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A N
E X A M I N A T I O N

O F A
C H A R G E

B R O U G H T A G A I N S T

I N O C U L A T I O N ,

B Y

D E H A E N , R A S T , D I M S D A L E ,
and other Writers.

B Y J O H N W A T K I N S O N , M . D .

Non mihi, sed rationi, aut quæ ratio esse videtur
Milito; securus quid mordicus hic tenet, aut hic.

SCALIGER.

L O N D O N :

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M, DCC, LXXVII.

T O
The VICE-PRESIDENTS,
TREASURER,

AND THE
Rest of the GOVERNORS

OF THE
DISPENSARY

FOR
GENERAL INOCULATION,
This EXAMINATION, &c.

I S

With the greatest Respect,

Inscribed,

By their

Most obedient

humble Servant,

JOHN WATKINSON.

S I R,

AS the Plan of the DISPENSARY for GENERAL INOCULATION seems to be the only one by which the salutary effects of Inoculation can be sufficiently extended to a very numerous and useful class of people, the Poor of the Metropolis, the success of the above Institution becomes a matter of public importance.

An objection has however been made to this Institution, by some who stand high in the esteem of the public, and whose opinion of it, therefore, ought to have been delivered with greater caution, lest, in endeavouring to prevent an imaginary evil, they should rashly nip in the bud a national benefit.

But, as the falsity of the objection alluded to, is clearly shown in the inclosed Treatise, it has been thought expedient to transmit it to you, that the cause of humanity may not suffer by the *assertions* of those who seem to have taken but little pains to inform themselves of *facts*.

I am, Sir,

Your most obedient Servant,

ROBERT SMITH, Secy

P R E F A C E.

THE Charge, which I have examined in the following pages, strikes at the very root of Inoculation, in this metropolis.

For, if it be true, that the practice of this art has, for a series of years, augmented the mortality of the natural Small-pox, it has certainly, been, hitherto, injurious to society ;—and, if from the extension of that practice, a proportional increase of the mortality is to be apprehended,

apprehended, as a late respectable writer seems to think, I cannot see on what principle, either of humanity, or policy, the further use of it can be justified.

But, confidently, as this charge has been brought, I have attempted to shew, that it is totally destitute of foundation.

Whether I have succeeded, or not, the public will determine.—
The Charge and the Answer are both before them.

E X A M I N A T I O N

O F A

C H A R G E,

WHICH HAS BEEN BROUGHT AGAINST

I N O C U L A T I O N,

By DE HAEN, RAST, DIMSDALE, and
other WRITERS.

AMONG the various improvements which do honour to the age we live in, the present method of inoculating the small-pox is far from being the least.

In the practice of this happy invention, we see human ingenuity opposing itself to the ravages of a dreadful disease, and the medical art triumphing, as it were, over the powers of death.

The numerous objections which malice, envy, and ignorance had brought against it, are now, as far as they respect its utility to *individuals*, gradually sinking into oblivion; and “time, who obliterates the fictions of opinion, and confirms the decisions of nature,” has given his testimony in its favour.

But, the victory is yet incomplete.—Inoculation is represented, as being hurtful to the *community*.—It is charged with spreading the variolous contagion, and increasing the mortality of the *natural* small-pox.

Whether this charge be well founded or not? is a question of public concern. The public have therefore a right, to all the evidence which is necessary to the just solution of it.

That evidence, with a few reflections naturally arising from it, I mean now to lay before them, with brevity and candour.

In support of the above charge, the London Bills of Mortality are appealed to.

These, indeed, shew that the mortality from the small-pox has been increased since the introduction of inoculation, but, they contain no proof that inoculation has occasioned it, on the contrary, they clearly

ly

ly demonstrate that the accusation is unjust.

But, previous to the stating of any evidence in exculpation of inoculation, it may be proper to inquire, what is the amount of the abovementioned increase, and in what manner the fact has been ascertained ?

According to the celebrated De Haen, the Bills of Mortality of this City evince, that, one sixth more have died of the small pox in the space of twenty two years since the commencement of inoculation, than in the same period of time before the introduction of that practice.

And, by a comparison of the same kind made by another opponent to inoculation, it appears, that, in the space of thirty-eight years the difference is still greater.

Baron Dimfdale, who follows Dr. Jurin's method of computation, finds, that, in a period of thirty two years, beginning with the year 1734, the deaths by the small-pox amounted to one-eighth of
the

the whole number ; and in the eight succeeding years to somewhat more than one-sixth. “ But ” says he “ if the eight years are divided it will appear that the deaths from the small-pox in the first four years are 8642 ; the medium for each of those years will be 2160.”

“ For the last four years the numbers are 10179, the medium for each 2544.”*

It is, however, manifest, that in a city, like London, where the number of inhabitants, from various causes, must be continually fluctuating, that no certain conclusion can possibly be drawn, with respect to the increase or decrease of the mortality of the small-pox, from the *absolute* number of deaths by that disease in one period, compared with the absolute number of deaths by the same disease in another period.

This material circumstance seems to have been wholly overlooked by De Haen ; and in the last mentioned calculation, it has likewise escaped the attention of the Baron.

* Thoughts on general and partial inoculations.

Other writers have avoided this error. They have endeavoured to trace the variations in the mortality of this distemper, not from the *absolute*, but the *relative* number of its victims, that is, from the proportion which they bore to those of all the other diseases at one time, compared with the proportion which they bore to them at another.

But, unexceptionable as this mode of investigation may at first sight appear to be, a slight examination will discover that it is not wholly free from fallacy.

