the arthritis extended to the wrist; diffuse inflammation seized upon the buttock and the parotid gland; and the case assumed the characters of consecutive arthritis, and terminated fatally with effusion into pleura, on the 15th.

The dark discolouration observed in puerperal typhus, purpuric typhus, purpura, and in cholera, demand elucidation.

The hemorrhagic and scorbutic complications, to which it is referred, give us little assistance, and, with Flint, I have little doubt that its explanation must be looked for in the capillary system, and perhaps in the blood itself.

FEVER WITH BLUE DISCOLOURATION OF SURFACE.

Mary Carton, aged twenty-seven, was attacked on the 11th of April, 1838, three days after a natural delivery, with aching abdominal pain—quick pulse, and high state of excitement. On the 12th, pulse 134; expression of countenance bad; hands and feet blue.

13th.—Blue tint extends to face and trunk; pulse almost imperceptible; respiration gasping; little abdominal suffering, unless on pressure.

14th.—Sinking; died at ten a.m., fifty-six hours ill. No *post mortem* examination permitted.

PUERPERAL FEVER, WITH DISCOLOURATION OF LIMB, VESICLES, GANGRENE, AND COLLAPSE.

Biddy Fox, aged thirty, was seized with rigor, followed by febrile excitement on the 5th May, 1838—the fifth day after her delivery of a seven months' child, caused by fright. She had only been three hours in labour. The fever continued without any local affection or abdominal tenderness throughout the 6th, and at three a.m. of the 7th, she was seized with acute pain in the calf of the left leg. At morning visit her state was as follows:—Countenance anxious, and expressive of suffering, and great constitutional disturbance; pulse small, 96; calf of leg swollen, tense, and acutely painful on slightest touch; no pain up thigh, but some uneasiness on deep pressure, low on left side of abdomen; skin hot; constantly crying about pain of leg; twelve leeches were applied to calf of leg; at one p.m. the limb became discoloured, the discolouration gradually extending up the thigh and down to the ankle and foot, where vesications rapidly formed. Some punctures were made in the calf of the leg and popliteal space by my able colleague, the late Surgeon Abraham Colles, in search of matter; but the pulse became rapid and indistinct, and she rapidly sunk, and expired at seven p.m., fifty-six hours from the occurrence of the rigor, and sixteen hours from the seizure of the limb. The femoral and tibial arteries continued pervious throughout, and the temperature of the left limb was higher than that of the right.

No examination permitted.

PUERPERAL FEVER, WITH GANGRENE OF UTERUS: SYMPTOMS PRECEDING DELIVERY.

Mary Anne Brennan was admitted at 11 a.m. on the 8th Jan., 1839, in the ninth month of gestation. About two o'clock the pupil on duty reported that she complained of great pain in her right thigh. On examination it was found large, tense, very painful along the course of the rectus muscle; but tenderness did not extend upwards as far as the saphena vein. There was also some soreness in the right iliac fossa. Pulse quiet and small; tongue furred. States that about a week before she had had a rigor, followed by sudden loss of power in right leg, and that yesterday she had another shivering fit. Twelve leeches were immediately applied to the limb, and within an hour she expressed herself much relieved. At half-past five o'clock p.m. she was conversing with the nurse, when she was observed suddenly to sink, her arms becoming powerless. Notice was immediately sent to the assistant on duty, who found her pulseless, eyes glazed, feet cold, features collapsed—in fact, moribund. Stimulants were administered, but she expired in a few minutes.

I performed the Cæsarean section instantly, and removed the child, but it exhibited no signs of vitality, nor could its heart's action be detected at the moment of the mother's death.

On drawing backwards the abdominal parietes, the ovary and Fallopian tubes, particularly on the right side, were gorged with blood, and the structure of the uterus in their immediate neighbourhood was dark and softened—in fact, in a state of ramollissement. The fimbriated extremity was glued by soft, freshly-effused lymph to the peritoneum.

The following cases, passing into erysipelas, purulent depôts, cellular sloughs of the character of anthrax, and arthritis, show more strikingly how puerperal fever, like typhus, is ascribable to a poison which vitiates the blood and passes through a series of consecutive states, due to what are termed poison depôts in various parts of the body:—

Mary A. Keating was seized, on the 10th of December, 1837, with erysipelas of the elbow-joint, on the tenth day after her delivery. The solid nitrate of silver was freely applied to it, with relief. The gluteal region became engaged on the 12th, and the nitrate of silver was freely applied. The pain, which was intense, was relieved. She had much prostration, diarrhea, and rigors, at

intervals, and great nervous anxiety. Potassa fusa was now applied over the glutei.

The inflammation here became circumscribed, and, on the 17th, the sterno-clavicular articulation became engaged. Not until the 29th could matter be detected in the hip, when a puncture was made. Some matter escaped, and ultimately a mass of sloughed cellular tissue separated. She gradually improved, the joints engaged, returning to their natural state, and she left the hospital on the 4th of January, becoming convalescent.

The hopeless character, however, of the more urgent cases, in which the large joints and adjacent structures take on this poison action, may at once be seen by looking at these drawings, which exhibit the trochanter eroded by ulceration, the two hip-joints engaged, the sacrum in a state of destructive ulceration, whilst the soft parts of the upper part of the thigh and gluteal muscles were in an advanced stage of ramollissement, and those on the left absolutely sphacelated. Surely, if ever the true εσθιομενος of the Greek fathers in medicine, justifying the synonym of the Latin, *exedens*, *depascens*, *corrosivus*, is to be found in modern pathology, we have it here.

Sydenham and other authorities have long established the fact that fever at different times and places may be characterized by peculiar and various local tendencies. And science, it is remarked, is no better prepared to explain their occurrence now than at any past period in medical history. By some, these varieties are merely considered complications or inter-current symptoms; by others pathognomic essentials, which justify the conclusion that the diseases are distinct in cause, symptoms, history, and pathology, and ought, consequently, to be so in name. In investigating both the predisposing and occasional causes which exist without the body of the diseased person, the "*circumfusa*," "*ingesta et applicata*," arising from the influence of external bodies, we too frequently lose sight of the assistance to be gained from the study of "Epizotia" or diseases of animals. In epidemic and contagious diseases this is much to be regretted. It was long since remarked by Guersent, that the circulatory and nervous systems of warm-blooded animals approach nearly to our organization, and their diseases present an analogy with ours. "The essential and symptomatic fevers ascribable to excitation of the nerves and blood-vessels, and the reaction of these organs upon each other, occur in them, and especially in the mammalia, with the same characters and almost the same names.

The resemblance between *their* maladies and those of *man* are sometimes so perfect, that it is impossible not to place them in the same "nosographic câdre," and not to assign them the same name. The discrepancies of opinions upon a name to be appropriated to the late epidemic might have been reconciled or harmonized had the phenomena observed in some of the epidemics which have prevailed amongst animals been looked to. For instance, in the epidemic typhus which prevailed amongst horned cattle in France in the year 1795, as well as in that of 1814, M. le Professeur Dupuis constantly observed "that the spinal marrow was more injected and soft than natural, the meningeal coverings were red, and contained in their folds a large quantity of limpid and transparent serum, and, in the lumbar region, the medulla was softened, and the cellular tissue of the lumbar and sacral nerves gorged with blood-serum. The brain in these animals dying of typhus was not so often observed softened as the medulla, but sometimes the meningeal coverings were more injected and red, and the ventricle often filled with a quantity of citrine serum."

Cerebral symptoms have also been observed in epidemic catarrh of dogs, assuming, in its progress, the form of epilepsy, or dance of St. Guy. Edward Jenner tells us of a gentleman who killed several of his dogs labouring under this disease, supposing it hydrophobia. Dissection displayed a soft brain; the ventricles full of serum, and the medulla softened, and fluid in its membranes. The "epizootic charboneuse" or typhus of domestic fowl, which prevailed in Paris in 1780, and whose chief complications were gangrenous angina and ophthalmia, was accompanied with convulsions of the wings, and other parts of the body, and the bird expired after a rattle of a peculiar kind, apparently convulsive, which resembled a plaintive cry proceeding from the bottom of the throat, not unlike spasmodic croup. On dissection, the brain was found gorged with blood, the interior of the bill and pharynx and external parts gangrenous, and ecchymosis in the different viscera.

We adduce these cases to show that, in the animal as well as man, modifications of known diseases occasionally occur, as in the puerperal cases observed by me in 1838, as well as in the latter cerebro-spinal cases *intercurrent* with the ordinary disease. The original type or generic characters prevailing and existing, whilst the varieties assume a more less prominent position, throughout its progress. Where these occur, as we might say, accidentally, in a peculiar epidemic, as, for instance, in the five cases of parotiditis out

of thirty-five recorded by Dr. Flint, and leave no permanent distinctive features, merely cropping up sporadically subsequently; they should be treated as complications due to causes in operation at the moment, evanescent in their nature, and in no way necessary to characterize the case, therefore, unworthy of a special name. When, however, on the other hand, we have a train of symptoms nearly constant in their occurrence, such as a peculiar eruption present, a peculiar structure engaged; non-contagious in its nature, not transient, not accidental in its occurrence, but permanently established amongst us, and traceable, as it is often, to cognate causes; then, I think, we must grant such a train of symptoms a right to be considered a distinct disease, and endow it with a special name, as we have done in the case of typhoid or enteric fever.

It remains still to be proved whether the group of symptoms observed to occur in the recent epidemic, shall recur and become permanent, as has been the case in enteric typhoid, or whether they shall assume those distinctive marks which have so isolated the latter disease, the very name applied to which evinces that it exhibits generic characters that assimilate it to a parent type.

The non-recurrence for nearly thirty years of an epidemic exhibiting the same characters as the cases described by me in this paper as puerperal typhus, would lead us to hope that such may possibly be the case as regards the recent epidemic also, and, consequently, that, failing in its element of permanency, it might only be referred to hereafter as a form of typhus prevailing in the spring of 1867, and limited to a small number of cases, attended with cerebro-spinal symptoms, discolouration, and collapse, and which may take its place with the puerperal typhus of the Dublin Lying-in Hospital of 1838, with epidemic typhus amongst horned cattle in 1795 and 1814, and with the typhus charboneuse in domestic fowl of 1780—all of which partook more or less of the same pathognomic characters.

It will be recollected that in the commencement of this paper we stated we had in view two practical objects—the cure and prevention of puerperal fever—but that the latter should occupy our special attention. To deal with both required a knowledge of what we termed the principles of the disease, or, more correctly, of the laws which observation and comparison satisfy us are those which govern it in its occurrence and recurrence.

We further stated that it was a disease purely zymotic in its character, consequently subject to the laws which hold in zymotic disease.

We stated as a motive to our investigation that growing knowledge had led to the conviction—a conviction every day strengthening—that zymotic disease is, in a great measure, preventable.

Hitherto we have been occupied in describing the varieties of metria with which I myself have been made familiar; and as it is of the utmost importance that no misapprehension, confusion, or deception should exist upon the subject, I wish it to be understood distinctly that every form of the disease described by me partakes of the zymotic character, is subject to the laws regulating this family of diseases, is capable of extension in the same manner as endemic, epidemic, and contagious disease, but, above all, that (by proper means) it *is preventable.*

Now, as it is impossible to be too explicit on this subject, where so much is at stake, we shall state that whilst we cannot, of course, object to the subdivision of diseases of the same type according to the structures engaged, for the sake of classification, we deem it quite important, for the sake of treatment, but, above all, of prevention, that diseases, especially of the zymotic type, should be so classed and arranged as to identify their common causes, predispositions, origins, treatment, and means of prevention. We mention this because the neglect of this rule renders our treatment empirical, frustrates our elucidation of the principles which develop the disease, prevents its recognition, and perpetuates its ravages.

We shall therefore classify the whole family under the head of "zymotic metria," in preference to that by which it has been hitherto received, of "puerperal fever;" and under this common head we shall treat, as modifications of the same disease, puerperal fever, metritis, peritonitis, pleuritis, phlebitis, arthritis, pyemia, purpuric, or cerebro-spinal metria, traumatic metria, erysipelas, and hospital gangrene.

This I feel fully justified in doing, because I have traced one and all of them to the same contagium; several of them have prevailed in the same patients, and again and again in different patients at the same moment, under my own observation, when the hospital under my care was charged with the same zymotocene; the varieties cropping up in adjoining beds, and referable, as in the traumatic form, to circumstances and causes traceable at the time.

Again, in the cases successfully treated, it will have been observed that the same treatment has been successful with all, supporting the strength, meeting local inflammation by mercury, local depletion, counter irritation and turpentine, issues of different kinds, and the decided but judicious use of stimulants.

The task now remains to us to state to what this contagion is due, or rather upon what combination of circumstances it depends; and in order to do this we must establish certain propositions:—

1. That puerperal metria is due to the absorption of a poison by the parturient female.

2. That this poison may be generated by any parturient female; and, where the circumstances are favourable to its imbibition, it may be absorbed into the system of the generator or that of any other parturient female exposed to its influence.

3. That the generation and absorption of this contagion is in a direct proportion[a] to the number of parturient females cohabiting in a given number of feet of atmospheric space, at their parturient period, or who breathe the same atmosphere when lying-in.

4. That in lying-in hospitals, where large numbers of patients are delivered under the same roof, this disease finds its habitat, appearing and reappearing at uncertain intervals.

5. That its appearance, although apparently capricious, is not infrequently traceable to the occurrence of other zymotic diseases, to a general unhealthy state of the hospital, the labours for some time being succeeded by bad recoveries, before the true zymotic metria exhibits itself.

6. That it is produced by contagion, long experience proves, following in the steps of certain practitioners, whilst others are totally free from it, and that in the same locality.

7. It is endemic, confined in its occurrence to certain localities.

8. It is not only confined in its occurrence to a given hospital, but it is observed to haunt certain wards of the hospital, and this to such a degree that I have been obliged to close up for many months wards in which it established its special habitat.

So much for our positive propositions.

Now for the negative; and whilst we freely admit that negative evidence is less valuable, we look upon it that here it is essential to complete the chain of reasoning.

9. Zymotic metria is not a disease peculiar to parturient women

[a] See page 10, *supra*, eleven lines from bottom of page.

confined in their own houses, occurring comparatively rarely among them.

10. It is, therefore, not a disease observed to prevail in small lying-in hospitals or cottages where only one or two patients cohabit in their lying-in.

11. The just and inevitable conclusion from the foregoing propositions is that by continuing the system of large lying-in hospitals we are causing the death by zymotic metria of a number of patients for one that would occur under any system that would secure the separation or isolation of women in their confinements.

12. But as hospitals possess advantages facilitating the cure of patients and as schools of instruction, it is quite possible to combine these advantages with those of the separate system by means of grouped but isolated cottage or pavilion hospitals, with only one, or, at most, two beds in each isolated room.

13. That consequently with our present knowledge of puerperal fever the conclusion is inevitable that the mortality among parturient women would be greatly lessened by an alteration in the construction and arrangement of lying-in hospitals.

As I have already dwelt sufficiently upon the first and second proposition in the first part of my paper, we shall now confine ourselves to the description of the subsequent propositions, beginning with the third and grouping the fourth along with it as a corollary, that the generation and absorption of the metria contagium is in direct proportion to the number of parturient females cohabiting in their lying-in, and that its habitat is the great lying-in hospitals. In affording proof of these positions, as in all the others I venture to propound, I shall give, in the first instance, my own experience, which I regret to repeat is but too comprehensive of this dread disease; and then the reliable experience of such other authorities as bears on the subject at issue.

The statistics of my own private practice, not of course including consultations, were, that out of about 3,500 deliveries, three fatal cases of puerperal fever or metria occurred. That is something under 1 in 1,200; whilst in the 13,157 patients delivered under my care from the years 1834 to '40 inclusive, in my mastership of the hospital, the number of fatal cases of zymotic metria were 117, or a little more than 1 in 112. Therefore, of the patients delivered under my own care, and for whom I was responsible to God and man, ten and eight-tenths, or almost eleven died of those in my charge in hospital for one in private, or in their own homes. This,

gentlemen, is an awful consideration; and, mark me, had we then possessed our present knowledge, this was all preventable, as the result of our investigation will prove.

It may be objected to this that the lying-in hospital admits the worst cases, many of them advanced in labour sent from considerable distances, exhausted and half starved. True, these matters must be taken into account as increasing the mortality; but, alas! there was a large margin to include all these cases, as the 117 deaths above reported merely included those who succumbed to metria. 101 more deaths also occurred in the 13,157, ascribable to child-birth and other diseases, a number amply sufficient to include every casualty and peculiarity to which hospital patients are obnoxious, and leaving untouched the grim array of 117 deaths by preventable metria.

Let us take the five years for which the Census Office in Dublin furnish data upon this subject, *i.e.*, from 1864 to 1868 inclusive. Although we do not obtain from these tables the number of deliveries, yet we can approximate to this pretty accurately by deducting the twin-births from the gross births, thus:—

Total births for the six years amount in the Dublin Registration District to	52,126
Deduct the usual ratio of twins, 1 in 60, as furnished by Collins's tables, the largest proportion of any country of which we have record, . . .	862
That gives deliveries	51,264[a]
Divide into this the total number of deaths, including childbirth (197), and metria (251), . . .	448

The result is one death in 114½. This result, however, includes the deaths by puerperal fever in the hospitals, as well as generally throughout the city and suburban districts.

If we deduct only the deaths in childbirth (197) this would leave the mortality one in 265; but this would be manifestly an objectionable standard of comparison, as a certain proportion of sporadic cases of metria occur throughout the district, and at times this even assumes an epidemic character. We should be justified, however, in deducting the cases of metria occurring in hospital from the gross number of deaths, and then allowing an equivalent for the average

[a] See page 74, *infra*.

proportion of cases of metria occurring in the District without the hospitals, for the five years calculated, thus:—

In the two Lying-in Hospitals, from 1864 to 1868 inclusive, there were delivered . .	D. L.	5,758
	C.	2,266[a]
Total		8,024
In the five years recorded the deliveries amounted in the city to		51,264

Outside of the hospitals the proportion by metria was 1 in 457, whilst in the hospitals it was 1 in 60. Allowing then an equivalent of deaths by metria within the hospitals for that without, one-seventh and one-half a seventh, including extra casualties and difficulties, that would leave it that 6 women died out of every 7½, from being delivered within the walls of the hospitals, instead of in their own homes, or properly constructed huts or cottage hospitals. But if we take the two preceding years, so as to give a seven years' equivalent to my mastership already alluded to, the deaths in the hospitals from metria amount to 1 in 50 for the seven years, from 1862 to 1868 inclusive; and taking it for granted that the deaths from metria bore the above-stated proportion outside the hospitals, then the result would be that in the City of Dublin alone 7½ women die out of every 9 from being confined in hospitals; in other words, that in all the deaths that have occurred in Dublin for the last seven years in parturition, out of every 9 deaths 7½ women have died, who would in all human probability be at this moment alive had they been confined in their own homes, or in isolated cottage hospitals.

The Registrar-General's reports for England and Wales give, for three years, an average of:—

1 death by metria in 726; 1 by childbirth in 306 deliveries.
From both causes, 1 in 223.

In London:—

1 death by metria in 515; 1 by childbirth in 429;
From both causes 1 in 239.

In 27 cities and towns in England the mortality was, in the two years, 1862–63,

1 in metria in 781; from accidents of childbirth, 301.
From both causes, 217.

[a] This sum ought to have been 2,366, but as the result is so slightly affected the original figures are retained.

The Registrar-General of Scotland gives, for the two years, 1861–62, deaths:—

1 by metria in 400; by accidents in childbirth, 1 in 345.

From both causes, 1 in 225.

In Glasgow the deaths were, from both causes 1 in 227; of these two-fifths died of metria, and three-fifths from accidents in childbirth.

In seven of the most trading towns in Scotland the mortality was in the proportion of 1 death to 170 deliveries. The deaths from metria were about a third less than those from accidents in delivery.

But it is stated that, even admitting the mortality from puerperal fever to be greater in large lying-in hospitals, that this mortality is more than counterbalanced by the lesser number who die in them from other causes, or, as it is termed, from childbirth, and that, as the saving of human life in the aggregate is the object aimed at, the lying-in hospitals best attain it; but the plea, I regret to say, is totally fallacious, as the mortality from childbirth exclusively in the Dublin and Coombe Lying-in Hospitals combined amounted to 1 in 130 from 1864 to 1868, whereas those in the Metropolitan District for the same period only amounted to 1 in 260; and, as we have just seen, the average mortality from accident of childbirth in England and Wales for three years was only 1 in 306. That of 27 large towns in England was, in childbirth, in the year 1862, only 1 in 290, and in 1863, only 1 in 312.

In Scotland the deaths from childbirth, independent of metria, were only 1 in 345 births in the years 1861–62. And yet those who propose the fallacy alluded to would still assert that the recoveries are better and the mortality less from childbirth in great lying-in hospitals, if we exclude deaths by metria, than in the houses of the poor, or in cottage hospitals.

The mortality in the Waterford Cottage Hospital from childbirth, independent of metria, we should have mentioned was only 1 in 180.

Is it useless now to dwell upon the retrospect of what has occurred? The mischief is done, and may only be alluded to as a beacon to warn us against the continuance in such a fatal course. Out of the 190,783 deliveries occurring in the Lying-in Hospital since its foundation, 2,627 deaths have occurred, or 1 in 72½. Now, taking the ordinary calculation of our present death-rates at 1 in 116, according to the Commissioners' return, that would leave a fearful avoidable or unnecessary loss of life in the difference between 1 in 72 and 1 in 116.

DEATH-RATES OF DUBLIN LYING-IN HOSPITAL, FROM ITS FOUNDATION TO 1868:—[a]

Year	Number of Patients	Deaths	Death	Deliveries
(b)	3975	45	1 in	88
1757	55	1	1 in	55
1758	454	8	1 in	50
1759	406	5	1 in	110
1760	556	4	1 in	139
1761	521	9	1 in	52
1762	533	6	1 in	88
1763	488	9	1 in	54
1764	588	12	1 in	49
1765	533	6	1 in	88
1766	681	3	1 in	227
1767	664	11	1 in	60
1768	655	16	1 in	41
1769	642	8	1 in	80
1770	670	8	1 in	80
1771	695	5	1 in	139
1772	704	4	1 in	176
1773	694	13	1 in	52
1774	681	21	1 in	32
1775	728	5	1 in	145
1776	802	7	1 in	114
1777	835	7	1 in	119
1778	927	10	1 in	92
1779	1011	8	1 in	126
1780	919	5	1 in	183
1781	1027	6	1 in	171
1782	990	6	1 in	165
1783	1167	15	1 in	77
1784	1261	11	1 in	114
1785	1292	8	1 in	161
1786	1351	8	1 in	170
1787	1347	10	1 in	134
1788	1469	23	1 in	64
1789	1435	25	1 in	57
1790	1546	12	1 in	129
1791	1602	25	1 in	64
1792	1631	10	1 in	163
1793	1757	19	1 in	92
1794	1543	20	1 in	77
1795	1503	7	1 in	214
1796	1621	10	1 in	152
1797	1712	13	1 in	131
1798	1604	8	1 in	200
1799	1537	10	1 in	153
1800	1337	18	1 in	74
1801	1725	30	1 in	57
1802	1985	26	1 in	74
1803	2028	44	1 in	46
1804	1915	16	1 in	120
1805	2220	12	1 in	185
1806	2406	23	1 in	104
1807	2511	12	1 in	209
1808	2665	13	1 in	205
1809	2889	21	1 in	137
1810	2854	29	1 in	98
1811	2561	24	1 in	106
1812	2766	43	1 in	64
1813	2484	62	1 in	40
1814	2508	25	1 in	100
1815	3075	17	1 in	180
1816	3276	18	1 in	182
1817	3473	32	1 in	108
1818	3539	56	1 in	63
1819	3197	94	1 in	33
1820	2458	70	1 in	35
1821	2849	22	1 in	129
1822	2675	12	1 in	220
1823	2584	59	1 in	44
1824	2446	20	1 in	122
1825	2746	26	1 in	105
1826	2440	81	1 in	30
1827	2550	33	1 in	77
1828	2856	43	1 in	66
1829	2141	34	1 in	63
1830	2288	12	1 in	190
1831	2176	12	1 in	181
1832	2242	12	1 in	187
1833	2138	12	1 in	178
1834	2024	34	1 in	60
1835	1902	34	1 in	56
1836	1810	36	1 in	50
1837	1833	24	1 in	76

[a] The red figures (8 in number) exhibit the *healthy* years, or those free from metria. The blue figures (13 in number) exhibit the years which were *tolerably healthy*, as tested by the fair rate line in curvilinear diagram. The black figures (92 in number) exhibit the *unhealthy* years, or those in which metria prevailed in the hospittal, confirming its endemic character.

[b] The first line gives the total deliveries and death-rate in Mosse's first hospital in George's-st. up to 1757.

Year	Number of Patients	Deaths	Death	Deliveries	Year	Number of Patients	Deaths	Death	Deliveries
1838	2126	45	1 in	47	1855	1060	35	1 in	30
1839	1951	25	1 in	78	1856	1600	25	1 in	64
1840	1521	26	1 in	59	1857	1509	33	1 in	46
1841	2025	23	1 in	88	1858	1084	30	1 in	36
1842	2171	21	1 in	103	1859	1389	21	1 in	66
1843	2188	22	1 in	99	1860	1404	26	1 in	54
1844	2176	14	1 in	155	1861	1135	59	1 in	19
1845	1411	35	1 in	40	1862	800	58	1 in	14
1846	2025	17	1 in	119	1863	1228	32	1 in	38
1847	1703	47	1 in	36	1864	1184	26	1 in	46
1848	1816	35	1 in	52	1865	1332	30	1 in	44
1849	2063	38	1 in	54	1866	1074	40	1 in	27
1850	1980	15	1 in	132	1867	1146	40	1 in	39
1851	2070	14	1 in	148	1868	1022	39	1 in	27
1852	1963	11	1 in	178					
1853	1906	17	1 in	118	113	190783	2627	1 in	72
1854	1943	37	1 in	53					

But in stating the gross mortality of the Dublin Lying-in Hospital now as 1 in 72 I should lead you into an error, as you will perceive by this table of the annual mortality, given from its foundation, that for the last fifteen years it has never been less than 1 in 64, and that it has in one year risen to 1 in 14. The average of these fifteen years is about 1 in 31⅓; and this is the state of things we have now to contend with, one sufficiently grave to demand our immediate attention; nor let it be said that this is an accidental epoch of exaggerated zymotic poisoning, and that if we have patience matters will be restored to their former less alarming rate of mortality. Fifteen years is a sufficient period in all conscience to test whether an epidemic is temporary or permanent in its nature, and we must not conceal from ourselves that, comparing the epochs, the death-rate increases in a fearful ratio. If you will cast your eye along the table it will give you at a glance the longest duration of these endemic hauntings to which the hospital has been liable since its foundation. Observe where there are two figures in place of three, and you will thus arrive at a rough result. From 1761 to '65 was one of these, again, from 1767 to '70, and from 1772 to '74. From this year down to 1800 we meet only an occasional unhealthy year, but it remains at two figures until 1804, and so on with only an occasional unhealthy year until 1818, the most crowded state the hospital ever was in; in that year there were 3,539 deliveries. *For three years the mortality then rose to* 1 *in* 44 ; in 1826 it arose again, and

continued for the next four years at 1 in 50. It returned to a state of unexampled health for the next four years of Dr. Collins's mastership, the deaths averaging 1 in 186, so much so that my lamented friend was in great hopes that by the system of ventilation, purification, and separation he had so strictly established, he had obtained the key to its annihilation from the institution. I succeeded him in the charge of the hospital, having been his assistant, and co-operating with him in carrying out all his plans. But, alas! I had too soon to learn that all his plans and precautions were unavailing to stop this fearful malady. The metria attacked the hospital in 1834, and from that year to the present hour, that is, a period of thirty-five years, only on seven occasions, namely, 1842, '44, '46, '50, '51, '52, and '53, have the proportions amounted to three figures. In my seven years' mastership, one year of which I never was without metria, notwithstanding every precaution (with experience gained in working Dr. Collins's system with himself) my gross mortality amounted to rather less than 1 in 61. This mortality was fearful enough, but when we state that out of the 18,906 patients delivered in the hospital for the last fifteen years 603, or 1 in 31⅓ have died, and that under the ablest superintendence and the most assiduous care, then, I affirm, there is clear convincing evidence that there is a something so defective and objectionable in the system of the hospital itself, that not a moment should be lost in correcting it in the manner that experience, science, and observation best dictate.

But let it not be supposed that we look upon the result of Lying-in Hospital practice in this city as the most fatal to be found; on the contrary, it is one of the least fatal of the great Lying-in Hospitals throughout Europe, showing that not to the want of skill and talent is due the fearful mortality upon which we dwell, but to an inherent defect of the gravest character in the system or construction of these institutions, and one that baffles human ingenuity to correct as long as they continue to be constructed upon their present vicious and faulty plan.

We shall, in proof of this position, now give a few statistics upon the Lying-in Hospitals generally, and request your close attention to the figures and the rateable mortality in reference to the size of the hospital and number of patients cohabiting:—

Large Hospitals.

		Mortality.		Deliveries
Liverpool, . . .	156 patients	1	in	99
London Hospitals, all, .	2,000 ,,	1	in	77½

		Mortality.		Deliveries.
Coombe, . . .	404 annually	1	in	72·25
D'Barnes' several Continental Hospitals, . .	14,253	1	in	56¾
Glasgow, . . .	352	1	in	54½
Dublin, . . .	1,334 annually averages	1	in	31⅓
St. Petersburgh Midwives' Institution, . .	1,714 annually	1	in	26⅔
St. Petersburgh Hospitals, .	1,715 annually	1	in	22
Vienna,[a] 1838, . .	4,453 annually	1	in	25
Paris Gen. Hospitals, 11, 1862	433 or 4,764 in 11 Gen. Hospitals,	1	in	18½
Paris, 1862, . . .	2,204	1	in	18½
Cottage Hospitals.				
New Ross, . . .	30 annually, total 924	1	in	185
Waterford, 23 years, .	115 annually	1	in	295
Limerick, . . .	367	1	in	367

We shall now give the statistics of the maternities which supply home attendance in midwifery cases, as furnished by Mr. Simon, and with which you are rendered familiar by Dr. Phelan's elaborate and convincing pamphlet, to which I beg to call your strict attention.

Royal Maternity, London, . .	18,751 in	5 years,	1 died in	334
St. Petersburgh, reported by D. Hugenbeyte, ...	207,582 in	15 years,	„	143
Glasgow Lying-in Hospital, patients attended from	729 in	2 years,	„	73
Coombe Lying-in Hospital, patients attended from	4,473		„	223

The statistical part of this report cannot be better summed up than in Dr. Leon Le Fort's calculations and words:—

"Of 888,312 women confined in maternities or hospitals, 30,594 died; and of 934,781 deliveries effected in the towns, 4,405 were followed by death." The mortality was, in the first case, one woman out of 29 confined; in the second it was only one out of 212 cases, but M. Leon Le Fort's three essential parts or propositions ought to be written in gold—they are as follows:—

"The women who are confined in hospitals and maternities, not only die there in much greater numbers, but die in quite unusual proportions compared with those who are confined in their own dwellings.

[a] See Appendix B, page 114.

"The cause of this frightful mortality must be attributed to puerperal fever, and it is by contagion that this destructive scourge exerts its ravages.

"It is absolutely necessary to take serious hygienic measures, and if the malady cannot be prevented from breaking out, it is however possible to impose barriers to it, and to say 'Thou shall go no further.'"

