

Cholera

CHAPMAN (John)

*article on Cholera.
(see page 272.)*

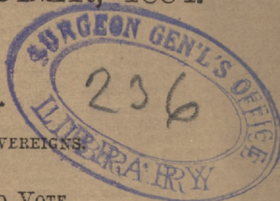
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the old system was swept away at a stroke, and the new Corporations were left untrammelled to carry out the wishes of their constituents. Such was the Bill as it entered the House of Lords; such was not the Bill when it was sent back to the Commons. Unable to save the corrupt old system *en masse* from extinction, they endeavoured to snatch as many brands from the burning as they could. All the freemen were restored to the burgess roll, all the old town clerks were made irremovable, all the existing aldermen were secured the possession of seats in the new Town Councils for life, all the justices who sat on the bench in their corporate capacity were secured fixity of tenure. New aldermen were to be elected for life by the Town Councillors, to form a kind of municipal life peerage in the heart of each corporation. The right to nominate justices was taken away from the Councils, a high property qualification was insisted on for Councillors, and Non-conformist Councillors were excluded from any share in the exercise of Corporation Church patronages. The appearance of the Bill as it emerged from the Upper House created a storm of indignation throughout the country. Ministers were implored to reject it outright, by refusing even to consider the Lords' amendments. This course was strongly pressed upon the Government by Mr. Roebuck. Mr. O'Connell was equally defiant of the Lords, but he thought the best method was to make what concessions they could, and then dare the peers to throw out the Bill. His advice was taken. The Ministers refused to assent to the perpetuation of the town clerks and aldermen of the old system, they refused to enact the Test Act in relation to Church patronage, but they assented, with modifications, to the property qualification, which lasted down to the last Parliament, to the continuance of the freemen, to the loss of the right to nominate justices, and to the institution of aldermen for a term of years. Thus the alderman was saved from extinction by the House of Lords, which renewed the lease of his existence, and enabled him to secure to this day in Liverpool* and elsewhere, a Conservative majority in councils where, but for his presence, power would have passed into the hands of the Liberals.†

The House of Lords [says the same writer] has contributed but little to the constructive legislation of the last half-century. . . . With the exception of the three-cornered constituency it has hardly created anything since the Reform Act of 1832 but the alderman. The alderman may indeed be regarded as almost the solitary monument of the legislative genius of the Upper House.‡

No one can say that in this collision between the Houses the House of Lords showed wisdom or statesmanship, or came out of it with honour or dignity.

A statesmanlike and patriotic Legislature would have been specially anxious to take every possible means to prevent and

* In November, 1883, Liverpool, for the first time, elected a Liberal mayor.

† "Fifty Years of the House of Lords," p. 69 *et seq.*

‡ *Ibid.* p. 68.

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hinder the spread of corrupt practices in the constituencies created by the Reform Act. We use those words to denote as well those Borough constituencies, such, for example, as Gloucester, Northampton, and Bodmin, where the £10 householders were added to the old voters, as those Parliamentary Boroughs, such, for example, as Manchester, Marylebone, Rochdale, and Devonport, which were originated by the Act. This is what a wise and patriotic Legislature would have done, and therefore what the Lords did not do. What has been their conduct in this matter? In the former class of constituencies they contrived to save the freemen from political extinction. "Those constituencies thus inoculated with corruption by the express action of the Lords have never rid themselves of the taint." *

The reformed House of Commons passed a Bill disfranchising the boroughs of this class, which, at the first General Election under the Act, had exhibited the most glaring cases of corruption. The Lords rejected the Bill. The Commons also sent up a drastic Corrupt Practices Prevention Bill. The Lords referred it to a Select Committee, which emasculated the Bill, and, moreover, inserted "clauses giving the peers a right to appoint five of their members to sit with seven members of the House of Commons to try bribery cases under the presidency of a judge." † This impudent attempt of the peers to interfere in the trial of election petitions naturally provoked the resentment of the Commons, and the Bill lapsed. We must not linger over this subject, but content ourselves with referring our readers to "Fifty Years of the House of Lords." ‡ Its author gives the history for that period of the proceedings of the House in reference to electoral corruption, and the facts he states are more than enough to justify the conclusion to which he comes:—"That if corruption has eaten into our democratic institutions the aristocratic branch of our Legislature is largely responsible for the failure of all the remedies devised for its extirpation." §

In 1871 the Ballot Bill first came before the Lords. In debate the usual commonplaces "un-English," "dangerous innovation," and so forth, were repeated; but, in fact, the measure is a return to the ancient manner of voting in this country. Our present mode of voting, not very appropriately or accurately called the Ballot, is in fact anonymous voting. It is not, however, desirable to change the old familiar term. || The

* "Fifty Years of the House of Lords," p. 48.

† *Ibid.* p. 50.

‡ See c. iv. p. 48 *et seq.*

§ *Ibid.* p. 53.

|| The late Mr. Henry Berkeley, M.P., in a resolution, moved towards the close of his long and persistent advocacy of the ballot, substituted for that word the words "anonymous voting."

principle of anonymous voting is no novelty in England. In the Parliament of 1628, which Macaulay calls "the greatest of all parliaments that England had seen in her history," the Committee of Privileges in the case of the Yorkshire election resolved: "That if an elector or freeholder being by the sheriff upon the poll, demanded his name, shall refuse it, he is not disabled to be an elector." The ground of this resolution was the same as that on which Bentham, Grote, James Mill, Berkeley, Molesworth, and all other modern advocates of anonymous voting based their argument—viz., "That it might be inconvenient to electors to have their names set down, because notice might be taken of them to their prejudice,"* or, as two centuries later, a member for Yorkshire more tersely stated it, "There should be no record kept against any man how he votes."†

In 1871 the Lords rejected the Bill, but there were symptoms in the autumn of an agitation against them. In the Session of 1872, therefore, they, with more celerity than dignity, executed a retreat, and, all facts and arguments remaining the same as the year before, passed the Bill by 88 votes to 58. In Committee, however, the majority followed their usual tactics, and rendered the Bill useless by making, by 83 votes against 67, secret voting optional. The Commons remained firm, and again the Lords retreated and rescinded their former vote. They showed their animus against anonymous voting, by limiting the operation of the Bill to seven years.‡ The only effect of which has been to give Parliament the trouble of inserting the Act in the yearly measure for the continuance of expiring laws. "The same animus," says the work to which we are so much indebted, "showed itself the same year in the rejection outright of the proposal to elect School Boards by ballot. The majority, however, was small, and the vote a few years afterwards annulled by the Lords at the demand of a Conservative Government." A notable illustration indeed of what has been well said—that the use of a Conservative Government is that it can tell the Lords to say black is white and be sure of a servile obedient response. The worst enemies of the House of Lords could not wish it to appear in a light more unfavourable to its dignity, its prestige, and its reputation for statesmanship and usefulness, than it has shown itself in dealing with measures of electoral reform.

* *Commons Journal*, April 17, 1628. See the particulars of the Yorkshire Election in Forster's "Life of Sir John Eliot," vol. ii. p. 272.

† Richard Cobden.

‡ "Fifty Years of the House of Lords," p. 56; but if our memory be right the period was ten years.

But it is in measures affecting religious liberty and equality that the House of Lords have chiefly interposed those "petty hindrances," and showed that "petty spite," to which Professor Thorold Rogers refers. Take for instance the question of Jewish Emancipation. The first Bill for that purpose which passed the Reformed House of Commons was carried by a majority of 137 votes. The Lords rejected it by a majority of 50. After a struggle of twenty-five years the Lords submitted to what was dignified by the name of "a compromise." It was proposed that either House of Parliament should be enabled to meet the case of the Jews by a resolution to omit in their case from the oath the words "on the true faith of a Christian." The Lords passed the Bill for this purpose and sent it to the Commons, and at the same time returned the Bill directly admitting the Jews, and assigned as a reason for so doing that "it would be impious to admit a Jew to sit in a Christian assembly." Lord Campbell's somewhat coarse comment on the incongruity and absurdity of this proceeding is well known, but will bear repetition: "It was as much as to say to the Commons 'we know that we should be damned if we agreed to admit a Jew to sit amongst us, but we give you authority to allow Jews to sit among you, and if you please you may do so, and be damned to you.'"^{*} During the twenty-five years' conflict on the Parliamentary disabilities of the Jews, a Bill for admitting them to municipal corporations was rejected by the Lords in 1841, and passed by them in 1845,[†] for no other reason than in 1841 the measure was proposed by the Whig Ministry, and in 1845, a Conservative Ministry invited their lordships to say black is white, and they of course accepted the invitation.

A glaring instance of this "petty spite" was the rejection by the Lords for seven years of the Bill for relieving Dissenters from making the declaration in favour of the Establishment, on which the Lords at the repeal of "The Test and Corporation Acts" had insisted as a qualification for municipal office; and of which Lord Russell said, "it had kept out nobody, and its repeal could therefore admit nobody."[‡] In this case, as in those of the admission of Dissenters to Universities and the abolition of Church Rates, "a long fight came to a close by a complete surrender by the Lords of every position they had undertaken to defend."[§] In each case we may apply to the majority of the House what was said of Sir Robert Peel in reference to his con-

^{*} "Lives of the Chancellors," vol. viii. p. 206.

[†] "Fifty Years of the House of Lords," p. 14.

[‡] "Recollections and Suggestions," p. 58.

[§] "Fifty Years of the House of Lords," c. v.; "Religious Equality," p. 57 *et seq.*; p. 62 *et seq.*

duct on the Catholic question and the Corn Laws. Either they foresaw the necessity of giving up their opposition to the measure or they did not. If they foresaw it, they were wanting in honesty when they persevered in their opposition. If they did not foresee it, they were wanting in wisdom, and are not fit to be entrusted with a veto on the measures passed by the representatives of the people.*

But it is Ireland which, during the last fifty years, has been the principal victim of the crimes and follies of the House of Lords, and to the majority of that House the disturbances and convulsions of that country within that period of time are mainly due. So far back as 1839, O'Connell told the House of Commons: "Though a majority in this House may be disposed to do us something like justice, all your efforts will be frustrated by the other branch of the Legislature," who, in dealing with Ireland, "treat everything of conciliation or justice with contumely and contempt."† "The Irishman," said Macaulay, "has been taught that from England nothing is to be got by reason, by entreaty, by patient endurance, but everything by intimidation."‡

In no part of the United Kingdom was it more desirable and necessary to create Municipal Government than in Ireland. A Municipal Corporation Bill for Ireland on the model of the English measure, passed the House of Commons in 1835 and each following year only to be rejected by the Lords. At length, in 1840, they passed it, but then only "in a mutilated form, with a higher franchise, differing from the franchise in England, so that the brand might still be left upon the country."§ And with the result "that nine-tenths of Irish borough householders outside Dublin remain to this day without that voice in the municipal government of their town which they enjoy as a matter of course when they migrate to an English or Scotch borough."|| Macaulay was guilty of no exaggeration when, in his great speech on Ireland in 1841, he said, "Every Bill passed by the advisers of the Crown for the benefit of Ireland was either rejected or mutilated."¶

We have not space available for the consideration of the long catalogue of woes inflicted on Ireland by the Tory majority in the House of Lords. We must, however, refer to their treat-

* See Lord Russell's "Recollections and Suggestions," p. 240, and Trevelyan's "Life of Macaulay," vol. ii. p. 455.

† Quoted in "Fifty Years of the House of Lords," p. 20-23.

‡ *Ibid.* p. 34.

§ Mr. Gladstone's speech of Aug. 30, p. 18.

|| "Fifty Years of the House of Lords," p. 29.

¶ "Miscellaneous Writings and Speeches," p. 648. Edition 1871.

ment of one question because it remains a question of practical politics.

In 1843, what was known as the Devon Commission, was appointed to inquire into the Irish Land Question, and after two years' inquiry it reported strongly in favour of legislation to secure the tenant compensation for his improvements. What followed, Mr. Gladstone shall relate in his own words:—

The question of Irish land was one on which an enlightened Conservative Government forty years ago perceived the necessity of making great changes, and it was hoped that, as a Conservative Government, it might perhaps persuade the majority of the House of Lords to listen to its voice. In the year 1845 Lord Derby, the father of the present Lord Derby, being then a member of the Government of Sir Robert Peel,* introduced a most important Bill into the House of Lords, for the purpose of giving compensation to Irish tenants for their improvements. And it is perfectly possible that if at this early date that mild and moderate measure had been passed, we to this hour never should have heard a word of the land question in Ireland. But what happened? Although Lord Derby spoke on the part of a Conservative Government, proprietary influence and class influences in the House of Lords were too strong for him, and he was compelled, most reluctantly compelled, to withdraw his Bill.†

And this, although the Commission had reported "that no single measure could be better calculated to allay discontent and to promote substantial improvements throughout the country."‡

Lord Aberdeen's Government, in 1853 and the following year, unsuccessfully attempted to induce the Lords to agree to a Tenant Compensation Bill for Ireland; to the principle of such a measure every leading statesman had given an avowed sanction, but a distinguished Conservative member, Sir J. Napier, regretfully admitted, "It is notorious that the House of Lords will pass no such measure, and that for a Government to propose it to them or pretend to support it is an imposture and a sham."§ It was not until twenty-five years had elapsed since the report of the Devon Commission that Mr. Gladstone, during his first administration, undertook to give legislative effect to its recommendation. "The Irish Land Act of 1870" was mutilated by the Lords in committee. Mr. Gladstone felt that their so-called amendments would, and were intended to do, more to mar than mend the measure, but he accepted most of them rather than

* It will be remembered that Mr. Gladstone was at that time a member of the Peel Government.

† Speech of Aug. 30, 1884, p. 18 *conf.*

‡ "Fifty Years of the House of Lord," p. 10.

§ Quoted in "Fifty Years of the House of Lords," p. 12.

sacrifice the Bill; experience showed the whole tendency of the Lords' amendments was in the wrong direction.*

Their next action in the Irish Land Question Mr. Gladstone shall describe: "In 1880 we passed through the House of Commons a Bill granting compensation for disturbance in Ireland, which, I believe, would have effectually checked and moderated the tremendous disturbances and convulsions of that country. That Bill was unfortunately lost in the House of Lords."† We quote with full internal assent and consent the remarks of the author to whom we are so much indebted. "To that vote can be traced the excessive exasperation of the tenants against their landlords, which enabled Mr. Parnell to make the Land League supreme in Ireland, and to intensify those feelings of national animosity which it has been the labour of generations to efface."‡

When, in 1845, Lord Derby abandoned his Irish Land Bill, Lord Brougham might have repeated to his peers the warning he gave them in the Reform debate of October, 1831. "Hear the parable of the Sybil: for it conveys a wise and wholesome moral;" and then, after applying the old legend to show that the consequence of rejecting the Reform Bill would be an increase in the popular demands, he concluded: "What may follow your course of obstinacy, if persisted in, I cannot take upon me to predict, nor do I wish to conjecture. But this I know full well, that sure as man is mortal, and to err is human, justice deferred enhances the price at which you must purchase safety and peace; nor can you expect to gather in another crop than they did, who went before you, if you persevere in their utterly abominable husbandry of sowing injustice and reaping rebellion."§

Mr. Gladstone accurately describes the consequences of the rejection of the Bill of 1845.

See the consequences that have flowed from that deplorable action. We are told that the influence of the House of Lords should be a corrective influence. Was that a corrective influence? Was that an influence for the purpose of moderating the action of a popular principle? No, it was a narrow view which declined and refused all just reforms, and the refusal of which, so far from leading to moderation, has led to the necessity for the adoption of vast changes in Ireland,

* "Fifty Years of the House of Lords," chap. i.: "The Irish Land Question."

† Speech of Aug. 30, p. 19.

‡ "Fifty Years of the House of Lords," p. 19.