It is true, that, by comparing, the number of persons who have perished at different times, by the small-pox, with the number of those who have been cut off by the other diseases, any excess or defect in the former with respect to the latter, may readily be detected. But, if the influence of any cause which tends to increase the general mortality, without having any effect upon that by the small-pox, should be diminished, it is evident, that the variations in the mortality by the latter, relative to the number of inhabitants, would

would not be shewn by the above comparison.—

Such a cause, for example, is the impurity of the air, the influence of which, there is reason to believe, has been considerably diminished by the various improvements which have been made in this metropolis. For it does not appear, that it is so much from the magnitude of a city that the air becomes contaminated, as from the narrowness and uncleanness of its streets.

The air of Edinburgh is as unhealthy as that of London, yet the inhabitants contained in the former of these cities do not amount to a sixteenth part of the number contained in the latter.

It has been computed by a very accurate and able writer,* that, about one in twenty of the inhabitants of this city dies annually; whereas in the parish of Holy Cross near Shrewsbury only one in thirty three dies in the same period of time; at Stoke Damarell in Devonshire only one in fifty four; and, according to a late publication
by

* Dr. Price.

by Dr. Percival, the disproportion in some places is still greater.

This striking difference between the duration of life in town and in the country, is chiefly to be attributed to the greater purity of the air in the latter than in the former.

If, therefore, in consequence of the improvements above alluded to, the state of the air in London, has in any degree approached to the state of that in the country, it follows, *cæteris paribus*, that the number of deaths, in proportion to the number of inhabitants, must be diminished.

But, as the small-pox is a disease which neither owes its existence to, nor seems to be affected by, that kind of impurity of which we are now speaking; a disease which rages with equal violence in the congregated city, and the thinly peopled village; it is *possible* that its victims may at the present time bear nearly the same proportion to the number of inhabitants, though not to the number of deaths, that they did formerly.

I shall

I shall admit, however, that the increase of the mortality in question, is accurately ascertained, and proceed to shew that the inference deduced from it is nevertheless false.

If inoculation have spread the infection, and augmented the mortality in the degree contended for, the inoculated small-pox must, necessarily, be very contagious. Whether it be so, or not, let reason and observation determine.

As the pustules in the artificial disease are generally very few in number, and the quantity of fresh air applied is large, it is rational to suppose that the effluvium arising is immediately combined with the atmosphere as a menstruum, and like other vapours, in a similar state, deprived of its peculiar properties : but, as the pustules in the natural disease are more numerous, the effluvium arising will be much greater, and the combination above mentioned not so soon effected ; and, if the patient be confined to his chamber, which is generally the case, the air must quickly be saturated with the effluvium
emitted

emitted, and, consequently, that which arises afterwards will float in it unaltered.

In the two latter instances, the contagion may readily be conveyed through the medium of the air; in the former, it can scarcely be communicated unless by contact.

The intensity of the contagion is therefore to be estimated by the number of pustules, directly, and the quantity of air applied, inversely.

The exceptions to this general rule, I am not solicitous to discover.—It is sufficient for my present purpose that the above reasoning evinces what observation (as I shall afterwards shew) confirms, that, there may be a very considerable difference between the natural and inoculated small-pox, with respect to their contagious power, though none with respect to their essence.

Whether, indeed, this disease be produced naturally, or artificially, it is far less contagious than it is generally supposed

to be. For, although, in either of these states, it may be communicated, with the difference above described, to some few, or, in technical language, may be propagated sporadically ; in neither, unless a certain constitution of the air is present, can it be spread epidemically.— When that constitution is present, the contagion is rapidly diffused, independently of personal communication between the infected and those liable to receive the infection ; when it is not present, the contagion soon ceases to multiply itself, though under circumstances the most favourable to its propagation.

Baron Dimfdale, who has delivered his sentiments on this subject, in the publication before mentioned,* tells us, ‘ that he
 ‘ knows it has been said, and even publicly declared, that the small-pox from inoculation is so mild, as *scarcely* to be infectious to others ;’ but ’ says he ‘ if this was
 ‘ true, how comes it that matter, taken from inoculated patients, conveys the distemper with equal certainty, as if it was taken
 ‘ from the natural small-pox ? Is it not
 ‘ morally

‘ morally certain, that the effluvia partake
 ‘ of the same infectious quality ? No phy-
 ‘ sician of any experience, I am sure, will
 ‘ ever countenance such an opinion.—But
 ‘ lest it should prevail, and do mischief
 ‘ among the ignorant and credulous, I think
 ‘ it incumbent on me to contradict so dan-
 ‘ gerous and unwarrantable an assertion.’

But ‘ dangerous and unwarrantable’
 as this assertion may be, Baron Dimf-
 dale maintains it himself, at least substan-
 tially, in the very next paragraph, and
 thereby gives his own testimony in fa-
 vour of that opinion which he is ‘ sure
 ‘ no physician of any experience will ever
 ‘ countenance.’

‘ In fact’ says he, ‘ it is certain that
 ‘ the small-pox is infectious *in proportion*
 ‘ *to the number and malignity of the pustles,*
 ‘ and so far there is usually less danger
 ‘ from the artificial disease, than from the
 ‘ natural.’

From which, I think, it clearly follows,
 that when the pustules are free from ma-
 lignity, and very few in number, which
 is

is generally the case in the inoculated small-pox, the disease may be 'so mild as scarcely to be infectious to others.'

But, whatever might have been the idea which the Baron intended to convey, I do not hesitate to affirm, that the above inference contains a well established truth.