The capricious occurrence of metria is proverbial, in so much so that no hospital physician can calculate for any length of time upon a healthy state of his hospital continuing. This is perfectly true; yet there are precursors that cause the physician of experience much anxiety, and lead him to dread an outbreak. The first of these is the occurrence of other zymotic disease, either epidemically in the city or more particularly in his own institution. Typhus[a] fever, scarlatina, and erysipelas, are those diseases that especially attract his notice, and the history of the Lying-in Hospital, as well in my own time as subsequently, has corroborated the fact that the appearance of these diseases, but particularly of typhus, is a fatal prognostic, and should put us at once on our guard. When such cases come into hospital they should be immediately removed to contagious hospitals, if this be possible, and if not separated and isolated, allowing no intercourse whatever with the atmosphere or occupants of the other parts of the building. The number of patients should be limited in admission, and if the disease shows itself the hospital closed altogether for a time.

Another precursor to the outburst of metria is the occurrence of bad recoveries after labour, particularly if operation, or manual interference had been necessary. This state may go on for some time, and that without a death occurring. When the hospital physician meets this threatening aspect of affairs let him adopt the same precautions just insisted upon in zymotic disease occurring; but above all, that of lowering the number of admissions or shutting up the institution, as, most assuredly if he do not, the outburst will speedily be upon him.

Prop. 6.—The proposition that metria is contagious is one that I am just as well satisfied of as that typhus or erysipelas is contagious, whilst it is not so markedly so as scarlatina, measles, or small-pox, I freely admit. The occurrence of the sporadic cases of this disease, leaving no possible trace by which they can have spread by contagion from another parturient female has, no doubt, given rise to, and confirmed the idea of, its non-contagious nature;

[a] See Appendix E, page 126, with Dr. Grimshaw's curvilinear test.

but exactly the same thing may be said of typhus fever and erysipelas. They occur no doubt sporadically, but will any one deny their spreading by contagion?

I doubt not, however, that many of those cases which we called sporadic cases of puerperal fever were cases of traceable contagion in which the contagium was carried from another case similarly affected by the medical attendant or nurse-tender.

I recollect being very much struck with the evidence afforded of this fact in the case of a most assiduous and indefatigable physician who was sent over by his Government from the North of Europe to study under me at the Lying-in Hospital. He was with me during one of the earlier outbursts of puerperal fever, when it became necessary to partially close the hospital and attend patients at their own homes. This gentleman was not remarkably attentive in exercising that virtue which is said to be only second to Godliness. He never appeared to change linen or woollen habiliments, and absolutely lived in a shaggy pilot coat by night and by day. He was so unceasing in his duties that he attended two patients for every other pupil's one; but the unhappy part of it was that I traced him through his rounds of duty like the pale horse in the Apocalypse, and the fatality attending his steps was such that I was obliged to request him to desist from visiting patients at their homes, when the proportion of cases sensibly diminished.

But the onus of proving this carrying of puerperal fever about does not rest with my northern friend. The same has often occurred with practitioners extensively engaged in midwifery practice. We have known of such who were obliged (for their own peace of mind and from conscientious feelings) to retire for a time from midwifery practice, or absent themselves from the neighbourhood. But the remarkable feature in some of these cases was, that whilst the disease haunted such practitioners as their shadow, the practice of most of the other practitioners in the same district was unattended by it. Whilst it has been my duty to see a large proportion of the cases attacked with metria in consultation occurring in this city, the proportion of them occurring to myself when I was engaged in midwifery practice, as already stated, only amounted to about one in 1,200; and of this I have not the slightest doubt, that this small proportion was due to the fact of my early conviction of the disease being contagious, and my having acted always upon this conviction. In fact, I never went to a parturient patient after visiting a case of metria or other

infectious disease without taking all the precautions I should have done in a case of scarlatina or small-pox, such as ablution and change of dress.

I admit it was troublesome, and often put me to serious inconvenience. Nevertheless I ascribe to this freedom from much painful afterthought; and it is, in my conviction, a practice that, no matter what a physician's opinion may be on the subject of contagion, he is not only not justified in omitting but highly culpable in neglecting.

It is all very well for a physician to form an opinion upon an abstract proposition of this or any other kind, and maintain it sturdily too; but for him to test it by exposing his patient to what a large proportion of his professional brethren believe to be almost certain death, is another and a much graver question, and one which, in my mind, don't admit of one moment's argument.

But if our views of a common poison be confirmed, many of these cases that we have hitherto esteemed sporadic may have been due to contagion, as hitherto little precautions have been taken by medical practitioners in separating zymotic diseases from parturient females, or in going from one to the other themselves, and possibly thus carrying the disease along with them.

Again, the practice of having lying-in patients in general hospitals, as they have in the Hôtel Dieu and in other hospitals in Paris and elsewhere—a practice productive of enormous mortality and most reprehensible in every way—goes some length to explain upon our views of the disease, not only the true nature of what has been termed sporadic metria to be contagious, but also the enormous fatality where this practice is adopted.

It having been fairly established by what has preceded that metria stands in relation of effect to cause with other zymotic diseases and crowded hospitals; how could a more mischievous and destructive plan be adopted than putting parturient females into general, and more particularly surgical hospitals,[a] where the special traumatic element is developed?

In asserting the non-contagious nature of metria, the last argument, that based on its sporadic nature must yield, if the principle of poisoning by self-contamination be admitted as here, the contagium is just in operation the same as if the patient to be contaminated were placed for the purpose in the same ward with one stricken. And this is not dwelt upon merely as a nice point of the principles of medicine. It is purely practical in its bearing—a

[a] See reports of Nightingale ward, King's College Hospital.

point which the admitting can produce no evil, but good—as at once we are led to inquire why this self-poisoning should occur in some cases and not in others.

In my own cases of sporadic puerperal fever already alluded to, the condition of the patients' chambers assimilated to that of the lying-in hospitals. The atmosphere was sensibly loaded; cleanliness and ventilation were comparatively less attended to, and the atmosphere was charged with exuviæ. Now this fact is not dwelt upon as implying that a want of cleanliness or neglect of the necessary precautions, such as removal of the excretions, foul linen, or attention to ventilation, could be chargeable to the lying-in hospital; on the contrary, I know that the reverse was the case, as well in my own mastership as in all those subsequent. But with every precaution that can be used in these respects, every hospital physician and surgeon knows that a loaded state of the atmosphere exists necessarily[a] in a ward more or less crowded with patients. Nay, that to the ordinary visitor effluvia are quite perceptible, and even effluvia varying in their character are detectable by the visitor gifted as I am with an unpleasantly acute sense of smell. In fact, I never could mistake the special odour of the lying-in hospital labour wards, or fail to distinguish it from that of a surgical hospital ward.

We all recollect when cholera was pronounced non-contagious, and when those who were hardy enough to resist the popular current upon this subject were, if not carried away and overwhelmed by the flood, at least half-stifled and choked by its brawling turbulence. Reason has at length had her sway. The simple and conclusive evidence of its growth in gatherings, its zymotic origin, and its spread along the lines of human traffic, have at length been listened to; and the hardy supporters of contagion who have, like myself, survived the hubbub and turmoil, remain with their belief in the doctrine unshaken, nay confirmed by the buffeting it has undergone.

As to metria, I should place it in its degree of contagion, in the same category with erysipelous and typhus fever, and rather more so than cholera.

The seventh and eighth proposition we shall, for brevity sake, discuss together.

[a] See Captain Langton's paper, and discussion upon construction of hospitals; also Report of British Medical Association in Leeds, p. 219, Aug. 21, 1869. Journal of Association.

Its endemic nature, and its prevalence in a given hospital, or the wards of the same hospital, are self-evident facts from what has preceded; and in dealing with the subject of endemic and contagious disease, I imagine much confusion has arisen from the attempt to create "a distinction without a difference." It cannot be denied that in endemic and epidemic disease, a physical entity or influence is present in the atmosphere, whether of the room, the house, the street, or the district, of a peculiar character, which produces a certain effect or train of symptoms, recognizable by us as ascribable to this endemic or epidemic influence. What this is has, hitherto, escaped our detection. Just as is the case in whooping-cough, measles, cholera, typhus fever, so it is in ague, metria, and influenza; accepting, as we may, the three latter as the best types of endemic and epidemic diseases. The fact of its being necessary to breathe the same atmosphere as that breathed by the whooping-cough sufferer, and to inhale the exhalations emanating on swampy localities, or to reside in the district visited by an influenza, does not alter the necessity of imbibing into the system, either by respiration or contact, a physical principle which acts as a poison, and, of course, this cannot be imbibed unless it exist, consequently it reduces the matter at the last to a poison or influence operating necessarily by contact; and thus most of these modifications must come to be considered as contagious, and due to contagion.

My convictions upon this view of endemic disease have long been confirmed by what I observed in the attacks of puerperal fever haunting certain wards in the lying-in hospital. This was especially the case with what most of my hearers will know as No. 11, with No. 10, and with No. 8, but particularly with No. 11. No. 11 consists of three small rooms in the old brick building, two of about eighteen by sixteen feet, and one about twelve by fifteen, communicating with each other, and containing about 3,400 cubic feet of atmospheric air; in these there were nine beds. Now, this ward was so subject to the disease, patients in the epidemics having been attacked in a proportion so much larger than the other wards, that I passed it over in its rota of receiving patients, and kept it empty—paying at the same time every attention to painting the wood-work, white-washing the walls and ceiling, scouring, with chlorate of lime solutions the floors, and washing, with the utmost assiduity, the bed-clothes, the nurses' wearing apparel; and obliging the nurses, wardmaids, and pupils to attend to ablution—a difficult duty I confess. Yet, on opening the ward, again and again the disease

showed itself, until at last, on one occasion, I shut these wards up for six months before allowing new patients to enter them.

The same fact is observable in the lurking poison of scarlatina—that most tenacious of all poisons to its habitat. I recollect a house in Mount-street, in this city, that was in the habit of being let furnished, and on three occasions, the last at an interval of twelve months from the preceding occasion, I had patients up from the country for their confinement, and members of their families, attacked in it with scarlatina. Nor can I easily describe my vexation on my being summoned to this house when it was too late to correct the mischief.

Now before leaving this subject it is right to mention that although I have not myself met fatal puerperal fever occurring epidemically in my own private practice, many other physicians of large experience have; and I have seen, in consultation, a larger proportion of such cases at a time when it was notorious that bad recoveries occurred in this city and neighbourhood than at others, ascribed to the epidemic prevalence of the disease.

Does not this, it may be said, go a great length to combat the contagious theory, and to disprove the greater hospital mortality? no such thing; it rather confirms both views if properly understood. Can any person of observation doubt for one moment that influenza is epidemic, endemic, and contagious? Witness its almost universal prevalence at times in whole districts. Again, its prevalence amongst all the members of a household in isolated cases; and lastly, the frequency with which a sociable friend carries it home from a visit of comfort to his suffering neighbour, or, what is more conclusive and less pardonable, how frequently the benevolent man walks in to pay a friendly call to his neighbour when he ought to be nursing it in his chamber—and imparts a month's torment to him in communicating rapidly the news of the day. Can any person doubt that if it was our habit to live in common homes influenza would not be *universal* instead of prevalent as it is at present? Or can it be doubted that scarlatina even when epidemic does not become a scourge when it gains admission to Eton, Harrow, or Rugby?

In fact, the conclusion practically to be drawn from the epidemic tendency is that the fact of this predisposition existing renders it the more imperative to avoid congregating those liable to zymotic disease and puerperal fever in particular, as it adds immensely to the likelihood of its occurrence and spread where the two influences co-exist.

We shall not recapitulate what has been stated in the earlier part of this paper about the comparative security to life of those delivered in their own homes, however humble they may be, as compared to those delivered in large hospitals. This fact is now so firmly established that it were only an insult to my hearers to dwell upon it.

Let us now see what the results are under a system of proper management in the housing of poor women in their trying period. Mark me, gentlemen, I say proper management in housing—in nothing else do I presume to call in question the management.

The housing that we venture to bring under your notice in contrast with that of the great hospitals, which I have so unhesitatingly condemned, is that adopted so successfully in the smaller towns in Ireland; and we shall rest satisfied with giving you simply the statistics of three of them which have been carefully kept.

Their success depends upon only having one or two patients in the hospital wards at a time, as their treatment in all other respects appears to be the same as that in our larger hospitals.

In New Ross, of which there is a published report for 30 years, to 1839, the total admissions were 924, or over 30 in each year, and the deaths were only 5, or 1 in 185.

In Waterford, of which we have the report published by Dr. Elliott, to whose exertions to draw attention to this matter we owe so much. His report reaches from 1838 to 1868, 30 years, in which period 3,534 deliveries occurred. The gross mortality in it has been for this period 1 in 288, and the mortality from puerperal fever only 1 in 328.[a]

In the Limerick Lying-in Hospital, however, the success was even more marked of the small hospital system, as it only gives 1 death in 367.

Dr. Elliott, who I am proud to claim as an old pupil of mine, puts the question under discussion so simply and so conclusively, that I must be permitted to quote the paragraphs referring to it from his report of 1868.

He describes his cottage hospital as "a small house rented for the purpose, in an elevated and airy part of the town.

[a] This should have been given in my first edition thus:—"The gross mortality in it has been, for the last 23 years of this period, 1 in 295, and the mortality from puerperal fever 1 in 1,328, out of 2,656 deliveries."—See page 43, *infra;* also pages 68 and 73 of "Reply."

"Of this house, only two rooms were available for the occupation of the patients. One was a very small room, in which were two narrow couches or beds, on which the patients were delivered; and the second, a larger room, in which were eight beds. Into this last-mentioned apartment the patients were removed at the end of some hours after delivery, and they continued to occupy it during their stay in the hospital. This larger ward was lighted by three windows with a louvred pane in the upper sash of each. The door was removed from its hinges, so that the entrance was rendered a mere door-way, necessarily open at all times; the bedsteads were plain iron couches, very roomy, but devoid of tester, curtains, or valance of any kind; the beds were of straw, inclosed in a sack or ticking, and changed for each patient.

"Into this hospital 753 women were received and delivered between March, 1838, and October, 1844, a period of six years and six or seven months. Of these, six died, three of the deaths being the result of puerperal fever in one or other of its forms, thus giving a total mortality of 1 in 125½, being a per centage of 0·79 or 4-5ths nearly; and a mortality from puerperal fever of half the amount, viz., 1 in 251, or 0·39 per cent.

"In October, 1844, the hospital was removed to the house which has been in continued occupation to the present time. This house is less favourably circumstanced as to its surroundings than was the former. It is situated in a narrow street, about 25 feet wide, in a poor and rather densely inhabited neighbourhood, and the ground on which it stands is rather low.

"It consists of six rooms. On the ground floor is a board-room to the front, and behind it a kitchen; on the first floor a small apartment to the rere, in which are two delivering couches; and to the front, an apartment or ward in which are four beds for the reception of patients after delivery, and during convalescence.

"On the upper or second floor are also two rooms; one to the rere occupied by the resident midwife, and another to the front, in which are four beds for the reception of patients.

"Each of the wards, in which are four beds, is lighted by two windows, with a louvred pane in the upper sash of each. The door of each is removed from the hinges as in the former hospital; and the bedsteads and bedding as before. Behind this house or hospital, if it be worthy of that appellation, is a small yard, in which are a wash-house, ash-pit, and house for the storage of straw, but the drainage and sewerage are by no means perfect.

These details, which may appear to some tedious or trifling, are given, because a knowledge of them is necessary for a due appreciation of the results.

"Into this hospital, from October, 1844, to this day, that is, during a period of twenty-three years, there have been received and delivered 2,656 women. Of these nine have died, two of that number of puerperal fever in the form of puerperal hysteritis, being a total mortality of 1 in 295$\frac{1}{9}$; or a per centage of deaths of 0·33, and a mortality from puerperal fever of 1 in 1,328, being a very small fraction indeed.

"It will have been seen that in the first hospital, where two apartments only were devoted to the immediate use of the patients, eight beds were grouped together in a single ward; and that in the present hospital three apartments are available for their use, so that the same number of beds is divided between two wards; and it will be noticed, as a significant fact, that whereas the mortality from puerperal fever was, in the first hospital, 1 in 251, during a period of six years and six months, the mortality from the same cause has been, in the present one, only 1 in 1,328, during a period of twenty-three years.

"In this, as in larger hospitals, and especially in lying-in hospitals, the number of patients together under treatment has varied greatly; there have been occasions when all the beds were occupied at the same time; and once or twice it has been found necessary to receive even more than the proper number; generally there have been only one, two, or three in hospital at the same time, and not unfrequently the hospital has been for some days, or a week, or even longer, completely empty."

What practical deduction do we arrive at from what has preceded?

That by the system of large hospitals certain death results to a much greater number of patients than when admitted to small, or when confined in their own homes.

Whatever question may arise as to the accuracy of the statistics in patients delivered at their homes, none occurs in the well-authenticated reports of the small hospitals, that does not exist equally in the case of the large. We shall, therefore, limit our comparison to the great and small hospitals. Let us then take first the three small hospitals:—

Mortality in New Ross is		1 in	185
,,	Waterford	,,	295
,,	Limerick	,,	367
			840

The mean of these three is 1 death in 282⅔.

This, then, is our standard of the proportion of deaths which occur where women in labour are treated in small hospitals in place of large. We are justified in concluding that the general treatment is the same in both, the only difference being the congregating large numbers of parturients into the same building in one case and not in the other. Let us further bear in mind that the increased mortality bears nearly a direct proportion to the increased numbers inhabiting each building. What then are the comparative results with the small hospitals as our standard? We shall first take the larger hospitals in detail.

That in the Liverpool Hospitals, 2 out of 3 die, who should not; or in other words, the deaths by proper management should be reduced to *one-third* of their number.

In the London Hospitals, 3 out of 4 die, who should not; or the deaths should be reduced to one-fourth of their number.

In the Coombe[a] Hospital, 3 out of 4 die (on their present calculation) that should not; which calculation extends over a period of the last 7 years, and the deaths ought to be reduced to one-fourth of their present numbers.

In the Glasgow Hospital, 4 out of 5 die, who should not; or the deaths ought to be reduced to one-fifth of their number.

In the Dublin Lying-in Hospital, 8 out of 9 die, who should not; or the deaths ought to be reduced to one-ninth of their present numbers.

In the Midwives' Institution, St. Petersburgh, 10 out of 11 die, who should not; or the deaths ought to be reduced to one-eleventh of their present numbers.

In the Hospitals generally in St. Petersburgh, 12 out of 13 die, who should not; and the deaths ought to be reduced to one-thirteenth of their present numbers.

[a] As some difficulty occurred in tracing the fatal cases sent out from the Coombe to the other hospitals, I cannot pledge myself that the death-rate, if taken, for the Coombe Hospital for the last fifteen years, as in the case of the Dublin Lying-in Hospital, might not prove different, and perhaps approach more nearly to the figures of that institution.

In Vienna Hospital, 10 out of 11 died, who should not; or the deaths ought to be reduced to one-eleventh of their recorded number.

In Paris General Hospitals, 17 out of every 18 died, who should not; or the deaths ought to be reduced to one-eighteenth of their number.

In the Paris Lying-in Hospital, or Maternity, 20 out of every 21 died, who should not, and the deaths ought to be reduced to less than one twenty-first part of their number.

Or if we take the mean of the death-rate of the 11 great hospitals as denominated above as 1 death in 44, and suppose the magic of our eloquence to reach and influence their managers in establishing small hospitals, or properly-constructed huts, instead of their present large hospitals, the saving to human life by this simple alteration would prove immense.

Be it recollected, these poor women flock to these hospitals under the impression that they are gaining a safe asylum in their hour of trial and distress; little do they imagine that they are, in their ignorance, taking a step that adds to their risks of death, in a ratio, at the very lowest calculation, of 3 to 1, and at the highest of 20 to 1, against their lives. Shall we then continue to keep these asylums open[a] upon their present faulty principle, when by merely remodelling the great obstetric institutions of this city ample and safe accommodation can be provided for them?

But it will be asked would you absolutely shut up the hospital, and destroy our world-renowned school of midwifery in this city? By no means;[b] I would establish the midwifery school upon a sound basis. I would take the lead, as Dublin has ever done, in this department of medicine, and correct the crying abuses that exist; abuses that may have been excusable when ignorant of them, but which are unpardonable alike in the eyes of God and man when dragged into the light of day. There is no difficulty whatever in the case of the Lying-in Hospital of Dublin in meeting the intentions of the founder by such an alteration of the present system as our increased knowledge of its vices suggests. The correction

[a] Let not the example shown by Dr. Priestly and Dr. Playfair be lost to us. These enlightened and conscientious physicians, on being satisfied of the incurable unhealthiness of the Nightingale maternity wards of the King's College Hospital, closed them at once and for ever as lying-in wards. Dr. Playfair proposes a very simple and inexpensive substitute, which we hope to find fairly tested by the Governors of the King's College Hospital.—See Dr. Playfair's letter, Appendix A, page 114.

[b] See Appendix F, page 129.

of these abuses will benefit the medical school of Dublin, and the obstetrical branch in particular.

The intentions of the founder, Mosse, would be best carried out by accommodating his great charity to those changes that progress in his art dictates, and which his great mind would have been the first to grasp and the boldest to execute were he now amongst us. Picturesque huts, in the Swiss or Italian style, sufficiently elevated above the ground, could be easily constructed to accommodate as many patients as are at present admitted, and could be built in the high ground in the Rotunda Gardens or elsewhere, or the pavilion system, lately introduced in some Continental and American hospitals, might be adopted.

The large hospital, at present such an ornament to our city, and such a benefit to humanity, through its diseases of female wards, can be appropriated exclusively to diseases of females, the brick building appropriated as a residence for midwives sent up for instruction from the country, and thus supply the greatest desiderata to our school.

Patients can be more generally attended at their own homes, and appliances, medicines, and even food supplied. In fact, all that is here insisted upon except the isolated huts is, and has been, for years in operation—ever since my establishment of the female disease ward; and all we want is to extend these and add the huts. I long foresaw the necessity of these changes, and have been gradually pressing them on the attention of the governors.

It becomes a trying effort to look on at avoidable loss of human life, when the responsibility of preventing this rests upon our own shoulders. In this painful position have I been for the last 30 years. As a governor and ex-master of the Lying-in Hospital my impressions have been confirmed as to the necessity of the change I now advocate. I feel that, with my knowledge and convictions, silence on this subject would be cruel, heartless, not to say criminal. This feeling of dissatisfaction increases with age and experience; so do our apathy and listlessness. Life runs on apace, but misery and redress of wrong lag. These convictions must plead my excuse for so tardily calling public attention through the influence of our common profession to this great grievance. I have now done what I conceive to be my imperative duty in this matter, and confidently leave the issue in the hands of a profession who have ever made the public good its first object.

DR. EVORY KENNEDY'S REPLY

TO THE

SEVENTEEN PHYSICIANS

WHO CRITICISED HIS PAPER ON "ZYMOTIC DISEASES,"

Delivered in the Hall of the College of Physicians, 10th July, 1869.

SIR,—In answering the criticisms on my paper, I must, in the first place, say I was not ignorant of the effect likely to be produced by grappling with an evil such as I felt it my imperative duty to expose, and, by the weight of public opinion, if possible, to remedy.

Neither was I ignorant of the fate that usually attends reformers.

I was warned on all sides not to bring a hornet's nest upon me, and cautioned not to disturb the equanimity of the physicians of Dublin by proposing impracticable changes in their darling institutions, hitherto the pride and boast of our city.

Sir, my motives forbade my giving ear to those timid counsels. My reliance upon the strength of my cause, which is yours alike, coupled with the estimate in which I hold the sound-thinking, liberal-minded, and philanthropic members of my profession, led me to the conviction that the profession, in this city, would never lend themselves to a feeble attempt to stifle or crush truth, if fairly and manfully brought before them.

It would have been easier for me to send my paper to the press, and avoid the ordeal of a *viva voce* discussion, with all its excitements, irritations, and personalities. But this plan would have failed in accomplishing the objects I had at heart, namely, eliciting fully all that could be said against as well as for the proposed changes; to meet objections fairly; and, further, to draw the attention of the whole profession to the importance of the subject, and what I considered the crying nature of the evil, as the best and speediest means of effecting its remedy.

From this it will be understood that I was quite prepared to expect an animated discussion, with much discrepancy of opinion. The mildest of our poets extolled the use of the *animated no!* and a discussion cannot proceed without it.

I am bound to say that this debate has proved no exception to the rule; for, although some important points have been quite overlooked, and although some statements have been repeated and reiterated, which, in my judgment, do not touch my arguments at all, or else misrepresent

them, still I will candidly admit that my opinions and facts have been *on the whole* fairly and fully as well as freely discussed.

I purposely and carefully abstained from using personal allusions, or warring with individuals. My hostility was directed against a system for which no person, living or dead, could individually be held responsible. The system of large lying-in hospitals was in operation before the birth of the fathers of the present generation, and it was due, in a large measure, to the benevolent exertions of the Physicians of an earlier period.

The somewhat novel position in which I find myself placed of having to answer, at one and the same time, the objections and criticisms of seventeen learned doctors poured upon me, "quick, thick, and heavy, like a thunder shower," imposes on me a difficulty not easy to surmount, with due consideration of your time and of the task before me. Fortunately that task has been lightened by the fact that most of my commentators have answered themselves and each other. I was in hopes that what remained to be done by me might have been done by grouping my critics; but this I found impossible to do satisfactorily. I am forced, therefore, to take them separately. I shall do so in the order in which they spoke.

Dr. Johnston, the present master of the Dublin Lying-in Hospital, began the debate, and I shall deal with the salient points of his paper. He says he would be *wrong* in *not* contradicting my assertion, that the intention of Mosse in relieving the miseries of lying-in women has failed, because the exertions of its masters have not rendered it as healthy as one could almost desire.

But it is proved that the mortality of the hospital has been for the last fifteen years 1 in 31. Unless, then, he looks upon the death of so large a proportion of patients as Mosse's intended means of relieving them from their miseries, it follows Dr. Johnston *was wrong* in contradicting me, and that he is equally *wrong* in his other conclusion, that the hospital has been rendered *as healthy as one could almost desire.*

Dr. Johnston says that he has succeeded in maintaining a perfectly pure and innocuous atmosphere in the lying-in hospital, and continues:—"I may *certainly* say that anything like a *miasm* or *poison* does *not exist*, and confidently affirm that nothing approaching to contagion has appeared." Yet afterwards Dr. Johnston answers his own assertions by saying:—"No doubt we have had deaths from metria, but in almost all instances they were either victims of seduction, &c., or others labouring under distress of mind."

Dr. Johnston says my statements are based upon wrong hypotheses.

Negatur; the hypotheses are strictly correct; that is, if Dr. Johnston's objections deal with what may be regarded as bearing upon the hypotheses he alludes to. Let us take them in detail.

Dr. Johnston cannot see why puerperal fever should prevail in the

lying-in hospital when fever, typhus, and erysipelas prevailed in medical and surgical hospitals.

Very possibly. Neither do I *quite*, although I fancy we shall all soon see. But I know by experience *it is a fact*. So did the late Dr. Collins and the late Abraham Colles; so did the late distinguished Dr. Joseph Clarke and Dr. Labatt.

Dr. Johnston can't conceive why, because it frequently happens that puerperal fevers follow the introduction of these zymotic diseases into the lying-in hospital, that hospital should be charged with being the habitat of such a poison, or that zymotic disease in any way engendered it.

I can't conceive how we can arrive at any other conclusion from the frequency of the one following the other than that they stand in the relation of cause and effect. Although the hospital is charged with being the habitat of puerperal fever—a fact which cannot be disproved—it is not so charged because it may be specially engendered by the introduction of a case of zymotic disease (see Propositions 4 and 5).

Dr. Johnston can't admit that puerperal fever prevails endemically in hospitals.

It must do so if the great hospital be its habitat; and this, although a self-evident fact, has been proved by me over and over again.

Dr. Johnston affirms it is not correct, in fact, unjust, to say that when puerperal fever appears it *generally* first shows itself in the great hospitals.

Not only is this the fact *generally*, but it is notorious that it often shows itself and commits dire havoc in our hospitals without extending epidemically or at all out of them (see Dr. Joseph Clarke's letter to Sir C. M. Clarke, p. 91, *infra*).

Dr. Johnston adds, after dwelling upon the precautions and advantages of the hospitals as compared to the hovels of the poor:—"It is hardly, if at all, possible that malaria, or miasm, or any poison, could find a habitat in the hospitals."

But it does, and does to such an extent that the mortality in all the great lying-in hospitals in Europe, notwithstanding all the precautions adopted, has forced itself upon public and professional consideration, and we are here to investigate the causes of this *impossibility* according to Dr. Johnston.

Again, Dr. Johnston and his assistant deny the existence of a noxious atmosphere in the hospital during their period of connexion with it, or from May, 1868, to May, 1869. The latter gentleman asserts that the present death-rate is much lower than Dr. Kennedy's calculation, based on the average of the last fifteen years, leads him to think it is. Fortunately, or, rather, I should say, unfortunately, he answers this question so completely in disproving his own assertions that nothing more remains but to adduce his own statistics. Need I say that it is to

me a matter of pain and regret, as a governor of the institution, my being obliged to prove that this gentleman's statistics do not confirm the statement of so marked an improvement as they led us to expect. His statistics for the year 1868 are that there were 1,132 deliveries and 29 deaths, but of these there were, as far as I can elicit, 4 deaths in the chronic ward, which are not to be taken into account. If I am in error in this deduction, it is not my fault, as the report did not, as it ought to have done, give us the exact number from all causes, metria, of course, included. But 25 deaths in 1,132 leave a little over 1 death in 45. Now, in the fifteen years, from 1854 to 1868 inclusive, the fifteen years for which I take my average as establishing endemic disease, there are six years in which the death-rate is more favourable to the hospital than that of last year, of which Dr. Johnston and his assistant speak so confidently of "*nothing* like miasm existing," in the very same breath that they admit 18 deaths by zymotic puerperal fever on the high average of 1 death in 63 from this disease alone. To prevent mistakes on this subject we shall give the death-rates for the seven years alluded to, bearing in mind that the gross death-rate of the last year by their table, errors excepted, is 1 in $45\frac{1}{6}$. In 1854 the death-rate was 1 in 53; in 1856, 1 in 64; in 1857, 1 in 46; in 1859, 1 in 66; in 1860, 1 in 54; in 1864, 1 in 46; and in 1865, 1 in 44.

Although I have deemed it necessary, in order to draw a fair comparison with other tables of mortality, to deal with the gross mortality in discussing this question, I am perfectly satisfied to take the statement of the 18 cases of metria out of 1,132 deliveries as conclusive evidence that the disease still holds its habitat and haunts the wards of the Dublin Lying-in Hospital, the large mortality of 1 in 63 *by metria alone* amply confirming this statement, and that upon the statistics furnished by the present resident assistant.

But, sir, I will candidly admit, had it been otherwise, and had the tables furnished from May, 1868, to May, 1869, afforded a different result, had the death-rate fallen to 1 in 66, nay to 1 in 100, or risen to its highest year (1862), 1 in 14, it would not in the slightest degree have affected the case I have made, or the necessity that exists for dealing with this important question. It did not require an occasional improvement in the mortality, extending, perchance, over a year or two, aye or three years, to convince those who have studied this disease philosophically and free from prejudice, that it is fitful and capricious in its appearance, disappearance, and duration. My two nights' efforts to elucidate the laws which regulate this disease must have proved nugatory if this, one of its universally recognized laws, has not been impressed upon you.

I have here selected the points of Dr. Johnston's speech which appear to require an answer. The rest of his observations may be described as a eulogistic description of the management and advantages of the lying-in hospital, in which I should fully agree if its defects in construction were

remedied. But with his conclusions, and with his arguments, meant to explain why it should be "as healthy as one could desire," and with his commendations of its construction, which I have pronounced and maintain to be "faulty"—with these I disagree, simply because the experience of the hospital has proved that it is *unhealthy*, and that upon Dr. Johnston's own showing. For, surely, where all the other advantages are so great, and arrangements so perfect, the tenor of his own reasoning leaves me no alternative but to ascribe its unhealthiness to its *construction*, which I affirm, and Dr. Johnston thus proves to be "*faulty*."