§ Speech on Second Reading of Reform Bill, Oct. 7, 1831. Works, edition 1873, vol. x. p. 384.

which naturally are the causes of great complaint to the same class of persons who applauded the rejection of Lord Derby's efforts in 1845.*

We might much enlarge our list of the malfeasances and non-feasances of the House of Lords, but we have said enough to justify the conclusion which Mr. Gladstone tersely expressed: "I cannot say that the legislative action of the majority of the House of Lords has for the last fifty years been a benefit or a blessing to the country."†

We are reminded by Mr. Gladstone that "it is often said, and said by clever men, that the purpose of the House of Lords is to represent, not the fleeting opinions of the people, not the passion of the moment, but the permanent solid convictions of the people."‡ The real state of the case, according to a late distinguished statesman of moderate views and judicial habit of mind, Sir G. Cornwall Lewis, is this: "It might indeed have been expected that the House of Lords would have redeemed its adherence to the interests of its order by its exemption from popular errors and popular fanaticism. Unhappily this has not been the case; on the contrary, it seems to have sought to atone for its maintenance of the interests of the aristocracy by embracing the prejudices of the democracy. Thus it has too often happened that when the people have been right, the House of Lords has been oligarchical, and that where the people have been wrong the House of Lords has been democratic."§

The futile Ecclesiastical Titles Act is a remarkable illustration of the truth of this description. Speaking of the Lords' resistance to Catholic Emancipation, Parliamentary Reform, and Free Trade, the same accurate thinker remarked: "It speaks little for the wisdom of the leading statesmen who had made so long and so stubborn a fight in defence of the established institution, and rendered each of these three great settlements a capitulation to a victorious enemy rather than a grant of acknowledged right. In each case the Legislature had the appearance of passing a wholesome measure upon compulsion, not because it was wholesome, but because it could no longer be withheld."||

No reasonable man can doubt that the Franchise Bill will add another illustration to those given by Sir G. C. Lewis. With the experience of 1832 before them, it is singular the

* Speech of Aug. 30, p. 18, 19.

† *Ibid.* p. 17.

‡ *Ibid.* p. 16.

§ "Essays on Administrations, 1783 to 1830," p. 468.

|| *Ibid.* p. 461.

Conservative leaders of 1884 should have adopted tactics as unwise as Lord Lyndhurst's celebrated amendment of 1832, and inspired some of their supporters with consternation and alarm. The very words of Lord Cairns' amendment, as Earl Grey points out, "take away the only reason the peers have for rejecting the Bill."* Another distinguished member of their House, Lord Sherbrooke, thus expresses his view of their action :

The House of Lords have been suddenly converted ; they profess unbounded zeal for a cause which they have hitherto detested. Never was conviction more sudden : what they abhorred yesterday, they have embraced to-day. It would be ridiculous to pretend for a moment that this astonishing change is the result of real conviction. I should have thought that the precedent of 1832 would have been a sufficient warning. At that time it was believed that, had the peers remained intractable, a creation of peers might be resorted to, and the belief was very general that this had its effect in preventing a prolonged resistance. To me it seems almost incredible that statesmen should be found so rash as to draw down upon themselves even the threat or the possibility of so lamentable an occurrence.†

And yet Lord Sherbrooke, when in the House of Commons, had experience of the agility with which Conservatives change votes on the Franchise question. In the Committee on the Reform Bill of 1867, contrasting the Conservative policy of 1866, of rejecting a £6 franchise, with their policy of 1867, of proposing Household Suffrage, he said : "Just as we are told that the world woke one morning and wondered to find itself 'Arian,' the House have awakened and marvel to find themselves reformers." And on the third reading of the Bill, he thus taunted its Conservative supporters : "You remind me of the Bishop's exclamation to Clovis : *Adora quod combussisti, combure quod adorasti.*" Both these remarks are equally applicable to the recent proceedings of the Conservative majority in the House of Lords.

The two Houses are now in a deadlock. The Marquis of Salisbury protests against the doctrine that, "whenever the Ministers of the Crown and the House of Commons are agreed, the House of Lords is bound to yield ;" and expresses his hope that the Lords will never yield to that doctrine ; and Earl Granville "protests against the converse doctrine, that the House of

* Letter to the editor of the *Times*, as quoted in debate by Earl Granville : see "The Lords and the Franchise Bill," p. 99.

† Letter to an inhabitant of Manchester, published in several of the daily papers.

Commons is always to yield whenever the House of Lords thinks differently." *

We agree with Mr. Gladstone

that at any rate in such a case as this, in a representative country, when issue has been deliberately joined, the representative chamber ought to prevail, and must prevail.†

That the House of Lords ought not to challenge the nation upon a question like the extension of the franchise, and ought not to push its opinions to extremes;‡ [and that] the worst foe of the hereditary principle is the man who places it in direct conflict, brow to brow, with the elective and representative principle.§

We commend to Lord Salisbury's serious attention the following weighty words of a former leader of the Lords, who certainly was free from all democratic tendencies. In moving the second reading of the Corn Law Repeal Bill (1846), which, like the Franchise Bill, had been recommended by the Speech from the Throne, and had passed the House of Commons by large majorities, the Duke of Wellington said :

This measure, my Lords, was recommended by the Speech from the Throne, and it has been passed by a majority of the House of Commons, consisting of more than half the members of that House. But my noble friend said that that vote is inconsistent with the original vote given by the same House of Commons on this same question, and inconsistent with the supposed views of the constituents by whom they were elected. But, my Lords, I think that is not a subject which this House can take into its consideration—for, first, we can have no accurate knowledge of the fact; and, secondly, whether it be the fact or not, this we know, that it is the House of Commons from which this Bill came to us. We know by the votes that it has been passed by a majority of the House of Commons; we know that it has been recommended by the Crown; and we know that if we should reject this Bill it is a Bill which has been agreed to by the other two branches of the Legislature, and that the House of Lords stands alone in rejecting this measure. Now, that, my Lords, is a situation in which I have frequently stated to your Lordships you ought not to stand. It is a position in which you cannot stand, because you are entirely powerless; without the House of Commons and the Crown the House of Lords can do nothing. You have vast influence on public opinion, you may have great confidence in your own principles, but without the Crown or the House of Commons you can do nothing—till the connection with the Crown and the House of Commons is revived, there is an end of the function of the House of Lords.||

* Lords' debate, Aug. 5, 1884.

† Speech of Aug. 30, p. 9.

‡ P. 17.

§ P. 15.

|| We quote from the *Liverpool Daily Post*, July 29, 1884. The speech will be found in Hansard for 1846.

The Lords, fortunately for themselves and the country, listened to the Duke's wise counsels and passed the Bill. In the present depressed state of trade and manufactures, were the old corn and provision laws in force the agitation against the House of Lords would not be so moderate and good-tempered as it is now.

"The question," says M. Daryl, "is often asked if the House of Lords is likely to continue and to survive the approaching era of universal suffrage. That is a question which it would be premature to decide."* But the Lords themselves have forced us to consider it. Mr. Bright lately expressed his conviction "that the time is near, or has come, when the nation must decide the future position of the House of Lords."† And he has also reiterated the conviction to which he first gave utterance in 1858:—"We know, everybody knows, nobody knows better than the peers, that a house of hereditary legislators cannot be a permanent institution in a free country. For we believe that such an institution must, in the course of time, require essential modification."‡ So deeply rooted in his mind is this conviction that, when he visited Dunrobin Castle and saw the vacant spaces left in the walls for the cognizances of unborn dukes and duchesses, he inquired, with good-humoured sarcasm, whether the family really imagined it likely that these vacant spaces would be filled.§ A similar opinion to that of Mr. Bright had long before been come to by an equally far-seeing, and in the truest sense of the word, equally Conservative statesman—Lord Macaulay. "I am quite certain," he writes from India || to Mr. Ellis, "that in a few years the House of Lords must go after old Sarum and Gatton. What is now passing is mere skirmishing and manœuvring between two general actions. It seems to be of little consequence to the final result how these small operations turn out. When the grand battle comes to be fought, I have no doubt about the event." And this feeling grew so strong that he addressed to his friend, the third Marquis of Lansdowne, "a carefully reasoned letter—a State paper in all but form, urging the imminent perils that threatened a Constitution in which a reformed House of Commons found itself face to face with an unreformed House of Lords, and setting forth in detail a scheme for reconstructing the Upper Chamber on an election basis."¶

* "Public Life in England," p. 157-8.

† Message to a meeting at Middlesborough. See *Daily Telegraph*, July 28, 1884.

‡ "Speeches," popular edition, p. 293.

§ "Reminiscences," by Lord Ronald Gower, F.S.A., vol. i. p. 48.

|| *Ubi vide post*, no date is given, but apparently it was written 1835 or 1836.

¶ Trevelyan's "Life of Macaulay," vol. ii. p. 57, 58.

If Mr. Trevelyan can for a time escape from the pangs of that form of martyrdom which men call the Irish Secretaryship and give to the world this State paper of his uncle's, it would be a gift equally opportune and valuable.

The question of the abolition or reform of the House of Lords is now well within the range of practical politics. Spite of the present irritated feeling against that House we do not think the people are prepared for or desire its abolition. We are struck by the fact that at the recent yearly meeting of the Trades Union Congress, the Congress—if the report we have seen be correct *—unanimously agreed to a resolution expressing satisfaction with the Franchise Bill, and regret at its rejection, and assuring the Government of the hearty support of the Congress in any steps they may take to secure its passing, but the Congress rejected a Radical amendment in favour of the abolition of the House of Lords, while a Conservative amendment expressing regret that the Government failed to introduce a Redistribution Bill along with the Franchise Bill did not find a seconder. If not an universal, it is a very general truth, that a second chamber is found to be an essential part of Parliamentary institutions. It is so in America, not only in the Federal Government, but in each individual State. It is so in each of our great monarchical republics in Australia. On the Continent it is so, not only in Belgium and Italy, but also in Republican France. Writing in 1873, Professor Thorold Rogers expressed this opinion:—"No one would advise that the English nobility should be disfranchised utterly, should be debarred all political rights. It is better therefore to leave them where they are most harmless, and where, as events progress, they will wield less and less of real power. It may be hereafter necessary to limit the power of veto which they possess, and which they occasionally use vexatiously and factiously." † That necessity, we think, has now arisen. The additional resolution passed at the Franchise Demonstration at Manchester of July 26 expresses the general opinion, "That the power of veto on legislation hitherto exercised by the House of Lords is an intolerable anomaly, is a constant hindrance to good and necessary legislation, and ought to be abolished." Other means of escape from the present difficulty are suggested. Mr. Arthur Arnold ‡ argues that the Queen should give Mr. Gladstone the same power of creating peers which in 1832 William IV. gave Earl Grey. Certainly the

* In the *Daily Telegraph*, Sept. 10, 1884, but the reports in that paper are often grossly unfair, still more often grossly inaccurate.

† "Cobden and Political Opinion," p. 267.

‡ *Fortnightly Review*, Aug. 1884.

Conservatives cannot reasonably object to this proposal. In the reign of Anne :—

On one occasion only did the House of Lords act directly counter to her views, and to those of the Ministry which enjoyed her complete confidence, and then the majority of the House of Lords was summarily swamped by the creation of twelve new peers. No other Ministry has dealt a blow at the independence of the Upper House of Parliament, or set a precedent for paralyzing the then majority, equivalent to the blow struck and the precedent set by the Tory Ministry which enjoyed the confidence of Queen Anne, whose own sympathies and action were in perfect accord with those of the uncompromising Tory party.*

Referring to this creation of peers, and to the Bill for limiting the number of the House of Lords, introduced in consequence of it, Lord Macaulay says :—

The theory of the English Constitution, according to many high authorities, was that three independent powers, the Sovereign, the Nobility, and the Commons, ought constantly to act as checks on each other. If this theory were sound, it seemed to follow that to put one of these powers under the absolute control of the other two, was absurd. But if the number of peers were unlimited, it could not be well denied that the Upper House was under the absolute control of the Crown and the Commons, and was indebted only to their moderation for any power which it might be suffered to retain.†

A sufficient creation of peers would of course remove the present dead-lock, but the objection to such a creation is, that, if once the precedent be set, each successive Ministry, whose measures may be defeated in the Upper House, will be justified in following the precedent, and creating a majority for their necessity until, to revive an old saying, "the House of Lords would have to meet on Salisbury Plain." Lord Lyndhurst‡ propounds a theory of a House of Lords that shall be at one and the same time hereditary and representative. This he would do by converting the English, Scotch, and Irish Peerages into one Electoral College, which should elect representatives out of their own body, and that these elected peers should be the House of Lords. The country's experience of the working of the representative principle in the case of the Irish and Scotch repre-

* Article "Bolingbroke," in the *Times*, Aug. 5, 1884, attributed by rumour to Mr. G. O. Trevelyan, M.P.

† "Life and Writings of Addison." Essays, edition 1874, p. 772. We understand Lord Macaulay to be narrating the opinion of the time on the question, not giving or even intimating his own.

‡ In the *Nineteenth Century* for August 1884.

sentative peers does not lead us to wish for its further extension. A House of Lords elected by the peers would only represent the interests, privileges, and prejudices of their class as strongly as, or more strongly than, the present House, and the altered state of things would be as bad or worse than the existing.*

The pressing and practical question, however, is not a general Reform of the House of Lords, but how to get the Houses out of the dead-lock into which they have been brought by Lord Salisbury. The best and simplest way would be not by direct legislation but, as in the case of the suspension of voting by proxy in the House of Lords,† by the tacit operation of what Dr. E. A. Freeman calls "that delicate system of understandings which forms our unwritten Constitution," the adoption of Mr. Bright's suggestion that the Lords should cease to exercise their veto on any measure sent to them for a second time by the Commons. If this suggestion be adopted, Mr. Bright will again vindicate the right he fairly claims to be considered a Conservative element in our politics.

We would respectfully call Lord Salisbury's attention to the letter written in 1845 by the Duke of Wellington to the late Earl of Derby, on his secession from the Peel Ministry in order to lead the opposition to the repeal of the corn laws. The Duke, deprecating the Earl's intended opposition, describes the manner in which he had led the Conservative majority in the Lords:—

For many years, indeed from the year 1830, when I retired from office, I have endeavoured to manage the House of Lords upon the principle on which I conceive that the institution exists in the Constitution of the country, that of Conservatism. I have invariably objected to all violent and extreme measures, which is not exactly the mode of acquiring influence in a political party in England, particularly one in opposition to Government. I have invariably supported Government in Parliament upon important occasions, and have always exercised my personal interest to prevent the mischief of anything like a difference or division between the two Houses, of which there are some remarkable instances, to which I will advert here, as they will tend to show you the nature of my management, and possibly, in some degree, account for the extraordinary power which I have for so many years exercised, without any apparent claim to it. Upon finding the difficulties in which the late King William was involved by a promise made to create peers—the number, I believe, indefinite—I determined myself, and I prevailed upon others, the number very large, to be absent from the House in the discussion of the last stages of the Reform Bill, after the negotiations had failed for the formation of a new Administration.

* See on this subject the remarks of Professor Thorold Rogers, in "Cobden and Political Opinion," p. 266 *et seq.*

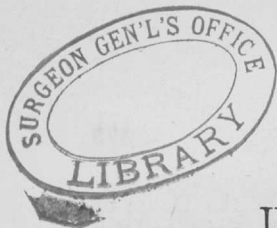
† In 1868, see "Reform of the House of Lords," p. 5.

This course at the time gave great dissatisfaction to the party; notwithstanding that, I believe it saved the existence of the House of Lords at the time, and the Constitution of the country. Subsequently, throughout the period from 1835 to 1841, I prevailed upon the House of Lords to depart from many principles and systems which they, as well as I, had adopted and voted—on Irish tithes, Irish corporations, and other measures—much to the vexation and annoyance of many. But I recollect one particular measure, the union of the provinces of Upper and Lower Canada, in the early stages of which I had spoken in opposition to the measure, and had protested against it, and in the last stages of it I prevailed upon the House to agree to and pass it in order to avoid the injury to the public interests of a dispute between the Houses upon a question of such importance. Then I supported the measures of the Government, and upheld a servant of the Government, Captain Elliott, in China; all of which tended to weaken my influence with some of the party; others, possibly a majority, might have approved of the course I took. . . . Upon the important occasion and question now before the House, I propose to endeavour to induce them to avoid involving the country in the additional difficulties of a difference of opinion, possibly a dispute, between the House on a question in the decision of which it has been frequently asserted that their Lordships had a personal interest, which assertion, however false as affecting each of them personally, could not be denied as affecting the proprietors of land in general.*

This letter shows that the Duke, towards the close of his life, had attained some degree of that civil wisdom of which, according to his brother, the Marquis Wellesley, he had in his earlier days absolutely none. We commend the Duke's example and his wise counsels to the serious and careful consideration of the Marquis of Salisbury. It is not too late for him to take heed to his ways, and make for his party a safe if not an honourable or graceful retreat from the untenable position into which he has unfortunately and mischievously led them.†

* We quote from the *Liverpool Daily Post*, *ubi supra*. The letter will be found in "The Wellington Despatches."

† On the subject of this article *conf.* WESTMINSTER REVIEW, N.S., No. CVII., July, 1878: article, "The House of Lords," and No. CXXIV., article: "The Jubilee of the First Reform Act."



INDEPENDENT SECTION.

[Under the above title a limited portion of the WESTMINSTER REVIEW is occasionally set apart for the reception of able Articles, which, though harmonizing with the general spirit and aims of the Review, may contain opinions at variance with the particular ideas or measures it advocates. The object of the Editors, in introducing this department, is to facilitate the expression of opinion by men of high mental power and culture, who, while they are zealous friends of freedom and progress, yet differ widely on special points of great practical concern both from the Editors and from each other.]