Medicus, a very eminent and experienced German physician, observes in one of his epistles to Dr. Petit of Paris, that the variolous contagion is so rarely propagated by the artificial disease, that although a prodigious number of people have been inoculated, not more than ten instances, perhaps, can be reckoned, in which it has communicated the infection, notwithstanding the pains which have been taken to discover that pretended quality of it.

Si donc, says he, nous voulons connoître et déterminer avec précision le vrai degré, la vraie force de cette espece de contagion, il nous faut consulter ce qui arrive dans le petites verole inoculées, ou l'infection d'un air epidemique-

epidémique n'a point lieu. Or la petite verole se communique si rarement par les inoculés, que sur la quantité prodigieuse qu'il y a eu d'inoculés on ne sauroit peut-être compter plus de dix exemples d'une pareille infection malgré les soins qu'on s'est donné pour tâcher de découvrir cette prétendue qualité de la petite verole inoculée.

Miege, a celebrated inoculator, declares that his own experience has afforded but one example of the contagion being propagated by the inoculated small-pox, and that, that happened by contact, *per osculum, ideoque proximum per contactum accidit.* Nierop. de contag variol.

Sulzer, who appears to have had considerable practice in this art, assures professor Schroëder of Gottingen, in a letter which he wrote to him in the year 1765, that he had not seen a single case in which the inoculated small-pox had by contagion given the disease to another.

Je puis vous assurer Monsieur, says he, que depuis 1758, que j'ai inoculé bon nombre toutes les années, et dans toutes les saisons,

sons, je n'ai pas vu un seul cas, ou j'eusse pu dire, la petite verole inoculée a donnée par contagion a un autre enfant ou adulte la maladie : encore moins a t'elle causée un epidemie de petite verole, quoique j'aye inoculé dans la ville et dans les villages, et jamais apart dans des maisons. Il est vray, que je prends les precautions dans le tems de la supuration de ne laisser aprocher du malade ceux qui pourroient etre infectés, et que je fais changer d'habit, laver, et parfûmer ceux qui pourroient aisement porter la contagion, sur tout si les patients ont bon nombre de petite verole. Vid. Nierop de contag. variol.

A similar assurance is given by Dr. Odier of Geneva to the author of the *Journal de medecine**. Speaking of the two letters that he had some time before addressed to De Haen, in which he had stated the objection derived from the bills of mortality in its full force, he says, *Jusqu'ici je n'ai fait que donner à l'objection que j'avois en vue toute le force dont elle me paroît susceptible ; il me reste à examiner jusqu'à quel point elle est fondée. Je suis si persuade qu'elle ne l'est point du tout, que depuis que je commence à pratiquer le medecine, je n'ai cessé*

* Journal de Medecine, &c. Vol. XLII.

cessé de recommander hautement l'inoculation, & d'inoculer moi-même, toutes les fois que l'occasion s'en est présentée. Si je n'avois pas été déjà parfaitement convaincu, le succès que j'ai eu jusqu'ici seroit plus que suffisant pour résoudre tous mes doutes, d'autant plus que je n'ai point encore observé que la petite vérole inoculée se communiquât à personne par contagion."

Mr. Holwell, who resided upwards of thirty years in the East Indies, and whose account of the manner of inoculating the small-pox in that country, shews the attention that he paid to this subject, informs us, that, 'notwithstanding the
' multitudes that are every year inoculated
' there in the usual season, it adds no malignity to the disease taken in the natural
' way, nor spreads the infection as is commonly imagined in Europe.*

But, the following fact attested by Dr. Schwenke a physician of distinguished reputation in Holland, is sufficient, I think,
to

* An account of the manner of inoculating for the small-pox in the East Indies.

to remove every doubt that may remain on this head.

About the end of the year 1767, and the beginning of the year 1768, two hundred people, at least, were inoculated at the Hague, who without much regard either to themselves or others, frequented all places of public resort; notwithstanding which, no epidemic was produced, nor in the whole year did more than eight persons die of the small-pox, and of these, three died in the spring, one by inoculation, and two by the natural disease which they had caught at some other place and carried with them to the Hague; the remaining five died towards the end of the year.—*Vid. M. W. Schwenke Epist. in Cel. Sandifort. Biblioth. med. Tom. 6.*

To these testimonies, the number of which might have been greatly augmented, I shall beg leave to subjoin my own. I have paid particular attention to the point in question, since the establishment of the dispensary for general inoculation, and can with truth affirm, that a single instance has not yet occurred in that charity,

charity, in which the contagion has been spread by an inoculated patient. Where the chance of spreading it has been apparently great, I have been very strict in my inquiries.—In many cases the circumstances have been such, that if the apprehensions of a celebrated inoculator were well founded, the distemper must inevitably have been communicated.

Some have been inoculated in narrow streets, in the midst of those who were obnoxious to the small pox, and others in little courts, where, according to the common opinion, the danger of communicating the disease was still greater.

In the latter case, the patient has sometimes been kept in a little room on the ground floor, the door of which opened directly into the court, and in the day time was seldom shut. Before this door, and within a few yards of the person inoculated, a number of children have continued to play during the whole course of the disorder, and, as has been already affirmed, without receiving the infection.

Baron Dimisdale, indeed, asserts, that instances of a contrary kind have frequently fallen within his observation. But, as the Baron does not seem to have been aware of the great influence of the epidemic constitution of the air—it is possible that what he should have attributed to this cause, he has sometimes imputed to simple contagion.—Be that as it may, a writer in the *Monthly Ledger*, * under the signature of J. S. who had an opportunity of seeing the practice of inoculation in the country where these instances happened, speaks of the consequences of it in a language very different from that which is held by the Baron.