Now, wherein lies this faultiness of construction? In its being a double house, with an intermediate corridor, converting 4 great wards, with 9 beds in each, and 8 smaller wards, with 3 beds in each, into 1 great warren or plateau of connected rooms, with a similar plateau over it, with perforated openings in the ceiling of the lower corridor, which secures the same atmosphere pervading both corridors and the chambers. A generally commingling atmosphere is thus secured through doors, staircases, and perforated openings, so connected throughout that miasm must pervade all, if once generated. In fact, so constructed that isolation is totally impossible, as it is impossible to open a door, or to go in or out of any room in the building, without diffusing the tainted atmosphere, if it exist, throughout the whole structure.

I submit, then, that my original statements are confirmed, and Dr. Johnston's objections answered in the most conclusive manner, by the statistics furnished of the present state of the hospital by his own assistant.

Dr. Kidd, whose views appear to me more advanced upon the subject under discussion, and to approach more nearly to the state in which experience has now landed us, than any other of my commentators, leaves very little at issue between us. In fact, after examining my tabulated analysis of the speakers, criticizing my thirteen propositions, I find that, directly or indirectly, he agrees with most of the propositions, and as he is about to give the best practical test of this in the manner in which the new wings of the Coombe Lying-in Hospital are to be constructed, I sincerely deplore that he should be only *almost*, and not *altogether*, convinced of the necessity of complete isolation; for an error in this respect now, will perpetuate this deplorable and yet preventable mortality, as if the modified[a] attempt at isolation, as recommended in the resolutions at the Paris conference in 1866, which he describes as about to be adopted in the Coombe Hospital, be carried out, it may help to delay the progress of this great question for an indefinite period. That it may lessen the mortality is quite possible, but that it will prevent metria I do not anticipate; and whichever it does, if it leave the impression on the public mind either that enough has been done in this experiment, or that

[a] See page 107, *infra*. The Paris administration has already recommended the substitution of single bedrooms.

nothing more need be attempted, as this has failed, then, I am justified in saying that the change contemplated, by its failure or partial success, only perpetuates the mortality.

Sir, I can use no language stronger in proof of this than Dr. Kidd's own. After admitting that the mortality of the Coombe, for the last 15 years, has been 1 in 65=730; and ascribing the occurrence of metria, to a large extent, to epidemic influences, he goes on to say:—"But let it not be supposed, when he spoke in this way of individual causes of the disease, and of epidemic causes, he for one moment doubted the influence of contagion. On the contrary, he believed contagion was a most *influential* cause of it, that the disease was highly contagious, and that if they were to prevent it, they could only hope to do so by directing their attention to this. Dr. Madden had told them just now, that he never saw two patients ill of puerperal fever in adjoining beds. They (*i.e.*, the Coombe Hospital) had one remarkable instance where they had a case of puerperal peritonitis, in which they thought it wise to apply leeches. This woman got erysipelas, and recovered, while the patients at each side died from puerperal fever, in one case, accompanied with effusion into the joints, and in the other, the patient died before that could have taken place." By a strange coincidence, Dr. Kidd's next sentence, and next branch of his subject, commences thus:—"The statistics of extern practice and of lying-in hospitals had been compared. If the results could be depended upon, lying-in hospitals ought to be closed at once and forever. '(Hear, hear.)'" After expressing his want of reliance in the statistics furnished by extern maternities, he continues:—"But, notwithstanding all this, he must admit that there was a large proportion of mortality due to the influence of the hospital. He believed it was impossible to collect a number of patients under one roof, and yet not to have an hospital atmosphere, and that that atmosphere was more or less injurious." Here, sir, we have the honest manly statement of this enlightened man—himself an hospital physician of no slight experience—conversant with his subject, and sustaining, to the letter, the views I have propounded, on the eve of building an hospital so constructed as to collect a number of patients under one roof, when he admits fairly, "it is impossible to collect them under one roof and not to have an hospital atmosphere, and that that hospital is more or less injurious." Is it to be wondered that I should, under these circumstances, implore him and his colleagues publicly, as I have already done in private, at least to hesitate in raising any structure that shall not render the isolation of parturients complete?

On the third night of our discussion, I made a few observations upon the papers that had been read that night; but, by some strange oversight the reporter employed by the Society to detail the proceedings omitted my observations upon the St. Petersburgh Hospital in reply to Dr. Kidd.[a]

[a] See Appendix B, page 114, par. 2.

Dr. Kidd observed, in reference to isolating parturients:—"When Dr. Kennedy spoke of isolating patients he meant it literally by having a separate ward for each patient. The plan had been tried in the hospital of St. Petersburgh, which in 1852 had been reformed by the Grand Duchess Helena. It was increased by the erection of a new wing of the figure of the letter L on the corridor plan; opening off the corridor were a number of small wards in which the patients were placed. But what had been the result of that? The reform was instituted in 1852. The mortality for the seven years preceding it was 3·12, and for the seven years succeeding 2·93. That was to say before the patients were placed in separate wards 1 out of 32·17 died, and that after this change—after they had undergone all this expense and trouble, and more or less injured the efficiency of the hospital—the only change was that 1 died in 34·09."

Now, had the Doctor himself happened by any process of transformation to be one of the two lives saved, I doubt whether the *expense* and *trouble* would have stood in the way of his recommending the reform, inefficient as it was.

On this subject Dr. Mapother, no bad authority, writes to me:—"Dr. Kidd did not think the gain of 2 in 34 worth the changes made in the St. Petersburgh Hospital. Sanitarians are proud of reducing a death-rate by 1 in 1,000."

But my answer to Dr. Kidd on his statement—and which I complain of having been most unjustifiably omitted—was based on the following extract taken from Dr. Oppert's volume on Hospitals and Infirmaries. In describing the Helena Hospital he writes thus:—

"In the new part, which has the form of an open quadrangle, the sick wards are small, for one or two inmates. Most of them contain open fire grates; there are tile stoves in some others. *The wards open into each other*, and are situated back to back on either side of the wall." And yet Dr. Kidd adduces this as an instance in which, as he himself expresses it, my plan of "isolating patients literally" was tried and failed. My answer simply is, that this is directly the *reverse* of my plan; that my plan has not been tested in the Helena Hospital, and consequently has not failed. Dr. Kidd might just as well have said, that the plan he and his confreres are about to carry out at the Coombe Hospital, of isolating or separating the patients *after* the attack of metria has shown itself, is my plan, and if it also fails, might hereafter affirm that in the Coombe my plan had been also tried and had failed. My plan recommended to certain parties in America who consulted me upwards of thirty years ago as to the best means of building to be adopted in the construction of lying-in hospitals, with a view to exclude metria, was that each building should be completely isolated, and with not more than two beds in each ward.

Dr. Mapother, in a few pertinent words, evinced how well he understood the subject of zymotic disease, upon which I may say, without flattery, he is "*facile princeps*," our highest authority in this kingdom. It affords

me great satisfaction to know that I possess in this arduous struggle the support and unbiassed disinterested approval of this enlightened physician, who has made zymotic disease, its causes, prevalence, and laws, his especial study, and whose writings and lectures have thrown so much light upon it. This liberal-minded physician, thoroughly versed in the use and value of statistics, has recorded his opinion, in the spirit of a juryman or expert, and one who attended this debate with no determination to support a foregone conclusion—no mistaken idea that his darling hospital was attacked and must be defended at all hazards, provided with no elaborated one-sided brief that occupied weeks in the preparation, and hours in the delivery, but, best of all, with no motive but to arrive at truth. And what did this indubitably competent, this unquestionably honest expert and juryman say? "He did not think one of the speakers had controverted the remarkable statistics which had been brought forward by Dr. Kennedy with regard to the mortality in the Dublin Lying-in Hospital, and that in small provincial towns, Waterford, Limerick, and New Ross."

Sir, in dealing with the pertinent remarks of Dr. Churchill I feel bound to state that he has entered upon the consideration of the question at issue in a philosophic spirit, and that his observations have helped us much in the elucidation of this important subject. Nay more, I feel called upon to add, that however I may regret the insertion of the last paragraph of his paper, in which he pronounces "the changes I propose both *hasty* and *injudicious now*," yet the tenor of his reasoning, the negative-pregnant contained in the words not *now*, which mean, if anything, the necessity that shall or may exist hereafter, but above all, his table of statistics, and the admirable rules he has so strongly insisted upon, in their reading and application, render his observations not only valuable, but of the greatest value, in confirming all the views I have propounded and insisted upon in my paper.

We shall pass over his strictures upon the statement attributed to me as to the non-occurrence of metria in patients in their own homes, by simply referring him to the words "*comparatively rarely*," which he republishes in my ninth proposition;—the oversight in which he, in common with Dr. M'Clintock, giving Dr. Joseph Clarke's death-rate by puerperal fever, places it at 6, not 3, as it ought to be, out of his 3,847 private patients, a rate which so nearly coincides with mine, being 1 in 1,282;—and the mistake which ascribes this statement to Dr. M. Clarke, whereas it was contained in a letter written by the late Dr. Joseph Clarke to Sir Charles Mansfield Clarke, that we shall again have occasion to refer to.

Dr. Churchill, sir, asks a very pregnant question, and one the answer to which contains exactly what we are looking for. If metria be produced *spontaneously*, why do long periods of freedom from it occur under precisely the same hospital conditions as those which accompany its appearance? This question is seriously asked me by my friend Dr.

Churchill. Why, sir, the learned president of the College of Physicians might just as well have asked that unanswerable question that puzzled all the philosophers of the last century, and remains, as far as I am aware, still unanswered, "What is the weight of a mail coach?" I will ask him in return to substitute the adverb epidemically for spontaneously, and answer his own question, explaining why *epidemics* should recur under precisely the same circumstances, and it is not impossible, when he succeeds, he may apply the same solution to both questions.

Neither shall I dwell upon his views of the epidemic prevalence and contagious extension of metria, views so strongly confirmatory of what I have expressed. But I pass to his statistics. In the first instance, reiterating his own remark, "That to be of any authority they must be extensive," to which he added a more doubtful proposition, namely, "In fact their value is exactly in proportion to their extent." Had he substituted the words comprehensiveness and accuracy for the word extent I should have accepted the latter as freely as the former proposition. But possibly he intended to imply or include these ideas in the word extent, and in that meaning, therefore, I shall deal with it.

Dr. Churchill treats of three classes of statistics, giving the proportion of death-rates in each. The first includes the mortality of six obstetric practitioners, some giving the deaths by puerperal fever merely, the others the general mortality. Dr. Churchill justly remarks that they afford no sufficient ground for estimating the death-rate after confinement, and passes them without comment, a line in which I shall follow his example, merely remarking that, if we could hope by any exertion of ours to bring the death-rates in hospitals to any reasonable approximation to those detailed in his private practice tables, we should be but too happy.

The next class of statistics given by Dr. Churchill is that collecting the number of cases and proportion of deaths to deliveries occurring in the attendance upon out-door patients from the maternities—and here Dr. Churchill has his own principle of *extent* fairly carried out—he adduces no less than 236,665 cases as attended from six maternities, giving 1,590 deaths, or the proportion of 1 in 149 deaths by puerperal fever, and this he contrasts with 71,090 cases in large hospitals, giving 1,156 deaths, or 1 in 62—that is, he gives

Maternities death-rate, 1 in 149
Large hospital death-rate, 1 in 62.

Now, although I do not bind myself to the accuracy of these statistics, I accept them on the authority of Dr. Churchill as his basis of argument, and merely so far as they support his negative-pregnant that "*now* is not the time to provide small parochial hospitals," and ask is it nothing to save 2¼ to 1, on his own calculation, of all those women at present confined in the great lying-in hospitals? and if *now* is not the time to do this, when will

that time be, and what will be the additional unnecessary mortality that we shall then be answerable for when that time arrives? Simply repeating, that I am not answerable for any confusion Dr. Churchill may have fallen into in his attempt to give the mortality of puerperal fever distinct, as, finding myself the impossibility of doing this from the unreliability of the returns as well from some of the hospitals as maternities, in consequence of the irregular manner in which the different varieties of metria were reported, I adopted, as the only reliable test of comparison the total mortality in all cases, unless when I state the contrary. I should much have preferred giving the mortality by puerperal fever in all cases, with the gross mortality, had this been available and reliable.

As Dr. Churchill has not drawn the distinction between deaths by metria and the total deaths, it would not be fair by him, nor would this discussion derive any benefit by continuing the comparison, or dwelling upon his returns of the large hospitals. We shall, therefore, pass at once to his third table, that of what he terms the small hospitals, and in doing this must be allowed to repeat Dr. Churchill's axiom—"In comparing numbers we must take care that we are comparing like with like." Now, on this his own ground of comparison, I altogether reject this table of statistics, and deny that any ground of comparison holds. Dr. Churchill collects twelve *what he calls "small hospitals"* to make his comparisons by, and yet of these twelve *only three are* small hospitals, the other nine exhibiting all the vices of the larger lying-in hospitals, with none of their advantages. I feel convinced that this was an oversight on the part of my friend, or he never would have dreamt of drawing such a comparison as ranking these nine hospitals in the same category, or classing them as small or isolated cottage hospitals, such as I recommend. Nine out of twelve of these so-called small hospitals have annual deliveries varying from 400 to 140, and some of them contained as many as thirty and fifty beds. Several of them were old houses, with a number of rooms communicating, with story above story, and the patients huddled together, with every disadvantage of small crowded rooms, and a stagnating atmosphere. Why, compared to these, the palatial edifices and well-ventilated spacious halls for dormitories, such as the hospital which is such a source of pride to us in our city, might even afford better chances of recovery to patients in their delivery.

Of the three cottage hospitals which I compared with the great hospitals, on the contrary, one had only 30 deliveries in the year, and the largest, the Waterford, had only 115, and these scattered over four rooms. Is it then to be wondered at, that in these three, the mortality only amounted to the average of 1 in 282? whereas deducting these three cottage hospitals from the twelve in which the mortality is given by Dr. Churchill as that of the small hospitals, I find the mortality

of the remaining nine amounts to 1 in 59. Why I doubt whether a stronger case could be made for the cottage or isolated hospitals, than that furnished in this table of Dr. Churchill, or, if properly investigated, that any stronger proof could be given of the accuracy of my propositions, No. 3 and 4, which asserts the mortality to exist in proportion to the number of parturient patients cohabiting and breathing the same atmosphere, and that metria finds its habitat where large numbers are delivered under the same roof. Propositions that imply number in proportion to space, as well as absolute number; and yet, these proportions have been cavelled at by almost all my critics. Thanks to Dr. Churchill, however, he has answered these unanswerably in this table.

But, we have one more matter to touch upon with my friend Dr. Churchill. He very properly insists in statistical calculations upon "the numbers being very large, and the time embraced in them being considerable." Very justly he remarks, "in a small hospital or private practice you may go on for years without a death, yet after the lapse of another year the average death-rate may be fearfully high, just because an epidemic intervened." Now, I apply this rule to his table, and I find that the first and second entries, Dr. Beatty and Dr. Churchill's hospital statistics, only embrace three years, and are, compared with the Waterford Hospital, embracing thirty. It is hard to say whether, if the statistical kaleidoscope to which we alluded on our first night's discussion had revolved for the other twenty-seven years, Dr.Churchill and I might not have agreed upon this point, as we do on most others, and thus the convictions of his own well stored mind, the result of years of ample experience and deep study, might have remained unchanged, and been applied in co-operating with me in this my arduous struggle for the benefit of our suffering fellow-creatures. Be this as it may, however, I would strongly urge upon him, when he is tempted to be led astray again by statistics, to hold fast by his own excellent maxim—"That in comparing numbers with numbers we must take care that we are comparing like with like."

Dr. Morgan's report of the mortality in childbirth in the Lock Hospital, 1 in 74, is extremely interesting in its bearings upon this discussion.

It is quite evident that the paucity of the births and the consequent segregation of the parturient element was a main cause of the comparative absence of metria in his cases. But, on the other hand, we had the parturient exposed to what we laid down as a mischievous influence in her inhaling the atmosphere of a surgical hospital, an elemen that should have increased the mortality, as we have found it do, in the foreign hospitals in which surgical cases and deliveries are congregated, unless some corrective principle was in operation.

We imagine we have exactly met a principle in the theory, old as the fathers in medicine themselves, namely, that morbid poisons cannot co-exist in the same individual. Here the syphilitic virus was in occupation and may have resisted the metria zymotocene.

This theory I am quite aware admits of some apparent exceptions, exhibiting modifications in the types of disease, depending upon intercurrent poisons; but here again the common original type may be the explanation.

I have, myself, witnessed strange struggles for the mastery in cases where two poisons had found a nidus in the same individual, at the same moment.

One of the most remarkable was a lady under my care about two years since. Her children were ill with mumps, and she sickened with fever and stiff jaws, but without the glands assuming the usual enlargement. The fever, however, continued, and with considerable, in fact, unwonted severity, causing me some anxiety, and after four days, a crop of measles, with all the usual accompaniments, appeared over the body, ran its course, and subsided on the fourth day; but the fever kept up. Difficulty of swallowing returned, the parotid and submaxillary glands became very much enlarged, and the case then ran the usual course of mumps.

Dr. Morgan's observation upon the distress of mind necessarily existing in the deplorable cases of labour in the Lock are of much value.

Whilst I freely admit the influence of mental depression in causing bad recoveries in childbirth, and even predisposing to puerperal fever, I am of opinion that its influence in the latter respect has been overestimated in the observations that have fallen from several of the speakers upon this subject, but the very interesting report furnished by Dr. Morgan, of the Lock Hospital, and the South Dublin Union report, appear almost conclusive on this subject. What combination of conditions could be more calculated to produce mental depression than those of poverty, destitution, and syphilitic taint in the married female? But when we couple with these the frequency of seduction and prostitution as additional ingredients, we can scarcely conceive any influences more depressing to the human mind; yet, with this combination of depressing influences, the mortality in Dr. Morgan's cases in the Lock, with a delivery ward containing only *two beds*, and therefore never overcrowded, was only 1 in 74; whereas, in the Lying-in Hospital, with its mixed occupants and incidental cases of seduction, it has been 1 in 31½. The only assignable cause of the excess being the proportion of the deliveries under the same roof, constituting the hospital the habitat of metria in the one case; whereas, every other ingredient of calculation, mental as well as bodily, was in favour of the excess in the other, but it was wanting in the habitat.

The South Dublin poorhouse furnishes us with a parallel nearly but not quite as strong as this. The mortality for the four years 1865-6-7-8, inclusive, was in it 1 in 238. See Appendix C.

In the North Dublin Poorhouse the mortality is stated to have been 1 in 223½ for thirteen years, from 1857 to 1869. But, as a report was not supplied to me on my application, I cannot vouch for the accuracy of this return.

The records of the English poorhouses are strongly corroborative of the views here insisted upon, and the statistics derived from them show in a remarkable manner the saving of human life effected by the poorer classes lying-in in the poorhouses, as compared with those confined in our great lying-in hospitals. This fact has already attracted the attention and obtained the confirmation of sanitary and parliamentary committees. I am indebted to my friend, Mr. John Lambert, P.L.C., Whitehall, author of "Enquiry into Vagrancy," for the following interesting and conclusive information on this subject:—

Extract from the Speech of MR. GATHORNE HARDY *upon the introduction of the Metropolitan Poor Bill, February*, 1867.

"One point is remarkable enough—it is that, however overcrowded these (Workhouse) Infirmaries may be, none of those diseases appear there which are known to result from overcrowding. There are no hospital diseases, and it has attracted attention abroad as well as at home how very few are the cases of puerperal fever which so often decimate lying-in hospitals, and in France, I believe, cause death to an extent of which we have no conception. Even in our own lying-in hospitals these cases occur to a much larger extent than in the metropolitan workhouses."

The following striking results are obtained from the statistics of poorhouse deliveries, in a statement of cases furnished for 5 years from 40 English poorhouses by metropolitan medical officers to the Cubic Space Committee:—

11,870 deliveries occurred, with 93 total deaths, or 1 in 127. In 13 of the 40 poorhouses, no death whatever occurred for these 5 years out of 2,459 deliveries.

In 9 of the more crowded lying-in poorhouse hospitals, where 6,044 patients were delivered, the proportion in each exceeding 365, and in St. George's, Lambeth, St. Pancras, and Marylebone, exceeding 5, 8, 10, and 12 hundred respectively, the mortality amounted to 56, or 1 in 107; whilst in the remaining 31 poorhouses, in which the deliveries were only 5,826, or an average of 187 deliveries to each poorhouse, the deaths only amounted to 37, or 1 in 157.

The Cubic Space Commissioners state in their Report, p. 4:—"One very remarkable fact disclosed by these returns is the infrequency of childbed fever, and the small ratio of mortality produced by it, in those (the English) poorhouses, where within the last five years nearly 12,000 poor women have been delivered."

As far as I can ascertain from the table above referred to, only 40 cases of metria occurred out of the 11,870 deliveries, or 1 in 296.

The Cubic Space Commissioners again remark (Report, p. 5):—"The general exemption of the lying-in wards in the workhouse infirmaries from serious disease, and especially from puerperal fever, and the very

small mortality which they present, have already been noticed. It appears, indeed, from a minute analysis of the returns laid before the Committee on this subject, that in point of fact a very large air space, much exceeding 850 cubic feet, has been enjoyed on an average by each of the parturient women in almost all the workhouses. The comparative freedom of the lying-in wards from disaster may probably have been owing in part to the ampler air space, thus actually shared among the women, though the Committee are conscious that much of the immunity was attributable to other causes."

This Report bears the signatures of Drs. Watson, Acland, Sibson, Markham, and Randall, of Capt. Galton, W. Corbeth, T. Holmes, and Charles Hawkins, as well as of Dr. Edward Smith, with the following paragraph amongst others appended by the last-named physician:— "The records of lying-in cases show that the mortality is less in workhouses than in any other institutions of considerable extent. It is six times less than that which occurs in the chief lying-in institution in the metropolis—viz., the Queen Charlotte Hospital, and it is more than ten times less than now occurs in the chief lying-in institutions at Vienna and other Continental cities. To whatever causes may be attributed this agreeable fact, it seems a natural inference that they are such in workhouses that they might be safely continued."

Sir, I was rather astonished when I heard it put forward by some of my critics, that the mortality in the extern cases attended from the Dublin Lying-in Hospital amounted to 1 in 61 deliveries. I was aware that this branch of the institution had not been much encouraged, neither had the records of cases been very regularly kept. As a governor of the hospital I drew, about two years since, attention to the necessity of encouraging the out-door attendances and of keeping a more accurate record. I therefore applied to the Registrar for a report of the last year's deliveries, in order to compare them with Dr. Johnston's year of the hospital, when it was, in his words, as healthy as one could almost desire, and find by it that in the year ending March 31st, 1869, the extern attendances amounted to 147, with but 1 death. The mortality within the hospital for the same time amounted, as we have seen, to 1 in 45, or three and one-fourth to one of that without the hospital; and be it recollected, that this is the year in which the improved condition of the hospital, commensurate with Dr. Johnston's mastership, has been dwelt upon by him. Would we could even effect this saving within its walls.

We now arrive at Dr. M'Clintock's strictures upon my paper, and as this gentleman occupied an hour and forty minutes in developing them, you must bear with me as patiently as you can in the time required for their refutation. Our time will not, however, I trust, be misspent, as, from his numerous objections, his ingenuity, and his peculiarly

pungent and asseverative manner of attack, I am quite aware that his observations produced a marked effect upon his hearers. I shall not follow his example, however telling it may have been, of assuring you before doing it that I am about to annihilate him for ever. I shall not seek to forestall your plaudits by declaring my intentions of conquest which, like those of the boastful opponent of Tekeli, may fail to be realized in presence of the adversary. But I shall ask your calm attention to my reply, free of all favour, affection, and prejudice; as, in replying to this gentleman's somewhat bitter attack I shall be also meeting the objections of several others of my critics, whose arguments he has repeated and amplified. It is therefore, I repeat, the more necessary that a patient, considerate, and candid hearing may be accorded to me.

The spirit in which Dr. M'Clintock approaches the great subject that engrosses us (the means of reducing the admitted mortality in our lying-in hospitals) will be best understood by introducing him to you as the propounder of a novel and startling principle applicable to lying-in hospitals, and one which he enunciates in the language of Drs. Bristow and Holmes, "that a high death-rate indicates, as a rule, that an hospital fulfils efficiently the purposes for which it was designed, and that a low death, on the other hand, indicates, *cæteris paribus*, comparative inefficiency." Why, sir, this principle, even as applied to general hospitals, admits of great question; but its application to lying-in hospitals I shall not trust myself to characterize. Such an assertion might be made in the heat of debate, but furnishes no excuse for a gentleman in his calm moments deliberately putting forth such a maxim as this before a society of enlightened physicians, namely, that "the principle enunciated (in these quotations) applied with little qualification to maternity as well as surgical or medical hospitals." We shall hear presently what Dr. Stokes says on the subject of labour as a natural process. How the learned Regius Professor must have shrunk, horror-stricken, from the avowal of such a principle!

After enunciating this paradoxical and shocking dogma, Dr. M'Clintock wanders off to descant upon reliable estimates of mortality in women in their own homes. But we shall not require him to go out of the four walls of the hospital in which he resided for ten years, to prove the case against himself. We shall not require to compare hospital with private practice to refute it. Let us take any period for which the hospital was free from metria, for a sufficient time, to give a fair average of deaths by the accidents of labour and diseases, independent of metria. Say the last three years of Dr. Collins's, and the first year of my mastership; that is four years, from 1830 to '33 inclusive. In these four years there were 8,844 deliveries and 48 deaths, or 12 annually; that gives 1 death in 182. Now let us take, say the last four years of the hospital, from 1865 to 1868 inclusive. The deliveries were 4,574, the deaths 149;

that gives 1 death in say 30⅔. The result of the comparison of the hospital with metria against the hospital without metria is to give 5½ unnecessary deaths out of every 6 that occurred in the last four years, as compared with the period 1830 to 1833. Now, does Dr. M'Clintock mean to tell this intelligent Society that that excess of death-rate in the same hospital, under identically similar circumstances in every respect save one, the presence of metria, furnishes us "with grounds to draw his inference, and satisfy him with its foundation." "That a high death-rate indicates as a rule that the Dublin Lying-in Hospital fulfils efficiently the purposes for which it was designed, and that the low death-rate of Dr. Collins's four years, on the other, indicates (*cæteris paribus*) its comparative inefficiency in his period?"

Dr. M'Clintock triumphantly collects and recapitulates, what I had already given in my tables, the instances in the Lying-in Hospital in which the largest number of patients had been delivered with the smallest death-rate; and after dwelling upon these as disproving my third proposition, he concludes by alluding to Dr. Labatt's most crowded period (the very one I had adduced as confirming my position) in these remarkable words:—"This was certainly enormous cohabitation, *and it did certainly breed an epidemic puerperal fever*, but there were three years of gestation before it appeared." Here we have the one grain of wheat in the bushel of chaff. But Dr. M'Clintock is in error in saying there were three years' freedom from puerperal fever at this period—there were only two, 1815 and 1816, and these he should have mentioned (a fact that he, as master, could not have been ignorant of) were the very two years that the auxiliary hospital was added, and the crowding lessened. In 1817 the mortality rose again from 1 in 182 to 1 in 108, and in the next year to 1 in 62, whilst in 1814 it had been 1 in 100, and in 1813 1 in 40.

After such an admission, which common sense and common honesty extorted at Dr. M'Clintock's hands, followed by my commentary of the addition made to the hospital, which he suppressed, what becomes of his statistics collected from my tables with such assiduity, and his cavils as to the comparative mortality in New Ross and Waterford cottage hospitals.

Sir, Dr. M'Clintock might possibly have better consulted the interests of our common humanity, as he certainly would have done his own consistency and good taste, if he omitted his charges of *exaggeration* and *unsoundness*, dealt so unhesitatingly against me, in the very same sentence in which he admitted that I had made a *strong* case against larger hospitals in my mode of dealing with them, and when he still further "*conceded readily* a greater mortality amongst women confined in these institutions as compared with the aggregate of women confined in their own homes."

Why these two points include the whole principle for which I have been contending.

My third and fourth propositions are the undeniable logical corollaries based upon these. Our eight nights have been occupied in testing the accuracy of these two propositions; propositions which, in slightly altered language, Dr. M'Clintock himself admits, and, as we shall see afterwards, explains; and yet, with rare inconsistency, whilst he admits the truth of my propositions, he occupies an hour and forty minutes of the valuable time of this Society, engrossed with a vital and all-important question, upon which so much depends, and in which *our profession is as much upon its trial as are the lives of those women he admits to be sacrificed;* he occupies, I say, this Society for an hour and forty minutes, whilst confirming my position, in trying to detect flaws in my arguments, in dwelling upon accidental slips of the pen in the transcription of my figures, and in magnifying them into grave errors of calculation. But, excuse me, sir, I am imitating his bad example, and forestalling statements to come.

But, it has been attempted by Dr. M'Clintock, Dr. Atthill, &c., with more ingenuity than candour, holding me to the strict letter of my proposition, that the mortality was in direct proportion to the number of parturients in a given space, to prove that I had asserted that that general statement applied *ever* and *always* to the *very time* at which the crowding was observed. I did not, however, make any such statement. On the contrary, the whole tenor of my reasoning goes to prove the reverse. I dwelt especially on the capricious nature of this zymotic disease, and referred to the statistics of the lying-in hospital as showing this peculiarity in its endemic hauntings. But I went a great deal farther. I showed in distinct, unmistakable language what was intended in this proposition. Yet, strange to say, every gentleman who has attacked this Redan proposition has found it convenient to ignore these facts.

Page 31, *supra*, of my paper states:—"If you will cast your eye along the Dublin Lying-in Hospital table there furnished, it will give you, at a glance, the longest duration of these endemic hauntings to which the hospital has been liable since its foundation. Observe where there are two figures in place of three, you will thus arrive at a rough result." Now, mark me, gentlemen. "From 1761 to '65 was one of these; again from 1767 to '70, and from 1772 to '74. From this year down to 1800 we meet only an occasional unhealthy year, but it remains at two figures until 1804, and so on, with only an occasional unhealthy year until 1818, the *most crowded state the hospital ever was in; in that year there were* 3,539 *deliveries. For three years the mortality then arose to* 1 *in* 44; *in* 1826 *it arose again, and continued for the next four years at* 1 *in* 50." If language means anything, it will be evident from this, that I state that the mortality arose and continued as the result of the previous crowding. It will be perceived this convincing evidence of the truth of

the sixth proposition has been inserted in italics, to prevent its again escaping the notice of my critics. Sir, I feel called upon to do this, and to dwell thus on this quotation, because, although I pointed out this passage and these figures of Dr. Labatt's mastership to Dr. Atthill on the first night of discussion, as refuting his assertion and confirming my proposition in the most conclusive manner, other gentlemen have followed his bad example, and ignored this passage in treating of this subject.

Had my commentators taken the trouble of examining the history of the hospital, they would have found, however, as above alluded to, an especial explanation of the delay in the outburst of the disease until the year 1817, although the numbers had been increasing from 1815 to 1818 by nearly 1,000 deliveries; it was this. In the year 1815 the crowding was checked by the purchase of what had been the hospital for the blind, which was then added to the institution. This lessened the crowding. It partially isolated, by separating the patients, and thus the outburst of the disease was delayed for two years. Not only this, but the death-rate absolutely fell from 1 in 100, where it stood in 1814, to 1 in 180 in 1815, and to 1 in 182 in 1816.

What could be more convincing, then, than this period of 1818 in confirming the relations between crowding and death-rate insisted upon by me?