(THE NON-CONTAGIOUSNESS, CAUSATION, AND) SCIENTIFIC TREATMENT OF CHOLERA.

1. *Diarrhœa and Cholera*: their Nature, Origin, and Treatment through the agency of the Nervous System. By JOHN CHAPMAN, M.D., M.R.C.P., M.R.C.S. Second Edition, enlarged. 8vo. London: 1866.
2. *Cases of Diarrhœa and Cholera* treated successfully through the agency of the Nervous System, chiefly by means of the Spinal Ice-bag. By JOHN CHAPMAN. London: 1871.
3. *A System of Medicine*. Edited by J. RUSSELL REYNOLDS, M.D., F.R.S. London: 1870. Article on "Epidemic Cholera," by EDWARD GOODEVE, M.B.
4. *The Science and Practice of Medicine*. By WILLIAM AITKIN, M.D. Sixth Edition. London: 1872. Article, "Malignant Cholera."
5. *A History of Asiatic Cholera*. By C. MACNAMARA, F.C.U. London: 1876.
6. *Further Reports by Surgeon-General Hunter on the Cholera Epidemic in Egypt*. Presented to both Houses of Parliament. London: 1883.
7. *Bulletin de l'Académie de Médecine*. No. 30, Séance du Juillet 1884, et No. 35, Séance du 26 Août 1884. *Le Choléra de 1884*—Toulon, Marseille. Par M. JULES GUÉRIN.
8. *Cholera and its Bacillus*. By ROBERT KOCH, M.D. An Address delivered before the Imperial German Board of Health at Berlin. Translated from the *Deutsche Medicinische Wochenschrift* for the *British Medical Journal*, Nos. 1235 and 1236. London: 1884.

IN India, the so-called "home" and "source" of cholera, where it is generally believed to be non-contagious, its presence produces no panic; the healthy relatives or friends of the sick minister to their needs without fear of becoming infected; the

physicians and nurses, on whom they are dependent for treatment and care, have as little apprehension of danger, when attending to them, as they feel when attending to other patients. Victims of cholera are not isolated, but, on the contrary, it is even customary to treat them in the same wards in which sufferers from other diseases are treated, and without any evil result. The clothes of such patients are not burnt, or even fumigated; all persons are free to go in, or go out, of cholera districts without let or hindrance; and the practice of disinfecting passengers at railway stations and the establishment of *cordons-sanitaires* are alike unknown.

In Europe how widely different are the beliefs and consequent customs which prevail! The doctrine that cholera is contagious is everywhere held and taught authoritatively by physicians, it suffuses the whole medical press, is generally echoed by the lay press of every country, and is accepted without question as an article of faith by all European peoples. While the last great discovery of the proximate cause of cholera—the comma-shaped microbe of Dr. Koch—is already subsiding into the Lethe which, during the last fifty years, has engulphed hundreds of parasitic fungi which had played a like rôle, the hypothetical *cholera contagium* holds its own, and is spoken of undoubtingly as a zymotic (fermentative) agent, capable of multiplying “in a ratio at least as great” as that of small-pox; it is dogmatically affirmed to travel from one district or from one country to another; to proceed, as a general rule, along the lines of human intercourse and, especially, along the courses of rivers; to be sometimes favoured by the wind, but not infrequently to advance in opposition to it; and, most certainly, to avail itself of ships in order to pass from one country to another separated by sea. When cholera appears in any town or village its inhabitants are seized with panic and seek safety in the most precipitate flight; contact with victims of the disease is especially dreaded as if fraught with the utmost danger; they are rigorously isolated from the healthy; their clothes are burnt and, sometimes, also, even the things they may have handled,* or the carriage in which they may have been conveyed. *Cordons-sanitaires*, transgressors of which run the risk of being shot,† are often placed round the “infected”

* In the letter of the *Times*' correspondent published on the 12th ult., he says: “A friend of mine who has just returned from Naples, told me that he saw a fruit woman tumble off her seat in the Mercato, and as she was carried away a bonfire of her chair, stand, and fruit was made on the spot.” Railway carriages conveying passengers, who while in them were seized with cholera, were this year burnt at Spezia.

† At Spezia, the soldiers were ordered to shoot any one attempting to pass the cordon.—*Times*, Sept. 10, 1884.

districts ; letters in transit are stopped and fumigated ; if the rigour of the *cordon* is so far relaxed as to allow trains to pass the passengers are fumigated also ; and, finally, in the hope of preventing the importation of the disease from one country to another, the several nations of Continental Europe enforce the useless and vexatious practice of quarantine. Every reflecting person who duly considers the wide difference here indicated between the beliefs and practices of India on the one hand, and those of Europe on the other, in respect to cholera, must be struck with astonishment, as it appears to me, by this marvellous discrepancy. And yet how few European physicians seem ever to consider it even, and how rarely any one of them is led to doubt the truthfulness of his own convictions, and to ask himself whether there be any real grounds for those precisely opposite convictions which prevail in India ! At the sitting of the French Academy of Medicine, August 26 last, a report was read concerning a proposal to organize a Commission for the study of the phenomena of cholera. In this report occurs the following statement :—"This [proposed] inquiry, it must be borne in mind, is not inspired by a preconceived doctrinal idea ;" and yet in this same report it is said that two of the principal objects of the Commission would be "to determine the length of the period of incubation of cholera, and whether it has been imported into the towns and villages of France, until then free from it, by man or by contaminated objects." It is evident from these words that the Commission of the French Academy of Medicine has started on its course of investigation already loaded with three assumptions—viz. (1) that cholera is the consequence of a specific poison ; (2) that this poison is zymotic, and therefore has a period of incubation ; and (3) that it is imported from without into towns and villages previously free from it ! That a body of scientific men, while holding firmly to these assumptions as if they were proven verities, can complacently assure itself that it "is not inspired by a preconceived doctrinal idea," is at once an astounding example of self-delusion, and an instructive proof of the great depth and strength of the current of prejudice which has to be drained off before European physicians as a body will become able to investigate the nature of cholera in accordance with that scientific method now recognized as the only sure guide to truth.

I believe I am justified in stating that there is only one member of the French Academy of Medicine who, in studying the etiology of cholera, exemplifies the method just mentioned. In doing so he has so distinguished himself that, were I writing for French readers, they would know to whom I refer, even if I abstained from naming the indefatigable, eminent, and venerable

Jules Guérin, whom the Academy, influenced, probably, "by a pre-conceived doctrinal idea," excluded from the Commission now conducting its inquiry on the assumption that the questionable propositions named above are axiomatic truths. But, notwithstanding this assumption by the authoritative medical body which directs the thought and moulds the belief of France on medical subjects, I venture to re-open the question—Is cholera contagious? On the answers which are given to this question hang great practical consequences. All European Governments, advised by their several medical counsellors, answer it, through their accredited organs and by their actions, in the affirmative; the Government of India, also advised by its medical counsellors, whose minds are most informed by experience of the matter, and who therefore may fairly be presumed to be the most competent judges, answers it in the negative. In thus advising the Government of India, Anglo-Indian physicians rely exclusively on experience; for up to the present time no investigator of the nature of cholera, where it may be most continuously observed, has been able to reveal to us in what consists its essence and cause. Sir J. Fayrer says that, notwithstanding his long experience, he is "totally ignorant of the nature of the disease;" and Professor Goodeve remarks, "it must be confessed that we do not know what is the exciting cause of cholera." Believing myself able to explain the mystery, to show how every symptom of the disease is produced, to supply an intelligible *rationale* of the known facts connected with it, and to present decisive proofs, both implicit and explicit, that cholera is not contagious, it seems to me especially expedient at the present time that I should lay my conclusions before the public, together with an exposition of the facts and arguments on which they are based. Compared with the magnitude of the subject, that exposition will necessarily be extremely brief; and, inasmuch as many physiological and pathological facts which it would mention in detail, were it addressed to medical men, must here be either ignored or merely referred to, it will of course be very defective. I hope, however, that by using language as simple and untechnical as I can command, to render the argument I am about to expound easily apprehensible by the general reader.

It is often observed that our first impressions of persons with whom we become acquainted are the truest, and scarcely less often experience justifies the observation: when English physicians first became acquainted with cholera in India, where it presents itself in its most terrible aspects, and before they had formulated any theories concerning it, they were most deeply impressed by its spasmodic character; and hence, regarding the

nervous system as primarily affected, they designated the disease *cholera spasmodica*. Since then pathologists have made a wide circuit, searching in every other element of the body for the essential nature of the malady; but they have not found it, and I shall endeavour to show that those first impressions of Anglo-Indian physicians, which originated the name *cholera spasmodica*, were true—that, in fact, cholera is essentially a disease of the nervous system. But before making this attempt, I must premise a few words concerning the nervous system itself.

In man, as in all the higher animals, there are, indeed, two nervous systems—the cerebro-spinal and the sympathetic. The former comprises the brain and spinal cord, together with the forty-three pairs of nerves which are given off from them. It is by the intervention of cerebro-spinal nerves that our voluntary muscles are brought into action, that the various glands and glandlets throughout the body are made to secrete, and that the manifold impressions of all kinds which are made on the peripheral ends of our sensory nerves are conveyed to the nervous centres—the spinal cord and brain. The Sympathetic nervous system consists, chiefly, of a series of little masses of nervous matter called ganglia (knots), arranged along each side of the spinal column, and connected by intermediate nerve filaments so as to form two knotted cords. These extend from the upper part of the neck along the spinal column to its lowest extremity called the “coccyx.” This ganglionic system communicates with every pair of cerebro-spinal nerves as they emerge from the spinal cord, and also unites with cerebro-spinal nerves to form numerous “plexuses,” from which branches are distributed to all the organs contained in the thoracic, abdominal, and pelvic cavities.

A striking and especially interesting feature in the distribution of the force emanating from the Sympathetic is the great amount of it which is supplied to the heart and blood-vessels (chiefly arterial) throughout the body. Every tube of the arterial system, beginning with the heart and terminating in the minutest arterial twigs, is surrounded by delicate, clinging filaments from the sympathetic ganglia in much the same manner as ivy twines itself around the stems and branches of trees. The functions of this remarkable assemblage of ganglionic nerve-centres long remained a mystery, and, in many respects, remains a mystery still. But, happily, its chief function, at all events, has been revealed; were it not for this revelation the essential nature of cholera must have continued unknown and unknowable.

The great and lasting honour of discovering and of demonstrating the main function of the Sympathetic was reserved for

that profoundly sagacious and pre-eminent physiologist, the late Professor of Physiology at the Collège de France, Claude Bernard. Every artery is surrounded by a series of muscular rings so intimately joined together as to form a continuous muscular tube—called the muscular coat—which formerly was supposed by its elasticity to aid in propelling onwards the successive waves of blood originated by the heart. In 1851 Bernard proved that the involuntary muscles constituting the muscular coat of arteries are as thoroughly dominated by, and subservient to, nerve-force as are the muscles which are subject to mental control (those of our limbs, for example) and which execute our volitions; and that the nerve-force which governs these involuntary arterial muscles emanates from sympathetic centres, and is distributed to those muscles by sympathetic nerves. When endeavouring to find out in what way the nervous system contributes to the generation of animal heat, Bernard divided the sympathetic, or ganglionic, cord in the neck of a living rabbit. Immediately afterwards, on the side on which the nerve had been divided, the blood-vessels of the conjunctiva (the mucous membrane covering the front of the eye and lining the eyelids) and of the ear became visibly and greatly distended; the temperature of the face and head on the same side rose *seven degrees centigrade* (about 12° F.) above that of the sound side; sensibility was increased, and, generally, the phenomena of an increase of vital action presented themselves.

In 1852 Brown-Séquard, the distinguished compeer of Bernard, and now his successor as Professor of Physiology at the Collège de France, first, and afterwards Bernard and the English physician, Dr. Augustus Waller, galvanized the upper part of the severed ganglionic cord, and found that the effects produced by doing so were the reverse of those consequent on its section—the blood-vessels contracted, the quantity of blood was lessened, the temperature was lessened, and generally the phenomena of a decrease of vital action presented themselves. In fact, galvanization of the Sympathetic can produce contractions so vigorous of the blood-vessels subject to the part of the nerve galvanized as to suspend or arrest almost wholly the circulation of the blood in them. Owing to its wonderful power of contracting arteries, it can render the parts supplied by those it constricts pale and bloodless; it can retard or arrest textural nutrition, and, therefore, the organic functions; and, as by arresting textural nutrition, it arrests those chemical processes which form an essential part of the changes associated with that nutrition, and in consequence of which heat is evolved, it can cool the body to the extent even of rendering it nearly as cold as a corpse. Considering the last-named results of the action of the Sympathetic, Bernard named it the *Frigorific* nerve.

The foregoing very imperfect, because necessarily very brief, explanation in respect to the nervous system will, I hope, enable my readers to understand without difficulty the following exposition of the nature, or immediate cause, of cholera.

During the last half-century many students of cholera have ascribed it to disorder of the nervous system. In 1831, Dr. G. H. Bell, in his work, "*Cholera Asphyxia*," endeavoured to prove that the disease is due to a morbid state of the Sympathetic. Several other Anglo-Indian physicians, impressed especially by the spasmodic phenomena of the disease, ascribed it, as already mentioned, to disorder of the nervous system. In 1832 a French physician, Dr. L. Auzoux, published the doctrine "that cholera is to the great Sympathetic what epilepsy is to the brain." Dr. Davey, in his work on the "*Ganglionic Nervous System*," published in 1858, claims to have shown that cholera is due to a morbid state of that system. The eminent English physician, Sir William Gull, refers the symptoms of the disease to "an early and severe depression of the ganglionic nervous centres;" Dr. Copland, in his "*Medical Dictionary*," expresses a like opinion; and Dr. Goodeve, one of the most recent of the English authoritative writers on cholera, says that the state of the lungs and intestines implies that the nervous system is under a morbid influence. But though the authorities in favour of the hypothesis that, to a large extent at least, cholera is a disorder of the nervous system, are numerous and weighty, all of them ascribe the disorder exclusively to the action of the Sympathetic: the rôle of the cerebro-spinal system is wholly ignored, and while several of these pathologists are of opinion that the sympathetic nervous system is profoundly depressed or exhausted, no one of them explains in what consist the several links in the chain of causation between the alleged disorder of this nervous system and the manifold symptoms of the disease.

The doctrine which I believe affords a complete and consistent explanation of all the seemingly mysterious phenomena of cholera may be thus expressed: All the symptoms of cholera are due to simultaneous and abnormal superabundance of blood in, and excessively preternatural activity of, both the spinal cord and the sympathetic nervous centres.

In my work on Cholera, named at the head of this article, I have enumerated thirty-eight characteristic symptoms of the disease; and of these twenty-one are, as I clearly show, produced by morbidly excessive activity of the spinal cord. In the early stages of the disease *all* the secreting organs are preternaturally active: the renal, hepatic, pancreatic, mucous (including the intestinal), and sweat glands are pouring out their appropriate secretions

superabundantly. Now, all these functions are carried on by virtue of force conveyed, there are valid reasons for believing, through spinal nerves, from the spinal cord. The question—What is the immediate cause of the immense outpour from the stomach and bowels? is at once so important and so interesting that I shall venture to advert to it particularly. It is remarkable, as already intimated, that nearly every writer on cholera, when adverting to the state of the nervous centres related to the intestines, speaks of them as being profoundly depressed or exhausted, and considers that the proper functions of secretion are arrested. It is probable that the large amount of serous or watery effusion which occurs has concentrated attention upon itself, and, to the eyes of the majority of observers, has masked the important fact that the actual secretion of mucus is enormously increased. The large amount of whitish flocculent matter constituting the characteristic element of the so-called rice-water evacuations has, however, been the subject of elaborate and prolonged researches and discussions, and has given rise to a great diversity of opinion as to its essential nature. In my work on Cholera, I have reviewed the evidence adduced on this question, and have, I believe, demonstrated decisively that, so far from the nervous centres related to the intestines being, as they have been generally said to be, profoundly depressed or exhausted, they are, in fact, in a state of the most extreme and tumultuous activity. I must, however, content myself here by adducing but one witness in support of this statement; and his evidence may perhaps be held to be of special value, not only because he has been a careful observer of the nature of the choleraic discharges, but also because, having put forth a theory of cholera which differs widely from mine, he will probably be regarded as an impartial witness: I refer to Dr. George Johnson, who says,—

The flocculi in the rice-water stools consist almost entirely of *perfectly organized epithelial cells, most of them of large size*. Of this fact I have satisfied myself by repeated examinations of the discharges from different patients. The peculiar creamy viscid secretion, which sometimes nearly fills the small intestines after death, is almost entirely made up of the same *fully formed epithelium*. Now, it is obvious that this large amount of epithelium cannot be explained by the peeling away of one or two layers of cells from the surface of the mucous membranes—the result of a local irritation during life, or of maceration by the fluid contents of the bowel after death. *This abundant cell-formation can result only from a very active vital effort.*

In presence of the large array of facts pertaining to this question now accumulated, it is impossible to avoid the conclusion, that the whole glandular system of the alimentary canal is in a state of the most energetic, tumultuous activity; that the

development of gland cells, which are shed in quick succession, is extraordinarily exuberant; and that a very large proportion at least of the cells, and cell-detritus, as well as the whole of the superabundant mucus found in the evacuations and intestines after death, is the product of this excessive development from the vast extent of the glands and glandular surface in question. A recognition that the functions of this large body of secreting cells are thus enormously augmented and intensified, produces the conviction that the nervous influence distributed to the entire glandular surface must have been overwhelmingly energetic and intense; and this conviction becomes a certainty when we consider the thoroughly ascertained fact that during collapse the temperature within that part of the bowels to which the thermometer can be applied is higher than normal; in many cases it is 103° F., and in some cases as high, even, as 105° F. In instructive accord with these facts, the temperature in the groin is found to be about 2° F. higher than it is in the armpit. My discovery that the mucous membranes generally may have their functional activity increased by the application of heat along the spine, and lessened by cold along the same region,* while revealing how it is that, simultaneously with general spasmodic contraction of the arterial system, such immense outpourings from the mucous glands of the alimentary canal are possible and effected, prove conclusively that hyperæmia of the spinal cord is at once the proximate cause and complete explanation of this hitherto mysterious phenomenon.