“ I have been witness, says he, to the progress of inoculation, from the introduction of the Suttonian method, thro’ a very considerable part of a populous country: at the introduction of that method, the subjects obnoxious to the disease were more numerous in proportion to the ex-
 empts, than they could possibly be in London at any period. Baron Dimisdale
 under

* Vol. I. Page 523.

under whose direction a principle share of the practice was conducted, was not deficient in imposing such restrictions on his patients as he thought necessary for the public safety ; but I believe these restrictions were not very scrupulously regarded. There were practitioners, whose practice was by no means inconsiderable and whose restrictions were less strenuously imposed and more frequently broken ; yet few instances of infection from inoculation were heard of ; that there were not twenty times more was matter of surprize to those acquainted with the contagious nature of the disease, and is to me an irrefragable proof of the truth of what I have asserted, that more mischief is likely to be done by one patient, in the natural confluent disease, than by fifty inoculated patients under the present mode of management. Your correspondent may probably object that there might be many more instances of infection from inoculation at the time I have mentioned, than could have come to my knowledge. But those who know most of the country know that it is a place where things cannot be secreted, a transf-

a tranfaction at ten miles distance is more talked of than a tranfaction at two streets distance in London. The practice was the general topic of conversation, I was far from being uninquifitive about its fuccefs, and there were opponents of it who would have made their advantage of *any injury* which it might have produced.”

Dr. Tiffot, in a piece intituled, *L'inoculation juftifiée*, very juftly obferves, that the fmall pox is indeed a contagious difeafe, but, that it does not propagate itfelf fo much by contagion, as by an *infection of the air*, produced by caufes which are unknown to us.

The truth of this obfervation, is exemplified in a very ftriking manner, by a fact which is related by Dr. James Sims in his *Observations on Epidemic Diforders*.

“ About the autumnal equinox,” fays he, “ bilious diforders declined, giving way to the fmall-pox, that with unheard of havock defolated the clofe of this year, and the fucceeding fpring of 1767. They had appeared above a year before, along
the

the eastern coast of the kingdom, and proceeded slowly westward with so even a pace, that a curious person might with ease have computed the rate of their progress. In this they were scarcely to be interrupted, as appeared by the following instance. The children of soldiers on their march, had brought them from other places to some towns here, during the preceding summer, and although they were of a malignant kind, the afflicted all dying, and therefore most fit to propagate the infection, yet *not one of the inhabitants received them, until in their regular progress they had travelled over the intermediate space.*"

Nothing, indeed, is more manifest, than, that the natural small-pox, though, in general, much more contagious than the artificial, does not readily multiply itself, unless favoured by a miasmatic constitution of the air.—The same remark may be extended to other contagious diseases.—*Etenim contagium morbosum, says Van Swieten, requiri causas prædisponentes, ut morbus ille nascatur certum est.* Tom. V. P. IV.

And

And, according to Sydenham, even the plague itself, without the concurrence of a fit disposition of the air, cannot excite an epidemic.—*Interea aeris dispositionem quantumvis λοιμωδη, pesti suscitandæ per se imparem esse, vehementer suspicor; quin pestilentia morbum alicubi semper superstitem aut per fomitem, aut per pestiferi alicujus appulsum, e locis infectis in alios deferri; ibidemque non nisi accidente simul idonea aëris diathesi popularem fieri.* Sect. 2. cap. 2.

It appears from the *Premier Rapport sur l'inoculation* of Dr. Petit, that the Hotel Dieu, a large hospital in the centre of Paris, is never free from the small-pox, and that at certain times the ward destined to receive those who are seized with that disease is extremely full; that notwithstanding the multitude thus crowded together, and the enormous quantity of infection produced, and that in a place too which is open to the public, and where there is continually an immense concourse of all sorts of people, the disease is not observed to be always present in the neighbourhood of this hospital, nor, even to be more common there, than in other parts of the city.

Vous n'ignorez pas Mess. qu'en tout tems il y a des petites véroles dans l'Hôtel-Dieu de Paris ; que dans certaines saisons, la salle destinée a recevoir ceux qui en sont attaqués, est excessivement remplie : or cette multitude de malades ramassée dans un même lieu ouvert a tout le monde, et dans lequel il y a sans cesse un concours immense de personnes de tous états, cette multitude, dis-je, ne forme-t'-elle pas un masse enorme de levain variolique, qui devoit au moins se répandre dans le voisinage de cet hôpital placé au centre de la ville, et serré de tous côtés par les maisons des particuliers ? Cependant on n'a point encore observé que dans ce voisinage la petite vérole dût durer toute l'année, ou que seulement elle y fut plus commune que dans le reste de la ville. Page 121.

A remarkable instance of the insufficiency of contagion alone to the production of this distemper is authenticated by Dr. Sandifort, the present professor of anatomy and surgery in the university of Leyden.

One of the children in the orphan house at the Hague was seized with the small-pox, and tho' the communication between

tween the patient, and the rest of the orphans was not interrupted, none of them caught the disease,

The fact is circumstantially related by Dr. Nierop Junior of Amsterdam. His words are these.

Celeberrimus Sandifortius, Professor in Anatomicis et Chirurgicis insignis, tempore, quo praxin medicam Hagae Comitum felicissime faceret, in Orphanotrophio Hagano variolis laborantem tractavit, non propogato ad reliquos infantes aliosve eam domum inco-lentes contagio, licet porticus, in quo decum-bebat variolans, cum tota domo commercium tam intimum haberet, ut sæpius per illum transfirent reliqui, et in eo quotidie deligaren-tur qui ope chirurgica indigebant, quemad-modum perbenigne mecum communicavit Cel. Vir.—Nullum adeoque dubium, adds he, quin miasma dispersum ab aliis contrahi potuerit, generalis si adfuisset conditio præ-disponens, quæ sufficeret ad morbum exci-tandum. De contag. variol.