Drs. M'Clintock, Atthill, and their friends oblige me to analyse in detail the saliently marked periods which show the marked relation between increase, diminution, and death-rates in the hospital, as they have done, a not uncommon thing with young philosophers, they have confounded cause and effect. They say:—"It so happens that during the year 1800, '1, '2, '3, '4, being the five years that immediately preceded those whose statistics I have just quoted, the annual deliveries averaged 500 less than the succeeding ones, being 8,900, yet the mortality was more than double, being 1 in 66." Now, let me ask, why did the deliveries average 500 less, and why did the mortality increase *pari passu* with this lessening? Simply because the overcrowding of the hospitals in the four preceding years, namely, 1796, '97, '98, '99, had engendered the disease, 6,474 patients having been delivered in it in that time, and their admission in 1797 having amounted to 1,712. Dr. Evory, whose astute mind and experience pointed out the cause of the increased mortality, consequently limited his admissions, and in 1799, when the disease showed itself, he reduced his deliveries to 1,537, and again in 1800 to 1,337; but it was too late. The disease had found its habitat in the hospital, and continued its havoc for four years.

The indiscreet master who succeeded, Dr. Kelly, would not take the example of his predecessor, but allowed the patients to crowd into hospital, in increased numbers, until the year 1803, when the patients numbered

2,028, and the death-rate reached 1 in 46. Now, as nearly as possible the same thing had occurred in the last three years of Dr. Joseph Clarke's mastership. The deliveries had amounted to 4,990; the highest average up to that time, and in his last year, 1793, they reached 1,757. In that year the death-rate rose from 1 in 163 to 1 in 92, and the disease having found its habitat, the death-rate got to 1 in 77 the next year.

Dr. Evory, however, lowered his deliveries to 1,543 in 1794—in 1795 to 1,503, and in the latter year the death-rate only amounted to 1 in 214. The same thing exactly takes place with Dr. Hopkins. From the year 1806 to 1809 the deliveries increase from 2,220 annually to 2,889. The death-rate rises from 1 in 205, where it stood in 1808, to 1 in 98 in 1810; and although the deliveries are reduced by nearly 400, or to 2,484 in 1813, the death-rate reaches 1 in 40, because the disease has now found its habitat.

A fatal blindness seems, however, to follow our ministrations from the earliest periods of the management of this institution. The diminution of deliveries is attended with its consequences more speedily on this occasion than usual. The death-rate is reduced to 1 in 100. Dr. Labatt's mastership commences in 1815; patients crowd into the hospital again; the admissions reach 3,276 in 1816, and the death-rate stands at 1 in 182. But despite the relief afforded for a time by the increased accommodation supplied by the addition of the blind house, the fatal result, although delayed, is certain. The next year the deliveries amount to 3,473, the death-rate rises, of course, to 1 in 108. Still the experience of former masters is disregarded, the deliveries are permitted to increase, in 1818, to 3,539, and the death-rate increases to 1 in 63; and although when too late the deliveries are diminished next year to 3,197, the death-rate rises still higher to 1 in 33, the disease having now found its habitat, and retains it for another year, although the deliveries were reduced by 600, or to 2,458.

By a strange infatuation the deliveries were again allowed to increase to 2,849 in 1821, and to 2,675 in 1822; and though lowered in 1823 to 2,584, the death-rate rose to 1 in 44. In 1824 the deliveries were still further lowered to 2,446; the death-rates were reduced to 1 in 122. In 1825 the deliveries increase to 2,746, the death-rate increases to 1 in 105. The habitat is established, and although the deliveries in 1826 are lessened to 2,440, the death-rate rises to 1 in 30.

In this year I first became acquainted with this fell disease, as a pupil of the Lying-in Hospital, and when I call to mind the deep impression made upon my mind by witnessing the death of eighty women in one year by this disease, and of 191 for the four years that I attended as a pupil, most of whom had entered the institution in youth and health, buoyed with the hope of a speedy restoration to their homes and families, it is not to be wondered at that forty years has failed to efface it.

But, sir, painful as it is, I must persist in the cruel task I have set myself. The habitat was now thoroughly formed. The poison miasm pervaded the hospital thoroughly. Dr. Collins's mastership commenced in 1827, and he adopted every precaution that the resources of our art could devise to combat it. The deliveries, however, were increased by upwards of 100 that year, amounting to 2,550; the mortality was 1 in 77. In the next year (1828) the deliveries were increased to 2,856; the death-rate rose to 1 in 66; and although the deliveries were reduced in 1829 to 2,141, yet the habitat was there, and the death-rate was 1 in 63.

The reduction of deliveries, which Dr. Collins never after this allowed to amount so high as those of the next year, 2,288, told, and with the other means so minutely detailed in Dr. Collins's work, diminished his death-rate to 1 in 190, and it did not rise beyond 1 in 178 for the next three years.

It may be better to dwell briefly at this period of the history of the hospital; as, with the termination of Dr. Collins's mastership, an entirely new phase in its history is developed. The conclusions to be drawn from what has preceded are, that on every occasion when the hospital was crowded, sooner or later metria showed itself. Secondly, when this occurred, its habitat became established, and it continued for a longer or shorter time, notwithstanding every means that could be adopted: and although a reduction in the number of deliveries had an effect in ultimately improving the healthiness of the hospital, even these measures repeatedly failed in abating the evil immediately, when once the habitat of metria was thoroughly established. Let us now recapitulate the third and fourth propositions set forth in my paper, namely: third, that the generation and absorption of the metria poison or contagion is in a direct proportion to the number of parturient females cohabiting at their parturient period, or who breathe the same atmosphere at the time of their delivery; and the fourth, that in lying-in hospitals, where large numbers of patients are delivered under the same roof, this disease finds its habitat, appearing and reappearing at uncertain intervals.

If ever propositions were established by multiplied facts, and confirmed by prolonged experience, I submit these propositions have been so by the preceding analysis. But, as the object of this paper is, not merely to obtain a barren victory by the ingenious manipulation of a number of figures, but to carry conviction to the minds of the profession, and through them, to the public, let us reason a little more closely upon the laws which appear to be derived from a study of the history of the metria hauntings in the hospital.—Firstly, that markedly as these attacks occur in direct proportion to the number of parturient females cohabiting at their parturient period, yet the contagion is cumulative, or growing in its contagious power, requiring a longer or shorter time to arrive at its full intensity or saturation. This law accounts for the interval of time observed

to elapse, before the outbursts of the disease, and after the cause which experience has proved to be the most invariable, if not the only known cause of its development, after it has been generated—I allude to crowding.

Again, although it is true that the spread of the disease is in proportion to the number, it is a mistake to suppose that this crowding *requires* a large number of patients to produce it. We have seen, by the diminution of the mortality in the Waterford Cottage Hospital, on changing the number of beds from six to two in the wards, that it was not the presence of a large number, but the crowding of what we might say was a comparatively small number, that sustained the mortality at its higher rate. The same fact is proved by the large mortality observed to occur in the London Hospitals, containing thirty and fifty beds, and in the other hospitals enumerated in Dr. Churchill's table.

Now, ingeniously and pertinaciously as my critics have endeavoured to keep this latter law in the shade, and pervert it as an argument against my proposition, neither their facts nor their logic will hold for one moment on this subject. No incompatibility whatever exists between the latter law and my general proposition. I affirm that a zymotic disease is conveyed by the absorption of poisonous contagion. I lay down a general proposition, that the generation of this poison will be in the direct proportion of the number of patients subject to the conditions on which it is generated, and breathing the atmosphere charged with it. I require that, to lessen or prevent its generation and spread, the number generating and exposed to the spread of the poison should be reduced to a minimum. That minimum might be one, but I should not object to two, at least, until the success had been sufficiently tested. Need I remind my illogical young critics, that the major comprehends the minor; and that if more than one patient inhabiting a ward increases the liability to generating the poison, and the certainty of its spread and extension, that *a priori* the "generation and extension must be in exact proportion to the number of parturients exposed to it, and breathing the same atmosphere." So far, then, from the mortality in the medium-sized or small hospitals, hunted up by my critics, and a fearful array it was, but especially the mortality in the York-road, the Glasgow, the Edinburgh, and Dr. Beatty's hospitals; I say, so far from their mortality making against my proposition, properly understood, they all confirm and strengthen them, because they present the conditions I object to in a more marked degree; possessing all the objections and vices, with none of the advantages of the great lying-in hospitals.

Dr. M'Clintock, by a process of reasoning of his own, based upon selected statistics, arrives at the conclusion, which I cannot at all agree with as what he terms "above suspicion," that 1 death in 134 is the right number to occur in parturition. And, by another process, he

increases the Registrar-General's rate, arrived at from data and sources at least equally "above suspicion" as his own. This he does by deducting one-fourth by his own arbitrary dictum from the Registrar-General's number, 171, for England and Wales, which reduces his ideal death-rate to 1 in 129. Dr. M'Clintock will excuse me, therefore, when I tell him that neither he nor the Registrar-General, were he innocent enough to adopt Dr. M'Clintock's calculation, would be justified, if their object be to give an accurate return of the deaths in childbirth, in deducting upon his abstruse *quaternian calculation*, which throws Professor Hamilton's into the shade, one-fourth or any other proportion for diseases proving fatal in parturition. Without entering into the subject as to whether these diseases may have been caused or called out by parturition, there is one broad fact patent, that the fatality was determined by the complication of the disease with parturition and non-sequitur, that any one of the cases would have proved fatal at all, and certainly not at that particular time, had not parturition been complicated with the disease, or it may be, produced the disease, and caused or accelerated the death. Therefore the Registrar-General's calculation of 197 mortality is strictly correct, and Dr. M'Clintock's deduction of a fourth is entirely untenable.

Dr. M'Clintock attacks my tenth proposition in his own ingenious manner.

He takes my general proposition and applies to it his own *literal*, not my *general* interpretation, when I state that puerperal fever "is not a disease observed to occur in small lying-in hospitals," and this, although I give the small hospital in which it occurs, with the rate of its occurrence, twice over, once in page 40 and again in page 42, *supra*. He then devotes a whole page of statistics and argument to prove that I denied that puerperal fever *ever* occurs in small hospitals, and eventually winds up by admitting that my true meaning in the tenth proposition, which was as manifest as the sun in daylight to every eye but Dr. M'Clintock's, "*might* logically be inferred from the admission of the author that the poison of zymotic metria might be generated by any parturient female," and that this disease "is to be met with in the hovels of the poor and the chambers of the wealthier classes." Sir, I shall not designate such a line of discussion as I think it deserves. I ought, perhaps, to apologize for occupying so much of your time in refuting it.

But Dr. M'Clintock, descanting on the Waterford Lying-in Hospital, gives "five deaths as occurring in it from puerperal fever, with an average of only 115 annual deliveries." It would have only been justice to Dr. Elliott and the cause of truth, which we are at least ostensibly investigating, to have added that these five deaths from puerperal fever occurred in the Waterford Lying-in Hospital out of 3,469 deliveries, and extended over a period of thirty years, being a mortality of 1 in 681.

A mortality that would quite satisfy me if I could bring it about at the Dublin Lying-in Hospital; aye, or one-half of it!

But, sir, I regret I am not done with Dr. M'Clintock and the Waterford Hospital yet. In adducing these statistics from it, he falls into the very same blunder that, you will presently see, he attacked me for, and out of which he succeeded, by my enforced silence, in making such temporary capital. He fails, unintentionally of course, to distinguish accurately the two periods of the hospital, and, in place of giving the mortality by puerperal fever upon the long, or twenty-three year period, with its improved ratio upon the diminished number of beds, which he ought to have done in common justice to Dr. Elliott and the objects of his argument, *like* me he takes the whole period, but *unlike* me he does not give the results upon the improved period; of course his doing this was, as in my case, a mere slip of the pen in transcription. Let us see what the difference amounts to in the correct way of giving these results. We have seen that in Dr. M'Clintock's manner of proving the rate of mortality, the death-rate of the more crowded Waterford Hospital was one in six hundred and eighty-one; but in the correct and fair way of calculating it amounted only to one in one thousand three hundred and twenty-eight. It will be perceived, to prevent mistakes, as Dr. Elliott requires, we have put his calculation, not mine, in letters.

So much, sir, for Dr. M'Clintock's purism in his own calculations, and his vaunted refutation of my tenth proposition, to refute which, he says, it were enough to cite the instance of the Waterford Lying-in Hospital. The Liverpool Lying-in Hospital is the next cited, but as he gives it a mortality of 1 per cent. it tells in favour of my position, although, as I have already said, I do not admit it as to be classed with the cottage hospitals. I only wish we could reduce the mortality of the Dublin Lying-in Hospital to 1 per cent.

The next instance adduced against the establishment of cottage hospitals is the South-Eastern, or Dr. Beatty's Lying-in Hospital. Dr. M'Clintock introduces his notice of it in these words—"I feel great satisfaction in appealing to the statistics of this little hospital, knowing they are worthy of implicit confidence." I confess honestly I cannot join in the great satisfaction he expresses upon this subject, and feel assured, as was the case in the great lying-in hospital, Dr. Beatty must have laboured under difficulties from the construction of his hospital that rendered it impossible to keep his mortality at a lower standard than 1 in 69. At all events, as I elsewhere stated, I admit no comparison between Dr. Beatty's hospital, with nearly 400 deliveries annually under one roof, and the cottage hospitals already alluded to, and to which we may now add the Killarney and two Dublin Union Hospitals, and the thirteen English poorhouses reported by the Cubic Space Committee, in which 2,459 deliveries occurred, without one death, in five years (see

page 58, *supra*); and be it remembered that these only afford an approximate test of the principle of isolation, as in none of them has the principle been carried out to the full extent required.

The last case adduced by Dr. M'Clintock against my tenth proposition is the small Kingstown Lying-in Hospital. Dr. M'Clintock says that "the annual deliveries, not exceeding 90, *it is in every point* of view an *unexceptionable* specimen of a cottage hospital." Judging from its statistics, I should arrive at a very different conclusion, and as these are my only data for forming my opinion, I must say that I pronounce it in every point of view an *exceptional* specimen of a cottage hospital. When Dr. M'Clintock analyses on the strict philosophical principles laid down by Mill, as applicable to exceptions to general rules, the true explanations of the causes that rendered it exceptional, then I shall attach value to the exceptional case of the Kingstown Hospital, with its total mortality of 1 in 68, or of 1 in 114 from puerperal fever. In the meantime we must class it with the exceptional case of Ballarat to be afterwards alluded to, adduced by Dr. Stokes as a mystery which, although unsolved, may not be insoluble.

Dr. M'Clintock, as well as several other of my critics, has called in question the statistics supplied by the General Registration Offices throughout these kingdoms. I am not here as the apologist of this department. Those indefatigable and distinguished men who direct and control this great department, one of the few scientific branches of the Executive in this country, stand too high by their labours and results to require me as their apologist. But, sir, I must be permitted to express my dissent from and regret at the flippant and offensive manner in which this great organization of the State has been treated. Sir, I deplore it the more because I am old enough to recollect the difficulties we had in getting this great desideratum—the Census Office—established upon a fixed and permanent basis; the jealousies it excited, and the difficulties there were to bring our legislators to adopt it, and our masses to co-operate with it and submit to it. It behoves thinking men to consider how far they are justified in breaking the faith of the public in so wise and so valuable a department.

For my own part, I totally disagree with all the slighting remarks that have been thrown out upon the statistical labours of, and results supplied by, the Registrar-General's Office. I rely upon the latter as the best information procurable upon the subject of statistics generally, and I have no doubt that, even admitting it to the fullest extent, that certain inaccuracies must creep into their calculations from accident, different views of facts, or even from design, yet that these are counterpoised by their opposites and corrected by their analogues in such a manner as to render the total results as nearly correct as any human machinery, which necessarily employs an immense and varying agency, can accomplish it. I therefore accept the Registrar-General's statistics as,

if not the best possible, at least the best possible with our present facilities, and feel myself quite justified in assuming them as the soundest attainable basis in the present discussion. Their very extent should render them unexceptionable to my critics, as they insist so much upon this element of proving statistical truths. I shall therefore say, that with the greatest deference to those of my commentators who have collected statistics of their own, bearing out their views of what should be the mortality of lying-in women, I shall prefer accepting the statistics of the Registrar-General, and his statement of what it actually *is*, to their selected figures, collected and marshalled to prove what it ought to be. This general statement will dispose of a host of those gentlemen's calculations who have favoured us with statistics of their own selection and procurement, but which are entirely at variance with those of the Registrar-General.

It will now be convenient to allude to another law that demands our attention and consideration. It is conveyed in the word habitat, which you will recollect is the subject of my fourth proposition—one which has been the object of nearly as many attacks as my third ; so many, indeed, that my belligerent critics must have looked upon it as the Malakoff of my position. They are right in so doing. So do I.

In answer to those gentlemen who are perfectly satisfied with the state of the Dublin Lying-in Hospital, as well as those who have so strenuously and unmeasuredly attacked me for attempting to introduce such improvements into its construction as will lessen the mortality within its walls : let me ask you—some of whom have even had the hardihood to assert that it is, quoad its mortality, in a satisfactory and an improving condition—let me ask you, I say, to look at the facts. The institution has had out of the 113 years passed since its foundation, 65 years in which it was so severely attacked as to bring the death-rate, upon the rough principle of calculation given in my paper, from 3 to 2 figures, and thus 113 divided into the following periods show with what results:—In the first 18 years after the establishment of the hospital tested in this way, metria prevailed extensively in 13 ; in the next 20 in 7 years ; in the next 19 in 7 years ; in the next 20 in 8 years ; in the next 20 in 13 years ; and, in the last 15 years, in every year of the 15, or, in the last 35 years in 28 years, leaving only 8 years comparatively free from the ravages of the disease, as thus tested. To this length I was prepared to go, on writing my original paper, and to leave the comparison of two or three figures as the test of the existence or non-existence of metria within the walls of the hospital. The violent and multiplied attacks to which this has exposed me, have naturally required my closer and more searching investigation of this subject. The conclusion which this reconsideration has led to, and confirmed, in my mind, is, that in this rough way of testing the existence of metria by the death-rate amounting to two figures in place of three, I was quite wrong and under the mark. That

it affords no true test, and that I must abandon it in favour of a more accurate and trustworthy comparison. This I shall be obliged to develop and amplify in dealing with Dr. Beatty's paper a little later, and merely allude to it at present to prevent misapprehension.

We have seen that the condition of atmosphere which predisposes to this disease, is, at times, of very slow development, or what we term cumulative, in its properties. When these properties are thoroughly fermented—this phrase is not mine, sir, it has been used by my critics—and right or wrong in its physiology, it describes what I want to convey, then, there is often an equally remarkable rapidity in the time occupied in infecting its victim. But when it has once shown itself, then the law of my fourth proposition comes into operation—the law of habitat; that law which fixes its endemic character, by which the atmosphere is changed from its cumulative to its actual poisoned, or saturated, state. What was an approximation to it, during its cumulative period, becomes now an absolute fixed quantity; and so persistent is it, so obstinate and so pervading, that for weeks, months, and years, as I have proved to you by the history of the Lying-in Hospital, every part of the numerous and spacious wards of this institution has been haunted and occupied by this dire disease. That haunting and occupation has continued, until, like other zymotic diseases, it has worn itself out, or until it has been checked by want of victims; the patients not being admitted into the hospital, offering often the only effectual check to its continuance. One reason why the term habitat is especially applicable to metria is the peculiarly obstinate and pertinacious character of its hauntings, as compared to other zymotic disease. In cholera, measles, whooping cough, erysipelas, and even small-pox, the disease runs a course and terminates in a reasonable time. It may, in most epidemic attacks as they are termed, very questionably I think, be traced to its introduction into a house or neighbourhood hitherto free from it, from a house or locality in which it prevailed. It runs its course—precautions are taken—it wears itself out. The same is observed in epidemic prevalences of puerperal fever, where it prevails in districts. In these it runs its course very much as the other zymotics do; but it is not so in hospitals. Here the law I dwell upon, that of habitat, comes into operation; it continues to haunt the now infected hospital for an indefinite period. Why? Because *here* it possesses the conditions ready for its spread when in existence, and for its regeneration and cumulation, if suspended for a time in its ravages. These are the considerations which justify and require that ill-constructed Lying-in Hospitals should be denominated the habitat of puerperal fever. Need we again recapitulate the proofs of this, adduced so often, but especially in treating of my third proposition? where I showed, amongst other instances, the deaths of 191 women in Dr. Collins's first four years' mastership, including some months of Dr. Pentland's, and 170 in my own seven years' mastership.

This consideration of the habitat, so conclusively proved, brings us, by a natural transition, to the more recent phase or change in the history of the Lying-in Hospital of Dublin referred to, and which embraces the last 35 years. In this period the principles of cumulation and habitat may be said to have culminated and become persistent. In place of occasional outbreaks of metria, with years of health, matters have changed.

This increase, or rather persistency of metria, would lead to the conclusion that true metria is changing in its character more and more every day, assuming the habits and laws of an endemic or indigenous disease in our great hospitals, which, instead of showing itself at intervals, as do other epidemic diseases, remains constant, and will continue to do so as long, at least, as those conditions which generate, cumulate, and develop it are permitted to exist.

This is the state of facts with which we now find ourselves face to face. The phase, the habits, the laws of metria are altered. The bygone history of the lying-in hospital is only so far of value as it traces out the progress of matters to this consummation. We have now not an occasional epidemic, but an established disease, having its generating cause, its cumulative cause, and its pabulum within the four walls of the hospital. The hydra is there within your reach. Deal with it as you would with any other "pest-house." Again, sir, this is the language of my critic, not mine. But, sir, do not misunderstand me. I am in this question, at least, a reconstructive, not a destructive reformer. The disease, bad as it is, is not in a state to require either disendowment or disestablishment. It merely requires that the hospital should be modified to meet our improved knowledge of zymotic disease, and the altered characters of metria in particular. Let the success of Joseph Clarke in trismus, and of Jenner in small-pox, encourage you in your efforts. Be not deterred by opposition. No valuable improvement was ever accomplished without it, and there is no abuse so untenable that it won't have its backers, no fallacy so palpable that it won't have its supporters. Jenner was nearly annihilated by the anti-vaccinators; he survived them, and the anti-vaccinator is now reduced to a unit. Our profession, enlightened and humane as it is, is not easily turned out of its habitual course of thought and action. Besides, the attractions to our revolving in our present path of thought and action in the case of our large hospitals are weighty and cogent. Let us not, however, try this out too long, or add a third case to the two instances that give the death-rate of the Dublin poor management an infamous celebrity, and one handed down in the statistical books of the last half century as a disgrace and blot on our national character. Let the infant parish mortality and the death-rate of the foundling hospital remind us that, not many years since, there were respectable men, and wise heads, aye, and learned doctors, who did not flinch from maintaining and advocating the per-

sistence in a system of mismanagement, the results of which are too disgraceful to dwell upon, and which led to the erasure from our charitable institutions of the Foundling Hospital, which was at that time, what the Dublin Lying-in Hospital has been, the pride of this city—an institution which, with proper management like similar institutions elsewhere, might have been rendered a means of combining a charitable ministration, with a powerful engine of civilization and enlightenment throughout this country.

Sir, I now approach a branch of my subject that I would willingly have avoided. Dr. M'Clintock has detected an error in my transcription, and this is at once characteristically denounced as follows :—"Here Dr. Kennedy has committed a serious arithmetical mistake in his data, which consequently falsifies all the conclusions he has laboured to draw from them." Now, when I tell my hearers that this statement is the substitution of thirty years for twenty-three years in transcribing from Dr. Elliott's book, the period for which an average calculation was adduced; that the result was strictly correct in the average as the basis of calculation founded on the twenty-three years' period; but, above all, when I tell them that the error in transcription was itself corrected, in the same page, by giving Dr. Elliott's text *in globo* in which the two periods were fully set forth, and the calculations based upon them detailed by Dr. Elliott himself, I think I am justified in asking whether it was a worthy or justifiable line for an honourable antagonist in argument to take, to put into circulation an assertion that "I had committed a serious arithmetical mistake in my data, which consequently falsified all my conclusions," knowing that for weeks or months this damnatory misrepresentation must be in circulation before its contradiction should appear. It will be recollected that Dr. Elliott, in his report of the Waterford Hospital, mentioned that after six years, for which his hospital was established, he removed to another building; and he gives the statistics of this hospital separate from the one he removed to. The statistics of the latter, extending over a period of twenty-three years, I, sir, of course, took the longer period for the basis of my calculation; firstly, because it *was* the *longer* period, and secondly, because there were fewer beds in the wards, and it consequently approximated more nearly to my idea of a cottage hospital. In the hurry of transcription I gave it in my last as the result of the thirty years, not the twenty-three years' period. In the table of figures in the next page[a] (304) I gave the accurate result on the twenty-three years' calculation, namely, 1 death in 295, and this it is that forms the basis of my calculation. It is therefore perfectly correct, and I repudiate entirely Dr. M'Clintock's figures, based upon a totally different calculation. It is a remarkable fact that by a

[a] Dublin Quarterly Journal of Medical Science, May, 1869.

typographical error a unit is omitted in the same paragraph that Dr. M'Clintock deals with, but he did not deem it necessary to dwell upon this, as the omission told 1,000 against my argument.

Good, however, comes out of evil, as Dr. M'Clintock's delicate exposure of "my serious arithmetical mistake and falsified conclusions" has drawn out a further commentary from Dr. Elliott, who writes to me to this effect:—"But the most useful and instructive mode of viewing these statistics is reached by dividing them into two periods. During the first period of six and a-half years we occupied an hospital in which eight beds were grouped together in one ward, and there during that period the total mortality was one in one hundred and twenty-five and a half, and the mortality in puerperal fever was one in two hundred and fifty-one. During the recent period of twenty-three and a-half years we have occupied our present hospital, in which eight beds have been divided between two wards, four beds only in each ward, and here we have had a gross mortality, from all causes, of one in three hundred and nine, and a mortality in puerperal fever of one in one thousand three hundred and ninety." Dr. Elliott considerately adds, "I use letters instead of figures, that there may be no mistake this time."

If further proof were required of the too palpable conclusion of the rate of mortality being in proportion to the number of patients occupying a given space, as insisted upon in my third proposition, so violently attacked as the Redan of my position, what could more completely resist their assault and turn their attack than the simple, wise, and thoroughly trustworthy statement of Dr. Elliott, that by the mere expedient of reducing his beds from eight in one ward to four in his two wards, he succeeded in reducing his gross mortality from 1 in 125½ to 1 in 309.

This admits of no refutation, and requires no comment beyond what Dr. Elliott states in his pamphlet, page 6, as a significant fact, "that whereas the mortality from puerperal fever was in the first hospital 1 in 251 during a period of six years and six months, the mortality from the same cause has been, in the present one, only 1 in 1,328.

Dr. M'Clintock's best specimen of statistical criticism remains. It is that in which his anxiety to arrive at truth is only equalled by his determination, in his own language, "to show my chain of reasoning fallacious," "inasmuch as a great error, represented by the number 8,688, lies at the very basis of my calculations, and vitiates *all* the conclusions derived from them." "The error is this: I give the total number of births in the Dublin district for the period specified as 52,126, whereas it is only 43,438." Dr. M'Clintock then naively adds—"I discovered this by the merest accident in the library up stairs when looking over the reports of the Registrar-General for Ireland;" and then, feeling that he has gone to the very verge of the line of veracity in his insinuation, adroitly turns the subject with the most inimitable

simplicity by the words—"Let me now return from this digression." Now, let me ask, if the object of these remarks was fair and candid criticism, why did not Dr. M'Clintock show to his hearers how this mare's nest of 8,688 of an error vitiates all or any of the conclusions derived from this calculation? Simply because he could not, as the calculation was not in the slightest degree affected, nor the results in the most remote degree influenced by the mare's nest he had discovered. Dr. M'Clintock knew when he penned this that I was discussing an average from these figures, and that whether this average was produced from forty or fifty thousand did not signify. Hence, then, the graphic description of the "accident in the library when looking over the reports." I had nearly forgotten to add the interesting detail, following the example of Mrs. Quickly and her seacoal fire, in order to insure precision and extreme accuracy. Dr. M'Clintock informs us that it was "upstairs;" and hence, also, the sudden and adroit, but, alas! now that the day of *reckoning* has come, the simple suggestion, "Let us return from this digression," will not avail in relieving him from his dilemma when I tell him, as I do, that the duty remained to him to prove in what manner the 8,688 "had vitiated all my conclusions." Sir, that was a duty he could not execute, because it was impossible for him, by any amount of ingenuity, to prove as a fact what was not a fact, and what he put forward as a fact, although the means of testing its falsity were before him "upstairs in the library," just as patent as they are before you and me at this moment.

The fact is, the 8,688 error in transcription was totally unimportant, and whether it was 8 or 8,000 did not affect the result one iota. You will perceive by this return, kindly furnished me by the Registrar-General for Ireland, and given beneath, that the total number of births are given in six consecutive lines of figures.

"REGISTRATION OF BIRTHS AND DEATHS, IRELAND.

"Return showing, for the years 1864–8, the total number of births and deaths *registered* in the Dublin Registration District;[a] also the number of deaths from puerperal fever (metria), and of deaths in childbirth, compiled from the weekly returns issued by the Registrar-General for Ireland.

	1864	1865	1866	1867	1868	Average number 1864-8
Total number of Births, . . .	8,753	8,903	8,695	8,241	8,846	8,688
„ Deaths, . . .	7,345	8,151	9,034	8,607	8,004	8,228
Deaths from puerperal fever, . .	42	32	66	37	32	42
„ in childbirth, . . .	18	35	27	38	46	33

"J. M. BURKE, *Medical Superintendent.*

[a] "The Dublin registration district, consisting of Dublin city, and suburban districts of Rathmines, Donnybrook, Blackrock, and Kingstown, extends over an area of 9,745 statute acres, and had, according to the census of 1861, a population of 314,409."

In transcribing the above numbers into a column for totting and reduction, the sixth line of figures was added in both cases from the words "average numbers," written in a small hand, causing them to escape my notice. Thus the average of births was given as an additional year, as well as the average of deaths reduced, leaving the resulting average, which was the object of the calculation, identically the same as if both had been omitted, and the five years only given. This, then, is the great error discovered upstairs in the library, that "renders my whole chain of reasoning fallacious, and vitiates all my conclusions."

But, sir, as this great error has been mooted in all the medical coteries of Dublin, and will fly over medical Europe on the wings of the press, I have deemed it due to myself and to the important subject into which I have thrown my advocacy, to place these *great errors* of transcription, so much dwelt upon, in their right light. I have, therefore, required the certificate of Mr. Francis Low, whose reputation as a notary public and statistician rank so deservedly high, as to their true nature and their bearing upon the subject under discussion. Here is his certificate :—

"At the request of Dr. Kennedy I have examined his computation of the rate of mortality from puerperal fever and childbirth which prevailed in the Dublin registration district during the five years from 1864 to 1868 inclusive, as given in his paper on 'Zymotic Diseases,' and deduced from the Registrar-General's return, and find that, notwithstanding an error in the transcription of the figures, *the result is correct*, as will be seen by the following explanation :—

"Dr. Kennedy, in summing up the total number of births, as also of deaths (resulting from the causes stated), for the five years alluded to, included, in error, a sixth quantity, viz., the average for the five years, which had been placed in a sixth column over against the other five, giving the births and deaths referred to. The including of this sixth quantity, though inadvertent, *plainly cannot affect the result*, because *it* is the mean (as stated) of the other five, and therefore the mean of the six must be identical with that of the five, and the mean number of deliveries, divided by the mean number of deaths, constitutes the rate of mortality sought.

"Dr. Kennedy also quotes correctly Dr. Elliott's report of the rate of mortality (1 in 295) prevailing in the hospital in Waterford intended to be referred to, viz., that occupied as such from October, 1844, the only errors being the statements that Dr. Elliott's report regarding the mortality in this hospital extended from 1838 to 1868, instead of from 1844 to 1867, and that the mortality from puerperal fever was 1 in 328 instead of 1 in 1,328, which latter rate manifestly strengthens Dr. Kennedy's position. "FRANCIS LOW, Not. Pub.

"DUBLIN, 26*th June*, 1869."

Sir, in my ignorance I had imagined that in a large mass of calculations such as the preparation of my paper necessarily involved, errors in transcription and typography, and even oversights in calculation, might be expected to creep in. I further imagined that in a discussion for the benefit of science and humanity, such as this purports to be, an honourable seeker of truth would have shrunk from making use of such accidental oversights as weapons with which to damage his opponent. However I have lived to learn. But, sir, I am too old to learn much. Therefore I cannot undertake to benefit to the full extent intended by the lessons I have had in arithmetic.