Another important group of symptoms consists of morbid conditions of the muscular system. These are—abdominal griping; excessive contractions, or expulsive activity, of the stomach and bowels; excessive activity of the thoracic and abdominal muscles; excessive contraction of the urinary bladder; tremors; muscular twitchings; fixed and stony expression of the face; tonic hardness of some, or of many, of the voluntary muscles; tightness across the lower part of the chest; cramps and convulsions. Now all these morbid phenomena of the muscular system are exclusively due to over-activity of the spinal cord, which discharges its tumultuous excess of nerve-force through the spinal-motor nerves, on the muscles with which they are connected. The whole of the morbid conditions already mentioned consist of an excess of function of the organs implicated. Hence, for convenience of description I call them *positive* phenomena; and the nerves which are concerned in producing them I call *positive* motor nerves. To the excessive

* For evidence of the truth of this statement, the reader is referred to the introductory chapters in my works on "Sea-sickness" and "Cholera" respectively.

action of these nerves is also due the phenomenon often observable in cases of cholera—viz., the presence of albumen in the urine, the production of which is explained at page 101 of my work on “Diarrhœa and Cholera.”

On the other hand, there is a number of symptoms which are exclusively due to the excessive activity of the Sympathetic, and the character of which is accurately denoted by the word *deprivation*. All the symptoms in question are due to the fact that the affected organs are deprived of their wonted supply of blood. Hence I call this group of symptoms *negative*, and the Sympathetic which produces them the *negative* motor nerve. The first group of these symptoms are cerebral—viz., slight headache, deafness of various grades, singing in the ears, dizziness, slight faintness, syncope, enfeeblement (but without perversion) of the mental faculties. These conditions are all clearly referable to the state of cerebral anæmia induced by the undue contraction of the brain-arteries caused by the constricting action of the Sympathetic.

As the disease advances, the arteries which supply the lachrymal and salivary glands, the liver and kidneys, become so powerfully constricted that blood ceases to flow to those organs, and hence their appropriate secretions become fearfully “conspicuous by their absence.” The negative character of the pulmonary group of symptoms is not less striking. The involuntary circular muscles, not only of the pulmonary blood-vessels, but of the bronchial tubes, are powerfully constricted. Hence the arrest, more or less complete, of the normal chemical changes which take place in the lungs, the shrinking of the lungs themselves, and the obvious consequences—short, struggling, and rapid respiration, enfeeblement of the voice, voicelessness, and cold breath—devoid, or all but devoid, of carbonic acid. Gradually the arteries of the skin become so contracted that the blood is shut off from the surface of the body, the result being the well-known and extremely characteristic group of symptoms called “Algidæ”—symptoms which include the shrunken, death-like aspect of the visage, the corrugated condition, dark colour (cyanosis), all but abolished sensibility, and corpse-like coldness of the skin. The fatally constricting energy of the violently excited Sympathetic finishes its work by depriving, first the voluntary, and then the involuntary muscles of their needful supply of blood—death consequently overtaking each in the same order of succession. And though the brain still lives, the patient, while possessing consciousness and more or less intellectual capacity, including the power of hearing and understanding what is going on around him, may yet be unable to give any sign that such is the case.

Finally, death overtakes the nervous system itself. As correctly stated by Professor Parkes: "two or three hours before death there is often some return of heat in the scalp and forehead, over the region of the heart or whole chest; it may be also over the abdomen; the extremities are still icy cold, and the cholera visage is unaltered. This partial return of heat on the head and trunk is an immediate forerunner of death, and, as far as I have seen, is invariably a fatal sign; it is occasionally confined altogether to the cardiac region, and is sometimes astonishingly great." This remarkable phenomenon is immediately due to the relaxation of the various branches of the pulmonary artery, of the bronchial tubes, and of those systemic arteries distributed over those parts of the body which, during the disease, have continued most vital, and have therefore preserved the highest temperature. The blood has continued to pass through these arteries most copiously, and is thus prepared to effect their dilatation, and to flow through them in fuller currents than before, the moment the energetic stimulus from the negative motor nerves, which has kept them in a state of tonic spasm, declines or ceases. Now already, when the phenomenon in question presents itself, the ganglionic nervous centres presiding over the arteries just mentioned have begun to die. Their convulsive grasp of the blood-vessels and air-tubes, which has already proved fatal to the system generally, is being relaxed, and they themselves are sharing the fate which, through their agency, has overtaken the entire organism. A last but vain effort for life is made, however, by the structures, released at length from the deadly influence of their excessive energy. The normal attraction between the venous blood in the pulmonary arteries and the air in the air-cells generates movement of the blood through the pulmonary capillaries; it reaches the left heart, and is thence forced most copiously into those systemic arteries just indicated, and thence, finding its way to the starving tissues, the usual vital changes occur. Meanwhile, as a result of the renewed oxydation occurring in the lungs, and in the parts supplied by the newly relaxed systemic arteries, heat is evolved, and constitutes the phenomenon in question. But this local struggle for life is too late: its possibility depends on the presence of death in the nervous system, which, soon seizing on the brain itself, closes the scene. This local increase of temperature before death is a strong proof that the blood-vessels are healthy; that the structures are healthy, and suffer only from lack of nourishment; that the blood itself is free from poison; and that the disease is seated in the nervous system.

The increase or long persistence of heat in the body after death, which is a well-established fact, is merely the continuance

and extension over the whole body, after the death of the brain, of the series of actions commenced before death, and explained in the preceding paragraph. The whole sympathetic system having ceased its functions, the arteries throughout the body relax; the small supply of blood in them is drawn through the systemic capillaries; every particle of oxygen which it can yield up combines with the surrounding structures; and while there are elements to continue these changes, the temperature of the body is raised, maintained, or prevented from declining with the rapidity usual after death from almost all other diseases. The reflux of blood throughout the body denoted by the general increase or unusual persistence of its heat must inevitably result in lessening the bulk of any organ which may have been unduly distended with blood. Such an organ in choleraic collapse is the spleen, which is found unusually large during life, but which after death presents in respect to size no constant appearance. It seems to me probable that when the evacuations are extremely abundant, the spleen is less distended than in other cases, and that the great variations of size which it presents after death depend upon the amount of the evacuations, and the extent to which the post-mortem arterial relaxations result in a temporary renewal of textural vitality, which, of course, implies to the same extent a diffusion of the previously pent-up blood throughout the body.

Those post-mortem changes in the aspect of the skin, which in cases of cholera are especially notable, result from the chemical changes occurring, and caused as described in the preceding paragraph. The skin becomes lighter in colour, even when it has been especially dark, and has continued so for some time, because the oxydation of the blood changes it from blue to red, while the relaxation and partial dilatation of the terminal arteries cause the shrivelled aspect of the face, hands, and feet, partially or wholly, to disappear.

Those extraordinary and, to the ignorant, terrifying phenomena—post-mortem muscular contractions—have never, I believe, received a satisfactory explanation; but the hypothesis here expounded reveals the cause of them at once. The arteries supplying the muscles were vehemently contracted before death, but are now relaxed; immediately before death the muscles were deprived of blood, and therefore enfeebled to the utmost degree; after death, receiving a new supply of blood, their hungering constituent elements receive fresh nourishment; with it new strength, and, under the stimulus of the still hyperæmic spinal cord, which is the last to die, contract in the manner described. It seems to me that if the waning life of the blood in the spinal cord be equally distributed, those apparently

co-ordinated muscular movements sometimes observable are most likely to occur, and that when special segments of the cord retain their excitability longer than the rest, convulsive movements of one or of a few muscles only will result.

The early onset of rigor-mortis is a fact characteristic of cholera corpses, and is in extremely interesting accordance with the requirements of the hypothesis now expounded. When the Sympathetic of an animal is divided, and one part of it is excessively stimulated by means of galvanism, the arteries to which that part is distributed become, of course, strongly contracted, and in this manner a condition like to that which I affirm to exist in the arteries of a cholera patient in collapse is induced. Now, in such cases, rigor-mortis invariably supervenes in the part of the animal thus experimented upon far more rapidly than elsewhere; and the fact that the same condition comes on with extraordinary rapidity after death from choleraic collapse, is a striking proof that the arteries of patients in that state are powerfully constricted by nervous agency.

Additional evidence in support of the hypothesis in question is derived from the post-mortem appearances in cases of death during collapse. The distribution of the blood generally is always in the veins. "Arborescent venous congestion" is the prevailing epithet used to describe the aspect of the vascular system, the arteries being empty. This is the condition which the hypothesis presupposes. Whatever may be the amount of hyperæmia of the spinal cord and ganglionic nervous centres during life, it by no means follows that they will be found in the same condition after death. Indeed, what has already been stated concerning the change in them after death, and its results in the arterial system, proves that their vascular state is modified when death occurs. It is, however, said that the spinal cord is found extremely congested, and that the sympathetic centres examined by Mayer were disorganized. These statements need confirmation. But if the sympathetic ganglia were disorganized, the fact would imply that their functional activity had been intensified to the extent of inducing destructive inflammation, and, if so, this is the strongest fact which could be adduced in proof of the doctrine here propounded.

This brief exposition of the proximate cause of cholera may be fitly closed by a reference to the state of the blood of patients dying in collapse. It has been the object of a large amount of careful investigation by numerous observers, who, on the whole, concur in the conclusion that whatever differences in its constitution from that of normal blood are discoverable, are differences mainly due to the withdrawal from it of a large amount of its most fluid portion. There is no evidence that the blood is in

any other respects unhealthy. The truth of this remark is strikingly confirmed by the experiments of Professor Parkes, who proved not only that, by inflation of the lungs after death, the dark blood in them becomes vividly red, but that when it is taken out of the body, and exposed to the air in thin layers, it does the same. "It is certainly a very singular thing," he says, "that in cholera the blood should retain its power, out of the body, of acquiring a red colour when exposed to the air in thin layers, and yet in the body, that this change, as well as the changes leading to the production of heat, should be interrupted. *This certainly looks like obstruction only.*" These words which I have put in italics show how near the truth of the matter Professor Parkes stood: though the morbid processes to which the phenomena of cholera are due are usually supposed to consist of chemical changes wrought in the blood by an organic poison, all that we know of the state of that fluid tends to prove that it is essentially healthy, that the changes effected in it are merely changes in the relative amount of its constituents, and that those changes are consequences of purely physical or dynamic agencies.

So far as is practicable within the compass of this article I have now shown that all the symptoms of cholera are produced by a preternatural, tumultuous energy and activity of the nervous system—both cerebro-spinal and sympathetic; and that, though extremely numerous and various, they are completely accounted for by the operation of one and the same immediate cause. In concluding this part of the subject, I may observe that while the hypothesis now briefly expounded does not exclude the possibility of the existence of a cholera poison, germ, or microbe, it is self-sufficing, and that this fact renders it extremely improbable either that any such poison, germ, or microbe is causative of cholera, or that the disease is in any sense more infectious or contagious than is sunstroke or epilepsy. Sir Andrew Clark, in his critical analysis of my work on Cholera, remarked that I might happily have prefixed to it for motto the axiom of Sir Isaac Newton: "*Causas rerum naturalium non plures admitti debere, quam quæ et veræ sint et earum phænomenis explicandis sufficient:*" now the nervous system in the state described above affords a sufficient and complete explanation of the production of the phenomena in question; therefore, to recognize even the possibility of a blood-poison as the immediate cause of cholera would be, according to Newton, not only needless, but contrary to the dictates of a sound scientific method.

I now invite my readers to a consideration of the *Causes of Cholera*. They are numerous, and their operation is often simul-

taneous and complex. I believe, however, they may be so classed as to prove, so far as evidence of identity of causes will avail as proof, that summer diarrhoea and cholera are one and the same disease; for it appears indubitable that they both arise out of the same conditions, and are originated by the same influences. The phenomena of cholera are ascribable to proximate, to remote or predisposing, and to exciting causes. The term, *proximate cause*, is generally understood to denote that morbid condition of any given structure the presence of which entails the symptoms characteristic of the disease in question as an inevitable consequence. This immediate cause is therefore justly regarded as the essence of the disease; and hence the determination and description of its seat and character constitute pathology. The pathology of cholera having been already discussed, I have only to advert here to its *remote* or *predisposing* and *exciting* causes. These terms are, in my opinion, objectionable; for, in respect to the causes of cholera, at least, they are often interchangeable: thus, if the pathology of cholera explained above be correct, atmospheric heat may induce hyperæmia of the nervous centres, and so predispose them to excessive functional activity. If now the person in whom this change has occurred drinks freely of alcoholic liquors, or water containing a considerable proportion of organic matter, he may directly, through the stimulating influence of the alcohol on the nervous centres, or indirectly, through irritating the nerves of the alimentary mucous membrane by the bad water, so excite those centres as to produce cholera; and conversely, alcohol or bad water acting as stated on nervous centres not already predisposed to cholera, may render them hyperæmic, thus inducing in them a predisposition to the disease, and when this condition has been established in this manner, the supervention of great atmospheric heat may excite in them that excessive activity resulting in cholera. Hence it appears that what is a predisposing cause in one person may be an exciting cause in another, and *vice-versâ*. The various causes of cholera are, I believe, essentially the same as those productive of diarrhoea; but, whereas one only of the causes hereafter mentioned may very often be productive of diarrhoea, the sources of cholera are more frequently complex, and thus effect, by their co-operation, results which, acting singly, they would be unable to produce.

It has been stated by Dr. Farr, the eminent medical statistician, that diarrhoea is as constantly observed in English towns when the temperature rises above 60 degrees Fahrenheit as are bronchitis and catarrh when the temperature falls below 32 degrees. In like manner, of all the influences which conduce

to the development of cholera none is so potent as that of *Solar Heat*. This truth is made manifest by the following facts:—

During the epidemic of cholera in England in 1831–2 the total number of deaths from the disease was 30,924; but of these no less than 24,613 occurred during the five months, June, July, August, September, and October. Again, during the epidemic of 1848–9 the total number of deaths from cholera in England was 54,398, and of these 50,521 occurred during the five months, June, July, August, September, and October. The epidemic occurring this year in France and Italy has, so far, been restricted to the months just named. In Bengal the hot seasons are those of the worst cholera epidemics. Observations extending over a period of eight years prove that the most fatal cholera months for European troops are from April to September.