Another instance of the same kind and not less remarkable, I remember to have
heard

heard related by the celebrated Professor Van Doeveren, of Leyden, in his lectures on the practice of medicine.

In the spring of the year 1762, a company of foot, with twelve children labouring under the small-pox, entered the city of Groningen, which was then entirely free from that disease.—These children were dispersed in the houses of the poorer sort of inhabitants, in the midst of numbers who had not had the distemper, and, who, constrained by their poverty, could not fly from its approach.—A fairer trial of the power of simple contagion could scarcely have been devised.—The event was such as convinced the learned professor that this power was inadequate to the effects which had been commonly ascribed to it.—The Epidemic threatened was not produced, nor, which is more extraordinary, was the disorder propagated sporadically; for, after the most sedulous inquiry, not an individual could be found to whom the infection had been communicated.

This fact has been likewise mentioned

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by

by Dr. Forsten Verschuur, an eminent physician at Amsterdam, in a dissertation on inoculation, written in the Dutch language, and published in the Year 1769; and also by Dr. Nierop, in his *Specimen Medicum de Contagio varioloso ex observationibus indagato*.

To multiply facts of this sort would be easy, but I trust that these will be quite sufficient to establish the position for which they were brought. I shall therefore quit this ground, and meet the antinoculists upon that, on which, they have hitherto thought themselves secure of victory. I mean the Bills of Mortality. To the accuracy of these I shall wave every objection. Their evidence I shall admit to be good, and on that evidence will hazard the credit of inoculation.

It is an axiom in philosophy, that the same cause, in similar circumstances, will always produce the same effect; and further, that the effect produced will be greater, or less, as the energy of the cause is increased, or diminished.

Let us apply this to the Bills of Mortality. Let us see whether the number of deaths by the small-pox has risen and fallen, in proportion as inoculation has been more or less practised. If this should be the case, there will be some reason for imputing that variation to inoculation. But if on the contrary it should appear, that the one has not corresponded to the other, it will be evident, that the increase of mortality, and the practice of inoculation are not connected together as cause and effect.

In a period of seven years, immediately preceding the introduction of inoculation into this city, that is from the year 1714 to the year 1720 inclusive, the mean annual number of deaths by the small-pox, compared with the mean annual number of deaths by all the other diseases, was as one to eleven.—In the seven succeeding years the proportion was the same. From the year 1728 to 1734, the number of victims to the small-pox was comparatively diminished; the proportion which it bore to the number of those who were cut off by the other diseases being
but

but as one to twelve.—From the year 1735, to the year 1741—and from the year 1742, to the year 1748, in the latter of which periods inoculation was more prevalent than it had ever been before, the share which the small-pox had in the General Mortality was reduced from the proportion of one to twelve, to that of one to thirteen. From the year 1749, to the year 1755—its share was the same as in the first period above mentioned prior to the introduction of inoculation.

Since the year 1755, according to the same mode of computation, the mortality of the small-pox, compared with the general mortality, has been augmented to the proportion of one to nine.—But it must be remembered, that, for a considerable part of this time, inoculation, tho' much practised in the country parts of England, made no progress in the capital. And if instead of the last seven, we take the last four years, during which inoculation has become very fashionable, we shall find, by a similar calculation, that the mortality in question is again diminishing.

That

That the prevalence of inoculation, and the increased mortality of the small-pox, have in no point of time coincided, I do not mean to insinuate. According to the laws of chance, this must sometimes have happened. But I contend, that the great irregularity of their coincidence may be considered as a fresh proof, that the one, is not the cause of the other.

Upon this fact, the defence of inoculation against the charge of increasing the mortality of the small-pox, might, perhaps, be safely rested; but I shall add to it another, derived likewise from the Bills of Mortality, which appears to be conclusive.

For the fact alluded to, I am indebted to my ingenious friend Dr. James Sims, who has very obligingly furnished me with it from the materials which he has collected for the history of inoculation. I shall state it in his own words.

‘ An objection has been made to inoculation, and lately supported with considerable

'derable warmth by several respectable
 'writers, which, if founded in truth,
 'would be sufficient to prove that the
 'practice of this art is detrimental to so-
 'ciety. It is asserted, that, by inocula-
 'tion the contagion is so much propo-
 'gated, that the victims to the small-pox
 'have been more numerous since, than
 'they were before, that practice obtained;
 'and that the mortality has increased in
 'proportion to the reception of the art'.

' To prove these assertions it has been
 ' usual to extract the deaths by the small-
 ' pox, from the Bills of Mortality, for a
 ' certain number of years previous to the
 ' introduction of inoculation, and to com-
 ' pare the general average of these, with
 ' the average of deaths since that time, and
 ' by dividing the latter into separate periods
 ' of years, to shew, that the proportion
 ' of deaths by the small-pox has been con-
 ' stantly increasing since the practice of
 ' inoculation began.

' Thus, one writer, who gives a view
 ' of the Bills of Mortality for eighty four
 ' years, shews, that in forty two years
 ' previous

' previous to inoculation, only seventy two
 ' deaths in every thousand were owing to
 ' the small-pox, whereas in forty two years
 ' afterwards, the deaths by that disease a-
 ' mounted to eighty nine in every thousand;
 ' and, that, by dividing the latter of these
 ' into lesser periods the average of deaths
 ' is as follows :—In the first twelve years,
 ' it is seventy four in a thousand, in the
 ' next ten, eighty three, in the next, ninety
 ' six, and in the last ten, one hundred and
 ' nine.