Be that as it may, I may say this much, that should I be required to advise a course of education to qualify a man for a similar encounter to that in which I have been engaged, I should be disposed to take a lesson from the schooling I have had, and advise his being educated upon the excellent system so admirably laid down by Mrs. Malaprop, and which you, no doubt, recollect she so judiciously insists upon in allusion to her female pupil. "I should not allow her to meddle with logic, algebra, simony, fluxions, paradoxes, or such inflammatory branches of learning; neither should I let her trouble herself with mathematical, astronomical, or diabolical instruments. But she should be taught a supercilious knowledge of accounts; sent to school to learn a little ingenuity and artifice, and as she grew up she should be instructed in geometry, that she might know something of contagious countries, but, above all, she must be a true master of orthodoxy, in order that she may not pronounce her words so shamefully as people usually do, and that people may reprehend the meaning of what she was saying."

The pleasing duty now devolves upon me of commenting on my friend Dr. Beatty's paper, which I look upon as one of the most valuable read to the Society throughout this debate. He took up one idea, he never lost sight of it throughout, and he exhausted it so completely that every commentator who has dealt with his subject has been feeble in comparison to him upon it; I allude to the identity of puerperal fever and erysipelas. His arguments, cases, and reasoning leave nothing to be said upon this subject, if indeed further evidence was required to confirm the views propounded by Dr. Sidey thirty years ago, and sustained by almost every hospital physician who had possessed an opportunity of observing upon those diseases since. Dr. Beatty is equally strong upon the contagious nature of this; according to him, common disease, erysipelas and puerperal fever, and his observations upon this branch of his subject merit our deepest attention. In fact, Dr. Beatty's support of my views is conclusive, and merits my warmest acknowledgment. Nevertheless it was necessary that he should also attack me; but I am puzzled to know why he, of all my commentators, should have been the person to read me and this Society a lecture upon the waste of time

occupied in vain attempts to discover the nature, origin, and best means of preventing metria, and to prohibit speculation on the origin and common nature of zymotic poisons, in the very same breath that he advocates and insists upon the identity of two of these poisons, erysipelas and puerperal fever. And yet Dr. Beatty asks this Society, in his own innocent manner, if any one is a bit the wiser after reading a passage which illustrates common poison on the principles of isomerism, as advocated by Dumas, for ignorance of which and its application to the subject in hand, any enlightened physician might justly have been condemned.

Dr. Beatty brings a charge against this Society of "*one-sidedness* as applicable to their recent proceedings, their speeches, and their arguments in contravention of my paper." This is his language, not mine. Sir, I should have scorned to bring such a charge against our common profession, whatever insinuations Dr. Beatty, or even the all-powerful press of this country, may make to the contrary. Well may his congeners say—had an enemy done this thing, but that it should have been done by our own familiar friend with whom we had taken sweet counsel together; and he adds, that not one single voice has been raised in favour of my paper. Why, sir, Dr. Beatty himself is a living instance to the contrary; his facts, his arguments, and his reasoning go most strongly in favour of my positions. I could not have wished for a better advocate, or more conclusive evidence than he adduces for my views—and he possesses this great merit, that he is perfectly unbiassed towards me. I can, therefore, only explain the hostile line he has taken by his labouring under this strange hallucination, that all the speakers on my paper are "one-sided," and conclude that his national tendencies so predominate that he must strike a blow on the one side even whilst his arguments go to support the other, on what he so naively designated this "one-sided discussion."

Dr. Beatty sets out by another rather sly cut at his friends in telling them that *their statistics have so well answered the purpose for which they were used*, that he will not weary the Society by any allusion to that dry subject, further than by giving a table of his father, the late Dr. John Beatty, and his own statistics in private practice. Their total combined deliveries were 7,680; deaths, 30; being a mortality of 1 in 256. I believe this death-rate to approximate much more nearly to the true general rate in private practice than the examples given in Dr. M'Clintock's or Dr. Churchill's tables.

I, therefore, accept them as far as they go as confirmatory of my views and calculations.

Dr. Beatty then passes to the support of my views upon a common poison, and succeeds in establishing the identity of two at least, indeed I might say three, ranking pyemia as one. This is a first step towards the accomplishment of what may eventuate as a more general recognition of this principle. He modifies this statement when he says we know that

zymotic diseases, but more especially measles and small-pox, are not convertable the one into the other. But, it will be in the recollection of the Society, that Dr. Stokes met this difficulty by admitting the occurrence of *mule diseases;* therefore, on these nicer points, we may safely leave Dr. Beatty and Dr. Stokes to fight it out.

I might here take an exception to the pathology of Dr. Sidey and Dr. Beatty, and state that the great variety of tissues engaged, and the not infrequent absence of peritonitis, and other serous inflammations, in metria, do not quite justify the conclusions that "puerperal fever is merely an erysipelatous inflammation of the peritoneal covering of the pelvic region, or other serous membrane, extending, as the disease advances, over the abdominal cavity, and the body of the uterus accompanied by true erysipelatous fever." Dr. Beatty says that when erysipelas is epidemic, metria is sure to appear, and that when erysipelas subsides so will metria, "if *separation* for a time be enforced, and the disease not perpetuated by contagion."

This latter admission is valuable, and the Society should not allow it to escape their notice, that on Dr. Beatty's authority, the condition upon which this common disease subsides is separation, which condition not being complied with, it does not subside. Could this gentleman have used a stronger argument in favour of isolation and the establishment of cottage hospitals? I think not.

Dr. Beatty next attacks my statement, that metria approaches more nearly to a constant quantity in great lying-in hospitals; and asks me why it is not always present in such institutions? and why it is more intense when the greatest numbers are collected together? which he answers is notoriously not the case.

Now, sir, is it really necessary for me, with the statistics of all the great hospitals of Europe before you, and with those of your own hospital at your door, to go back and prove to this Society what Dr. M'Clintock has denominated, "*the great excess of metria cases in hospital?*" If language expresses facts, or facts are capable of expression by language, what language could more accurately express this state of facts than that metria approaches more nearly to a constant quantity in a great hospital; but indeed the tenor of Dr. Beatty's own paper goes to prove this, although he explains the excess of metria on different grounds.

I may here mention, in addition to the dread array furnished in my former paper, that in the Maternity at Turin, where 700 or 800 women are annually delivered, the mortality for the last 10 years amounted, according to Professor Faye, to 1 in 25, or 4 per cent. in the students' department. In the department for midwives it is said to be still greater, which fact, together with the more recent experience of the Vienna hospital, would appear to lead to the conclusion that too much stress may have been laid upon the extension of contagion through dissections.

However, as this is a step in the right direction, we shall not dwell upon it.

In the lying-in hospital in Dresden the average mortality amounts to from 2 to 3 per cent.

The lying-in institution in Copenhagen, according to Dr. Stadfeldt's report, gives 1,237 deliveries in the year, with 41 deaths, 1 in 30, or 3·3 per cent. This approaches very near to the mortality in the Dublin Lying-in Hospital as it does in the number of deliveries. This report, however, is stated to be, on the whole, more unfavourable than that of the preceding year.—*Med. Chir. Rev.*, Jan. 39? T. 26?

On the other subject, the mortality in proportion to the numbers, Dr. Beatty offers a very simple, but to me inconclusive denial, that "it is notoriously not the case." The Society might not, perhaps, be quite satisfied were I to answer Dr. Beatty in his own language, and simply assert that it notoriously is the case, as I have proved by the statistics of the lying-in hospital, and it notoriously has been the case since its foundation. On no occasion[a] in which the hospital was crowded was not the crowding succeeded by metria, and the metria was not banished until the numbers were lessened, and separation, as insisted upon by Dr. Beatty, carried out. I explained this upon a principle, which must attract more attention than it has hitherto done, in the investigation of zymotic disease; namely, the principle of cumulation. The duration and development of zymotic disease generally, will acquire much elucidation by the study of the habits of metria, and the application of the principle of cumulation in our great lying-in hospital. Dr. Beatty asks "why have there been long intervals of years, sometimes when the largest number of women have been confined during which no metria has occurred?" In the first place, I deny the long intervals of years. The tables of the Dublin Lying-in Hospital prove directly the reverse. Let us test this by Dr. Collins's death-rate, calculated from the one solitary instance of four years of freedom from the disease, which stood at 1 in 182. Now let us see how many years of the 113 that the hospital has been open for, have ranged above, and how many below this death-rate. Out of the 113 years, 104 have ranged above it; therefore, we might insist upon 104 years out of the 113 having exhibited metria; but, as this would include some years that approximated too closely to the limit, we shall strike off thirty from Collins's average mortality, and calculate that metria was present in the hospital every year that the mortality arose to within thirty of his average death-rate for his four successful years—a calculation that can not be objected to. Now what is the result on this calculation? that out of 113 years of the hospital's

[a] See Appendix D, and also Appendix E, Dr. Grimshaw's report and diagram.

existence it has only been free from metria for twenty years, twelve years of which twenty it has only been partially free, and has been haunted by it ninety-three years, or about five-sixths of the time that has elapsed since its opening. Again, let us test the assertion of the long intervals of years without metria, when the largest number of women were, as Dr. Beatty says, confined; an assertion in which he by no means stands alone, as Dr. M'Clintock, Dr. Atthill, and several other learned doctors, too numerous to mention, make the same assertion. If we abstract Dr. Collins's exceptional period of four years' freedom from metria, 1830–1833, inclusive, which providentially affords us a sufficient duration of healthiness to base sound sanitary calculations upon, in analysing its history, for no period, throughout the existence of the institution but one has it been three years free from metria. That was from 1780 to 1782, inclusive. Only thrice has it had two years' freedom, namely, 1785–86, and 1807, 1808, and 1815–16; and, in the other years for which it was free, it was only for one year at a time that the immunity existed. No long time for the cumulative process to develop its poisoning principle.

So much for the assertion of the long intervals of years in which no metria has occurred, and for Dr. M'Clintock's joke of the long period of gestation. He would have been more philosophic had he used "incubation," and better still "saturation," at the expense of his joke.

Let us now turn to the appendix D, cast our eye over the tables, and test the effect of crowding by showing the relation in which increased numbers have stood to mortality, from the foundation of the hospital to the present day. I regret delaying you with this, but it is imperative.

Let my readers also examine the table of curves and report in the appendix,[a] which indicates this relation at a glance, for which I am indebted to my friend Dr. Grimshaw, our authority upon this admirable plan of illustration. If these, with the detailed enumeration set forth in the appendix, and the mortality table, page 30, *supra*, fail to satisfy the most sceptical of my opponents of the relation between crowding and mortality, and of the Dublin Lying-in Hospital having been for 93 out of 113 years of its establishment the habitat of metria, as well as of its being now a persistent endemic in that institution, it only remains to decide whether the failure results from want of proof, or from wilful blindness and their being incorrigible.

It will be seen that I have, in the Appendix, gone into the details of each year of the last or altered period of the hospital, since the disease became truly endemic, to show that even in this period a further increase was added on each occasion to its permanent high rate of mortality by increasing the number of women delivered under the same roof, in the same manner exactly as was the case when the disease assumed more of

[a] See Appendix E.

the epidemic character in its attacks; I mean in those periods of the history of the institution when occasional healthy years cropped up, with alternating outbreaks of metria more or less severe.

This, then, brings our annual analysis of the hospital to its conclusion, and confirms, with a confirmation "strong as holy writ," our two positions, the fourth and fifth, that have been so much attacked, and which we submit are as now placed beyond the possibility of cavil.

Dr. Stokes states, in his observations upon my paper, that there are two elements in the production of a disease from poison, such as metria, receptivity, and poison. Now, with great deference to my learned friend, if he will read my propositions correctly, he will find there are three. The first is the being a parturient female. This may be defined as the condition-precedent. The second is the existence of circumstances favourable to its imbibition; and the last is the existence of the poison.

Dr. Stokes overlooked the second altogether, and disregarded his logic by the substitution of a middle term different from mine when he put such language into my mouth as—"That a natural process, such as parturition, should be *of necessity* coupled with a tendency to develop a fatal poison."

My language, my ideas communicated, and my logic, are exactly the reverse of this. My proposition stating—"That this poison *may* be generated *by any* parturient female, and *where* the *circumstances* are *favourable* to its imbibition it *may* be absorbed into the system of the generator, or that of any other parturient female exposed to its influence."

From this it is patent that my intention was not to convey such an extravagant or erroneous idea, as that a natural process, "such as parturition, should *be of necessity* coupled with a tendency to develop a specific and fatal poison," which, no doubt, would have been, as Dr. Stokes characterizes this creature of his own fancy, "a startling proposition." But to convey what *is* conveyed as plainly as language can convey any idea, *ipsissimis verbis*—"That this poison is not of necessity generated by every, but *may* be generated by *any* parturient female *where* the circumstances are *favourable* to its *imbibition*, and absorbed into the system of the generator, or that of any other parturient female exposed to its influence."

I am not, then, placed in the position that Dr. Stokes had designed to land me in, of proving that the "natural process that secured the existence of man upon the earth is necessarily coupled with a tendency to develop a specific and fatal poison."

Dr. Stokes next lays down a very sound rule in philosophizing that we should, where there are two methods of explaining a certain phenomenon, accept the most probable. Now, let us test his own views upon this principle. He says, I myself have not the slightest doubt that "contagion is less the cause of the spreading of puerperal fever in

hospitals than epidemic influences. What these epidemic influences are no man can say," &c. Again, one of the most remarkable instances with which I am acquainted in favour of the epidemic influences in producing puerperal fever as distinguished from contagion in lying-in hospitals, "is the great fact that women are simultaneously affected by puerperal fever in the hospitals where they have been entirely separated, and where even the attendants have not been the same."

Now, let us apply Dr. Stokes's test of probability to this position. Is it possible, let me ask him, to find the state of things he describes in the lying-in hospital without a case of puerperal fever in the district outside of it, a state confirmed by my own experience as well as that of most other masters of the hospital; and yet to persevere in ascribing this disease to epidemic, not endemic, and contagious influences as the more *probable cause?*

Is it more probable that a disease that has haunted the Lying-in Hospital of Dublin, as I have proved by its statistics for 93 out of the 113 years it has been opened, should be more due to endemic and contagious causes, or to epidemic causes, when the districts surrounding the hospital, taking all the years it has prevailed epidemically in Dublin would not amount to a tithe of those in which the hospital has been scourged with it? Is it more probable that a disease existing, and repeatedly existing, *in* the hospital[a] and *not* appearing *outside* should be due to causes operating within the hospital, than to causes operating without?

But Dr. Stokes begs the question in his proposition. He says the women are simultaneously affected. This is not my experience of even what we term an outburst of puerperal fever. On the contrary, patients are consecutively attacked. Unlike the occurrence of influenza—the best specimen of epidemic disease—which often spreads to hundreds of cases in the course of one day or night, and depends, as we often observe, upon sudden atmospheric changes, metria begins usually by a dropping case, then in a few days another, then the plot thickens as the atmosphere becomes pervaded with the poison, and fresh victims are in the state of receptivity. Again, the influenza disappears or dies out with the alteration of the physical or atmospheric causes that produced it. Not so metria in hospitals, as we have had too painful evidence in what has preceded. Is it, therefore, *more probable* that with this state of consecutive development of the disease, it should not be more ascribable to endemic and contagious than to epidemic causes?

But Dr. Stokes again begs the question. He says, "Where they have been entirely separated in hospitals the disease has spread." But have they been entirely separated? This is the very point I have been devoting all my energies to accomplish. This is the very question at issue—

[a] See Dr. Joseph Clarke's letter, page 91, *infra*.

Whether they are to be entirely separated?—and this is the very view of the question that I regret my friend Dr. Stokes has thrown in the weight of his great influence to prevent. If he will join me in this one point—namely, the entirely separating the patients, I shall hail him as an advocate of the cause I have so much at heart. But what is the fact? It is impossible, and I know it by painful experience, "to entirely separate" a number of people living under the same roof, and who must necessarily breathe the same atmosphere. I have tried it, and I might just as well, to use a homely adage, have attempted to stop the tide with a pitchfork.

But Dr. Stokes alludes to the selection made in patients being attacked or contaminated in different parts of the same building, and looks upon this as incompatible with contagion or endemic influence. Why, it is the very essence of its nature, that contagion—*that* the pestilence that walketh in darkness should charge an inclosed atmosphere and hang about or haunt it, moving from room to room with the currents that invade the building, resting for a time in one from stagnation, and then moving away to another. And here Dr. Stokes's receptivity comes into play in explaining the selection of its insidious attack. The circumstances are favourable to its imbibition, and the victim is struck. What can be more probable than all this? Contrast with it the probability of an epidemic metria, which *does not exist outside*, permeating the hospital, selecting one victim after another, within its walls, say even in distant chambers, and never going beyond the hospital, the disease dying out after a time on the law which regulates these strange variations, and returning in the same manner; and this often occurring, again and again, as I have known to be the case, before selecting a case for attack in the district:—These cases frequently traced, as I have done, to the patients of the same practitioner, who was at the moment attending the infected hospital, and thus dying out in the district only to crop up in the hospital again at a short interval. And then let us repeat the question, to which cause, epidemic or contagious and endemic influences, are we to refer hospital metria as most probable? Surely there can be no hesitation, to use Dr. Stokes's own language, that of these two methods of explaining the phenomenon, metria, we should accept that which is most probable, and which, with much deference to him, I submit I have thus, on his own test, established to depend upon endemic and contagious, not epidemic, influences.

But, sir, before leaving the subject of epidemics, the derivation of the word implies that it is a generally prevalent and atmospheric pervading influence, operating physically upon man, animals, and plants. It may be by alteration in the physical properties of the atmosphere simply, as sudden changes from heat to cold, from drought to moisture, or the reverse; or from changes either thus directly or indirectly produced in

its electricity. Under this class of cases may be mentioned as epidemic, influenza, rheumatism, facial paralysis, dropsies, produced by checking the insensible perspiration, &c. It may be from the loading of the atmosphere with a noxious or poisonous exhalation of a vegetable or earthy nature, such as ague or those fevers of tropical climates, pervading certain localities, and from which our soldiers and sailors suffer on too near approach to the coasts. Or it may be from the atmosphere being loaded by animal poisons or exhalations, such as we observe from defective sewage in cities, when typhoid fever, diptharite, and typhus prevail. Or, lastly, it may be a disease requiring contact or approximation of the diseased with the person to be affected, and ascribable to the direct communication of a poison from the one to the other. Whooping-cough, the exanthemata, mumps, furnish us with such examples.

But why do we dwell upon these truisms? Because there is a great scientific distinction and a great practical lesson to be learned from their recapitulation and study. The first group described should be classed as quite distinct from the others, as all pervading, and not necessarily depending for its production upon the contamination of a poison. It is non-preventable, and in its attacks all are susceptible to its influence. Nevertheless, some of the diseases of this class, as influenza, are capable of transmission subsequently by a poisonous influence generated in the person attacked.

The second group described, that produced by noxious vegetable exhalations or poisons, is, in simple ague the form in which we are most familiar with it, a disease that retains the germ of the poison for a long time in the system, lying latent for weeks, months, or longer, and seizing the patient at intervals, apparently capriciously; although the seizures generally recur on exposure to an ague or swampy district. This, however, may be looked upon as a fresh poisoning in a susceptible patient. This class of case is incapable, generally, of communicating its poison to another. Not so the remittent fevers, as recently elucidated by Dr. Bryden and Dr. Farquhar, Surgeon-Major, who point out the importance of distinguishing decidedly between these types. The latter distinguished physician, whose remarks upon crowding demand our deep attention, says, in his report upon hospital accommodation in the Indian army:—

"The next class of fevers to be considered are the remittent and continued, which are very important in regard to their effect on the suffering and efficiency of the army, and on the question of barrack accommodation.

"These, as before noticed, are usually considered to be severe forms of malarious fever, and dealt with, in consequence, as non-contagious; and it is undoubtedly a true observation that a neglected intermittent fever will occasionally pass into a remittent or continued form; as also that exposure in Terai jungle will give rise to similar fevers of a severe form. We see, besides, that with the increase of heat fevers of a severe form are multiplied.

"Dr. Bryden's valuable statistics show, however, very clearly that the suspicions formerly entertained as to the possibility of their occurring in an epidemic form, and behaving as if contagious, are but too well founded.

"It may possibly be that one or more contagious forms of fever are sometimes mixed up with these remittent and continued fevers, and from the similarity of their symptoms are not recognized. . .

"Seeing, then, that these fevers do not agree with intermittents or truly malarious fever in so many other particulars, it is not surprising to find that they obey the further laws of severe fevers in spreading with more or less certainty among bodies of men congregated together in one room or building. It is very distinctly illustrated in the sufferings of our European troops living in barracks in India, among whom 14,495 cases of this fever occurred during the last three years. Also in the suffering of the jail population, who had 6,974 admissions into hospital from this cause during two years. The Sepoys,[a] again, had only 1,875 cases during the last three years. . . . . .

"Besides in some parts of the country, as Rohilcund, intermittents are comparatively very rare, and yet we see the same barracks being built there as at Peshawur or in Bengal. In most or all parts of the country, indeed, the generation of malaria may be controlled or removed by cultivation and drainage. Not so epidemic fever, which will always be specially assisted in spreading by the congregating of numbers together in one room.

"In regard, next, to epidemics of cholera and the influence of the present barrack system on these, it is found that herding men together has an equally bad effect as it has been shown to exert on epidemics of fever. This is peculiarly interesting at this time, when one of the vexed questions is as to the advisability or necessity of moving European troops into camp on the appearance of cholera in their barracks."

In the class of diseases ascribable to foul animal poison, typhoid fever is conspicuous, and this disease is now well known not to be communicable by contagion. Not so typhus; it is purely a contagious disease, generated and developed in crowded and animal-tainted atmospheres, especially in those predisposed to it by debilitating habits. Like erysipelas and metria, it is a disease which may be either communicated to the patient, extending from another contaminated or poisoned patient, directly or indirectly; or it may be, as we have stated in our first paper on metria, generated in the patient herself when exposed to the influences that generate and develop it.

Now, the reasoning resulting from this bird's-eye view of these zymotic diseases amounts to this, that we have had hitherto very vague and indistinct notions of epidemic, endemic, and contagious diseases as to their laws and habits of generation, development, and extension or spread.

[a] The Sepoys reside in isolated huts.

Our want of precision upon them has led to most disastrous results, by causing us to look upon diseases as epidemic, and consequently beyond our control and prevention, when they were due to endemic causes that were easily remediable, or to contagion that had only to be prevented by strict attention to separation of the diseased from the healthy. The light that the investigation of the habits of cholera in its generation in crowded caravans amid the fetid Indian rivers, those of yellow fever in New Orleans, and those of plague at the sepulchre and haunts of the pilgrims in Egypt, has thrown upon the so-called epidemics, has gone a step towards correcting our views on this subject of epidemic and contagious influences—both influences preventable in their character, and which are the root of our enormous mortality and scourging by diseases which belong to their groups. What says an able writer on these zymotic diseases, in a recent number of that excellent popular journal the *Echo*, showing us that the public are becoming alive to these distinctions, although we may shut our eyes to them. In commenting upon Dr. Farr's admirable *résumé* of the causes of death in 1867, he writes:—"The origin of these diseases may be traced to the propagation of certain molecular atoms, not as yet scientifically classed or understood"—(This, I may say, is scarcely a matter of astonishment, when in attempting to investigate this subject by bringing Dumas' views upon isomerism to bear upon them, the bare attempt was ridiculed as beyond our comprehension by a worthy and excellent friend of mine, Dr. Beatty, who, at no distant period, sir, sat in your chair, and at a more recent one graced the chair of the President of this College, but who evidently thinks otherwise.) To return to the *Echo*—"It may be traced," he says, following Dr. Farr, "to certain living molecular atoms, not as yet sufficiently classed or understood, which, entering into man's body, transmute his tissues into their own substance, so that he can no longer live his own life. The process is analogous to fermentation, and each zymotic atom is the little leaven that leavens the whole lump. Hence"—(he continues, in language plain enough, I am sure, but I fear not plain enough to convince an unwilling audience)—"Hence the danger of overcrowding, for the aggregation of numbers within narrow limits facilitates the transmission of the seeds of the disease."

Dr. Stokes admits that if I establish my position of the relation between the number of women delivered under one roof and the mortality, that then he must admit that zymotic disease acquires a great impulse from crowding, and on the same grounds also, that he must believe with me that the parturient woman has in herself a power of generating a poison which infects herself, and which produces a similar process by contagion in her neighbours. Now, I submit I have proved this relation to a demonstration by the enumeration of the 113 years' statistics of the Lying-in Hospital, and disproved its converse, which was unnecessary, as the proof of the affirmative was sufficient, by reference to the most

authentic and reliable sources of information available, notwithstanding the few instances adduced to the contrary by certain of my commentators. But least he require further evidence, I may refer him to Dr. Farr's last report just published, in which he will find in contrast to the grim death-rate, 1 in 31 of the Dublin Lying-in Hospital for the last fifteen years, Dr. Farr gives as the average of the last twenty-one years in England and Wales, the rate of mortality in child-birth as 1 in 200. In the healthier parts of England there are 43 deaths in 10,000 deliveries, or 1 in 232; in Wales the deaths amount to 61 in 10,000, or 1 in 164, which difference is ascribed to what may be called the civilization of the districts, the three last years in England and Wales being 1 in 223. Thus although the general average of twenty years has improved throughout England and Wales by 1 in 23, it has in the last twenty years nearly doubled in the lying-in hospital.

Sir, although Dr. Stokes arrives at the conclusion that self-poisoning in puerperal fever is not proven, there are those who have thought deeply and written well upon this subject who are of a totally different opinion. Dr. Graily Hewitt, for instance, who has written more to the point in 21 pages of a pamphlet upon the disease than any writer I am acquainted with, says:—"It is impossible to escape the conclusion that it consists in nothing more or less than an introduction into the general circulating fluid of a poisonous material of animal origin—that it is a form of pyemia, for the production of which the minutest portion of the morbific agent may prove sufficient." But, sir, he goes further, and agrees with me in the self-poisoning. He says:—"That puerperal fever may occur in a well-marked form, and apart from such introduction from without of morbid material, is undoubted, but in such cases the explanation is virtually the same—the secretions from the surface of the uterus may become fetid, and may also be absorbed, in which case we have the idiopathic disease."

But Dr. Hewitt is not the only physician who has arrived at these conclusions, had we time to dwell upon them.

Dr. Stage, of Copenhagen, after treating of catarrhal affections, and uterine lesions in particular, in connexion with sporadic metria, in which he quotes Winckel, adds:—"The explanation of almost all these cases is, therefore, this, that the individual herself, in some mode or other, develops putrid matters, which are absorbed, and produce an infection—the so-called self-infection, in contrast to an infection from without."

Sir, there are some facts so conclusive to our mind that they do not require proof, and some equally convincing although they do not admit of proof. Let the learned professor but look to the sun descending behind the horizon this evening, and then prove to me that he will ever see it on the morrow; but does he doubt it? Sir, although I draw his recollection to this fact, I am not prepared to say, nor is he perhaps

prepared to deny, that the principle of self-poisoning does not admit of proof conclusive to the mind of any reasoning man. Can he deny it in empyema? can he deny it in syphilis? Then why deny it in the case of metria, which, to my mind, is the most conclusive of all? Let us test it by the simplest rule of evidence, as glanced at by himself. There is one axiom upon which all jurists agree, an axiom which rules the decisions of our judges in dealing with our lives and property, as laid down by Lord Mansfield:—"As mathematical or absolute certainty is seldom to be attained in human affairs, reason and public utility require that judges and all mankind, in forming their opinion of the truth of facts, should be regulated by the superior number of probabilities, on the one side or the other, whether the amount of probabilities be expressed in words or arguments, by figures or numbers."

Again, Phillips says:—"It is of the utmost importance to bear in mind that when a number of independent circumstances point to the same conclusion, the probability of the justness of that conclusion is not the sum of the simple probabilities of those circumstances, but is the compound result of them."

Now, testing the comparison of proof between the probability of poisoning by atmospheric or epidemic influence, with self-poisoning by an inherent generating power in the individual attacked sporadically by metria, when no other case in the district is attacked, who can for one moment hesitate in concluding, after what has preceded on the subject, that, with Mansfield, the superior number of probabilities—and, with Phillips, the compound result of them—go to confirm the self-poisoning; whilst, with Butterworth, the force and effect of the circumstantial evidence depending upon the incompatibility of the explanation by atmospheric influence, or in fact upon any other supposition, reduces anything else to an approximation to an *argumentum ad absurdum*?

Sir, Dr. Stokes, as others of my commentators have done, has availed himself of the opportunity afforded by this discussion of pronouncing a censure upon the Registrar-General's Department. He says, "The reliability of the returns of deaths to the Registrar, as far as puerperal fever is concerned, is worth very little." "I can state that, from long experience. Now, I think it hardly fair to compare these statistics with those of a regular hospital." Such a condemnation as this from a physician of Dr. Stokes' weight and eminence, is no trifling matter, and yet Farr, Wilde, Donnelly, and Burke, are not men likely to have lent themselves to the compilation or circulation of unreliable returns, or statistics that would be worth very little. I am quite aware that in all statistics capable of collection errors must creep in; nay more, that the statistics collected at present are capable of improvement. Dr. Farr himself uses nearly as strong language on this subject as Dr. Stokes. I am also aware that in determining the exact

class of case which should be denominated puerperal fever, a difficulty often arose, but this difficulty applied equally in the hospital returns as those out of hospital. Indeed Dr. Stokes seems to have admitted this when he adds, "It also seems to me that we are very deficient in accurate statistics as to the mortality from puerperal fever in hospitals and private practice." If such be the case I look upon Dr. Stokes' objection to the comparison of the statistics in and out of hospital as untenable, on his own line of reasoning; as the same elements of what he terms unreliablity, and worthlessness extend to both. In adducing this answer to Dr. Stokes' objection, whilst I freely admit that a difficulty did exist in statistics classed under the head of puerperal fever, which applied equally to all statistics, a difficulty which is much remedied by adopting the general nomer of metria, I object by my silence to appear to approve of the language applied to the Registrar-General, and to the wholesale attacks made upon a valuable department of the state, which, as scientific men interested in the acquisition of sound knowledge, to constitute the basis of our reasoning in diseases, it is so much our object to sustain, and our duty to respect.

We have now a matter to deal with in Dr. Stokes' statement, the consideration of which no doubt will go some length in explaining the line of argument he has adopted in this discussion. He utters the following language which, with the weight of his authority, would prove conclusive, were it not that he himself so markedly disclaimed all knowledge of obstetrical practice in his opening observations:—"Now," he says, "If the mortality in private practice, with all these favourable circumstances, be as great or greater, which I believe it to be, than in the lying-in hospitals, what becomes of the argument that the fatality is in proportion to the number congregated under one roof?" Now, I cannot for the life of me dismiss the idea that Dr. Stokes has by some unusual to him obscurity of thought confounded the words greater in degree and greater in quantity, or absolutely greater, and what leads to this conclusion is that in the preceding sentence he says, "I have been repeatedly called in consultation to cases of puerperal fever in the better class of life, and I am sorry to say the result is, that in these forty years I have never seen one case of puerperal fever in private practice that was not fatal; not one."

This general sentence of death, pronounced by Dr. Stokes upon all cases of puerperal fever, evidently astounded the Society, and called for a pointed inquiry from Dr. Murney. I can, therefore, quite understand why Dr. Stokes should have *felt himself* justified in pronouncing a greater mortality in degree, as all *his* cases in private practice were fatal; and greater than this there could not be, and possibly even as *far as his own experience went*, a greater mortality in quantity, as he does not tell us whether he even attended cases of it in hospital. But the

general tenor of his conclusions would lead to a contrary impression. Therefore it is quite possible that *he* might have been justified in drawing the same conclusions from *his own* experience in quantity also. But it remains for the Society to be satisfied or otherwise with this reasoning, more especially when they recollect the annual statistics of mortality for 113 years, the detail of which they have had to submit to as administered at my hands, and Dr. M'Clintock's honest admission of the great mortality in lying-in hospitals.