When cholera, in its fully developed form, prevails, and, generally, preceding its advent in any given place, diarrhœa (cholera in its initial stage) is much more frequent than usual in the same place. A great increase of diarrhœa preceded and accompanied the invasion of cholera in 1832, in 1848–9, and in 1865. From June 3, 1865, when the first case of cholera was reported in London, to November 25, when the disease disappeared, the number of deaths from diarrhœa was 3,137, the deaths from cholera being only 182. The temperature of that year was at its highest in England throughout the month of July, and during that month the deaths from both diarrhœa and cholera were much the most numerous—viz., 1,284 from diarrhœa, and 75 from cholera. Moreover, the annual mortality from diarrhœa, like that from cholera, is greater in hot than in temperate climates. In England for the seven years 1848–54, it was at the rate of 86 per million; but for European troops in Bengal during the fifteen years 1830–45, it was at the rate of 4,555 per million. In fact, diarrhœa is destructive of life, especially during periods when, and in regions where, cholera is rife, to an extent which will surprise every one who is not familiar with the facts of the matter. Within the period of twenty years 1847–66 there were two cholera epidemics in England. During that period diarrhœa and cholera destroyed 417,199 persons, and of this number about three-fourths, or 311,200, were destroyed by diarrhœa. During the year 1857 the deaths in England from cholera were 1,150; but those from diarrhœa were 21,189. During the summer months of the present year the deaths in Paris from cholera have not exceeded one or two a week, but the deaths from diarrhœa, chiefly infantile, *each week*, have been as follows:—In May, 56; in June, 67; in

July, 209; in August, 224; and in September (up to the 25th), 119. From the evidence here tendered it appears then that great solar heat is the chief cause of both cholera and diarrhœa; that when cholera prevails diarrhœa is increasingly prevalent, and that where cholera is endemic diarrhœa is endemic also.

Now, seeing that solar heat of a certain intensity induces cholera, and seeing that, as a rule liable to certain exceptions, the number of deaths from the disease increases and decreases with the rise and fall of the atmospheric temperature, we must conclude either that heat alone suffices to produce in the nervous centres, *directly*, that condition of superabundance of blood associated with great excitement which is the immediate cause of the phenomena of cholera, or that it produces that condition, *indirectly*, in some inscrutable way—for example, by originating a poison in the blood. I venture to affirm that the first of these hypotheses claims our preference, because of its simplicity, because it accords with all known facts in respect to the action of solar heat on the nervous system, and because it suffices to explain, by the agency of a known factor, the origination of the condition in question without the necessity of conjuring up an additional agent, the presence of which afterwards becomes much more embarrassing than helpful. The following facts are, indeed, thoroughly established: Summer diarrhœa is induced by heat; cholera is induced by heat; in the great majority of cases of cholera, especially those occurring in temperate climates, the initial stage of the disease consists of diarrhœa exactly like summer diarrhœa; summer diarrhœa is often associated with cramps and notable coolness of the skin very suggestive of cholera; and, indeed, in the United States summer diarrhœa, the victims of which are chiefly children, assumes the likeness of cholera to such an extent as to have obtained the name Cholera Infantum.

The opinion has been expressed by C. T. Kiërluf, near Bergen, by Mr. Orton, in 1832, by Professor Parkes, who relates a case in support of his belief, and by Professor Aitken, who adopts it, that the diarrhœa which generally prevails during invasions of cholera is capable of infecting healthy persons with "true cholera;" but I doubt if any one except these gentlemen would affirm that the ordinary summer diarrhœa in Europe, the cholera infantum of the United States, and the endemic diarrhœa of India are the consequences of a blood poison or of microbes in the intestines. And yet, wherever cholera prevails, either summer diarrhœa develops into cholera or the symptoms of the initial stage of most cases of cholera are to all intents and purposes identical with those of summer diarrhœa. The question therefore arises: at what stage does the sufferer from summer

diarrhœa, or from the diarrhœa which is the prelude of cholera, become the victim of the alleged cholera poison or cholera microbe? I believe all physicians having practical acquaintance with cholera are of opinion that the progress of the disease may be arrested in a large proportion of cases if prompt treatment be resorted to during the initial stage of diarrhœa; but would this be possible if the disease is from its beginning the result of a poison or microbe? And if it is not, I again ask, at what stage of the disease, and how, does the poison, or microbe, enter or become developed in the patient? I would also ask, how it comes to pass, if cholera be the result of a microbe or blood poison, that many patients recover from the disease in a wonderfully short time. And how can we explain the well-known fact that mental emotion can exert so powerful an influence on the sufferer from cholera as rapidly to hasten either his death or his recovery? Fear is as little likely to render the alleged poison in the blood more intensely poisonous, or to increase the destructive power of the microscopic invaders of the alimentary canal, as is assurance of recovery to neutralize that poison or to paralyze those invaders. Mental emotion exerts no such influence on the progress and termination of those diseases which there are valid reasons for believing to be results of blood-poisoning—viz., typhoid fever, scarlet fever, or small-pox.

Wide Ranges of Temperature, along with a high temperature, are also peculiarly conducive to the development of cholera. In the thirty-sixth week of 1854, when cholera raged in London, and the deaths from all causes rose to their maximum (3,413), the maximum range of temperature was 38·1, and the average daily range was 30·9, *the greatest in the fifty-two weeks of that year.* Extensive observations in India prove most conclusively that, as a rule, when the range of temperature is greatest the deaths from cholera are much the most numerous. Now, is there anything in this remarkable and well-attested fact favouring the idea that cholera is the product of either microbes or a blood-poison? Is it conceivable that wide ranges of temperature can conduce to originate either one or the other? On the other hand, it is easy to understand how the dynamic influence of the cause in question produces an instability of circulation in, and therefore preternatural excitability of, the nervous centres—conditions alike conducive to the production of cholera.

Disturbances of Atmospheric Electricity are, it is believed, especially favourable conditions for the development of cholera. Many delicate, nervous women are painfully affected by such disturbances, which, according to the testimony of several

authoritative medical observers, exert a powerful influence in producing puerperal convulsions, and in causing that disease to assume an epidemic character. In proof of the great influence of these disturbances on the nervous system I may mention that, in Paris, after the last Revolution, when the hospitals were crowded with the wounded, a very severe thunderstorm came on, and that the mortality was greater in all the hospitals on the night of the storm than on any previous or subsequent occasion. I have become acquainted with several cases in which persons in the presence of thunderstorms become troubled with diarrhoea; and of all the symptoms produced by attacks of atmospheric electricity diarrhoea is the most frequent. Moreover, it has been thoroughly ascertained that nearly every symptom of cholera is observable in one or another of the patients struck by lightning. It is recorded that at St. Petersburg during a cholera epidemic the magnetic needle did not obey its usual natural attractions, and that a magnet which usually sustained a weight equal to about seventy-five pounds to the square inch gradually lost this power as the disease increased, until at last, when the disease was at its height, it had only the sustaining power of fifteen pounds. As the disease decreased the power of the magnet increased until it sustained its proper weight. A similar fact was observed in Ireland during the epidemic of 1849. That cholera often succeeds a severe thunderstorm is well known. It seems impossible to avoid recognizing the existence of a causal relation between facts of this kind and the origination of cholera; but while such facts enforce the conviction that atmospheric electricity, which has, at least, close affinities with nerve force, exerts, when in a state of perturbation, a powerful and pre-eminently exciting influence on the whole nervous system, they are far from either suggesting or countenancing the notion that that influence poisons the blood or generates microbes in the intestines.

It seems to me likely that when the mode of attack of cholera is gradual, when it is characterized by premonitory diarrhoea, and when it presents itself over a wide area, the main factor in its production is solar heat, which, as already shown, acts the most potently when the diurnal range of its intensity is especially great; and that when the mode of attack is sudden, concentrated, and intense, the main factor in its production is that form of solar force we call electricity. Reasons have been adduced for alleging that there is probably a causal relation between the periodical famines which have been wont to occur in India, and a special influence in those years in which sun-spots appear. At the recent meeting of the British Association at Montreal, in the course of a discussion

on the connection of sun-spots with terrestrial phenomena, Mr. E. D. Archibald stated that "in years of sun-spots the barometric pressure increases, *giving clear skies and great heat*;" and the Rev. S. J. Perry said that "there is a decided connection between solar spots and terrestrial magnetism, that he can foretell aurora, and that *the greatest magnetic disturbances occur when the largest spots are seen*." Countenanced by these statements, the idea will not, perhaps, be considered far fetched, or, indeed, improbable, that those years in which cholera is *at once extensively and intensely* epidemic will be found to be sun-spot years. Certainly the subject is fraught with intense interest, and needs to be thoroughly investigated.

An especially remarkable and very interesting proof of the existence of a cosmical influence, rendering the atmosphere of those localities where cholera is epidemic unhealthy, is afforded by the fact that birds leave such places. The occasions have been so numerous and in so many different countries, that it is impossible to regard the coincidence as merely accidental. In 1848 birds of all kinds deserted the towns of St. Petersburg and Riga, and it was in that year that cholera broke out there. The same phenomenon was observed, and chronicled at the time, in Western Prussia in 1849, and in Hanover in 1850. In the little town of Przemyśl, in Galicia, all the jack-daws took flight from the streets into the country on September 26, 1872, and cholera broke out there two days afterwards. On November 30 these birds returned from their spontaneous quarantine, and by that time the last case of cholera had been recorded. In 1873 the disease appeared in Munich and Nuremberg, and not only the larger birds, but the sparrows and swallows, deserted both these towns. The inhabitants of Nuremberg looked with joy on the return of the sparrows, which occurred as soon as the cholera had disappeared from the town.* P. Hinckes Bird, Medical Officer of Health at Lytham, states that a physician, referring to the presence, about thirty years ago, of epidemic cholera in "a favourite sea-side resort," observed that the atmosphere appeared to be poisoned, for both the fishmonger and butcher told him that the fish and meat would not keep beyond a few hours; and, remarked one of them, casually, "Have you noticed that all the birds have left?" The physician added, "True enough, not one was to be seen or heard in the place, and the first sign of an improvement in the sad state of things was a return of the feathered songsters." A similar fact was observed at Scutari during an outbreak of cholera there.† "On the afternoon of July 23, 1883, at about

* The *Times* (quoted from the *Globe*), September 3, 1874.

† *Ibid.* September 8, 1874.

5.20, a marked change in the atmosphere took place" at Cairo, and on that day "birds seemed to have deserted the city, or, when still present, all signs of activity among them were suspended. On the night of the 23rd cholera appeared amongst our men at Kasr-en-Nil, the citadel, and at Abbasieh."* In the letter of the Marseilles correspondent of the *Times*, published August 2, 1884, occurs the following sentence:—"It is said that swallows desert a district afflicted with cholera, and it is certain that there are none just now in Marseilles." At a subsequent page I shall suggest an explanation of these remarkable migrations.

Mr. Glaisher, in his report on the Meteorology of London during the three cholera epidemics of 1832, of 1848-9, and of 1853-4, says:—"The three epidemics were attended with a particular state of atmosphere, characterized by a prevalent mist, thin in high places, dense in low. During the height of the epidemic, in all cases the reading of the barometer was remarkably high, and the atmosphere thick." On the day preceding the night when cholera broke out at Cairo last year, there was noticed in the atmosphere a haze "of a yellowish colour, suggesting the Arabic name for cholera, the translation of which means 'yellow air.'" It seems as if some men know, as well as birds do, when cholera threatens; for last year at Ramleh (Egypt), August 3, when the temperature was high, and the moisture in the air approached saturation point, "*Men who had served in India remarked that there was cholera in the air, and at 10.30 A.M. the first case was reported.*"

The Absence of Ozone in places where cholera prevails has often been observed. In 1855, at Strasbourg, the invasion of cholera coincided with a period of antozone, and the decline of the epidemic was accompanied by a return of the ozone; and in 1865 the same negative phenomenon was noticeable in England: in districts in which cholera prevailed not the smallest trace of ozone was discoverable by the test papers. This deficiency is likely, as I have elsewhere shown,† to facilitate that hyperæmia of the nervous centres which is the condition precedent of cholera, but in no sense helps us to conceive how the alleged cholera-poison is produced.

Lowness of Site is a condition remarkably favourable for the invasions of cholera. Dr. Farr, who has fully investigated this subject, observes: "The elevation of the soil in London has a more constant relation with the mortality from cholera in London than any other known element. The mortality from cholera is in the inverse ratio of the elevation. The mortality

* Brigade-Surgeon McDowell's Report to Surgeon-General Hunter.

† In my work on "Diarrhoea and Cholera," p. 152 *et seq.*

of the nineteen highest districts was at the rate of 33 in 10,000 and of the nineteen lowest districts 100 in 10,000." The deaths were at the rate per 10,000 as follows: In districts at an elevation of 20 feet, 102; at from 20 to 40 feet, 65; at from 40 to 60 feet, 34; at from 60 to 80 feet, 27; at from 80 to 100 feet, 22; at from 100 to 120 feet, 17; at from 340 to 360 feet, 7. "The most favoured seats of cholera all over the world," says Dr. Goodeve, "are places not high above the sea; along the banks of rivers, and the estuaries of great streams." This established fact—that in proportion to the lowness of site of any locality it, *cæteris paribus*, conduces to the presence of cholera—offers, in my opinion, a far more intelligible and satisfactory explanation of its frequency and of the extent of its ravages along the borders of rivers and the main lines of human traffic than does the common allegation that cholera "travels" or is propagated along these lines by contagious influence. Experience has long ago established the fact that in proportion as the air we breathe is pure it conduces to health, and that as a rule low sites are less healthy than those which are lofty. Moreover, students of cholera know that in proportion as the general health of individuals is impaired are they likely during the prevalence of cholera to become its victims. But in whatever manner and to whatever degree lowness of site may operate as a predisposing cause of cholera, this condition certainly cannot be regarded as a toxic cause, unless the lower strata of the atmosphere in low lying regions of the earth's surface are also, as a rule, to be regarded as poisonous.

Prolonged Marches of soldiers in India facilitate in a remarkable degree the development of cholera: it was proved by Dr. Balfour that of the native soldiers of the Madras army thirty-two died of cholera in cantonment and eighty-six when marching, to an average of 10,000 in strength. Moreover, it has been ascertained that the longer the marches the more frequent the attacks. Many facts of this kind are recorded. I have just been informed by a French physician that the marches of French soldiers in Algeria operate as an exciting cause of cholera to an extent appallingly impressive. But it is not only soldiers who are attacked on march: it is well known in India that people of all conditions who travel on foot are specially liable to be attacked, and that pilgrims, on their way through Lower Bengal and Orissa, strew the road to Juggernaut with their bones. Walking in hot weather, most especially when the back is exposed to the sun, induces in the spinal cord a condition of hyperæmia which is extended to the collateral ganglia of the Sympathetic, and thus a state of the nervous centres conducive to the onset of cholera is established.

Bad Food and Eating to Excess have often converted a tendency to cholera into its reality. Half-putrid fish, bad shell-fish, bad pickled pork, are known in several cases to have been followed by violent attacks of cholera. Dr. Carpenter mentions "an outbreak of cholera and choleraic diarrhœa among a number of school children who had eaten plentifully of spoiled oysters, and by which eleven of the sufferers lost their lives." It is recorded that immediately after the arrival of a cargo of bad oysters in New York diarrhœa and cholera prevailed to a great extent. Unripe fruit and crude vegetables, and even wholesome food eaten to excess, will operate as exciting causes of cholera when atmospheric conditions predispose to it. It has been observed that cholera seizures appear to be especially frequent in natives of India after a full meal. It is well known that her Majesty's 14th Regiment when at Berhampore was attacked with cholera after it had received an allotment of prize money. The disastrous effects of irritants of this kind have often been observed, but no one dreams that, while thus acting, they are capable of generating an infective poison in the blood.

Alcoholic Drinks are powerful aids in producing cholera. It was found that during the cholera epidemic of 1848-9 in England the deaths from cholera on Saturday, Monday, Tuesday, and Wednesday were above, and on Thursday, Friday, and Sunday, below the average. The weekly wages are generally paid on the Saturdays, and the Mondays in London and other cities are days on which a certain proportion of the population indulge in intoxicating drinks. During the epidemic of 1865 the mortality in Berlin suddenly rose on certain days, and was clearly referable to excess in drinking. In 1866, Dr. Andrew Clark stated, in respect to the London Hospital, that "immediately after pay-day among workmen there was a great influx of cholera patients." When the epidemic of cholera broke out at Naples in the beginning of September, the *Pungolo*, a Neapolitan newspaper, attributed the increased number of cases to intemperate living on the 31st of August, which was a *fête* day.

Sudden manifestations of an increase of cholera cases as effects of even one day's excessive drinking are established facts, so that alcohol, whether taken habitually or only on special occasions, is proved to be capable of acting either as an exciting or predisposing cause of cholera. The blood poison, zymotic, and microbe theories of the disease afford no explanation of this fact. But just as in respect to opium, the pathological doctrine I have put forward at once explains and is confirmed by the fact in question, the special affinity of alcohol for the nervous centres, its great exciting influence on those centres, and its power of producing vomiting and purging in many cases, when taken in considerable

quantities, are well-ascertained facts; it therefore not only produces, in an especial degree, hyperæmia of the nervous centres, and thus, according to my doctrine, an especial predisposition to cholera, but often also two of its leading symptoms.