' This constant increase is attributed to
 ' inoculation, and the argument appears
 ' to be properly stated, as it guards against
 ' any deception which might arise from the
 ' variations in the general number of deaths.
 ' I have endeavoured to state this objection
 ' in the strongest manner, and hope
 ' that I shall be able to give a decisive an-
 ' swer to it.

' The reason why the above objection
 ' has not hitherto been satisfactorily an-
 ' swered, is this ; those who have attempt-
 ' ed it, have taken the Bills of Mortality as
 ' garbled, and unfairly stated by the ob-
 ' jectors

jectors to inoculation, without giving themselves the trouble of further examination.

The circumstance in which the objectors have dealt unfairly by us is, that in taking the medium of deaths for a certain number of years prior to the practice of inoculation, as a fixed standard, they have not once hinted that the mortality of the small-pox had increased in the same proportion before, as it has done since, the introduction of that art; and by prudently publishing only a part of the bills, they have given us no opportunity of making this discovery. Had they given the whole of the bills, is it to be supposed that any man in his senses would have joined with them in blaming inoculation for an increase, which commenced ninety years before inoculation was heard of in this country, and continued progressive through the whole of that period?

I shall not pretend to say what the causes are which have produced this increase of mortality, all that I mean to aver is, that the increase has been constant

'stant from the first bill, wherein the dif-
 'eases are specified, to, almost, the present
 'day. I say almost, because, for the last
 'twelve years, when I believe, every person
 'will allow that inoculation has prevailed,
 'perhaps ten times as much as at any
 'time before, a considerable decrease has
 'taken place.

'That I may, however, treat the reader
 'fairly, I have in the first of the following
 'tables given every Bill of Mortality that I
 'could procure, and which, I have the
 'utmost reason to think, are all that are
 'now extant, wherein the small-pox is
 'distinctly specified.

'Having these before him, if I should
 'draw any unjust conclusion, he will easily
 'detect the error.

'The first column of this table shews
 'the date of each bill; the second, the
 'number of deaths in each by all diseases;
 'the third, that part of the number which
 'was owing to the small-pox; and the
 'fourth, the amount of this part in every
 'thousand of the whole.

'From this table I have formed six o-
 'thers

' thers.—The first, consists of periods of
 ' four years ; the second, of eight ;—the
 ' third, of twelve ; the fourth, of sixteen ;
 ' the fifth, of twenty ; and the sixth, of
 ' twenty four.

' In these tables, the first column contains
 ' the concluding years of each period ; the
 ' second, the annual average of all the
 ' deaths during that period ; the third, the
 ' average of deaths by the small-pox ; and
 ' the fourth, the number in every thousand
 ' of the whole sum of deaths occasioned by
 ' the above disease, as in the last column of
 ' the preceding table.

' The inspection of these will serve to
 ' convince every person however prejudiced,
 ' that the increasing mortality of the small-
 ' pox has existed, at least, from the origin of
 ' the registers of mortality.

' So regular a progress cannot be ex-
 ' pected in the first tables, as smaller pe-
 ' riods of years must in some measure par-
 ' take of the yearly inequality. But in the
 ' last table the progression is perfectly re-
 ' gular, down to the year 1772 inclusive,
 ' the series being 48, 56, 72, 77, 101, and
 ' for the last four years 96.

' I know

‘ I know, that by an unequal division of
 ‘ years I could have produced a series re-
 ‘ gularly increasing from the first number
 ‘ in table the second of 19, to 119 in the
 ‘ same table, and then regularly decreasing
 ‘ to 96 :—But I have avoided this mode of
 ‘ division as too artificial, and made choice
 ‘ of the present as the fairest and the least
 ‘ liable to exception.

‘ The second and third table will shew,
 ‘ that this mortality instead of increasing,
 ‘ is at present considerably declining. For
 ‘ it appears by the second, that the aver-
 ‘ age of deaths for four years preceding
 ‘ 1760, was 119 in a thousand, and in
 ‘ the third, the average for the period pre-
 ‘ ceding 1765, was 112 ; for the next
 ‘ eight years to 1772, it was 105 ; and
 ‘ for the last four years only 96.

‘ To shew this declension more ac-
 ‘ curately, and place it beyond the mis-
 ‘ representation of those, who, by taking
 ‘ such a particular number of years as
 ‘ happens to answer their purpose, lay
 ‘ false conclusions before their readers,
 ‘ under the sanction of apparently true cal-
 ‘ culations, I have constructed the eighth
 ‘ table :

‘ table: the columns are the same as in
 ‘ the former tables.

‘ It consists of eleven divisions, in
 ‘ which the number of years compared,
 ‘ increase regularly from one to eleven.
 ‘ In each of these I have, for obvious rea-
 ‘ sons, begun with the last bill of mortality.
 ‘ In the first, the bill of the last year is
 ‘ compared with the bill of the year im-
 ‘ mediately preceding. In the second,
 ‘ the medium of the bills for the last two
 ‘ years, is compared with that of the same
 ‘ number of immediately preceding years.
 ‘ In the succeeding ones, a year is regu-
 ‘ larly added, until the last, in which
 ‘ the mortality of the eleven years,
 ‘ from 1766 to 1776 inclusive, is com-
 ‘ pared with that of the years from 1755
 ‘ to 1765 also inclusive. It will appear in
 ‘ this, as in the former tables, that an in-
 ‘ equality prevails in all lesser divisions ;
 ‘ but an evident decrease being seen in
 ‘ all the larger, will force us to conclude
 ‘ that the small-pox does not destroy so
 ‘ many now as formerly, which happy
 ‘ diminution can I think scarcely be at-
 ‘ tributed to any other cause than the
 ‘ present prevalence of inoculation.