I therefore submit that if Dr. Stokes has no stronger grounds for refusing his assent to the greater proportional mortality in lying-in hospitals from puerperal fever than the result of his own practice with his reasoning upon it, that I am justified in pronouncing that he has arrived at an impotent conclusion.

I shall not, however, be so ungenerous as to withhold from the Society a fact with which, as our object should be more to arrive at truth than victory, it should be acquainted—namely, that Dr. Stokes does not stand alone in his convictions of the mortality of puerperal fever generally. Sir Charles Mansfield Clarke, whose experience as an obstetric physician was very great, has uttered the same opinion.

Dr. Joseph Clarke writes in March, 1829 :—"In looking over the *Medico-Chirurgical Review* for January, 1829, my attention has been excited by a paper entitled, 'Rapidly Fatal Puerperal Peritonitis. Atmospheric Constitution of 1825-6-7-8. By Doctor Farrell.' Here it is stated, on authority, I presume, not to be doubted, that one practitioner in London lost 7 patients in a few weeks, and another 4 within one month, all by similar disease; and that one consulting practitioner saw 13 cases, 11 of which died. In hospital practice," continues Dr. J. Clarke, "such events do occasionally occur; but in this city (Dublin), I can speak confidently, since the year 1781 they are unknown to us among the upper ranks of society." Sir Charles Mansfield Clarke, in his answer, March 10, 1829, writes :—"But I should not say that my own experience in consultation cases agrees with the statement to which you allude as to the mortality amongst lying-in women in London during the last four years; but as I believe one form of peritonitis is infectious, and as this form admits little in the way of remedy, several such cases may occur in the practice of one man, whilst another in the same district may be entirely exempt from them. Many examples of this kind have come within my knowledge. I wish it were in my power to say that I knew anything about the run of this disease; but every year's experience convinces me of the intractable nature of this complaint, and of the entire inadequacy of any mode of treatment with which I am acquainted in curing it. I do not here speak of a pure acute peritonitis, but of that disorder of which we spoke together when I had the pleasure of meeting you in London."

I have been induced to give these extracts from the correspondence of these great obstetric physicians, not merely as elucidating Sir Charles M. Clarke's views of the fatality of what he considered the true puerperal fever, but from their bearing upon questions of contagion, as well as from Dr. Joseph Clarke's views of the hospital being its habitat, and its spread in private practice evidently having been there traced to contagion not epidemic influences. But the really valuable part of the correspondence, and that to which I wish especially to draw the attention of the Society is, the statement of Dr. Joseph Clarke, who was as well the ablest obstetrical physician of his day, or perhaps of any day, as the physician of the largest consultation practice for nearly sixty years in Dublin, "that between the years 1781 and 1829 he could speak confidently they (puerperal fever cases) are unknown to us among the upper classes of society, although in hospital practice such cases do occasionally occur."

Dr. Stokes asks—"Is simple pregnancy the cause or the source of the development of a malignant poison? Who would believe that?" And continues—"The condition of a parturient woman had been compared to that of a person after a surgical operation. The analogy is by no means perfect." In the one case he says—"The patient is the subject of a natural process, a process which the Almighty intended to be perfectly consistent with health and with life. In the other case, the patient, who is subjected to a great surgical operation, is the subject of an entirely different process. The analogy is good in a mechanical, but it certainly fails in a physiological point of view."

Now, that there is malignant poison developed in pregnant females evidence has been adduced over and over in the course of this discussion, and that it is malignant, Dr. Stokes' recorded opinion of the invariable fatality of puerperal fever is the best proof. That pregnancy, or more properly, parturition, is the cause or the source of it is evident from its being a disease "*sui generis*" only to be met with in pregnant women. As to the analogy between parturition and a surgical operation, denied by Dr. Stokes, I cannot conceive any two processes, not identical, between which a fairer or a stronger analogy could be drawn. The reception which this analogy has met from every speaker in this debate who has alluded to traumatic metria, is remarkable.

Dr. Stokes' denial of this analogy, and his insisting upon labour being "a natural process which the Almighty intended to be perfectly consistent with health and life," is strangely at variance with our finding this eminent and philanthropic physician in the ranks of those who deny the necessity of effecting changes in our great hospitals, which are calculated and directed towards lessening a death-rate occurring in them "amongst those subjects of a natural process, a process which the Almighty intended to be perfectly consistent with health and life," in the

proportion of one out of every thirty-one delivered. Surely if it be a natural process, one out of thirty-one should not die in it; and if such a mortality occur, in what Dr. Stokes denominates and takes so much trouble to prove is a natural process, then it is his duty to detect the cause of this great mortality in a natural process intended by the Almighty to be consistent with life and health, and to remedy it in the manner that experience has already proved it can alone be done.

Had Dr. Stokes ever once witnessed a labour which, unfortunately for the cause of the parturient now at issue, he says he has not, I doubt not he would have detected the analogue he denies, and changed his mind upon the true nature of the process of labour. He would have then felt the full force of those words—"in sorrow and anguish shall thou bring forth"—which appear to have escaped his recollection in reasoning out this question. He would have borne in mind that labours are divided in our school-books into natural, *preternatural*, and tedious. He would have had before him the fact that in the most rapid cases it is often the most dangerous; whilst in the tedious cases it is almost unendurable. That the shock, *physiologically* speaking, inflicted upon the female constitution is quite as great as that produced by a capital operation, whilst the amount of pain is infinitely greater. Again, the delay in the suffering and the amount of pain is such that, were it to continue uninterrupted, and without the intervals of freedom, no female could survive it. If he will add to all this that, in a very considerable proportion of cases, where manual interference is had recourse to—when the labour is too rapid or very tedious—lesions of a serious, sometimes of necessarily fatal nature occur. Had he, in fact, witnessed one labour, I doubt not he would have changed his mind upon the perfectly natural process he makes so light of, as well as of the intentions of the Deity in regard to it.

Dr. Stokes, on the authority of a friend, drew the attention of the Society to a very remarkable statement. Puerperal fever is the endemic fever of Ballarat, prevailing throughout the scattered cottages of the country to a most alarming extent, and, in fact, being the only prevalent fever of that settlement.

If such be the case, then, we must look upon it as unique in its occurrence, and constituting an exception to the known laws which regulate this disease. At present, we should be only justified on Mill's principle applicable to exceptions, first, to make quite sure of the fact, and then to investigate the reasons and causes which are sure to exist, if we can detect them, why in this case there should be such an exception to the generally prevalent laws with regard to it. In illustration of the necessity of the first precaution, we may mention its having been asserted that puerperal fever had not shown itself in Kilkenny for twenty years, or within the memory of any existing man. That Kilkenny has

not at least been free from it, is conclusive, from the answer of Dr. Lawlor, who practised for many years in that city. He has answered my inquiries on this subject to the effect that he and the late Dr. Cane attended a fatal case in private practice, in the year 1847. Under no circumstances should we admit such an exceptional case to influence us in the results to be drawn with regard to the vital question of isolation in our present discussion.

What would the learned Professor or this Society think of my logic and conclusiveness in reasoning, if I told him that there was a district of the Ojibaway country in which the Indian tribes were never free from small-pox, and then argued on this exception that all children attacked with pyrexia in London and Paris, and the other great cities, might be moved at once into the small-pox hospitals with perfect safety, because they were equally liable to have their pyrexia eventuate in small-pox outside as inside the small-pox hospital? Now, I admit, this sounds very absurd, but to my mind not one particle more so than any argument based upon Ballarat.

We may remark, in passing, that although in the case of poisoning by animal and vegetable exhalations, we have generally the advantage of the physical safeguard afforded by the sense of smell to assist us in their detection and removal, yet in the group of diseases generated and communicated by crowding we are generally deprived of this safeguard. All we can usually detect is a close atmosphere and certain exhalations, as in the lying-in hospital wards, that prevail, whether disease be present or not. This want of notice is unfortunate in this most deadly class of disease; for although we have known of cases in which physicians have spoken of experiencing a certain whiff from the breath of a patient in typhus fever, which they felt at the moment was infecting them, yet this is so rare as not to prove generally valuable as a precautionary symptom. On the contrary, both in the typhoid and ague groups of fever the odour is often a great protection. This is especially observable in tropical climates, where these diseases, and particularly the latter, so much prevail. I was so struck with a communication I had from my son, then serving as ensign in the 55th Regiment, of his experiences on this subject in the Bhootan campaign, that I am tempted to give it to you. He says:—"In India, we are pretty well broken to bad smells. A dead elephant, no uncommon nuisance to be encountered in a campaign or forced march, is very bad. I have seen a column go round half a mile to avoid one. But, for a good sustained smell, I know of nothing that beats that of decaying vegetation in the Indian jungle. The first time I experienced this was on the march to Bhootan. We were in the Assam country, just on the edge of the Dewars; the weather was very hot, and at first I thought that the offensive odours resulted from the effects of the heat on the large body of men, so I separated myself from the

column and went off alone, when, instead of abating, the smell grew rather worse, and I began to think I must be carrying it with me; but on examining, I found that the stench—for it was nothing else—arose solely from the masses of decaying vegetable matter that filled the jungle. Within a few days the men began to suffer from fever, and within one month from the date at which I first perceived the smell, out of our officers, 26 *in number, only two others and myself had escaped fever;* and among the men the rate of sickness was 60 *per cent.*"

Mr. President, you have thought it your duty, whilst in occupation of the chair of this Society, to throw yourself into this debate, as you have a perfect right to do; but whilst dealing out criticism as a member of this Society, you must recollect that you have stepped down to the level of your compeers. You, sir, like them, must be satisfied, therefore, to accept such retort to your observations as they seem to require at my hands. Before dealing with your criticism, sir, you will permit me respectfully to refer to certain preliminary observations made by you, in behalf of yourself and the Council, in answer to my remonstrance upon the highly inconvenient, if not objectionable, manner of conducting this debate. I already declared that my object in producing my paper at this Society, in place of simply publishing it, was to secure full discussion. But, sir, "Il est a des fagots et des fagots." Can you for a moment imagine that I ever contemplated that I should have been called upon to sit out—I thank you for the word—the "crambe repetita" objections of fourteen speakers of written treatises, each extending to an unlimited period of time, some to an hour and a half, and one "horresco referrens" to one hour and forty minutes? Can it be supposed that in my most sanguine aspirations for fame, I could ever have anticipated that the rules which bind all societies of limiting the speakers in a debate to reasonable and practicable allowance of time should have been totally disregarded in this instance? And that I should have been martyrized to the extent I have been, and obliged to sit out in silence the same, I shall not call them futile objections or platitudes; but the freedom of debate enables me to denominate them the same erroneous statements, the same unworthy imputations iterated and reiterated, "iterum iterumque," by fourteen learned but verbose doctors.

No, sir, such a trial was beyond human endurance, and I remonstrated, but alas without effect, and I was eventually obliged to discontinue my attendance. If the Council of your Society determined upon adopting such a system of *discussion*, if so you designate the preparation and utterance of fourteen elaborate papers in answer to my one, they should, at least, have informed me of their intention as well as the fourteen learned doctors, and I should have well considered whether I should have submitted myself to such an infliction. Does any gentleman mean to tell me

that he would have gone through these fifteen papers, my own included, in his own study, were he a free will agent, and not caught in such a trap as I was? I therefore maintain, sir, that I was justified in remonstrating, as I did, against the novel system of debate by bill and answer of unlimited length, adopted especially in my case, and that I was not only justified in doing so, but as the founder of this Society, in warning you, that although it may survive one such *irregularity*—that is your own language, sir, not mine, and a very appropriate word it was—although, as I say, it may survive one such *irregularity*, repetitions would prove its destruction.

Sir, I regret to be obliged to state that, exactly upon the same grounds I have felt it my duty to abstain from noticing all those gentlemen[a] who have commented on the few observations made upon treatment in my paper: I feel constrained to abstain from noticing any observation in that branch of your criticisms.

Respect for the Society over which you preside, and regard for yourself, have determined me not to apply the same rule to your other criticisms.

We shall, therefore, with your permission, pass to the second point, as you term it, in which you put the word "pest-houses" into my mouth as applied again to the great lying-in hospitals. Sir, I tell you the word is yours, not mine. And sitting as you do in *quasi* judgment in that chair, it is a serious thing for you to use such language lightly, to whomsoever imputed. Be careful, sir, I beseech you, how you repeat such a phrase. These epithets stick. I am the more induced to offer you this advice, unasked, from your conveying to me that you are still open to conviction, and not a member of that overwhelming "organization" alluded to by the naughty press, "got up to stifle free discussion on a great sanitary question." I admire your defence of the speakers upon one side of this question. It is worthy of all praise, the breaking a lance with the all-powerful press, if done in the right way. But, sir, I confess I think you, the chairman of this Society, whose duty it was to secure free discussion, and to act the even-handed judge upon the occasion, ought, at least, to defend the Society at large, and not merely one side of it engaged in a discussion. And you must excuse me, as a well-wisher to the Society, when I say you have gone further by your defence to convict the Society, or a portion of it, of the charge brought against it, than all the straightforward criticism of the press has done. Sir, it may appear ungracious my being thus explicit upon this subject, more especially when my eye is attracted a little lower in your paper to the graceful words applied to me, in which you conclude your remarks upon the criticism of the press.

Sir, much as I feel obliged and gratified at your good opinion, you

[a] See page 105, par. 3, *infra*.

cannot misunderstand me when I say I decline to accept a compliment, paid even by you, to my self-apparent objects and motives at the expense of my judgment, my discretion, capacity, and duty.

But, sir, let us deal briefly with your reasoning upon my figures. These you call in question by a general denial as regards the Dublin Lying-in Hospital, without any grounds, and by a special denial as regards the Coombe Hospital, of which you have been for twenty-eight years physician, upon the following grounds.

You deny my calculation that 3 out of 4 die in the Coombe Lying-in Hospital "that should not," or in other words, from "preventable causes," and say certainly my calculation cannot be based on the return made to me of the Coombe Hospital for the last seven years; and you add that the tabular statement subsequently furnished by Dr. Kidd in no wise corroborates it. Now, with Dr. Kidd's or any subsequent returns or with how they may vary or how affect the death-rates I have nothing to do. I have only to deal with the authentic paper upon which I based my calculations, furnished me by the Registrar of the Coombe Hospital. This places the death-rate of the Coombe Hospital for the last 7 years at 1 in 72·25. Now, sir, the mean of the three small hospitals with which the Coombe was compared, gave a death-rate of 1 in 282¾. Four times 72 make 282, therefore if 1 in 282¾ was taken as the basis of our calculation of the average proportion of inevitable deaths in labour, we were justified in treating this great sanitary question, to state that 3 out of 4 of the deaths that occurred in the Coombe were unnecessary, in other words, if only one constituted the necessary or inevitable proportion of deaths *cæteris paribus*, that the 3 others were in excess evitable and consequently unnecessary, and if this be so, you, sir, by a parity of reasoning were not justified in your special denial.

You will, sir, I trust, excuse my declining to take in detail your explanations or ideas showing why the deaths should be taken, in the case of the Coombe, out of the general category, or admitting that your cases, many of which, in all likelihood, died of metria, should be classed under different heads, and not metria. The forceps may be used, version may be necessary, that dire necessity—craniotomy, or cephalotripsy may occur, and yet it is but too familiar to us to know that all these cases may die of metria. Thereupon the three pages in which you have amplified upon the distinctions of the state that led, possibly to metria, in its different modifications, is not germane to our discussion. In entering upon this inquiry I distinctly disclaimed attempting to base my calculations upon the numbers of cases of metria. I found the difficulties of arriving at what was metria so great that I determined to rest upon the general mortality as the only true basis of calculation. It is a very simple matter for you, sir, or any other inquirer, to arrive at your

own conclusions upon the proportions of metria and accidents in childbirth, but as it has nothing to do with the calculation at issue, I must simply reiterate that Dr. Ringland has failed to shake my statement, based on the authority of his own registrar's return of the Coombe death-rate for the seven years furnished to me, which leaves me no alternative but to arrive at the conclusion that 3 out of 4 evitable deaths occur in the Coombe Lying-in Hospital.

With Dr. Ringland's third point I quite agree. He says, and saves me much trouble by saying manfully, when dealing with the alteration of our hospitals, to prevent contagion—"I believe the true prevention is isolation." Had he stopped here I would have accepted his opinion as conclusive and valuable, but unfortunately he adds—"instant and complete isolation of the infected from the healthy." That is, he is satisfied that the true prevention is isolation, but he would not insist upon this until probably the mischief was done. But he corroborates this opinion by begging the question as to the time contagion develops itself, which he has fixed at some convenient season after the attack that admits of the removal of the patient; yet, on the other hand, you have Dr. M'Clintock and others assuring you that the attack sets in at any period, and spreads at the earliest period after, or almost simultaneous with, its own commencement; and you have me, as well as Dr. Stokes and others, telling you that it occurs before delivery.

What becomes, then, of the statement that "contagion does not exist in the incipience of the attack?" Sir, it does not require to prove even this, as the fact is, in the cases where the disease shows itself, it don't require immediate contact. The poison pervades the hospital. The hospital is its habitat, and be the patient removed or not the taint spreads, when it generates, haunts, and destroys. You, Mr. President, give us one crumb of comfort in the debate. You, like Dr. Kidd, announce, and speak with confidence of a plan to be adopted in the Coombe Hospital, in future, of keeping an isolated building, entered by a separate stair, for metria patients, to be removed to when attacked. I accept this announcement as that of a step in the right direction. But I could have wished you had been able to announce a step farther, and that isolation in its true sense had been determined upon in your new structure.

I may mention before passing from this subject, that a very valuable testimony is afforded on the subject of isolation in a report furnished by Dr. Graily Hewitt, of the British Lying-in Hospital, in which he remarks:—"During the early part of the year 1862, and before I had taken, in conjunction with Dr. Murray, charge of the in-patients of the hospital, several patients were the subject of puerperal fever, and four of them died; two others were discharged in a very weak state; and the hospital was for a time closed. As soon after taking charge of the

in-patients as circumstances admitted, steps were taken by Dr. Murray and myself to isolate the patients to a greater extent than before. The number of beds in each ward was reduced from six to four, and the communication between the wards as much as possible cut off."

Let us now see what was the result of the partial approximation to isolation, for Dr. Hewitt does not lead us to suppose it was quite perfect. The mortality in the four years previously, in which time there were 428 deliveries, amounted to 7, that is, to 1 in 61; whereas in the next four years there were 10 deaths in 761 deliveries, which was only 1 in 75, and this although the number of deliveries was nearly doubled. This calculation I look upon, therefore, as extremely valuable, as showing the advantage to be gained by even an approximation to isolation, whilst it also shows the check to the benefit that would in all likelihood have been derived from it had not the crowding on admission of an increased number of patients counteracted the advantage of the attempt at isolation. It is thus very valuable in showing, that partial and not complete isolation will only disappoint our expectations of preventing the disease.

I feel convinced, sir, that you and this Society will agree that whilst I have left no argument or counter-statement adduced by any of my commentators unnoticed, I have occupied sufficiently your time in dealing with individual criticism, and that you will be prepared, at this stage of our discussion, to expect me to deal with the subject generally. We shall therefore ascend *de particulare ad generalem*. In doing this I should mention that I attempted to draw out a tabulated calculation of the opposition and support given by the members who have spoken on the debate, and I find, as nearly as I can calculate, that in dealing with my thirteen propositions, 16 speakers expressed 76 opinions. That would arrive at very nearly an average of 5 opinions each proposition. Of these 53 supported the propositions and 23 opposed them, or supported their converse. But this gives us little information of how far they supported my views, or what views exactly they supported and what they opposed. I am sure I shall be excused in making this analysis when I say, although many of my critics attacked me and the proposed changes I advocated, most of them were fain to admit the basis upon which my arguments were fixed, whilst they objected to my conclusions and remedies for grievances as patent as the sun at noonday.

We shall give one example of this which will apply equally to several other of my propositions. Ten gentlemen freely admitted metria as contagious. But, sir, it must result from the simplest process of reasoning that every gentleman who has admitted the contagious nature of metria, in however small a degree, must, as a matter of course, and, as a consequence of the admission, agree in the truth of twelve of my thirteen propositions, including the three conclusions resulting from them. Let us just test them *seriatem*.

Proposition No. 6.—It is contagious consequently.	Proposition No. 1.—It is due to dissemination of poison.
It is contagious consequently.	Proposition No. 2.—It is absorbed into the system either of the generator or of another parturient exposed to its influence.
It is contagious consequently.	Proposition No. 3.—The generation or spread must be in proportion to the number of parturients exposed to the poison.
It is contagious consequently.	Proposition No. 4.—In lying-in hospitals where large numbers of patients are delivered under the same roof (if it exists) it *must* find its habitat; and we may add prove destructive in proportion.
It is contagious consequently.	Proposition No. 6.—It follows in steps of certain practitioners, whilst others are totally free from it in the same locality.
It is contagious consequently.	Proposition No. 7.—It is endemic, or occurs in the locality where the contagion prevails.
It is contagious consequently.	Proposition No. 8.—It is not only confined to a given hospital, but is observed to haunt certain wards of that hospital in which it has its special habitat.
It is contagious consequently.	Proposition No. 9.—It occurs comparatively rarely among women confined in their own homes, where they are only exposed to the risk of self-contamination or other zymotic diseases which it is observed to succeed.
It is contagious consequently.	Proposition No. 10.—It is not observed to prevail in small lying-in hospitals, or cottages where one or two patients cohabit, and the elements of its generation and spread don't exist, or only to a slight degree.
It is contagious consequently.	Proposition No. 11.—We are causing the death by metria of a number of patients by continuing the gregarious system of large lying-in hospitals, who would escape under a system of segregation or isolation.
It is contagious consequently.	Proposition No. 12. — If lying-in hospitals are to be continued for the cure of patients and as schools of instruction, which they undoubtedly ought to be, they can only be so continued by substituting isolated cottages or pavilion hospitals with one or two beds in each isolated room.
It is contagious consequently.	Proposition No. 13.—With our present knowledge the conclusion is inevitable, that the mortality among parturient women would be greatly lessened by an alteration in construction and an arrangement of lying-in hospitals which would secure segregation and prevent congregation.

But, sir, the contagionists must go much further. They must use their reason, their experience, and their common sense, in testing the value of any apparently adverse statistics—I mean statistics that appear at variance with the principles that apply in all other contagious

diseases; and if they find that any collation or coaptation of figures eventuates in bringing out results at variance with reason, experience, and common sense, and with those laws that apply immutably in all diseases of the zymotic and contagious types, then they must "hold fast by sound words," and reject the figures as either exceptions which prove the rule, the explanation of which they are ignorant, or fallacies beyond their detection. Such are the figures, from what quarter soever they may come, that would go to prove that contagious diseases will not spread, *cæteris paribus*, in a direct ratio to the number exposed to the contagious influence. This, supplementing the effect of crowded houses in producing disease, is, in fact, the gist of the whole question lying at the root of this discussion. If a disease be contagious it can only spread by exposing a susceptible party to the influence of the contagion. If two be exposed the risk is doubled; four, quadrupled; and so on in an arithmetical ratio. Exactly in the same ratio is the risk in the spread of metria in the larger in comparison to the smaller cottage hospitals, arguments, elaborate treatises, and statistics *notwithstanding*.

Most of my commentators have started on a wrong basis in this discussion.

They have dealt with me as an antagonist in a matter in which there can be, or at least there ought to be, no antagonism.

We are here as a scientific body, discussing an abstract sanitary question.

I affirm that there is a preventable loss of human life from a particular disease.

I suggest the method of lowering this mortality, and how am I met?

By a denial that the mortality exists.

I go into my proofs, and my facts are not disproved.

But a standard of mortality is set up different from that selected by me.

The invariable rule hitherto with sanitarians, whose ostensible motive is to improve the death-rates, is to place the highest standard before them, and direct their efforts to accomplish this.

The rule adopted by my commentators has been to select the lowest standard, and to level down to this.

What should we think of the physicians of Newcastle-upon-Tyne if, in answer to Dr. Farr's strictures upon the unhealthiness of their town, and his remonstrances upon their high death-rate, 37 in 1,000, they, in place of directing their attention to ascertain the causes and removal of their unenviable notoriety, denied its existence, and then set to work to prove their case by a comparison with the mortality of Manchester, which is equally notorious?—not Hull, in which the mortality amounts to only 23 in 1,000.

Yet this is exactly what has been done by my commentators, who

have questioned my statistics, and adduced counter statistics showing a higher death-rate in bar of correcting the unnecessary death-rate that I have pointed out. A large proportion of this debate has been directed to this futile, this irrelevant object. Our attention has been misdirected. The "*tu quoque*" is no justification. The "I am no worse than my neighbour" is no excuse.

A grievous mortality exists in these great hospitals. I care not *where else* it exists if I can prove to you that it does not exist *elsewhere;* if I can show you that human life is preserved in the proportion of 2 to 1 or 20 to 1 in a different place by a different plan from that adopted in the hospitals or places where the mortality exists; and that such a plan is available and practicable, or even not impossible in these also, my case is made. Now, sir, I have proved all this, and nothing remains to my opponents but to withdraw an antagonism that has already, I regret to say it, attracted to this discussion an unenviable notoriety. I have proved, sir, not only that the high rate of mortality from metria exists in our hospital, but that it has done so with little intermission from its foundation to the present day. What says a resolution of the governors on the subject, passed on the 17th June, 1815, fifty-three years ago:—"A crowded state of the wards appears to the committee rather incompatible with the health of the patients, as under such circumstances the hospital will thereby remain liable to a recurrence of *that infectious and fatal fever which has on several occasions been so severely felt.*"

What says the first authority of the present day upon zymotic diseases, Dr. Farr, of the Registrar-General's department, in his Report on Causes of Death in 1867, fifty years later, on this subject:—"The mere aggregation of people together in close apartments *generates* or *diffuses* the zymotic matter. Thus, *place lying-in women in close proximity* to each other, or mix them up with the patients of a general hospital, and they *died of puerperal fever*. Place many wounded men in a ward where cleanliness is neglected, and erysipelas, pyemia, gangrene spring up; imprison men within narrow walls, or crowd them in rooms, and typhus breaks out. The general and special hospitals of the country have been until quite recently erected without any special reference to the dangers accruing from the assemblage of great masses of sick people within the walls of one building, so that the efforts of the most skilful medical officers are frequently defeated." Again, Dr. Farr says—"The mere *accumulation* of masses of living people within narrow limits either *generates* or *insures* the diffusion of epidemic disease." Again, page 220—"The suppression of the generating beds of disease in unhealthy populations can scarcely fail to be efficacious. To suppress plague, suppress the wretched sanitary condition of Egypt; to suppress yellow fever, go to St. Thomas, New Orleans, and its other feeding grounds; to put a stop to epidemic outbreaks of cholera, cleanse the waters of India, and improve the condition

of the population; to extinguish enteric fever and typhus in our cities, extinguish the rookeries." And he might have added, to lessen the mortality by metria, if not to "get rid of the generating beds of this disease," reform the lying-in hospitals.

But to return to the attempts made to involve me in a discussion upon a collection of statistical tables, in which this higher standard of death-rates is dwelt upon as that to be aimed at, and with which we are called upon to rest satisfied. I once for all tell my commentators, that I shall fall into no such palpable trap; nor be led away from the true issue by any such—excuse the word—clap-trap.

I, therefore, disregard, with the utmost self-complacency, all their broad-sheets and narrow-sheets, teeming with figures and quantities, with a high death-rate, as the aim for us to attain. Nay, I will, if it be any comfort to them for treating the ponderous result of their labours cavalierly, I will admit them all *in globo*, and that without examination, as not germane to the question at issue, and only calculated to draw us aside from our great object, "how to lessen the mortality from metria." I want not to be reconciled to it; on the contrary, my object in coming before this Society, amongst whom I see so many of my alumni, is that of the Carthegenian of old, I would urge them to swear with me eternal hostility to our common enemy, metria, and, if possible, unite the Knights Hospitallers, who have so strangely mistaken their antagonist in this discussion, to make common cause with me in our efforts at victory.

Sir, I may say generally that my commentators in this controversy have supported every one of my positions, although they may have cavilled at the extent to which I have carried them. When I say cavil, I mean that they have taken objections on points, and failed to grapple with the broad principles. They have attempted to draw the attention of the Society off from the discussion of a great question, boldly, and, I admit, as my friend Dr. Beatty has said, startlingly, propounded, by involving it in a mass of what we shall term discrepancies of detail. They imagine that by showing a want of harmony and exact correspondence in all the collections of statistics they could scrape together, that they can break down the notorious facts which I arrayed before this assembly on introducing this subject—an array of facts, proverbial, and confirmed by the history of the great institutions of Europe, from their foundation to the present hour. Can they deny my tables in which the mortality in Paris, St. Petersburgh, Vienna, Dublin, London, York, and Glasgow Hospitals are given, and the combined mortality of which is 1 in 44; whilst the death-rate ranges even so high as 1 in 18½ in Paris? Can they deny the average death-rate of the Dublin Lying-in Hospital, standing for the last 15 years at 31½? Can they deny that for four years of Dr. Collins's mastership, it stood at 1 in 186, showing what the

true death-rate is—accidents of labour and diseases included, metria excluded? Can they deny that the death-rate of the South Union Workhouse Cottage Lying-in Hospitals had only amounted during the last four years to 1 in 238, and that no metria showed itself there during that time? Do not they themselves admit that the death-rate of the North Dublin Union Cottage Hospital has only amounted to 1 in 213½ for the same twelve years that the average Dublin Lying-in Hospital death-rate has been 1 in 30½? Can they deny the report of the death-rates as given in Drs. Thomas and John Beatty's private practice, as furnished by Dr. Thomas Beatty himself, as 1 in 256? But, above all, can they deny, or, rather, have they not failed deplorably in their attempt to disprove, the average mortality of the three cottage hospitals, Waterford, Limerick, and New Ross (to which we may now add Killarney), as standing at 1 in 282?

Sir, they cannot deny. They cannot vitiate. They cannot shake in the slightest degree one of these reliable and unanswerable bases of the reasoning upon which my conclusions are established.[a] My conclusions, my deductions, and my propositions are the necessary sequence of their establishment. Sir, I am certain, and maintain without the fear of contradiction, that the effect of the ordeal my paper has undergone has been its entire confirmation.

As to the discrepant statistics adduced by them—their very discrepancy may be said to go a long way in proving the accuracy of mine. The laws of evidence have long established the fact, that want of exact harmony or correspondence in different testifiers is esteemed rather a corroboration than a denial of truth—a principle that is adduced as well by our ablest theologists in proof of the inspiration of Holy Writ, as by our petty sessions attorneys in the establishment of an alibi. But, sir, it was a primary axiom in my days of pupilage, that "one affirmative proof overturned a host of negative evidence." If the combination of philosophers who have been dealing for months with my propositions cannot reverse this axiom, then I submit that my propositions, and especially that one which has been so much cavilled at—the proportion of deaths to numbers and crowding—has been proved, aye, over and over again, by every great Lying-in Hospital in Europe. Why, sir, my first impression on hearing it objected to was one of what I might describe

[a] The above Reply, confirmatory of my original Paper, was uttered on the 10th of July, on which occasion, without reason assigned, Dr. Johnston, the present Master of the Dublin Lying-in Hospital, absented himself; and yet, on the 17th of the same month, in the Irish Times newspaper, he uses the following language in reference to my Reply:—"He (Dr. Kennedy) commenced by attempting to prove his assertions by statistics, but on being shown the incorrectness and unreliability of these—the basis upon which he grounded his argument—he was obliged to acknowledge their utter worthlessness."

provoked amusement, that any man could be found so innocent, or so obstinate, as to deny such a self-evident proposition. It was in this spirit, I confess, that I, upon the third night of our sederunt, called the attention of the gentleman, who has the merit of first making this great discovery, to the case of Dr. Labatt's great mortality,—the very case to which he referred as corroborating the reverse. His mortality I repeat—1 in 63 in 1818—the year of the greatest number ever in the hospital. I showed him that the next year the habitat was complete, and the mortality rose to 1 in 33, although the deliveries were reduced 342; that it continued at 1 in 35 the year after, when the deliveries were again raised; and it gradually wore itself out in the course of the next two years, when the crowding was again permitted, and that in 1823 it again rose to 1 in 44.