Opium is now generally admitted to exert great power in conducing to the development of choleraic collapse of a type peculiarly fatal. This is the conclusion of numerous and eminent authorities—men who have had a large experience of the treatment of cholera in India. Opium is often prescribed, and not infrequently with seeming advantage, during the stage of premonitory diarrhoea; but in a large proportion of cases, even in this stage, its failure to arrest the progress of the disease is signal. Referring to my assertion and explanation, in 1865, of the dangerousness of opium, Professor Maclean, of Netley Hospital, said “he could bear the strongest testimony, not only to the inefficacy, but to the very great danger in the use of opium in cholera.” Professor Goodeve and Dr. Macpherson have expressed themselves to the same effect; and the medical journals contain many reports of the power of opium to favour or induce the development of cholera after the disease has fairly set in. Now, opium is known to have a peculiar affinity for, and to exert a specific influence over, nervous tissue. Therefore, when co-operating with other causes, or when acting alone in the production of choleraic collapse, it does so, as will generally be admitted, by virtue of its great and peculiar power of modifying the vascular and functional condition of both the cerebro-spinal and sympathetic nervous centres. The pathology of cholera already expounded reveals for the first time the *modus operandi* of opium in developing the disease; while the admitted facts that opium has often induced collapse, that collapse so induced is more than ordinarily fatal, and that when reaction succeeds to it, that reaction is often unusually protracted and dangerous, constitute additional evidence of the truth of the pathology in question.

Purgative Medicines have acted as the exciting causes of many attacks of cholera. This assertion, considered in connection with what has just been said of the power of opium to do the same, will no doubt seem paradoxical; but that it is nevertheless true is proved beyond the possibility of doubt. Ample evidence to this effect is presented in my volume on “*Diarrhoea and Cholera*” (p. 164 *et seq.*); but I must content myself with mentioning several physicians, mostly Anglo-Indian, whose experience constrains them to testify to the truth of this statement—namely, Dr. Twining, Dr. Morehead, Dr. Macintosh, Dr. Painter, Dr. Durham, Sir Ranald Martin, Dr. Macpherson, Dr. Barlow, and Professor Goodeve. In fact, that it is dangerous to take aperients when cholera is prevalent has become, as Dr. Macpherson says,

"the common doctrine of Europe, and the latest experience in France confirms it." The Medical Council of the London College of Physicians found that the treatment of cases in collapse by castor oil resulted in a mortality of 77·6 per cent. And yet Dr. George Johnson's great remedy for cholera is castor oil! As his high professional position lends importance to his advice, and therefore, in this case, makes it the more dangerous, it is necessary that this danger should be distinctly pointed out. "With regard to castor oil," writes Dr. Macnamara, "I was acting as house-physician to King's College Hospital, in 1854, when Dr. G. Johnson was treating his cholera patients on eliminative principles. I caught some of his enthusiasm on the subject, and came out to India the same year full of confidence and hope in castor oil. . . . In the following year I was left at Bhangul-pore in charge of a field hospital. I was the only medical man in the place, when cholera burst out among the Europeans and natives under my care. I went boldly to work with castor oil, but it absolutely and completely failed; *the mortality from the disease was fearful*. I have since, on several occasions, tried castor oil in cholera, but I have now finally abandoned it, having never seen any benefit arise from its use." The *modus operandi* of purgative medicines is, I presume, now generally recognized as being through the agency of the nervous system, even in those cases in which the medicine enters the circulation. The enteric nervous centres are, in any case, unduly excited, become foci of an excessive afflux of blood, and thus preternaturally energetic. Now this is precisely one of the conditions which obtains during an attack of cholera, and which, when the causes already mentioned are exerting their influence, is likely to induce the disease. And, conversely, when, by the action of purgatives, the functions of the enteric nervous centres have become intensified, the hyperæmic condition of those centres only needs the co-operative influence of either excessive heat or some one of the other epidemic agencies to ensure the development of the disease. A consideration of the *modus operandi* of purgative medicines here indicated, in connection with the pathology of cholera already expounded, explains at once why, in cholera times, their use is so beset with danger, and how not unfrequently they become potent causes of the disease itself.

Painful Dentition.—The process of teething, which, by the excessive irritation of the dental nerves, and the consequent hyperæmia of the medula-oblongata (the topmost part—which is within the skull—of what Marshall Hall called the "true spinal cord") in which they converge, is, in ordinary times, a most fruitful source of diarrhœa, and becomes in cholera times, and often in those summers when adults have a complete

immunity from cholera, a very active cause of the disease in children. My readers are now aware that many factors are concerned in the production of cholera. Sometimes several co-operate, sometimes not more than two, and sometimes the disease is engendered, there are reasons for believing, by one alone. Many of these factors can act as predisposing or exciting causes of cholera only when two or more act together. Many children pass through the ordeal of teething with but little pain, and without suffering grave consequences of any kind during any part of the year; and in temperate climates those children in whom dentition is decidedly painful, and the cause of more or less constitutional disturbance, generally escape any serious consequences from that process during the winter months. But during the summer months such children are in great danger. Their rapidly growing nervous systems, like those of children generally, are already suffused with blood to a maximum degree in order to supply material for their exuberant nutrition, and are therefore in a condition especially predisposing them to be easily wrought up to a pitch of morbid excitability; the painful dentition of which they are victims supervenes as an exciting cause of additional hyperæmia of the nervous tissue, and the high atmospheric temperature of the summer months co-operating with these causes suffices to induce that morbidly exalted state of circulation in, and tumultuous excitement of, the nervous centres which induces that fearfully fatal disease called in Europe infantile diarrhoea, and in America cholera infantum.

Noxious Effluvia are powerfully co-operative with solar heat in the production of the disease. As Dr. Goodeve justly says: "In spite of exceptions, the places in which the air is most vitiated from privies, cesspools, drains, decaying animal and vegetable refuse, or from over-crowding and concentration of human emanations, are those in which cholera has generally been most fatal and most widely spread." The terminal branches of the sensory nerves spread over the large surface of the pulmonary mucous membrane, transmit directly to the nervous centres the unhealthy impressions made on them by foul emanations or noxious effluvia; and as the primary receptive surface of these impressions is especially great, the converging effects produced on those centres are great also, and hence the profoundly depressing influence through the myriad paths of reflex action exerted on the whole organism.

Impure Water is, probably, of all avoidable causes of cholera, the most common and the most baneful. In proof of its potency I shall mention only one experiment, which was performed several years ago on a large scale, and which was as free as possible from error. The population experimented on numbered

between 400,000 and 500,000. The Lambeth Water Company drew its supply from the Thames at Ditton, above the influence of the London sewage and tidal flux; the Southwark and Vauxhall Company drew its supply from the river near Vauxhall and Chelsea. The water of the Lambeth Company was tolerably pure; that of the Southwark and Vauxhall Company was very impure. The water of both companies was distributed in the same district at the same time and among the same class of people, the pipes of the two companies being laid pretty evenly in the same areas, in many places running side by side in the same streets, and the houses supplied being pretty equally distributed. The deaths in the houses supplied by the Lambeth Company were at the rate of thirty-seven, and in the houses supplied by the Southwark and Vauxhall Company at the rate of 130 to every 10,000 persons living. It thus appears that of the drinkers of the foul water about three and a-half times as many as those who drank the purer water died of cholera.

The explanation given above of the mode of action of noxious effluvia is precisely applicable to the mode of action of bad water in causing cholera, the difference being only that whereas noxious gases act on the terminal branches of the sensory nerves of the pulmonary mucous membrane, bad water acts chiefly on the terminal branches of the sensory nerves of the alimentary mucous membrane. I am well aware that noxious gases and bad water may be said with much reason to exert their pernicious influence by poisoning the blood. They are supposed to do so when they engender typhoid fever; but even typhoid fever is not contagious, and with respect to their action as causes of cholera it must be borne in mind that they can, and, therefore, probably do, operate chiefly in the manner explained above, and therefore as excitants of the nervous system in the same manner, and with the same result as are characteristic of all the other causes of cholera already passed in review; and this conclusion is justified by the consideration that inasmuch as those causes are proved to operate dynamically in either predisposing to, or in effecting the development of, cholera, so it is probable that noxious effluvia and impure water, when causative of cholera, operate in the same way.

Nocturnal influences favouring the advent of cholera have often attracted attention. It has been noted by many observers that cholera begins most frequently during the night, and especially between two and four o'clock in the morning. In my work on Cholera I have enumerated several circumstances, among others the fall of temperature during the early morning hours, which conduce to this result; but here I can only advert to that cause

which I regard as the most potent. I have elsewhere * adduced proofs that during normal and profound sleep there is a maximum afflux of blood in the automatic nervous centres, the brain being meanwhile anæmic. Now, during the hours immediately after midnight sleep is, as a rule, the most profound ; and, meanwhile, the spinal cord and sympathetic nervous centres are in a state of hyperæmia. This condition, when excessive, is, as I have shown, the proximate cause of cholera ; and, therefore, while it obtains in a lesser degree during sleep, those nervous centres are at that time susceptible, in a maximum degree, of those influences which originate cholera. Hence it is that this disease, as well as epilepsy, is most prone to surprise its victims in the night.

Fear, that especially potent cause of cholera, needs only to be named in order to suggest how exclusively it does its deadly work through the agency of the nervous system. There is an old and well-known story of an encounter outside an Eastern city between the plague-demon, when about to enter the city, and a citizen. The citizen asked the demon what he was going to do there, and the demon said he was going to slay three thousand people. When the demon came out of the city, the citizen taxed him with lying, for thirty thousand people had been slain. The demon replied, "But I only slew three thousand ; fear slew the rest." This story contains a great truth, which is attested by many medical witnesses in India, Europe, and America. Innumerable observations prove that those persons who are depressed or alarmed are most likely to become victims of cholera, that in the presence of a cholera epidemic panic intensifies its force, and in many instances speedily develops simple diarrhœa into cholera itself, and that confidence proves both helpful in warding off an attack and in the struggle for life of patients already in actual danger. When at Southampton during the epidemic of 1865 I was fully assured by observations in several cases of the great influence which fear, on the one hand, and confidence, on the other, exerts on the progress and termination of the disease. Sir Thomas Watson, referring to diarrhœa, says, "A curious exciting cause is to be found in *mental emotions*, and especially the depressing passions, grief and, above all, fear. A sudden panic will operate on the bowels of some persons as surely as a black dose, and much more *speedily*." It is well known that many soldiers, especially young ones, are attacked with diarrhœa when going into battle. Several cases have been observed in which diarrhœa has occurred almost immediately after a railway colli-

* "Neuralgia and Kindred Diseases of the Nervous System : their Nature, Causes and Treatment." By John Chapman. London : 1873, p. 171.

sion.* In short, as pithily remarked by Dr. Forbes Winslow, "during an attack of cholera the patient who has the least fear of dying has, *ceteris paribus*, the best chance of living." Can these facts be accounted for by the organic poison or microbe hypothesis? It is impossible to answer this question except in the negative.

In cases of the kind here referred to, congestion of the nervous centres appears, physically speaking, to be the primary fact; but I apprehend that the order of causation is as follows:—Terrifying or exciting impressions, suddenly communicated, are conveyed to the sensory ganglia, and are thence distributed to the cellular structure constituting the cerebral convolutions; these are thrown into tumultuous excitement, which is propagated along the motor tracts with the rapidity of lightning down the whole spinal axis, and laterally to the ganglia of the sympathetic; these becoming suddenly swollen with blood, instantly act with intensely vehement energy, and diffuse their subtle stimulus in all directions. As the source of the powerful impressions which they have, in this case, received is cerebral, so the *chief* direction in which the vaso-motor impulses are reflected is towards the brain; hence, quick as thought, the cerebral arteries are contracted with preternatural energy, and thus, in extreme cases, the brain being rendered comparatively bloodless, the person is stunned as if by a blow, the face becomes pallid, cold sweat sometimes exuding from it, if consciousness is not abolished, mental power is still greatly enfeebled, the temperature of the surface of the body is lowered, the stomach and intestines are preternaturally stimulated, their mucous membrane exudes its appropriate secretion superabundantly, their peristaltic action becomes excessive, and thus diarrhœa and cholera, originating in mental emotion, are clearly due to the same *proximate cause* as that which, as I have endeavoured to show, is operative in all the other cases in which the predisposing or exciting causes of the disease are widely different.

Insanity has been found to be conducive to the development of cholera. A physician acquainted with the facts has informed me that when a certain lunatic asylum was within the sphere of a cholera epidemic, a much larger proportion of its inmates than of the population outside were struck down by the disease. This fact is in striking accord with the theory above expounded; for, evidently, the nervous systems of persons suffering from cerebral disorder are sure to be much more excitable, and much more prone therefore to become excited and

* "Injuries of the Spine and Spinal Cord, without apparent Mechanical Lesion." By H. W. Page. London: 1883, p. 162.

hyperæmic than are the nervous systems of persons in ordinary health.

The rapid survey now accomplished of the proximate, predisposing, and exciting causes of cholera, presents, I venture to affirm, a complete and satisfactory explanation of the genesis of the disease, of every individual symptom of it, and of every *post-mortem* phenomenon observable in the bodies of those to whom it has proved fatal. This characteristic will be admitted by all scientific thinkers to be a strong argument in favour of its truth. When, moreover, it is also found to afford a complete explanation of the mode of action of every force and influence known to become, directly or indirectly, causative of cholera, and when it is borne in mind that no other theory yet put forward affords any explanation whatever either of the essential nature of the disease, or how the known causes of it operate, the presumption that the *rationale* here expounded is strictly true amounts as nearly to certainty as is possible within a region in which mathematical proofs are not available. Now this theory implies in no sense whatever the existence of a cholera poison; and by accounting for every fact connected with the disease, not only without assuming the existence of such a poison, but in a manner which virtually negatives its existence, it presents evidence precisely as forcible as is that in proof of its own validity that cholera is originated and developed independently of any specific poison, microbe, or cholera germ, and that, therefore, *as causative of cholera*, they probably exist only in the imagination of certain pathologists and of those who too credulously accept their dicta.

If cholera is not produced by a blood-poison it is not likely to be contagious, and that it is not so experience of it affords convincing proofs.

During the cholera epidemic in Paris in 1831 over 55,000 persons were attacked, and of these over 18,000 died. Of the fatal cases only 164 were cases of persons whose duties or profession called them to nurse, or prescribe for, the sick; and these 164 formed part of over 2,000 persons thus employed. At St. Petersburg, of 58 persons employed in the hospitals only one had the disease. At Moscow, out of 376 persons attached to the hospitals only six were attacked. During the epidemic of 1866 in New York, 123 out of 800 inmates of the work-house died; but of the fourteen house physicians and surgeons who were employed in the institution, some of them being in constant attendance on the sick, not one suffered from the epidemic. Dr. Goodeve, late Professor of Medicine in the Medical College, and first physician to the hospital connected with that

college, at Calcutta, whose professional life has been mainly spent in India, states in his valuable paper on Cholera that "the majority of medical men in India, accustomed to see cholera year after year, to be in constant intercourse with the cholera sick, and to see the general immunity of hospital attendants and of themselves, doubt the contagiousness. . . . I should, as far as my own experience goes, say that cholera does not spread from the sick to the whole by any rapidly acting emanation." Dr. Morehead's observations support the view of the non-spreading of cholera in hospitals through contagion. They were carefully conducted through three epidemics in Bombay, and though he refrains from drawing positive conclusions, his facts are not in favour of contagion. Another important Indian authority, Sir Joseph Fayrer, states that "he had seen hundreds of cases of sporadic and epidemic cholera, but had seen nothing to make him believe there was anything of contagion in connection with the disease." Another and equally authoritative witness, Dr. J. R. Lewis, whose experience of cholera was also gained in India, says: "For fourteen years he had studied cholera, and had never seen anything to lead him to think it contagious. *It was the custom in India to treat cholera in the same wards as other diseases, and no evil resulted.*"

Surgeon-General J. M. Cunningham, Sanitary Commissioner with the Government of India, states that "from the record of about 8,000 attendants on cases of cholera in India it is proved that they suffer no more than other people living in the same place. There is no danger in attendance on cholera cases." This statement is cited by Surgeon-General Hunter in his report on the cholera epidemic in Egypt, and he then adds:—

My personal experience of cholera in India is in accordance with the opinions above expressed. The professional staff, a large body of students and attendants, of the Medical College and Hospital, Bombay, who were more or less in frequent communication with cases of cholera, and many of whom were also engaged from time to time in performing *post-mortem* examinations, appeared to enjoy comparative immunity from the disease without any special precautions being taken. Experience gained during the recent epidemic in Egypt confirms still further these facts. It was no uncommon thing to hear from medical officers and others that their clothing and persons had been covered with the discharges from cholera patients, which had been allowed to become dry; yet no evil results followed. Circumstances rendered it necessary that the British officers serving with the Egyptian army should attend on the cholera sick, wash the bodies after death, according to Moslem usage, and afterwards bury them, and yet in no single instance, if I am correctly informed, did they contract the disease.