T A B L E I.

T A B L E I.

Year.	Total of Deaths.	Deaths by the Small-Pox.	In 1000	Year.	Total of Deaths.	Deaths by the Small-Pox.	In 1000
1629	8771	72	8	1675	17244	997	58
1630	10554	40	3	1676	18732	359	19
1631	8562	58	7	1677	19067	1678	88
1632	9535	531	55	1678	20678	1798	87
1633	8392	72	8	1679	21730	1967	91
1634	10400	1354	130	1680	21053	689	33
1635	10051	293	28	1681	23971	2982	125
1636	23359	127	5	1682	20691	1408	68
1647	14059	139	10	1683	20587	2096	102
1648	9894	400	40	1684	23202	156	7
1649	10566	1190	112	1685	23222	2496	107
1650	8764	184	21	1686	22609	1062	47
1651	10827	525	48	1701	20471	1095	53
1652	12569	1279	102	1702	19481	311	16
1653	10087	139	13	1703	20720	896	43
1654	13247	812	61	1704	22684	1501	66
1655	11357	1294	114	1705	22097	1095	50
1656	13921	823	59	1706	19847	721	36
1657	12434	835	67	1707	21600	1078	50
1658	14993	409	27	1708	21291	1687	79
1659	14756	1523	103	1709	21800	1024	47
1660	15118	354	23	1710	24620	3138	127
1661	19771	1246	63	1711	19833	915	46
1662	16554	768	46	1712	21198	1493	92
1663	15356	411	27	1713	21057	1614	77
1664	18297	1233	67	1714	26569	2810	106
1665	97306	655	6	1715	22232	1057	48
1666	12738	38	3	1716	24436	2427	99
1667	15842	1196	75	1717	23446	2211	94
1668	17278	1987	109	1718	26523	1884	71
1669	19432	951	49	1719	28347	3229	114
1670	20198	1465	72	1720	25454	1440	57
1671	15729	696	44	1721	26142	2375	91
1672	18230	1116	61	1722	25750	2167	84
1673	17504	853	49	1723	29197	3271	112
1674	21201	2507	118	1724	25952	1227	47

TABLE I. continued.

Years.	Total of Deaths.	Deaths by the Small-Pox.	In 1000	Year.	Total of Deaths.	Deaths by the Small-Pox.	In 1000
1725	25523	3188	125	1751	21028	998	47
1726	29647	1569	53	1752	20485	3538	172
1727	28418	2379	84	1753	19276	774	40
1728	27810	2105	77	1754	22696	2359	103
1729	29722	2849	96	1755	21917	1988	90
1730	26761	1914	71	1756	20872	1608	77
1731	25262	2640	104	1757	21213	3296	154
1732	23358	1197	51	1758	17576	1273	72
1733	29233	1370	46	1759	19604	2596	132
1734	26062	2688	103	1760	19830	2187	110
1735	23538	1594	67	1761	21063	1525	72
1736	27581	3014	100	1762	26326	2743	104
1737	27832	2084	74	1763	26143	3582	137
1738	25825	1590	61	1764	23202	2382	102
1739	25432	1690	66	1765	23230	2498	107
1740	30811	2725	88	1766	23911	2334	97
1741	32169	1977	61	1767	22612	2188	96
1742	27483	1429	52	1768	23639	3028	128
1743	25200	2029	80	1769	21847	1968	90
1744	20606	1633	79	1770	22434	1986	88
1745	21296	1206	56	1771	21780	1660	76
1746	28157	3230	114	1772	26053	3992	153
1747	25494	1380	54	1773	21656	1039	48
1748	23869	1789	75	1774	20884	2479	119
1749	25516	2625	102	1775	20514	2669	130
1750	23727	1229	51	1776	19048	1728	90

TABLE

T A B L E II.

Containing Periods of Four Years.

Conclud- ing Year of each Period.	Annual Medium of the Total Deaths.	Annual Medium of Deaths by the Smallpox.	In 1000.
1632	9355	175	19
1636	13200	461	35
1650	10820	478	44
1654	11682	688	58
1658	13176	840	63
1662	16549	972	58
1666	35924	584	16
1670	18187	1399	76
1674	18166	1293	71
1678	18930	1208	63
1682	21861	1761	80
1686	22405	1452	64
1704	20839	951	45
1708	21208	1145	53
1712	21862	1642	75
1716	23573	1977	83
1720	25941	2191	80
1724	26760	2260	84
1728	27849	2310	83
1732	26275	2150	81
1736	26603	2166	81
1740	27472	2022	73
1744	26364	1767	67
1748	24704	1901	76
1752	22689	2097	92
1756	21190	1682	79
1760	19580	2338	119
1764	24183	2558	105
1768	23348	2512	107
1772	23028	2401	104
1776	20525	1978	96

T A B L E III.

Containing Periods of Eight Years.

Conclud- ing Year of each Period.	Annual Medium of the Total Deaths.	Annual Medium of Deaths by the Smallpox.	In 1000.
1636	11277	318	28
1654	11251	583	51
1662	14862	906	60
1670	27055	991	36
1678	18548	1250	67
1686	22133	1606	72
1708	21023	1048	49
1716	22717	1809	79
1724	26350	2225	84
1732	27062	2230	82
1740	27037	2094	77
1748	25534	1834	71
1756	21939	1889	86
1764	21881	2448	112
1772	23188	2456	105
1776	20525	1978	96

T A B L E IV.