I was in the hopes, that this statement of figures would have set the fallacy of confounding cause and effect, in this case at rest for ever. I imagined in my simplicity that ascribing the greater rate of mortality to the diminished number of patients, in place of the diminished number of patients to the greater mortality, was at an end, and that so preposterous a proposition should not be repeated. I have, therefore, to remind you that this step was taken to relieve the hospital from continuing to be the habitat of the poison. I am fully aware sir, that I am repeating myself, but I am doing so designedly, because, to my astonishment, the fallacy has been repeated, and dwelt upon again and again in this discussion, notwithstanding my exposure of it. Dr. M'Clintock repeated it; Dr. Beatty repeated it; and a host of other doctors, too numerous to mention, repeated it, and nothing is left to me but to repeat the refutation of it.

I purposely avoided dwelling much upon treatment in my former paper, my object being to grapple with the true nature, causes, and means of prevention of metria. I thought, however, that the profession might have reason to condemn me if, in a treatise of this nature, I had entirely omitted to allude to it. My reticence in this respect has not relieved me from the usual fate of "mine enemy who writes a book." My treatment has, in fact, been attacked as unmeasurably as my other views. I admit it is equally open to candid criticism, if this had been extended to it; but when the reverse has been the case, as it has in every instance in which my treatment has been called in question, I shall give these attacks but one general answer, namely, that when garbled extracts are taken and misrepresentations, proved by the context, put forward, I deem it both unnecessary and unbecoming to notice them, and I feel called upon to omit all mention of the critic and his objections—a practice that I also adopt when commentators outstep the fair bounds of criticism and descend to personalities.

By this simple plan the crime carries its punishment along with it, whilst temper and offensive personalities are prevented displaying themselves on my part, in what I esteem as a purely scientific inquiry, and

one into which personal feeling should not be permitted for one moment to enter.

Sir, if another night had been added to this debate, it is not impossible that the conclusion might have been arrived at by some latent essayists of the Knights Hospitaller ranks—some of the more courageous and *advanced* advocates of this *retrograde* movement in the nineteenth century—that the true cure and prevention of metria would be best accomplished by crowding all our parturient populations into large hospitals, and that for this purpose not a moment should be lost in enlarging our existing lying-in institutions, and extending them upon the present principles, which have been found so satisfactory.

Some general objections have been raised to remodelling our hospitals, based upon such grounds as cost, expense, disinclination to change, architectural beauty, and soforth. They should be left as they are. The Deity has ordained that people should die epidemically; for his own wise purposes epidemics have always existed. Now, sir, I shall not insult your understanding by combating such objections. Most of them were used over and over again against Jenner; and although the establishment of vaccination was retarded by them, it was not prevented. Could we now calculate the amount of human life and human misery saved by the introduction of vaccination, and of that loss and misery caused by the objectors' opposition to it, this calculation might be turned to good account in the present discussion.

Already has this discussion produced good fruit. Attendance at the patients' homes has been encouraged by the authorities of the Dublin Lying-in Hospital. The strong and universal opinion, I may say, pronounced upon the contagious nature of the disease, and the attention that has been more especially called to its spread by means of the indiscriminate mixture of the male pupils with the patients after attending autopsies, has, I understand, elicited an order precluding this practice, and thus for so far our common object has been gained—namely, the taking steps to endeavour to lessen the mortality by metria and other zymotic diseases. As I doubt not these precautionary measures will extend to other diseases communicable by contagion, the knight's service rendered by several of my critics upon this point reconciles me to much of their shortcomings in other branches of this inquiry. And if we applied the same principles of preventing the spread of contagion to the other equally important and equally obvious means by which contagion is necessarily spread through crowding together patients similarly affected, under a common roof, there would virtually be no question at issue between us. We should be, on many points, of the same opinion, although still, I regret to say, on one or two essentials, "far as the poles asunder."

I might have appealed to the benevolence of this Society, their love

of science, and desire for improvement, as a motive for acquiescence in my views, and urged their adoption with arguments and persuasions thus adduced. But, sir, in a severe inquiry such as this, I should by such a line only load my case "with superfluous armour, that encumbers rather than defends." I might tell them that communications had reached me from various quarters, such as that I now quote, in which Dr. Handyside, the distinguished surgeon for so many years to the Edinburgh Royal Infirmary, writes:—"I take this opportunity to congratulate you on the work as to wholesome village hospital wards instead of large maternities, in which, from the reports of the journals, I see you are engaged. Your experience and position ought to carry conviction, and it surprises me much to see so many of your profession opposed to the proposal. So far as my experience as hospital surgeon led me to see, we must, in the end, be led to give up the large hospital buildings.—Edinburgh, June 10, 1869."

But I shall not detain you with these from the result arrived at on the discussion of this subject in the Surgical Society of Paris, in which Trêlat, Le Fort, and Jardier were the principal speakers.—See the *Gazette des Hôpitaux*, No. 67, 1866.

The following conclusions were arrived at:—

1. It has been proved by statistics that the ravages of puerperal fever in lying-in hospitals are greater now than formerly. This can only be referred to the hospital atmosphere; therefore, the infirmaries and hospitals should be reduced in extent, and assistance provided for the poor at their own houses.

2. Puerperal fever is infectious; and, therefore, hospitals conducted on the best principles may become the seat of great calamities.

3. Besides the usual sanitary measures which are recommended for hospitals, special precautions should be observed in lying-in institutions.

4. To avoid importation of disease, strict cleanliness should be observed. Empty wards should be thoroughly cleaned, not only the walls white-washed, but the beds purified, &c.

5. To avoid the spread of the disease the healthy should be removed from the ward, where any have been attacked, to small rooms for one, or at most four beds.

6. The attacked should be removed to a separate building.

7. If, nevertheless, the remaining females get the fever, the whole building must be emptied.

8. In cities where lying-in hospitals cannot be dispensed with, they must be small.

Since the above conclusions were arrived at by the ablest of our Parisian confreres, as we are informed by Oppert, "The administration in Paris have had the wards divided by double glass partitions; but, recently, they have gone farther, and recommended single bed-rooms;"

and, still more recently, M. Le Fort has, we are informed, been trying a plan of canvas or tent hospitals.

This is striking at the root of the evil; and we trust that they will speedily go farther still, and have chambers or huts totally isolated, and devoid of all closed communications capable of sustaining and communicating a poisoned atmosphere from the sick to the healthy. Anything short of this must prove inefficient, and end only in disappointment.

You see then, sir, whilst we are discussing this question, it is deciding itself. Nay, sir, it has decided itself; and decided itself, gentlemen, not as should have been the case, *with* the strenuous assistance of those members of the obstetrical branch of the profession who have ranged themselves to oppose my feeble efforts (to the knife), as they say themselves, but in the teeth of their resistance. Sir, the enlightened press of England, of Ireland, and of Scotland, has decided this question. It is an accomplished fact. Let the recusants hasten to join in our efforts, or they will rest under the inglorious conviction that they have wedded themselves not merely to an unsuccessful cause, but to one that is being stamped with the just reprehension of the medical and general press throughout this country. The House of Commons is discussing the merits of this question. Hear what the able member for Finsbury, Mr. W. T. Torrens, says, in his speech on the Poor Law Bill, June 7, 1869:—

"Did the results of scientific inquiry, or of practical observation, tend to encourage perseverance in the old way of aggregation? Was it true or was it the reverse of truth, that the accumulation of large numbers, under the same roof, who were suffering from accident or disease was the best method of promoting their recovery? This was a very serious question; and it was one which he would not have felt himself justified even in raising had not the voice of warning reached him from all sides, and had he not been satisfied, by comparing the testimony of the ablest and the best of men, that a more excellent way of relief for the sick and suffering lay in their dispersion, and as far as it was possible in their separate treatment in their own homes."

Again, the press—that great organ that sits in judgment upon men's reasoning, their motives, and their actions, and draws its conclusions for the universal benefit—what does it say, gentlemen, upon the merits of this discussion? Do not be alarmed. I am not going to accumulate and reproduce the numerous articles that have dealt with it. I shall merely give you the brief view of one of the ablest of the London journals, the *Echo*, upon your own case in its article headed "The Harvest of Death, June 10, 1869." "It is worthy of note," says the *Echo*, "that the idea of maternity hospitals, which were once so popular, *has been exploded by scientific* experience. It has been found that such institutions are merely

nurseries for puerperal fever." Now, gentlemen, what say you to this? The press of England have arrived, without the assistance and in the teeth of your opposition, at this conclusion, that maternity hospitals are the nurseries or habitat of puerperal fever. But it is not done yet. Take to heart, I beseech you, the next paragraph of the *Echo*, and mend your hand in time. This, mark me, is the *Echo's* language, not mine. But here it is:—"The best hope of mitigating the danger under this cause (that is the cause of continuing the maternity hospitals as nurseries for puerperal fever) is derived from the gradual but steady increase of medical knowledge, and the extinction of that time-honoured institution personified in 'Sairy Gamp.'" But now for the "keenest cut of all." "A new race of Gamps, intelligent and instructed, is one of the greatest social needs of our time."

Sir, in contrast to this view of the *Echo*, I cannot deny you the quotation of a private letter bearing upon the responsibility that rests with our profession in using their best endeavours in the prevention, as they do in the cure of disease. It was an acknowledgment written to a medical friend on receipt of his sanitary pamphlet lately published.

"MY DEAR,—Many thanks for your pamphlet. I am glad to see that you feel a physician's duty extends beyond the cure of disease, and that the public have a right to look to him as well for its prevention. If our profession recognized and acted on this principle, generally, it would exercise a position, and earn an influence in the state commensurate with its true value. But until it does so we must not be astonished that they are treated as cyphers, not to say mere craftsmen by the State; when they rest satisfied with doing mere craftsmen's—I was going to say tinker's—work. I am glad to see, however, that you are an exception to this criticism.—Yours faithfully."

This, I think, requires no comment.

It would be amusing to reproduce some of the absurd accusations of imputed evil intentions with which I have been charged in this discussion. Mr. Sandham Symes, for instance, deals with the subject of architecture. At first I imagined his communication a hoax; but no, it is in too serious a strain. He seems really anxious to protect the ornamental buildings of the city, and like my friend Dr. Churchill, deprecates my proceedings in so precipitous a manner in a matter in which he graphically adds, "there has been as yet but a mere trifle of experience." But of what, think you, does he accuse me? Of nothing short of proposing to remove[a] the beautiful structure, the Dublin Lying-in Hospital, with its world-famed Rotunda, bodily, and replace it by cottage hospitals

[a] See page 355, Dub. Med. Quarterly for August, 1869.

to be built upon its site, one of the worst sites that could be selected for such a purpose. Why, sir, if made, such a proposal would justify my residence in an hospital raised by the eccentric Dean in another part of our city. This gentleman might, at least, have read my paper[a] before presuming to criticise it, in which I distinctly recommended the converting the present hospital into an institution for diseases of females (with the power of admitting children), and suggested the high ground, the most elevated part of the Rotunda Gardens, as a most suitable site for the erection of isolated cottage hospitals for the labour patients.

Another charge, or rather an insinuated imputation, was made even more absurd in its nature, in a few excited words, that said little but implied a great deal, by Dr. Sinclair, on the third night's discussion.

This gentleman introduced the name of the Prime Minister, with a suggestive insinuation that he was going to deal destructively with the hospitals of this city. No doubt, sir, in admitting that Mr. Gladstone's acquirements are as varied, and his information as extensive as conceivable, yet I may affirm, without assuming any knowledge of state secrets, or committing myself in their divulgence, that he knows just about as much of this discussion or its objects as of that important question that engrossed the attention of the three tailors of Tooley-street. Sir, I believe in my heart Mr. Gladstone's aspirations, wishes, and intentions are, as all may know who will be at the pains to inquire, to benefit the poor of this country in every manner that his enlarged mind, great experience, and devotion to sanitary improvements so peculiarly befit him to do. Sanitarians, at least, must agree that fortunately for Ireland the power, the knowledge, and the will to exercise them in the right manner, are combined in one man, at this moment, and that man is one who has taken the improvement of the sick and impotent in this country seriously to heart. Fortunately for us, as Irishmen and physicians, the practically benevolent pursuits to which Mr. Gladstone and his family have always been devoted have well prepared him to deal with the improvement of the medical charities of this country.

And still more fortunate for us in Ireland, the manner in which his efforts have been so ably seconded by the partner of his cares (a title nobly earned by her whose exertions to benefit the London charities have rendered the name of Gladstone, as that of Nightingale, a household word with the benevolent). Sir, the medical men and citizens of Dublin have a right to know the fact, as this name has been so invidiously, and, I may add, so injudiciously introduced into a purely scientific discussion: that the same anxiety to improve and extend our Dublin charities that has so well succeeded in London, prevails in that benevolent, large-hearted

[a] See page 45, *supra;* also Appendix F, Letter to his Excellency the President, Guardians, and Governors of the Dublin Lying-in Hospital, dated October 28, 1867.

mind, and unless frustrated in its exertions to accomplish its object, I doubt not the same success will attend them.

If, after the experiences, the facts, and the reasonings, and the reliable statistics that have been produced, there be still amongst my hearers and readers more than one man who denies that our large lying-in hospitals cannot, and ought not, to be improved in their rates of mortality, and that, even upon the showing of those gentlemen who have taken an adverse line in this discussion, I should like to see him and hear him say so boldly. If there be not, then there is at least one point upon which every man in this Society, and, I might add, every man who knows anything of the subject, out of it, must agree, that is, that a necessity exists for lessening the death-rates in lying-in hospitals. The means of doing that is entirely another question, and one on which there may be as many different opinions as there are individuals present. Taking it for granted, then, that a necessity exists for an improvement, it may be further affirmed that every one of the recusants who carried, as one of them pugnaciously remarked, "war to the knife" against my inquiry—the hospital physicians and their congeners who admitted so many of my propositions, and confirmed the real basis of my arguments—and the general listeners, including the sons of Korah, who have exhibited such patience and zeal in sitting out the nine nights' discussion, I say, it may be affirmed that every one of my hearers and readers who has arrived at the above conclusion, and who as far as in him lies, does not endeavour, from this moment, to correct it, is assisting in sustaining that death-rate. Do not deceive yourselves, gentlemen. He is not merely in the position of an accessory after the fact; he is from this day out an accessory before the fact. And what is the fact? It is the taking away of life—precious life, a gift that none of us can restore.

The law recognizes two descriptions of responsibility—that by commission and that by omission; and culpability attaches as well to the last as to the first. I now say, and say with deliberation, that every one of us who possesses the power, by act, word, or deed, of preventing this unnecessary loss of life, and who exercises his influence against that prevention, or omits to exercise it for it, is morally in one or other of these positions.

Following, sir, the example of my commentators, I might have been perfectly justified in a discussion of this nature in turning to account the artifices of oratory. Like them, I might have attempted pathos and perhaps accomplished travesty.

I might have appealed to them to use their own language, not mine, not to give over "these poor women to die in the proportion of 3 to 1 that ought to die in their hospitals." I might have besought them as parents, as husbands, to throw their shield over those doomed to become motherless and bereaved.

I might have attempted to arouse their feelings by depicting the mother led in tears from her humble but happy home to the hospital to endure the pangs of labour, her family cheered with the confident hope of a speedy release in health and strength—a hope, alas! how bitterly to be disappointed!!!

I might harass them by attempting to depict that death-bed scene, and its consequences, with which I am but too familiar.

The strong, hopeful woman, but yesterday, grasped by the unrelenting hand of death, in a silent, lonely, strange chamber, separated from her friends, and tended by the hand of the hireling stranger, unsoothed, unsolaced by those tender attentions of her family, that deprive even death itself of half its terrors.

I might have changed the scene and depicted the shrouded victim stretched on the table of the ghastly charnel-house, until her husband and surviving relatives attend to convey the remains of her they loved, and parted with so hopefully a few hours before, to her last resting place.

But who could attempt to calculate or depict the consequences of this bereavement to that sorrowing family? A father obliged to neglect his motherless children in the discharge of his daily duty. The children deprived of their guide—now Arabs on the street, swelling the huge catalogue of crime. The males, perhaps, winding up as felons—the females, as worse.

But, sir, neither my inclinations nor my respect for my audience approve of such a course, in a strictly practical inquiry, "*non tali auxilio nec defensoribus istis tempus eget.*"

Nay, I might have remonstrated, with perfect justice, with those gentlemen who oppose this attempt at lessening the mortality by an improved arrangement of our lying-in institutions, and begged them not to rest satisfied that 1 death in 45 deliveries proves to us that matters are "tending to render the hospital as healthy as one could almost desire."

I shall, however, appeal to them in the language of the patriarch, pleading for the cities of the plain, and say:—Peradventure we might find a way of saving 20 more women in every thousand. Shall we not do it? Or even if, peradventure, we might save 10 more in every thousand—let us do it. Or to the recusants who object to the expense or inconvenience, with a comparatively small improvement, resulting to the hospitals, such as the modified improvements in St. Petersburgh, Vienna, and Paris, we might add—Peradventure we might even find a way of saving 5 lives out of every thousand—let us do it.

It is doubtful whether those gentlemen, my critics, whom I have not specially answered may not be more dissatisfied with my breach than my observance of this mark of respect. Should such be the case, I can only apologize to them in the words of Napoleon I. in bidding farewell to his

army on leaving France for Elba:—"Soldiers, would that I could embrace you all! As this is impossible, I salute your generals."

Sir, it remains for me now merely to express my apologies for the imperfect manner in which I have executed this great, this painful duty, and to thank you for the patience with which you have borne with me.

Nine nights have been devoted to this important investigation; the amount of mortality in our great hospitals has been brought to light, and the means of preventing this have been elucidated. It rests with us now to apply our knowledge—and that without delay—for, in the words of the American poet—

"Man-like is it to fall into sin,
Fiend-like is it to dwell therein."

APPENDICES.

APPENDIX A.

Dr. Playfair's Letter.

"5, Curzon-street, Mayfair, W.,
"*August*, 1869.

"My Dear Sir,—The plan I proposed as a substitute (which I trust may yet be carried out) would have cost but little more. I propose having five lodgings in separate houses, in the immediate vicinity of the hospital—each consisting of two rooms, and each to contain two lying-in women and one pupil-nurse. The lodgings will be ordinary rooms, kept nice and clean, and ought to be got for about £1 a month each—so that the entire extra expense would be about £60 a year.

"The house physician would visit the rooms twice a day, and the head midwife and sister as often as necessary, the former attending every labour, as in the ward. Of course each room would have the simplest possible furniture, and there could be no difficulty in changing them as often as might be deemed advisable.

"In this manner, I think, we could train our nurses even better than before, as giving them more practical work, and also have a valuable addition to our school; and, indeed, such a scheme might be adopted without much difficulty in connexion with every hospital where there is a medical school.

"I am, dear Sir,
"Sincerely yours,
"To "W. S. Playfair.
"Dr. Evory Kennedy, Dublin."

APPENDIX B.

[Lest it should be objected that I have omitted any part of what was even ascribed to me during this discussion, I here reprint the imperfect and inaccurate account of what I am reported to have said on the first night of the debate.]

Dr. Kennedy said the president had been good enough to ask him to give a *résumé* of his paper, but he thought that unnecessary, as it

had now been for two nights under discussion, and the paper itself was in the hands of the members. There was, however, one point in which he wished to set himself right before the discussion was proceeded with. In the statistics which he furnished, he gave those of the Vienna Hospitals only up to 1838—the only statistics which were then available to him; but, on looking over the *Quarterly Review*, he found that Professor Faye, of Copenhagen, mentioned that there had been an improvement in that hospital, and he thought it his duty to lay that statement before the Society; it was simply a hearsay statement. He gave no statistics, but merely stated that it was one per cent. If he had known this before, he (Dr. Kennedy) would have stated it, but he now gave it for as much as it was worth.

[Again, the following specimen of unintelligible jargon[a] has been ascribed to me. Is it possible that my readers should require my disclaimer of it?—E. K.]

Dr. Kennedy thought he ought to be allowed to say a word in reply to what has been put forward; but, indeed, he had been left very little to do. Fortunately there had been such a discrepancy of opinion amongst the gentlemen who had spoken since his paper was read, that he thought they had answered each other pretty well. There were, however, a few points which require a little elucidation, and with these he would occupy their attention for a few moments. In the first place Dr. Johnston mentioned that he said that there was a necessary recurrence of puerperal fever after any epidemic disease showed in the hospital. He (Dr. Kennedy) merely stated that it had been observed to occur. He used no such language as had been attributed to him. He had stated distinctly that it was of a capricious character, and if they wanted any proof, the opinion of the gentlemen who had spoken had left little doubt upon this. It was often quite impossible to arrive at its cause any more than that of any other zymotic disease. If he asked Sir Dominic Corrigan to explain, on all occasions, why fever should appear, he would smile; and if he turned to any other gentleman there and asked him why a certain zymotic disease should appear at a given time and place, he would be equally amused; and he, therefore, did not feel called upon to explain this. All he had done was to endeavour to throw some light upon it, by tracing, as far as he could, after an experience of 40 years, certain predispositions to it. One gentleman said he ought to consider it a little longer before forming an opinion. Why he had been considering it since before he was born—he had been 40 years thinking of it. Many an anxious night it had cost him, when he had been obliged to visit his patients every six hours, and from that time to this he had never lost sight of it. To ask him at this

[a] See Report of Proceedings of Dublin Obstetrical Society, D. Q. Jour., p. 253.

period of his life to sit down to consider it a little longer was, he thought, a little unreasonable (laughter). The gentleman who next addressed the meeting spoke of sporadic puerperal fever, and expected him to explain the whole phenomena of self-poisoning. He thought he went very near it, as near it as man could go, without having the thing before him, but he could not explain the whole phenomena. He got a schooling from another gentleman because he did not allude to the effect of examinations in producing a disease that was contagious. Well, he thought it did not require a philosopher to explain to this gentleman that the disease that he specified as contagious would be contagious to the patient if applied by the finger. He thought the question recoiled a little upon the speaker himself. This same caustic gentleman had dealt very strongly with him upon another point. He had his own little book there, and in order that he might not misrepresent him, although he mended his hand since he wrote it, he would give the quotation :—

"I cannot admit that the mere accident of a number of lying-in women under the same roof of itself gives origin to puerperal fever, though when once it appears under such circumstances it spreads. The deaths bear no relation to the number of women admitted."—*Extern Maternities*, by Loombe Atthill, M.D.

Then he instances Dr. Labatt's celebrated period, commencing in 1815 ; omits all mention of the opening the auxiliary hospital, and boasts of this period being conclusive on the point that deaths bear no relation to the number of patients admitted. Now let us look at the tables. In the year 1816 the deliveries were 3,276 with 1 death in 182. In 1817 the deliveries were increased by 200 ; the death-rate rose to 1 in 108 ; the deliveries were increased from 3,473 to 3,539. In 1818 the death-rate rose to 1 in 63, and although the deliveries were reduced next year to 3,197, the habitat was confirmed.

And what occurred after that? Although they lowered the admissions to 3,100 next year, there was 1 in 33. What he wished to convey in his paper was, that this mysterious disease had been on the increase, and particularly for the last fifteen or twenty years, especially for the last fifteen, and this was what drew him out at this moment, and what made him urge upon his enlightened, intelligent, brother professional men the necessity for meeting the urgency of the case. For fifteen years the death-rate had never fallen lower than 1 in 31½, and in one year it was so high as 1 in 14. Was it not time to raise his voice before he was swept away from amongst them? He had got very little more time in which to do it, but he had got upon the housetops. He did not mean to say that the number of patients was as great in the Lying-in Hospital ; but he meant to say that the disease had now got firm hold, and that it would not be removed unless some steps

were taken, and he knew of none more effective than the one he had suggested. He had only suggested what statistics and his own observation proved, and he wished to draw the attention, first of the governors of this hospital, and secondly, of the profession, when he failed with the governors. They were all very well aware of the efficacy of statistics at that moment, and of the rage there was for collecting them. He had been obliged to use them, though he did not rely upon any that he had not a perfect knowledge of. His calculations were never arrived at from the statistics he collected, but from observation—the result of close, protracted observation, extending over years, and growing with his growth and age. There was a marked difference between observation and statistics. Observation was the result of facts observed by a man himself, of which he had an inherent cognizance that could not be displaced from his mind. What were statistics? Talleyrand said language was made to disguise ideas, and he might have extended the remark to figures. He might have said "figures are made to disguise quantities," and with perfect justice. What was a figure? It was a symbol of quantity. There was nothing in a figure more than in a word. One could tell lies with figures as well as with words, and bolder ones. It was very important to know this and to bear it in mind. He thought there were three kinds of statistics. Some of those present might recollect in reading those clever writings of Dickens' that one of his heroes, when he is asked if he likes sausages, answers, "Like sausages?—that depends very much if I know the girl that makes them" (laughter). That was his feeling about statistics. His value of statistics depended very much on whether he knew who made them. There was a friend of his who had arrived at certain conclusions from statistics, and he changed his mind, but not on the principal of the young man who got sausages from the girl he knew. There were also what he called kaleidoscope statistics. A gentleman put in his figures, there was a process of *reflection*, and he turned round the instrument, and each time a new form was produced, and they were just worth what they found them. Then there was another form—the Babbagean. Babbage made an instrument, into which he could put a figure, and amongst a million it would come out at the end of 50 years at the right spot, when he wanted it. Statistics were valuable if they were honest, and fairly put forward, and reliable. One of the speakers had said that he had drawn a comparison between the small provincial hospitals and the mortality in the maternities. He never drew a comparison with any maternity, because he had no confidence in their statistics, and he purposely avoided drawing a comparison with anything he had not confidence in. He hoped anything he had left unanswered had been met by the gentlemen on the other side, for, in the words of an eminent judge, "I agree with the two doctors for the reasons assigned by the other two who differ from them (laughter)."

The meeting then adjourned.

APPENDIX C.

"South Dublin Union,
"Clerk's Office, Board-room, James's-street,
"*26th day of June*, 1869.

"Dear Sir,—I have carefully examined the birth register, and the register of midwifery cases, for the years 1865–6–7–8, and beg to give you the following return :—

Year.	No. of Births.	No. of Deaths of Mothers.
1865,	124	Nil.
1866,	116	Nil.
1867,	114	2
1868,	122	Nil.
Total,	476	2

"Faithfully yours,
"George Hepburn, *Clerk of the Union.*

"Evory Kennedy, Esq., M.D."

APPENDIX D.

Detailed Report of Mortality in the Dublin Lying-in Hospital, year by year, from its foundation to 1868, *proving it the habitat of metria, and that puerperal fever is now endemic in it, and also shewing the relation existing between the mortality and crowding.*

The present Dublin Lying-in Hospital was opened in 1757. In 1760 the deliveries were increased from 406 to 556; the mortality, which was in this year 1 in 139, rose in the next year to 1 in 52. The deliveries were increased from 533 to 681 in 1766, when the death-rate stood at 1 in 227, and the death-rate rose in the next year to 1 in 60, and the year after, with the deliveries at 655, it was 1 in 41. The deliveries were increased to 1 in 704 in the year 1772, when the death-rate was 1 in 176, and the next year the mortality was 1 in 52, and the year after 1 in 32. The deliveries rose in 1778 to 927, and the same year the death-rate rose from 1 in 119 to 1 in 92. In 1780 the deliveries were diminished by 100, as compared to the preceding year, and the mortality fell from 1 in 126 to 1 in 183. In 1783 the deliveries were increased from 990, when the mortality was 1 in 165, to 1,167, and the death-rate rose to 1 in 77. In 1786, 1787, the deliveries stood at 1,351 and 1,347; the mortality at 1 in 170 and 1 in 134. In the following year the deliveries increased to 1,469; the death-rate to 1 in 64. In 1790 the deliveries were again

increased to 1,546, when the death-rate was 1 in 124; and in 1791 the deliveries were still farther increased to 1,602, when the death-rate mounted to 1 in 64. But the curious feature in the relation of crowding and mortality was that the hospital seemed for a time to acquire a power of accommodating itself to the increase of numbers. Human life being compatible after a time with a state of things that at first seemed destructive to it. Whether this occurred as referable to the law that we know exists in most epidemic and contagious diseases of wearing out, it is difficult to say; but by some such law this scourge has been tempered to the destitute resorting to this institution; otherwise, had the disease gone on increasing with every addition to the numbers without intervals of suspension, I need not say the doors of the hospital would long since have been closed for want of victims. The next year, 1792, was an illustration of this law, as in this year, although the deliveries were increased by 29 to 1,631, the mortality fell to 1 in 163. The cumulation principle, however, soon evinced itself, and what between this and the increase of deliveries to 1,757, in 1793, the death-rate rose to 1 in 92; and, although the precaution was taken of lowering the deliveries by 214, or to 1,543, in 1794, the death-rate continued high, and even rose to 1 in 77. The deliveries were still further lowered in 1795, and the mortality fell to 1 in 214. The deliveries were again allowed to rise in 1796, and the deaths rose to 1 in 152. Again they were allowed to rise in 1797, and the death-rate rose to 1 in 131. In 1798 they were lowered to 1,604, and the death-rate fell to 1 in 200. Still the cumulative tendency must have been in operation, but occult, for although the deliveries were lessened to 1,537 in the next year from the house becoming unhealthy, the mortality gradually increased to 1 in 153; and although the deliveries were still further lowered to 1,337, in 1800, the outburst was upon them, and the mortality rose in this year to 1 in 74. Whether it was that the pressure for admission was not to be refused, or that the master became hopeless of checking the mortality by checking the admissions, I know not, but the admissions were permitted to increase, and with their increase increased the mortality.

In 1801 the admissions increased to 1,725, 389 upon the previous year; the mortality to 68. In the next year the admissions were 1,985, the mortality 1 in 74; and in 1803 the admissions amounted to 2,028, the mortality to 1 in 46—the highest rate, save one year, it had then reached from the foundation of the hospital, and the greatest number of patients up to that time admitted to the institution in one year. In 1804 the patients were reduced 113 in number, the death-rate falls to from 1 in 46 to 1 in 120; this gives the master confidence, and the admissions are increased again to 2,220; the effect of this is not felt, however, in that year, the death-rates falling to 1 in 185, which was the average rate of Dr. Collins's four healthy years; but in the next year, when the

admissions were increased by 184, the death-rate rose to 1 in 104. We have now two years in which the principle of wearing out or accommodation of the hospital to the crowding come into operation; and that, although the deliveries were increased by 105, and still further by 154 the next year, the death-rate standing at 1 in 209 in 1807 and at 285 in 1808. The admissions rose 224 in 1798, the death-rate increased to 1 in 137, and increased to 1 in 98 in the next year, when the admissions were 2,854. The admissions were consequently lowered by 290 in 1811, and the mortality lessened to 1 in 106. In 1812, despite the experience of the effect of the lowering of the admissions upon the rate of mortality, the deliveries were increased by 205, or to 2,766. The consequence that might have been anticipated occurred, the death-rate rose to 1 in 64, the habitat was confirmed; and although the deliveries were decreased by 282, the death-rate rose to 1 in 40, the highest death-rate ever attained up to that time in the hospital.