The recorded experiences of the epidemic of 1849 in England established the fact that the washers of large collections of linen soiled by cholera patients in many large hospitals did not suffer seriously in that year. An immense number of *post-mortem* examinations in cholera cases have been made in most places, the contents of the intestines being submitted to all kinds of examination, and, as is well known, without any evil consequences to the examiners. Professor Aitkin says: "During my experience as demonstrator of anatomy in the University of Glasgow, for a period of six years (including the severe epidemic of cholera there in 1848-49, and during which time almost all the subjects for dissection had died of cholera), not a single student suffered from cholera." The experience in Edinburgh was, according to Dr. Alison, precisely similar. "It is certain that the dissecting rooms there were supplied during the greater part of 1848-49, as they were in 1832, almost exclusively by cholera subjects, and in neither year was there a single case of the disease among the numerous students attending the rooms." In fact, the intentional inhalation of the emanations from the blood and evacuations of cholera patients have been followed by no evil results; and the attempts of the most varied kind which have been made to impart the disease by inoculation and by introducing its different products in every way into the system have been altogether without success. Dr. Koch has made experiments of feeding mice and other animals "on the dejecta of cholera patients and the contents of the intestines of cholera corpses. Although these experiments were constantly repeated with material from fresh cholera cases, our mice," he says, "remained healthy. We then made experiments on monkeys, cats, poultry, dogs, and various other animals, that we were able to get hold of; but we were never able to arrive at anything in animals similar to the cholera process." The significance of these negative results derives especial importance from the fact that animals do suffer from cholera. Various animals, including dogs, cats, hares, birds of different kinds, and even fish, thus suffered during an epidemic of cholera in Austria. It is expressly stated that the disorder affecting the animals was "too generally spread, and too closely connected with the epidemic character of cholera, to admit of its being traced to accidental causes, or its being considered unconnected with the prevailing constitution." When in 1832-3, cholera was epidemic in Edinburgh, horses and cattle suffered from diarrhoea and cramps; their blood became viscid, and the post-mortem appearances were similar to those observed in man. At Lucknow, in 1872, the horses of the 19th Bengal Native Cavalry suffered in the same way, and Surgeon-General J. Murray states that during a cholera epidemic at Agra in 1864,

his dog suffered from the rice-water evacuations characteristic of cholera, with which he is convinced the dog was affected. Birds suffer most *—a fact in interesting accord with, and alone explicable by, my hypothesis; for the circulation of the blood in them is more active, and it is more highly oxydized than it is in any other class of animals. Probably their peculiar susceptibility explains the often observed fact that they leave places where cholera is epidemic. Dr. Koch's microbe has attracted a great deal of attention, but according to the alleged discoveries of Drs. S. Maurin and Lange, it is only one of the organic phases of four successive transformations. They assert that the second of these "is the immediate agent in the causation of the disease, and that the bacilli, which in due course are derived from it, and consequently appear in the evacuations, are, as Koch found them, perfectly harmless as such.† But the most decisive quietus which Dr. Koch's comma-shaped bacillus, considered as the cause of cholera, has received, is from the hand of Surgeon-Major T. R. Lewis, Assistant-Professor of Pathology at the Army Medical School, Netley, who, as mentioned above, has worked at the subject with great assiduity for a long period, and whose observations were made during several years in India, and this year at Marseilles: referring to the various microscopic organisms found in choleraic dejecta, one kind predominating in some cases, and another in others, he says, "therefore, the selection of the comma-shaped bacilli as the *materies morbi* of cholera appears to be entirely arbitrary." He then adds these words, which are absolutely fatal to Dr. Koch's theory:—"Comma-like bacilli, identical in size, form, and in their reaction with aniline dyes, with those found in choleraic dejecta, are ordinarily present in the mouth of perfectly healthy persons."‡ Indeed, the more the subject is examined the more the reasons strengthen for disbelieving altogether that either Dr. Koch's or any other microbe can ever originate cholera. It is true that a like idea has often been put forward, but only to be rejected. During the last forty years the notion has been from time to time entertained that cholera may be due to microscopic organisms in the air and water of the vicinities where the disease prevails, as well as in the evacuations of cholera patients. At the International Medical

* See "Notes on Cholera in relation to Animals." By Surgeon-General C. A. Gordon, M.D., C.B., in *The Medical Press*, Sept. 10, 1884.

† See article on "The Cholera Fungus," in *Medical Times and Gazette*, September 6, 1884.

‡ A Memorandum on the "Comma-shaped Bacillus," alleged to be the cause of Cholera. By Surgeon-Major Timothy Richards Lewis, M.B., Assistant-Professor of Pathology, Army Medical School. This masterly and important memorandum was published in the *Lancet*, September 20, 1884.

Conference on Cholera held at Weimar in 1867, the subject was fully discussed and evidence was adduced of the existence of swarms of microscopic, fungi-like bodies, in the evacuations of cholera patients. Since then like discoveries have often been made and published.

Dr. T. R. Lewis and Dr. D. Cunningham, medical officers attached to the Sanitary Commission in India, have conducted researches for the discovery of a specific organism in connection with cholera. Their inquiries and experiments, extending over a long period, afforded no evidence in favour of "the existence of a specific poison contained in cholera excreta peculiar to them alone, and giving rise to special phenomena." The researches, conducted jointly by these two physicians until 1878, were continued afterwards by Dr. Cunningham alone; and in a paper "On the Development of Certain Microscopic Organisms Occurring in the Intestinal Canal," he "shows that microscopic parasitic organisms are by no means uncommon in excrementitious matter; that they increase in numbers under known conditions; that certain parasitic forms are specially associated with particular forms of disease, *without holding any causal relation to them.*"

Messrs. Roux and Strauss, who last year were sent to Egypt by the French Government to investigate the cause of cholera, and who have since been to Toulon for the same purpose, have indeed found microbes or bacilli in the intestines of cholera patients. They report that in cases of very sudden death from the disease a bacillus, comma-like in shape, is constantly found in the mucus of the small intestine; but, as Dr. Strauss himself stated to the *Société de Biologie* on the 19th of August last, it is impossible to affirm that it is the cause of cholera, for it has been found in London water, in the dejections of dysenteric patients, and in the mucoïd secretions of women enjoying ordinary health. The various facts above mentioned seem to me to deprive Dr. Koch's microbe of whatever importance has been ascribed to it, and reviewing the result of all the researches hitherto made with a view to discover a specific kind of micro-organism as the cause of cholera, I am constrained to conclude that they are wholly negative.

The evidence already adduced in support of the proposition that cholera is not contagious might be indefinitely increased, but it amply suffices, as it seems to me, to prove that contagiousness is in no sense of the term a characteristic of cholera, and, in so far as it does so, to confirm implicitly the truth of the hypothesis above propounded. Facts, however, of another kind—viz., those illustrative of the mode of onset of the disease, point indubitably to the same conclusion.

The literature of cholera teems with evidence that when the
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disease appears in cities it very generally attacks individuals in different localities, who have had no communication with each other, either simultaneously or in very quick succession. As a general rule it gives warning of its approach in the shape of premonitory diarrhoea; but the suddenness and simultaneousness of its deadly onslaughts at different points far distant from each other are only too often most impressively manifested. When it appeared in Lord Hastings' camp in 1817, it destroyed 5,000 people within the first five days of its visitation. It broke out in Paris in 1832 on the same day, and, so to speak, at the same hour, in four different quarters; within eighteen days from its first appearance it had reached its climax, had invaded every quarter of the city, and had already destroyed 7,000 of the inhabitants. At Kurrachee, in 1846, according to the report of Mr. Thom, the disease suddenly burst forth in a few hours in every European regiment, whether in camp or in barracks, in every tent and in every house, and it was at its acme in forty-eight hours afterwards, when, instead of spreading further, it gradually and steadily declined. Dr. Wakefield, in his book on "*Asiatic Cholera*," says that, when in India, he, on several occasions, saw cholera present itself simultaneously in different parts of the same village, but exclusively along the course of a straight line passing through the locality. He also records that, about fifteen years ago, a dust storm, accompanied by rain and hail, burst over the fortress of Gwalior, which is 300 feet high, and which until then was free from cholera. The storm had scarcely ceased when the disease suddenly broke out at several different points of the fortress, and at places surrounding it on the rock. The actual outbreak of the epidemic now prevailing at Spezzia is alleged to have occurred immediately after a storm, and within a few hours afterwards to have destroyed the people by forties and fifties in different parts of the town.

The same distinctive feature of cholera—viz., its outburst at the same time in different places remote from each other, was observed both in France and England during the epidemics of 1849, 1853-54 and 1865. Physicians in the United States, report the same phenomenon, which was especially noticeable at Memphis on the Mississippi as well as at New Orleans in 1848, and at Philadelphia in 1849. Notwithstanding the vast array of facts here adverted to—facts which prove absolutely that cholera may originate afresh and independently in any locality presenting conditions conducive to its development—believers in the existence of a specific poison as the cause of the disease persist in maintaining that that poison is generated in India, and that, directly or indirectly, it is imported from thence and diffused from time to time over different parts of the globe. Last year

the assertion was made, and was echoed and re-echoed by the continental newspapers, that the cholera epidemic which devastated Egypt was brought there by the steamer *Timor* from Bombay; and yet it was proved that there was no case of cholera on board either during the passage or when the steamer reached Port Said, and that cholera had existed in Egypt some months previous to the outbreak at Dametta on the 22nd of June. Again it was alleged that cholera was this year imported into Toulon; but the first result obtained and proclaimed by the commission of inquiry instituted by the government, was a conviction of the physical impossibility of assigning the origin of the outbreak of the epidemic to an importation of the disease, or of the germs of it, from without. Within the first twenty-four hours of the appearance of cholera at Toulon, there were five fatal cases, which occurred in different parts of the town distant from each other, the patients having had no communication with each other. On the 22nd there were thirteen deaths, says Dr. Bourgarel, "at the most opposite points and everywhere at the same time." In like manner "the first fifteen deaths from cholera this year at Marseilles took place, without exception, at fifteen different points, in fifteen different streets, and more or less distant the one from the other." In short, the more the history of the origin of cholera epidemics is studied the clearer and more assured becomes the conviction that the disease is not spread by means of contagion.

Moreover, epidemics of truly contagious diseases do not cease suddenly; but cholera often does so—just as it appears, so it sometimes disappears, suddenly after a storm. In India this fact has been observed. It has been known to cease there after a heavy fall of rain. It declined after a hurricane which took place in Madras in 1818. A regiment suffering severely from cholera in camp, on the march has been known to lose it when getting into barracks. Prof. Goodeve mentions that "H.M.'s 63rd Regiment suffered extremely during the greater part of its march from Poonah to Bellary, but entirely lost the disease in two or three days after getting into barracks in the unhealthy station of Bellary."

Again, it is well known that individuals who, while suffering from cholera, are removed to healthy situations, do not become centres of a new infection. Instances are adduced in opposition to this statement; but their force is lost in the overwhelming number of those which may be advanced in support of it.

There is no exaggeration in the statement that at least 30,000 inhabitants of Toulon and Marseilles fled from those two theatres of the epidemic; this large number of men and women were, according to the believers in the contagiousness and importability of cholera,

suffused with the choleraic principle, and ought to have carried it with them, and to have sown the germs of the disease as they went along. There were thus 30,000 experiments, the results of which we can now appreciate. Well, what has come of the dissemination of the cholera poison in every locality overrun, and now or recently inhabited by these fugitives? In fact, nothing: in no town, in no village, I do not say between Toulon, Marseilles and Paris, but in all the countries of Europe, has there been one new cholera focus, the origination of which can be attributed to a traveller from Toulon or Marseilles.

This important statement made to the Academy of Medicine by the eminent authority, Dr. Jules Guérin, is confirmed by Dr. Rochard, who, in his despatch of June 27 to the Minister of Marine, says:—"Up to the present time there is not a single case in the hospitals, not a single fact of transmission either in the surroundings of the patients, or in the town, or in the adjoining villages, where some of our cholera patients have gone to die."

Having now placed before my readers a body of evidence sufficient, I hope, to convince them that cholera is in no sense whatever due to a blood poison, or the importation of cholera germs, and is in no degree contagious, I proceed to consider the measures which should be adopted in order either to avert or to cure the disease.

The attainment of correct conceptions concerning the nature and causes of cholera is one thing, but to be duly armed in readiness to meet its attacks, and to know how, effectually, to resist them is quite another thing. The truth of the adage, "knowledge is power," may, however, be exemplified, I believe, in respect to cholera, as it is in all other spheres of human experience. Having learnt what are the several factors in the production of cholera, and what are their several powers, positive and comparative, when acting separately or conjointly, we have learnt that some of them are avoidable altogether, some of them partially avoidable, and that though the cosmical factors are not avoidable, their power for evil is reducible to a minimum by preventing any co-operation with them of one or more of those factors which are capable of being subjected to our control.

In a very large number of cases, cholera is induced by the conjoint operation of a cosmical cause, and of one or more of those which are avoidable, and would not be developed without their conjoint agency. When adverting to infantile diarrhœa, I showed that, as a rule, the disease is the product of at least three factors—viz., (1) extremely rapid growth, and, therefore, extreme vascularity of the nervous system; (2) irritation of the dental nerves; and (3) great solar heat. Youths, after the

advent of puberty, may cut their wisdom-teeth, and persons of any age may suffer extremely from dental disorders causing great constitutional disturbance, in the midst of an especially hot summer, but one of the three factors just mentioned—namely, extremely rapid growth of the nervous system—being absent, the other two are insufficient to produce fatal diarrhœa, which the three conjointly engender to an appalling extent. Or, again, if in the case of the child, either of the other two factors, dental irritation or summer heat, be absent, the two remaining are generally insufficient to engender the disease; but, of course they would suffice if the action of one or both were especially energetic. In like manner great summer heat, insufficient, however, to produce cholera in inhabitants of lofty positions, may easily produce it in persons dwelling in low lying regions; but abstract the factor, heat, and such persons may continue to occupy low sites with impunity. Prolonged marches, pilgrimages, and ordinary travelling on foot in temperate climates, do not induce cholera, because each of them is a feeble factor, and needs therefore the conjoint action of an especially powerful one in order to become effective: the summer heat of Europe is not sufficiently powerful, but that of India is, and hence the reason why these factors do not manifest themselves as causes of cholera in Europe, and why in India they do. Noxious effluvia, impure water, bad food, eating to excess, purgative medicines, opium, alcoholic drinks, and, above all, fear, are severally able, in combination with one or the other of the cosmical or epidemic forces, to engender cholera in temperate as well as in tropical climates; and it is obvious that if two or more of these endemic, or more or less avoidable factors, co-operate with a cosmical one, the cholera engendered will, in proportion to the number of factors which are co-operative, be the more rapidly developed, will be of a type increasingly severe, will be more fatal, and, in cases of recovery, will be recovered from with more difficulty.

The lesson inculcated by these considerations is easily learnt: in tropical climates, he who is intent on escaping cholera, must avoid *all* its endemic or avoidable causes during *every part* of the year; but in temperate climates a less rigorous discipline is consistent with comparative safety. Of course in proportion as climates increase in resemblance to those of the tropics, the statement just made respecting tropical climates becomes applicable to them. During winter in Central and Northern Europe, its inhabitants may inhale noxious effluvia, may drink water containing a considerable amount of organic matter, may eat bad food, may eat good food to excess, may take excessive quantities of alcoholic drinks, may even take opium or purgatives as freely as they please without inducing cholera; but during ordinary summers, to do

all or any of these things is attended with danger of developing the so-called "sporadic" or "European" cholera, and during unusually hot summers, especially if they are characterized by the presence of an "epidemic influence," is very likely to bring on what is called "Asiatic" cholera.

It is evident that some of the avoidable causes of cholera just mentioned are easily avoidable at any time; that some are more or less easily withstood, and that even gluttony and drunkenness, together with the several phases of indulgence in alcoholic drinks between drunkenness and temperance, may be to a considerable extent restrained. We know also that the evil effects of bad water may be to a great degree, if not wholly, neutralized by boiling and filtering it. A good deal also can no doubt be done by the inhabitants of even the least cleanly and healthy dwellings to free them from their impure air and sickly odours, and generally to render them healthier than they usually are.