Containing Periods of Twelve Years.

1650	11125	371	33
1662	13802	833	60
1674	24092	1092	45
1686	21065	1473	69
1712	21303	1246	58
1724	25424	2142	84
1736	26909	2208	82
1748	26180	1896	72
1760	21153	2039	96
1772	23519	2490	105
1776	20525	1978	96

T A B L E

T A B L E V.

Containing Periods of Sixteen Years.

Conclud- ing Year of each Period.	Annual Medium of the Total Deaths.	Annual Medium of Deaths by the Smallpox.	In 1000.
1654	11214	450	40
1670	20958	948	45
1686	20340	1428	70
1716	21870	1428	65
1732	26706	2227	83
1748	26285	1969	74
1764	21910	2168	98
1776	22300	2297	103

T A B L E VI.

Containing Periods of Twenty Years.

1658	11637	528	45
1678	21551	1091	50
1712	21635	1390	64
1732	26080	2177	83
1752	25566	1990	78
1772	22265	2298	103
1776	20525	1978	96

T A B L E VII.

Containing Periods of Twenty-four Years.

1662	12463	602	48
1686	22578	1282	56
1724	23363	1694	72
1748	26544	2052	77
1772	22336	2269	101
1776	20525	1978	96

T A B L E VIII.

Containing the Medium of the last Year's Bills of Mortality, compared with that of the same Number of Years immediately preceding, in a regularly increasing Series.

Concluding Year of each Period.	Annual Medium of the Total Deaths.	Annual Medium of Deaths by the Smallpox.	In 1000.	
1776 1775	19048 20514	1728 2669	90 130	One Year each.
1776 1774	19781 21270	2198 1759	111 82	Two Years each.
1776 1773	20148 23163	2292 2230	113 96	Three Years each.
1776 1772	20525 23028	1978 2401	96 104	Four Years each.
1776 1771	21631 22262	2381 2166	110 97	Five Years each.
1776 1770	21655 22945	2261 2333	104 101	Six Years each.
1776 1769	21481 23512	2221 2568	103 109	Seven Years each.
1776 1768	21777 23765	2190 2535	100 106	Eight Years each.
1776 1767	21983 22880	2283 2559	103 111	Nine Years each.
1776 1766	22045 22219	2273 2441	103 109	Ten Years each.
1776 1765	22216 21916	2277 2334	102 106	Eleven Years each.

The body of evidence now adduced in favour of inoculation, amounts I may venture to say to a demonstration, that the charge which has been preferred against it, of spreading the contagion, and increasing the mortality of the small-pox cannot possibly be true. It is a question therefore, which will naturally be asked in this place, to what cause, then, is the increase to be ascribed? I answer it is, perhaps in some measure to be ascribed, to a difference in the number of inhabitants, on the principle before explained; and likewise to the communication between those afflicted with the natural small-pox, and those liable to take the infection, which seems to have been growing greater, as the dread of receiving the distemper has grown less. But as these, and some other causes, which I pass over, have not operated from the commencement of the increase, it is clear, that the principal one is yet to be sought for; and this, I am inclined to believe, will be found in the more frequent recurrence of varilous Epidemics. But, why these should be more frequent at present, than they were

were

were a Century ago, is an inquiry which cannot be prosecuted with any probability of success until we are better acquainted with the nature of the cause from which they originate. That they do not arise from simple contagion has been fully proved; and, indeed, independent of that proof, the regularity of their recurrence in many places, which are never free from contagion, might be sufficient to convince us, that they spring from a very different cause.

In London, variolous Epidemics do not seem to be governed in their visitation by ~~any~~ any fixed period of time; but in the Hague, where the quantity of contagion is probably always as great, in proportion to the number of Inhabitants, as in London, they have been observed to return every five years. In Leyden, the time of their return appears to be the same. In Groningen, it is every five or six years. In Switzerland, every six. In some parts of Norway, every seven, in others, every ten or twelve. And in Bengal, every seven years.

But

But, whatever may be the cause of the increase in question, the only mean of counteracting its operation, which appears to be adequate and practicable, is a *more general* inoculation; the efficacy of which, is plainly evinced by the influence that, this practice even in its *present state*, seems to have had on the bills of mortality.

But to inculcate the use of this salutary art to the affluent class of the Inhabitants of this City, who have long experienced its utility, would be unnecessary; and to recommend it to the poor, without furnishing them with the power of adopting it, would be offering an insult to humanity.

To considerations of this kind, the institution for inoculating the poor of London at their own habitations owes its birth. But, as some of the wisest and best schemes in favour of humanity, have at first been stigmatized as mischievous, or treated as ridiculous, it was not expected that this would meet with a better fate. It has, accordingly, been represented “ as
“ fraught

“fraught with very dangerous consequences to the community,” and attending, by spreading the contagion, to increase the very evil it was designed to lessen.

This is the charge which has been brought against the above mentioned establishment in particular, as well as inoculation in general, a charge, which I flatter myself has been compleatly refuted in the preceding pages. But, that refutation will at once produce an acknowledgement of conviction, and convert opposition into patronage, I am not so sanguine as to expect.

The clamours of prejudice, envy and self interest will, for a time, engage the public ear, but, I trust, that the voice of truth and justice, of humanity and sound policy, will at length prevail; that the practice of inoculation will become universal, and the mortality of the small-pox be nearly annihilated.

F I N I S.