In the course of the year 1814 the deliveries were within 24 of the previous year, and the death-rate was 1 in 100. In 1815 a change tantamount to diminishing the number of admissions occurred; a large brick building, capable of containing 30 beds, previously occupied as a blind institution, was obtained and added to the institution. This building stood at a little distance from the main building, and was detached or only connected by an open piazza. It thus afforded not only beds for the accommodation of a considerably larger number of patients; but it enabled more separation of the existing number, and a longer interval for the wards to remain unoccupied between each take-in of patients, as each successive relay of patients admitted to a purified ward is technically denominated. The effect of this told for a time, but I regret to add only for a time, and that a very short time. In 1815, when the auxiliary was opened, the deliveries were increased 575; the death-rate fell to 1 in 180—within five of Dr. Collins's four healthy years' average. The deliveries increased 201 the next year; the death-rate stood at 182. In 1817 the deliveries were increased to 3,473; but the bow was strained too tight, the death-rate rose to 1 in 108, Notwithstanding this hint the admissions were still further increased by 66, or to 3,539—the greatest number of admissions ever known in the institution—and, as might have been anticipated, the death-rate rose to 1 in 63. The habitat was again formed; and, notwithstanding the lowering the admissions by 342, the rates still rose until they reached 1 in 33. The authorities, shocked at the death-rate still continuing and reaching a figure that it had never before approximated, determined to act with more decision; they refused the admissions and lowered the deliveries in 1820 by 739, or to 2,458 in the two hospitals. The habitat continued, however, and the death-rate only lowered to 1 in 35; and, as the disease wore out, the authorities again increased the admissions,

which were permitted to reach 2,839 in the year 1821, with a death-rate of 1 in 129. The admissions were reduced in 1822, and the mortality fell to 1 in 220. But this was only a momentary flicker, to be followed by a long period of darkness in the history of the institution; for although the deliveries were still further reduced by 96 in 1823, the death-rate rose to 1 in 44.

They were again reduced by 138 in the year 1824, when the death-rate fell to 122. This encouraged to increased admissions; they were increased 300 in 1825. The natural consequence resulted; the death-rate rose to 1 in 106 that year, and to 1 in 30 in 1826, although the admissions were reduced by 106. The habitat was again fully established. The admissions did not rise above 2,550, 106 over the last year the mortality was 1 in 77. The admissions were increased in 1828 by 306; the death-rate rises to 1 in 66. Dr. Collins, who was now in charge of the institution, with that practical, sound sense, which he so largely possessed, saw that the crowding was at the root of the evil. He obtained the sanction of the governors to reducing and keeping reduced the admissions. He took every precaution that strict attention to cleanliness, purification, and ventilation could afford; but he did the most vital of all things in his case, he reduced his admissions by 715 in the year 1829, when his mortality reached 1 in 63; and in the next year, 1830, the mortality fell from 1 in 63 to 1 in 190. From that year he never allowed his deliveries to range higher than 2,288 for the next four years, and in those four years his average mortality amounts, as we all so well know, to 1 in 186.

We now enter upon what I may term a new phase in the history of the Lying-in Hospital. The disease, which had at intervals disappeared, becomes constant—converted, in fact, from an epidemic to an endemic, established and confirmed in its habitat. From the year 1833 to the present day the mortality has never, on any one occasion, fallen to the healthy scale of Dr. Collins's four years to which we have so often alluded; and only on one occasion—the year 1852—has it come so low as to allow the hospital to be classed, on the principle of calculation we have adopted, as tolerably free from puerperal fever.

We are consequently perfectly justified in saying that metria is now endemic, and possesses its habitat in the Dublin Lying-in Hospital.

In my former paper, it will be recollected, concluding that the true statistics of the hospital were so notorious, I rested satisfied with giving an approximate calculation, and drew the contrast of the healthy and unhealthy years by simply referring to those years, the death-rate of which amounted to 2 figures and those that reached 3; but as this simple plan has not satisfied my commentators, it has become my painful duty to analyse the whole table, and satisfy this Society and the professional public, by a detailed annual calculation, of the truth of the

conclusions arrived at. I was prepared to have my conclusions confirmed, but I confess I was not prepared to find that the more detailed analysis would so thoroughly discomfit and put to shame the hardy contradictions of my criticisers, or that it should go so far beyond the statements I had made upon the bird's-eye view of the table of mortality.

If these observations apply to the portion of the table up to 1833, they apply much more strongly to the portion since, as 6 of the 7 years that we were disposed to admit as healthy, from their exhibiting 3 figures in the mortality table, on the closest scrutiny, called for by my commentators, must be excluded from the healthy category, and only one of the remaining six—1852—be allowed to fall within the range of 30 below Dr. Collins's four years average death-rate.

Having satisfied ourselves of these facts, and further, that the death-rate has for the last 35 years amounted to 1 in 69, and for the last 15 years to 1 in 31½, we are fully justified in pronouncing it as a fact that metria is now a stationary disease in the institution, and requires our consideration to be directed to it at present, not merely as an epidemic appearing and disappearing at intervals, capable of being expelled, and the hospital restored to a healthy state as formerly by reducing the rates of admission to a minimum, but requiring a decided and effectual application of our knowledge of its now altered laws and character. Notwithstanding that metria was now endemically confirmed, the mere reduction of admissions did, I should mention, continue to exercise an influence in the reduction of the mortality in certain years of the 35, of this changed period of the epidemic to an endemic disease. The years 1843 and 1844, the two years in which the greatest number of patients were delivered for the last 35 years, were followed by a severe attack of metria. The death rose from 1 in 155 to 1 in 40. The admissions were lowered from 2,179 to 1,411, *i.e.*, 765, and the following year the mortality fell to 1 in 119, or 200 per cent. The deliveries were, however, increased again to 2,025, *i.e.*, 614, and in 1847 the death-rate rose again to 1 in 36. The death-rate kept to 1 in 52 and 1 in 54 for the next three years, the admissions being 1,816 and 2,063, and the disease mitigated in 1850, the death-rate falling to 1 in 132, with 1,980 deliveries, and to 1 in 148, with 2,070 deliveries. 1852 also was fated to put an end to our hopes as to a return of health to the institution. This was our culminating year; the admissions were continued high, amounting to 1,963, and the death-rate only 1 in 178 — the nearest approach, of late years, to Collins's successful period. In 1853 the rate of deliveries were sustained at 1,906; but the dire disease was upon us, and the mortality rose to 1 in 118, or 35 per cent. The admissions were increased in the following year to 1,943; the mortality rose to 1 in 53, or more than cent. per cent.; and although the admissions

were lowered the next year to 1,060, or nearly 200, the mortality rose to 1 in 30. It lessened the next year to 1 in 64, with 1,600 deliveries; but the increase again told, and the deaths were 1 in 46, with 1,509 admissions. This rise in the deaths was again followed by a diminution of admissions to 1,084, or 435. But the disease, in its most aggravated form, continued, and the death-rate stood at 1 in 36. Notwithstanding, the deliveries were increased to 1,389 in 1859, and 1,404 in 1860. In those two years the death-rates were 1 in 66 and 1 in 54. The effect of this increase, however, told, even on the then high rate of mortality, and in the year 1861 the death-rate rose to 1 in 19, although the admissions were reduced to 1,135 in that year; and the death-rate rose to 1 in 14 in the year 1862, although the deliveries were reduced 335, or to 800. This year was that of our greatest mortality, and the smallest number of admissions since 1776. It is a matter of astonishment that the gentlemen who adduced Dr. Labatt's greatest year of admissions—1817—with 3,539, and a mortality of 1 in 73, did not also adduce 1863, with its only 800 deliveries and 1 in 14 deaths, as a conclusive argument in favour of their views of retaining their present system of large lying-in hospitals. The reasoning in the one case would have been just as convincing as our analysis of the true cause of the mortality has proved it to be in the other, and establishes our 4th and 5th propositions fully; whilst it proves them guilty of the charge brought against them, of confounding cause and effect.

To continue our analysis, however. The lowering the admissions to 800 told, and in the year 1863 1,228 patients were delivered, and the death-rate lessened to 1 in 38. In 1864 the deliveries were 43 less; the mortality was 1 in 46. In 1865 the deliveries again rose to 1,333, and the mortality, which was 1 in 44, rose the next year to 1 in 27, although the deliveries were decreased 258, or to 1,074. The deliveries were increased to 1,145 in 1867, and the deaths, which were that year 1 in 39, rose to 1 in 27 in 1868, although the deliveries were lowered to 1,022.

APPENDIX E.

[I beg to acknowledge my obligations to Dr. Grimshaw, Physician to the Cork-street Fever Hospital, our Irish authority upon the system of curvilinear test, who has been good enough to accede to my request to apply this test to the tables and statistics upon which my conclusions have been based, and also to investigate the relations between the recorded zymotics and puerperal fever as above dwelt upon. The accompanying diagram has been prepared by him, and he furnishes me

with the following report upon the matters submitted to his investigation.]

Dr. Grimshaw's Report.

Dear Sir,—The accompanying diagram consists of a sheet ruled into squares (by what I shall call guiding lines). Along the upper margin of this ruled space runs a row of figures corresponding with the years to which the statistics contained in the table apply, namely, from 1757 to 1868 inclusive. Under each date runs a column of squares, the horizontal guiding lines of which correspond in number and position with the figures on the scale placed along the left hand margin of the diagram. This scale runs from 100 at the top by alternate figures (two being allowed to each square to avoid confusion) to zero at the bottom.

Two curves run through the diagram from left to right; one represented by a continuous line shows the variations in the number of deliveries; the other represented by an interrupted line, the death-rate in the Dublin Lying-in Hospital during the periods indicated by the dates along the top of the diagram. The scales are as follows:—for the admissions, the curve is so constructed that if it rose to the level of 100 (to the top of the ruled space) it should represent 4,000 deliveries per annum; the highest really reached was 3,539 in the year 1818. It will thus be seen that the space between any two horizontal lines corresponds with 80 deliveries. For the death-rate, the curve is so constructed that if it rose to a level with 100 it should represent a death-rate of 100 per 1,000, or 1 in 10; the highest really reached being 71·4 per 1,000, or 1 in 14 in the year 1862. Each space of the diagram, therefore, corresponds with 2 deaths.

A horizontal line, passing 6·5 of the scale, parallel with the ordinary guiding lines, represents a mortality of 1 in 156 (6·49 per 1,000 deliveries). This number (1 in 156) is selected by you upon the following grounds:—You consider Dr. Collins's death-rate of 1 in 186, (between lines L and N of the diagram) for the four years referred to you in your reply, as representing a really healthy state of the hospital; but lest this should be considered too strict a rule of health, you have the rate of 1 in 186 (by subtracting from it one-sixth) to a rate of 1 in 156, and grant this as a tolerably healthy state of the hospital. I shall call this line (without assuming that it represents an absolutely healthy state of the hospital) "the fair-rate line." Any part of the interrupted curved line lying above this "fair-rate line," shows the certain presence of metria; any part below shows the possible absence of that disease, and therefore a tolerably healthy state of the hospital.

I now proceed in accordance with your wish to consider how far it illustrates the truth of the propositions you have advanced in your paper.

The propositions which the diagrams alone can illustrate are those you have numbered 3, 4, and 5.

1st. Proposition 3.—"That the generation and absorption of this contagion is in a direct proportion to the number of parturient females cohabiting at the parturient period."

From this it necessarily follows that the larger the number of deliveries in the hospital, the greater the number of cases of metria, and, therefore, the greater the number of deaths. This, you state, is shown by a rise in the death-rate, almost always accompanying, or immediately following, an increase in the number of deliveries in the hospital, and *vice versa*.

On looking at the diagram, your statement is at once forcibly confirmed, the exceptions being very insignificant; it is unnecessary to point out the examples illustrating this point, as they are evident to any one inspecting the diagram, and have been sufficiently and correctly pointed out in your paper. The apparently notable exception of the year 1815 (line G) and following years, when the accommodation was increased, is well illustrated in the diagram, where we find the cases diverging instead of following one another. The best example of a great fall in deliveries, accompanied by a rise, although followed by a fall in the death-rate, is seen in the year 1845 (line Q), when a rate of mortality but seldom exceeded is accompanied by a fall in deliveries—this fall being, I believe, due to an intentional reduction in the number of deliveries, with a view of diminishing the puerperal epidemic. This fall in deliveries is (in accordance with your rule) followed by a decrease in the death-rate for the year 1846. A similar state of affairs arose in the years 1861 and 1862, when a rise in death-rate (apparently produced by the rise in deliveries of the preceding years, 1859 and 1860) is followed after a special reduction in deliveries by the most considerable fall in the death-rate curve shown on the diagram.

The most notable real exception is seen where the curves cut the column for the year 1813 (line F), where a decided fall in admissions is accompanied by the highest rate of mortality (but one) known up to that period. The habitat had been confirmed by further increasing the number of deliveries in 1812, when the mortality stood at 1 in 64—the mortality appears, consequently, to have increased in 1813, but even here a fall in death-rate occurs the next year.

From the year 1834 (line O) up to the present time, the diagram shows that metria has established itself endemically in the hospital, as although we frequently find the death-rate curve rising or falling as the deliveries increase or diminish, yet only twice did the hospital temporarily recover its healthy condition, once in the year 1844, and again it was nearly healthy from 1850 to 1853 inclusive. From the year 1854 to the present time, a much less close relation than formerly appears to exist between the death-rate and number of deliveries. The hospital

appears saturated by the metria poison, and the disease is spread by an ever present and violent contagion.

2nd. Proposition 4.—"That in lying-in hospitals, where large numbers of patients are delivered under the same roof, this disease finds its habitat, appearing and reappearing at uncertain intervals."

This is well shown in the diagram, and well pointed out in your paper. The rarity with which metria disappears, and for how short periods, is well shown by the small portion of the death-rate curve which lies below the "fair-rate line."

Report on Co-existence of Metria and other Zymotic Diseases.

3rd. Proposition 5.—"That its appearance, although apparently capricious, is not infrequently traceable to the occurrence of other zymotic diseases."

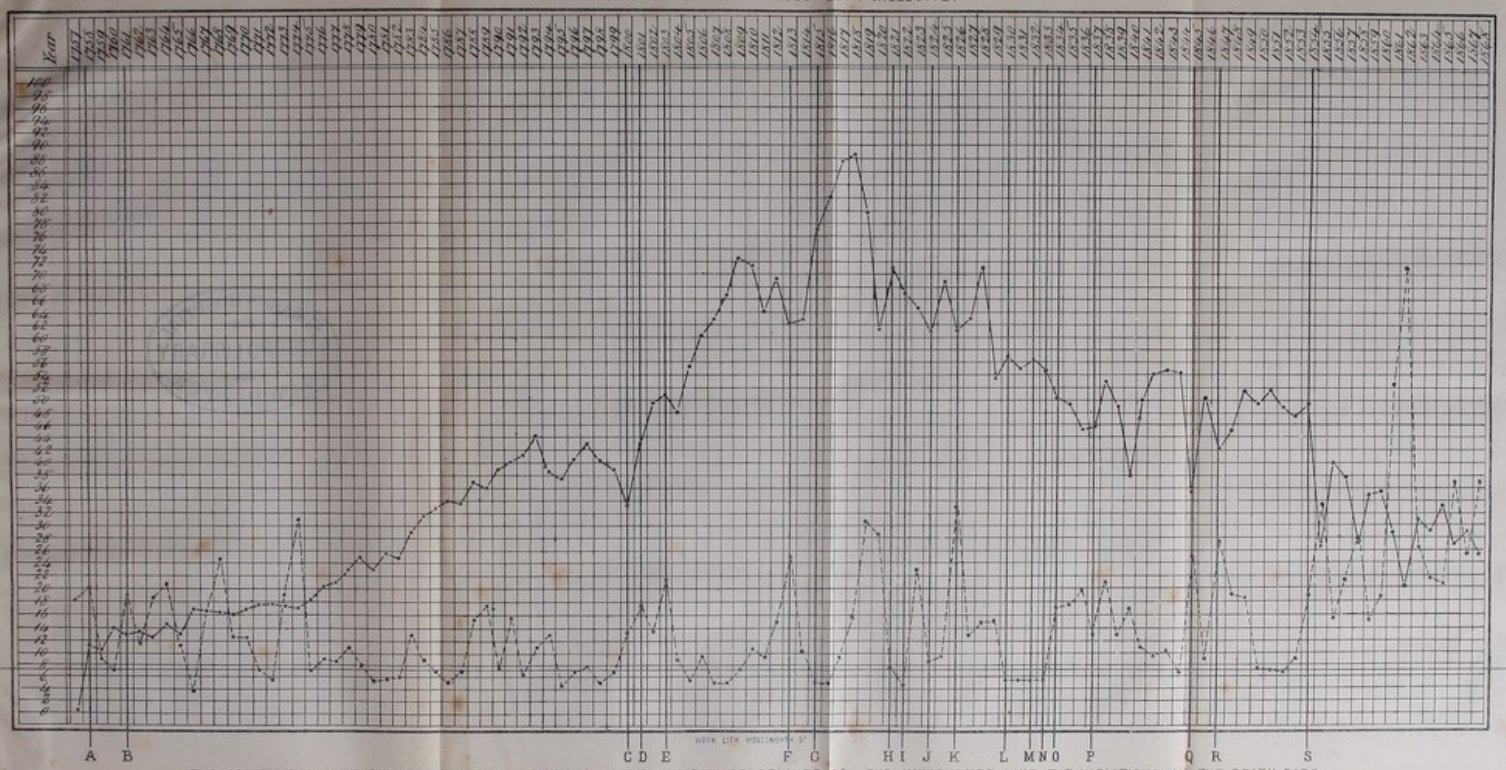
I have carefully investigated the relation between a high rate of mortality in the Lying-in Hospital and the occurrence of epidemics during the period over which your observations extend. It is chiefly through the able report on Irish epidemics, compiled by Sir W. Wilde for the Irish Census Report of 1851, that I have obtained my information with regard to the epidemics of the last century. For the present century I have relied chiefly on the Reports of the Cork-street Fever Hospital. I have investigated the relation to metria of typhus, erysipelas, cholera, scarlatina, influenza, small-pox, ague, dysentery.

Typhus.—I find that this is the only disease which bears anything like a constant relation to puerperal fever. Thus, of 24 well-marked typhus years, 17 were noted for high mortality in the Lying-in Hospital, the most remarkable of these were 1758 (line A), 1761 (line B), 1800 (line C), 1801 (line D), 1813 (line F), 1824 (line J), (a year of great mortality from typhus when but slight puerperal prevailed), 1826 (line K), in which the greatest typhus epidemic on record prevailed, and was accompanied by the greatest puerperal epidemic known in the hospital up to that time; the hospital mortality for that year was 1 in 30, or 33·3 per 1,000 deliveries—a rate of mortality which has been exceeded but seven times since. In 1845 (line Q) a similar correspondence is found. In 1847 (line R) another great rise in the death-rate accompanied the famine fever of that year. Exceptions are found on several occasions, the most remarkable of which are the years 1821 (line H), 1822 (line I), 1824 (line J), 1837 (line P).

Erysipelas.—The records of this disease are too meagre to enable us to draw any positive conclusions, but its connexion of late years with metria is too well known to require proof.

Scarlatina.—I have been able to have twelve distinctly recorded scarlatina years. Of these I find six associated with increased metria—namely, 1763, 1800, 1819, 1843, 1845, and 1866; in the years 1800

DIAGRAM SHEWING THE RELATION BETWEEN THE NUMBER OF DELIVERIES AND DEATH RATE IN THE DUBLIN LYING-IN-HOSPITAL FROM THE YEAR 1757 TO THE YEAR 1868 BOTH INCLUSIVE.



THE CONTINUOUS LINE REPRESENTS THE VARIATIONS IN THE NUMBER OF DELIVERIES, THE INTERUPTED LINE THE VARIATIONS IN THE DEATH RATE.

and 1845 typhus was also present, and in the year 1843 the rise in puerperal mortality was slight. Six scarlatina years were unaccompanied by any rise in the metria death-rate—indeed in the years 1798, 1831, and 1833, the death-rate curve is below the fair-rate line.

Yours faithfully,

T. W. GRIMSHAW.

MOLESWORTH-STREET, DUBLIN,
August, 1869.

APPENDIX F.

Letter to His Excellency the President, Vice-Presidents, Governors, and Guardians of the Lying-in Hospital—abbreviated.

"MAY IT PLEASE YOUR EXCELLENCY, MY LORDS AND GENTLEMEN,—The great Institution confided to our guardianship, which has for upwards of a century constituted an ornament, and, as the first of the kind, a source of justifiable pride to our city, requires to be remodelled. The time has arrived in which, like all human institutions, progress and changing circumstances have produced such an influence upon it, that neither the intentions of the founder, the wants and just rights of the public, nor the claims of humanity, by securing the greatest preservation of life, are accomplished by its instrumentality. These considerations have so long pressed upon me—considerations, strengthened by my occupations and habits of thought, and so confirmed by a connexion of nearly forty years with this Institution in the various capacities of pupil, assistant, master, and governor—that I venture to hope no excuse is required for my present intrusion.

"The preamble of the charter sets forth its objects as fourfold. First, that of preserving the lives and relieving the miseries of numberless lying-in women, including the wives of His Majesty's soldiers, and their infants. Second, that of preventing child desertion and infanticide. Third, the prevention of gentlemen from going abroad for instruction in midwifery. And lastly, that by 'admitting and instructing, in such an hospital, women who, after some time spent there, being duly qualified, may settle in such parts of our said kingdom, as most stand in need of such persons, it will be a means of preventing the unhappy effects owing to the ignorance of the generality of country midwives.'

"That these objects *have* been accomplished it cannot be denied; but that they *are* accomplished to the extent and degree commensurate with the present resources and capabilities of the foundation, no man can for a moment affirm.

"This proposition will appear evident, when it is stated that the patients admitted in the year 1818 amounted to 3,801; those delivered to 3,539; whilst those admitted in 1866 only numbered 1,324; those delivered only 1,069. In the year 1837, when the Institution was under

my care, the admissions having fallen under 2,000, the Governors, on my application, allowed me to establish a dispensary for treating externs, and to open a ward for the diseases of females. Since that time upwards of 4,000 females, labouring under diseases peculiar to their sex, little understood and less attended to in other hospitals, have been admitted and treated within the walls of your hospital—treated, I say, by competent physicians, who have made this class of disease their especial study.

"And yet this is nothing to what might have been accomplished had the admissions been increased in the wards for the diseases of females as the admission of labour cases diminished. Let us now inquire why have the admission of labour cases diminished to nearly a fourth the number of those in 1818? At its foundation, the Dublin Lying-in Hospital was a unique institution, and for 80 years no other asylum or means of caring for lying-in women in their hour of need existed in this city. Subsequently, the Coombe Hospital and other maternities were opened, affording facilities for attendance upon the poorer classes in their own homes. Again, the North and South Union Poor-houses afforded asylums for the more destitute. These combined causes lessened the number of applicants for admission, until at length they have fallen from nearly four to little over one thousand annually. No doubt, the prevalent idea of recoveries being better at their own homes than in hospital, since public attention has been called to the comparative statistics in each case, has had its influence in lessening the number of applicants to hospital. Be this as it may, the Governors of the hospital are not responsible for the diminution in the numbers, as they have received all that applied. They have thus, as far as in them lay, carried out the intention of the founder in relieving the miseries of all the lying-in women that have applied for admission. But how have we acquitted ourselves of our trust in the other requirement of this part of the preamble, that of 'preserving the lives' of the patients confided to our charge? My conscience obliges me to admit a conclusion, which has been long pressing upon my mind, that in this respect we have failed. Failed from no want of care, of talent, or assiduity, on the part of our medical officers—failed from no niggard supply of food, medicine, or appliances—failed from no neglect on the part of our efficient staff of nurses and superintendents. But, simply, because the original plan and construction of the hospital was faulty, and because our great founder, Mosse, did not possess the power of divination, or foreseeing what experience of the working of such an institution for many years could alone reveal, namely, that the congregating a number of lying-in women under the same roof engenders and spreads amongst them a disease, '*sui generis*,' and of the most fatal character.

"Puerperal fever, although occasionally met with, and even epidemic

in the homes of both rich and poor, is known to haunt our lying-in hospitals as its peculiar '*habitat*,' and so great are its ravages in lying-in hospitals, and such the proportion of victims swept away by it, in proportion to other fatalities in parturition, that there are physicians and philanthropists who even question whether lying-in hospitals, as generally constructed, do not prove rather a curse than a blessing to the lying-in patient. Upwards of twenty-five years since my attention was called to this subject by having been consulted by some benevolent people in America as to the best principle on which to construct a lying-in hospital. And I then unhesitatingly advised that it should be constructed in detached buildings, capable of containing only two or three beds in each building, and one story high, or, if two stories high, that separate entrances by outside stairs should exist to each room.

"It is this plan that I now beg to suggest to you for adoption at the lying-in institution under your care; and peculiar facilities exist by possessing a high plateau, particularly well suited to this purpose, in that portion of the Rotunda Gardens next to Palace-row. This plateau is about 400 feet by 200. On this I would recommend that 30 cottages, 25 feet by 15, with avenues 20 feet wide intervening, be built; that these be fitted with 3 beds in each, according to the accompanying plan. Fortunately, through the forethought of your former master, Dr. Collins, to whose exertions the finances of the hospital owe so much, the ground of the square has become the property of the hospital, so that the required buildings can be constructed on your own ground. The money for this purpose, as I explained to your Board on a former occasion, can be obtained at 3 per cent., paying itself off in 12 years.

"Into these cottages the lying-in patients should be admitted by an entrance at Palace-row. Your lying-in patients now only average about 37 in hospital at the same time, and 60 beds would afford an ample supply to secure the proper cleansing and ventilation between each fresh admission. By this means, and the never having more than two patients and one nurse under each roof, there can be little doubt that puerperal fever would, if not banished, merely show itself in the same proportion of cases, and only prove as fatal as it is observed in private practice. These buildings might be constructed in the style of the cottage ornèe, and have the effect of a Swiss chalet, as seen from the houses in the square, proving rather an ornament than otherwise.

"You were good enough, my lords and gentlemen, to accede to the resolution I proposed, and directed that your Master should afford assistance to all poor lying-in women who required obstetrical attendance in their own homes, and also that a registry should be kept of those so attended. It is gratifying to find that this plan has been in successful operation, and is fully appreciated by those patients who have taken advantage of it.

"As exception has been taken by the learned Professor, Sir J. Simpson, of Edinburgh, to the preference reserved in the charter for those who have served the office of assistant in the election of a master, we deem it due to the Founder's memory, and the interests of the Hospital, to dwell upon the importance of this provision being strictly adhered to. Nothing more clearly evinces the astuteness and wisdom of Mosse than its inculcation.

"We decline to base his motives upon the lower ground of merely encouraging his own alumni, and holding out the important, and, as it has transpired, most lucrative office of master, as a reward and motive to exertion in the great school he was founding. This, no doubt, may have influenced him to a certain extent, and with perfect justice. If his reason for making such a provision was this, his arrangement should be respected, and his wish in this, his will and testament, to provide for his own, should be disturbed by no rude hand. But Mosse's motives were far deeper, broader, more catholic. The founder of such an institution, to his own impoverishment, was influenced by no narrow or exclusive motives in selecting a physician to be entrusted with the charge of the institution for which he had sunk his all; the establishment of which had been the labour of a life; and the sustentation of which, in the highest efficiency when he passed away, was no doubt the first object of his care. Mosse, himself a physician accoucheur in large practice, knew well that a man may succeed to a large practice, be a popular physician—nay, even sustain a reputation as a writer and improver of his art, and yet not be either a safe or a sound physician accoucheur, or such an one as he would feel justified in entrusting with the lives of the patients in his darling Institution, or with the instruction of the pupils of that medical school he was engaged in establishing. He knew that the opportunities of study within his hospital would be unequalled. He knew that the extent of observation enjoyed by a physician in however extensive practice could only be counted by fifties, whilst those to be enjoyed in his hospital would be counted by thousands. He knew that in midwifery practice especially, the varieties were so sparse that their numerical proportion rendered it impossible to be conversant with them *all* unless in a large hospital or great maternity; and that even at the end of a long life, several of them might be left unseen by the private practitioner. He knew that he did not want a master who was only acquiring his experience when he was past his work, but one whose ample opportunities—whilst his youth and vigour remained—secured his fitness to undertake so arduous a trust. All these matters he was fully conversant with; and he concluded, and most wisely concluded, that nothing better secured this than making a master, before his election, serve an apprenticeship as pupil and assistant in the largest available field of observation.

"It now remains to be considered to what purpose the great hospital

and the auxiliary hospital should be applied, and this consideration involves the last two objects set forth by the founder in the preamble—the preventing gentlemen going abroad for instruction, and the supplying duly qualified women as labour nurses throughout the kingdom.

"In no manner could the former object be so satisfactorily and carefully carried out as by extending the principles of your present diseases of female ward, and appropriating several additional wards to this purpose, and two, if possible, to diseases (not contagious) of children. The results that have already followed your twenty-eight years' adoption of this plan fully justify its extension; and this, together with a large increase of attendance of labour cases at their own homes, aud the attendance on the hospital patients in the detached cottages, would afford facilities to the obstetrical students that Mosse himself would have been satisfied with.

"I have now only to report to your Board in reference to the remaining object of our founder, that of facilitating the instruction of qualified women to practice throughout the kingdom, that I waited upon Mr. Power and Dr. M'Donnell, the Poor Law Commissioners, who expressed themselves as much gratified at the resolution of the Board to admit women for instruction to be sent up by the Poor Law Unions on the payment of £5. But although they fully felt the great want that exists for a supply of midwives, and how great an object it is to obtain them, they regretted that the state of the law is not such as to authorize them to require the unions to present even £5 for this purpose. It therefore remains for us to consider whether, after existing interests lapse, arrangements may not be made before a new election of officers, by which this portion of the master's fee be not also discontinued, as the present master so liberally yielded the other portion.

"As no part of our trust is more important than affording an increased supply of educated midwives, the arrangements proposed above will place the auxiliary hospital, and some wards of the great hospital, at our disposal for the purpose of accommodating any additional number we may require, and thus every requirement of the charter will be complied with. By first accommodating every patient who prefers being admitted to hospital, in wards so constructed as to afford them every comfort and advantage, without incurring the risk from puerperal fever; by affording home attendance to every one that applies; by establishing an additional number of wards for those diseases incidental to parturition, and the female specially; by enlarging the opportunities for the male student, so as to make it a perfect obstetrical school; by doing the same for the female student, and thus increasing the supply of well qualified midwives throughout the country.

"It might be necessary in carrying out these suggestions to add to the medical staff of the hospital, as the master and assistants would be

unable to devote the time necessary; for instance, an additional assistant might be necessary. The consulting physician and surgeon, in place of being, as they are at present, sinecure officers, would have to undertake the hospital duties, and possibly additional physicians and surgeons might be appointed to take alternate periods of duty.

"The surgeons and physicians should be required to deliver clinical lectures to their pupils upon their cases. The female pupils admitted should be of a better instructed class then those hitherto sent up from the country. . . . . .

"Your were good enough, in the year 1839, to approve of my establishing within the walls of this hospital, a society for the encouragement and spread of obstetrical knowledge, open to all persons devoted to these inquiries. I have now the gratification of informing you that the Dublin Obstetrical Society, established under your auspices, has developed into a national institution of European reputation—numbering amongst its members many of the most distinguished improvers of our art at home and abroad. That it has assisted in carrying out the designs of your founder, not exactly as he expresses it, by preventing such gentlemen as mean to practice midwifery from going abroad for instruction, but by drawing many gentlemen from abroad here for that laudable purpose.

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"I have now merely to state that I transmit this letter for your perusal previous to the meeting of the Governors, which will take place at the Rotunda Hospital at 3 o'clock on the first Friday in November, when it is my intention to draw the attention of the Governors to the propositions herein contained, in the confident hope that so far as they are deserving of your approval they will obtain your support; and thanking you most sincerely for the attention with which my suggestions have always been considered by your Board, I have the honour to remain, your Excellency, my Lords, and Gentlemen,

"Your obedient servant,

"Evory Kennedy.

"1, Upper Merrion-street, Dublin,
"28*th October*, 1867."