I confess, however, that I have not much hope that the measures tending to avert attacks of cholera just indicated, are likely to be adopted to any notable extent by the present generation. The habit of drinking to excess, if once thoroughly established, is eradicable with great difficulty; and even the temporary arrest of that habit in the presence of danger can scarcely be expected from those classes of the community most prone to indulge in taking alcoholic beverages. Though the danger from drinking water containing organic matter may be minimized by boiling and filtering, it is probable that the cost of fuel for boiling it and of an effective filter would prevent the great majority of lower-class families from either boiling or filtering the water before they drink it; and their unwillingness to incur such an expense is likely to be strengthened by the consideration that cholera is but an occasional and comparatively rare visitor. Those inhabitants of inferior and more or less unhealthy neighbourhoods, who are acquainted with the causes of the unsanitary conditions which prevail, and who would be glad to co-operate in removing those causes, feel painfully that the work to be done exceeds their powers, and that unless the owners of the dwelling-houses in the locality will effect the needful improvements, those conditions must continue to be endured, at least until some governing power, municipal or otherwise, is constrained to intervene. In short, it seems to me that the general abolition of the abolishable causes of cholera will be one of the many general results of human improvement manifesting themselves simultaneously in numerous and different directions—physical, intellectual, and moral—as consequences of general and efficient education. Until then, those cosmical forces which determine the advent of cholera at different periods will continue pre-

eminently fatal to the generally intemperate, and especially to the intemperate drinkers of alcoholic liquors, to the drinkers of water containing a large amount of organic matter, and to the dwellers in localities unhealthy in consequence of defective drainage and other deleterious conditions already alluded to.

If cholera visits any place several times at more or less distant intervals, such repeated visits should, in my opinion, be regarded as indubitable evidence that that place is in urgent need of sanitary reform of some kind. Persons residing in it, or in the part of it in which cholera has declared itself, are partaking of its insalutary influences, and are therefore predisposing themselves to be easily acted on by one or other of the cosmical forces just mentioned. Now, while repudiating the doctrine that cholera is contagious, and consequently having no fear that the disease may be communicated by the sick to the healthy, I am strongly of opinion that persons ought not needlessly to expose themselves to such insalutary influences, especially when they are more than ordinarily dangerous; and therefore, that when cholera appears in such a place, every person not needed to tend the sick, and not constrained by other causes to remain, should leave it as speedily as possible. Of course, such persons can safely return, at least so far as cholera is concerned, as soon as the epidemic influence has completely ceased to operate.

It has been shown that mere lowness of site is itself conducive to the development of cholera, and hence the well-known fact that the disease is specially wont to manifest itself in towns or villages along the borders of rivers. When, by means of its herald, "summer diarrhoea," cholera threatens to invade such low positions, safety should be sought by moving to higher levels. Many of my readers will probably exclaim, "But to carry out these prescriptions will, as a rule, be extremely difficult, and in a large proportion of cases impossible!" I know it, but still can only rejoin, the inhabitants of such places who remain in them when cholera is hovering near them do so at the peril of their lives.

Having, in the foregoing remarks, indicated the lines of duty in respect to the removable or avoidable causes of cholera, I will now advert to certain causes wholly beyond human control, and not wholly within the reach of human comprehension. I refer to what I have designated the cosmical causes of cholera.

I have already said that there are reasons for believing that cholera may be originated by one factor alone. It is indeed probable that in certain cases heat and electricity are each sometimes solely operative in producing the disease, just as heat alone produces the disease called "sunstroke." Such cases are often what the French call *foudroyants*, and are known in India as *cholera-sicca*—cases characterized by the absence of

premonitory diarrhœa, and by the awful swiftness with which men seemingly in the full vigour of health are destroyed. "Thus at Kurrachee, in 1846, people are said to have died within less than one hour from the time they were seized." Many died within a few hours from the time of seizure, and in these cases, says Mr. Thom,

Vomiting and purging were not always present. Sudden collapse, ending in profuse sweating, were the most prominent symptoms—in fact, asphyxia had already taken place. It was often found that the pulse had ceased at the wrist, the eyes turned up, the voice hollow and feeble, before the natural hue had given way to that horrible lividity which is characteristic of the disease, so instantaneously was the power of life arrested.

Dr. Milroy states that at Teheran, in 1846, "those who were attacked dropped down suddenly in a state of lethargy, and died at the end of two or three hours without convulsions or vomiting, but from a complete stagnation of blood."* Although the majority of cases at Kurrachee in 1846 lasted longer than those just mentioned, yet the swiftness of onset, development and decline of the disease presented the likeness of a storm suddenly bursting over the city. I have already said that it seems to me not unlikely that when the mode of attack of cholera is gradual, is characterized by premonitory diarrhœa, and presents itself over a wide area, the main factor in its production is solar heat; and that when the mode of attack is sudden, concentrated and intense, the main factor in its production is that form of solar force we call electricity: now the question arises, whether in those cases in which cholera is seemingly due, either exclusively or mainly, to cosmical forces, anything can be done independently of medical aid to avert its attacks.

One very important duty in times when the diurnal range of temperature is great, as it is especially apt to be in September and October, is to take especial care that the body is kept thoroughly warm during the night. I believe many attacks would, by the observance of this simple precaution, be prevented. Then, in proportion as the disease seems to be due to solar heat, the utmost efforts should be made to ensure very free ventilation of every dwelling, to make constant use of cooling appliances of every available kind, including, especially, the daily use of cold baths; and, as far as possible, to migrate during the time of danger from low-lying to elevated ground, it being borne in

* It is worthy of remark that these two examples of extremely sudden attacks of cholera occurred in the same year—one in India and one in Persia; whether or not they both occurred at the same time of the year is a question of great interest, which, however, I am not now able to answer.

mind that the temperature becomes lower and the air purer in proportion as the elevation increases. When, owing to the extreme suddenness and intensity of an outbreak of cholera, there is reason to believe its main cause to be rather electric than thermal, the affected area is likely to be comparatively limited and fairly defined. If so it will not be difficult for a large proportion of its inhabitants to protect themselves by leaving it for a time, and it is obvious that the more quickly they do so the more certain their safety. But during all outbreaks of cholera, however effective may be the protection afforded by flight from the region in which the disease presents itself, the great majority of people, being poor and constrained to earn their subsistence in the locality where they live, cannot leave it, and hence must face the enemy on his own chosen ground. Here, therefore, arises the question, what aid, if any, can medicine bring to the sufferer from cholera?

In attempting to form any estimate of the comparative value of different medicines used in the treatment of cholera, the following facts must always be borne in mind: the mortality of the disease differs—*first*, in different years; *second*, in different seasons of the year; and, *third*, during the different periods—the outset, development, and decline—of each epidemic. This last fact is well illustrated during the outbreak at Kurrachee. Of the

First	100	patients	admitted	into	hospital	79	died.
Second	„	„	„	„	„	66	„
Third	„	„	„	„	„	50	„
Fourth	„	„	„	„	„	40	„

The treatment all the time remained essentially the same. Moreover, the effects of medicines differ widely according to the stage of the disease in which they are given. During the premonitory stage and during reaction they are powerful for both good and evil; but during partial collapse they are absorbed only slightly, and during profound collapse scarcely at all. These considerations, as well as many others which I need not recapitulate, must be taken into account before an *accurate* appreciation of the statistical results of different methods of treatment can, if it ever can, be arrived at. The following returns of the Medical Council of the College of Physicians must, therefore, be accepted as an *approximative* estimate. It is, however, the best available, and probably sufficiently near the truth for practical purposes. When the different methods of treatment were applied in the various stages—from choleraic diarrhœa to profound collapse—the *general* percentage of deaths following each plan was as follows:—

Eliminants	71·7 per cent.
Stimulants	54 "
Alteratives, calomel and opium	36·2 "
Astringents, chalk and opium	20·3 "

But when the different methods of treatment were tested in their application to cases of collapse only, the results were as follows:—

Calomel and opium	59·2 per cent.
Calomel (large doses)	60·9 "
Salines	62·9 "
Chalk and opium	63·2 "
Calomel (small doses)	73·9 "
Castor oil	77·6 "
Sulphuric acid	78·9 "

It is obvious that in a very considerable proportion of the cases, from the treatment of which the first of the above statistical statements is generalized, the absorbent process was still very active; for otherwise the wide difference in the results of the different methods would not have been possible—differences ranging over 50 per cent. In the more advanced stages of the disease, or during collapse, the differences, according to the second statement, have a range of nearly 20 per cent.—a fact which indicates that absorption in a slight degree still goes on.

It will be observed that, according to the report just given, the average number of deaths in proportion to the number of cases treated is very great—viz., 45·5 per cent. of the cases varying in severity from choleraic diarrhoea to profound collapse, and 68·1 per cent. of the cases in complete collapse. Now, in my opinion, if the medicines given in the cases in question exerted any influence, it was, on the whole, an injurious one; and this opinion is strengthened by considering how especially high is the mortality following the use of eliminants and stimulants in the first group of cases, and of calomel, castor oil, and sulphuric acid in the second. It is evident that if the sufferers had been spared the use of those drugs, the proportion of fatal cases would have been less than it actually was. All experience of the treatment of cholera during the stage of collapse by means of drugs proves that they are useless. The numerous and careful experiments made at the London Hospital in 1866, in order to determine the comparative value of various drugs said to be curative of cholera, resulted in confirming the mournful conclusion arrived at during previous epidemics of the disease—viz., that no drug yet discovered exerts any appreciable power of rescuing patients from the state of choleraic collapse. This fact, like all others characteristic of cholera, is now for the first time rendered quite intelligible

by the hypothesis explained above; for whereas, according to that hypothesis, the automatic nervous centres are in a condition of intense hyperæmia, physicians are acquainted with no drug which has a distinctively ascertained power of abolishing that condition. But while, as it appears, drugs can do no good, they can do much harm, and the reason why they can do so is not difficult to understand.

When cholera has reached its most distinctively characteristic stage, designated *collapse*, the power of absorption by the alimentary mucous membrane exists, if at all, to so slight a degree that, even if drugs which are given are not ejected by the stomach, most of them, at all events, are likely to remain inert so long as the state of collapse continues. If the patient should survive that condition and reach the stage of reaction, the medicines previously administered may seriously interfere with Nature's efforts to emerge from that stage, or from the secondary fever which not infrequently supervenes. Moreover, as the essential nature of cholera has not been understood, and as the medical world is still disputing concerning its nature and causes, medicines have meanwhile been given either at random or without the guidance of any principle which has obtained extensive professional recognition. It appears from the above figures that the three drugs, the use of which has been attended with the greatest mortality, are calomel, castor oil, and sulphuric acid. Considering that purgatives are proved to be especially dangerous to cholera patients *in any stage* of the disease, we easily understand why calomel and castor oil co-operate so effectively, as they are shown to do, with the disease itself in bringing it to a fatal issue. The influence of sulphuric acid appears to be even more deadly than that of castor oil. This acid is supposed to act as an astringent; for this reason it is given in cases of hæmorrhage with the intention of restraining it, and for a like reason it has been given to sufferers from cholera in the hope of lessening the excretions; but to whatever extent it may act as an astringent generally, it will to that extent act as a contractor of the capillary blood-vessels and small bronchial tubes, and, therefore, if the pathology of cholera already explained be the true one, as an intensifier of the algide condition already established. From this point of view, the extremely dangerous influence of sulphuric acid on cholera patients is thoroughly intelligible.

The drugs which, in my opinion, might be given during the stage of collapse most safely and most expediently are bromide of potassium and bromide of ammonium. Bromide of potassium so affects the nervous tissue as to lessen, it is believed, its vital activity; and bromide of ammonium, while supposed to act somewhat similarly, seems also to counteract the tendency to

congestion and to facilitate the capillary circulation, especially in the nervous system. At all events, it is certain that in the treatment of the whole group of convulsive or spasmodic affections, the chief proximate cause of which is hyperæmia of the nervous centres morbidly operative, these drugs are more efficacious than any other of the vast number which have been tried. The conclusion is, therefore, reasonable, if the proximate cause of cholera be what I allege it to be, that the bromides of potassium and ammonium, which should be given together, will, in some cases, exert a beneficial, possibly a curative, influence on the disease; and this conclusion derives strength from actual experience, which attests that the use of these drugs in cases of diarrhœa is attended with decided benefit. I may add that in any case they are, of all known drugs, precisely those which are likely to work the least harm after reaction sets in.

It cannot, however, be too strongly impressed on the public mind that it is during the early phases of cholera—"summer diarrhœa," "premonitory diarrhœa," or "cholerine"—that medication can exert its beneficial influence most potently. So long as the skin of patients suffering from diarrhœa retains its normal warmth, both sulphuric acid and opium may be given with safety and often with signal benefit; but in cases in which the skin has already become abnormally cool, I do not venture to prescribe either of them. My chief reliance, if obliged to rely on drugs, is then on the bromides and such other medicines as, while tending to check the discharges, will neither constrict the blood-vessels nor increase, as opium would, the hyperæmia of the automatic nervous centres.

But even as means of combating the premonitory diarrhœa drugs very often fail utterly. This fact is attested by the enormous mortality due to diarrhœa, not only in those years in which cholera presents itself in its fully developed form, but every year; for during every summer infantile diarrhœa, which I affirm to be a form of cholera, is fearfully fatal. Indeed, whoever recognizes the truth of the doctrine concerning the essential nature and causation of cholera which I have explained, can scarcely be surprised by the great fatality of the several forms of the disease. It is evident that an effectual remedy for it must be one which can exert a great, and at the same time swiftly operative, sedative power over the spinal cord and the sympathetic nervous centres, without lessening the general vitality of the patient meanwhile. As already stated, no medicine yet known can do this; and even if it could, the sufferer from cholera would, probably, be unable to derive benefit from it, seeing that precisely when he most needs it his stomach most persistently rejects it.

Moreover, if the nature of the morbid processes constituting cholera is, as I have shown, wholly dynamic, it is in the highest degree probable that the most effective remedy of the disease will also be found to be dynamic. Now, in 1863, I discovered that the circulation in the sympathetic ganglia, as well as in the spinal cord, and consequently their energy, can be lessened by the application of ice, and can be increased by the application of heat, along the region of the spine. Since that time, my twenty years' experience of the reality of the discovery has been abundantly confirmed by the experience of other physicians in Great Britain, Ireland, France, Germany, the United States of America, and various parts of the British Colonial Empire. In my exposition of the nature of cholera, I divided all its symptoms, prior to the stage of reaction, into two groups, and explained how one group is produced by morbidly excessive activity of the spinal cord, and how the other is produced by a like condition of the ganglia of the Sympathetic. Now, if this explanation be true, and if the power of lessening the energy of those nervous centres, alleged to have been discovered by me, be a reality, the possibility of treating cholera both scientifically and successfully is at length within our reach. And this presumption is greatly strengthened by experience in the treatment of many other diseases which, though not severally but collectively, present symptoms like to those of cholera. I have proved that excessive sweating and excessive secretion by the mucous membranes—indeed secretion generally—can be restrained by a suitable application of ice along the spine; that vomiting, purging, cramps, and coldness of the surface of the body, when met with separately as symptoms of other diseases, are, in the majority of cases, capable of being subdued by the same method. I have demonstrated that the vomiting of all grades of severity incident to pregnancy, the vomiting symptomatic of various diseases, and especially the vomiting of sea-sickness, can be thus arrested. Indeed, when sea-sickness becomes very severe, it is often accompanied by diarrhoea, and assumes a remarkable likeness to mild forms of cholera; and a large amount of evidence has now been accumulated, and partly published,* proving that, in the great majority of cases, at all events, this disorder can now be remedied. A very severe form of it is presented in the case described by Dr. Lee of Philadelphia—a case which affords striking evidence that vomiting, tonic convulsions, cold sweat, and coldness of the surface of the

* See "Sea-sickness, and How to Prevent It." An Explanation of its Nature and Successful Treatment through the Agency of the Nervous System, by Means of the Spinal Ice-bag. With an introduction on "The General Principles of Neuro-Therapeutics." Second edition, 8vo, price 3s.

body are remediable by the spinal ice-bag. I repeat the doctor's words:—"The effects of its application were little short of miraculous. In three minutes the retching ceased, and the spasms were calmed. In a quarter of an hour the patient had fallen into a quiet sleep, and in half an hour her hands and feet were of natural warmth, and her face had regained its wonted colour." Now in this case several of the leading symptoms of cholera were present; and while the vomiting, tonic convulsions, and cold sweating were arrested by the sedative action of the ice on the spinal cord, a like action on what Claude Bernard rightly designated the "frigorific" nerve-centres, so lessened their energy as to enable the lady's hands and feet, within half an hour, to regain their natural warmth and her face its wonted colour. Surely these are the very results desiderated in the treatment of both diarrhœa and cholera.

I have already published a number of cases of diarrhœa, some of them advancing into the dangerous cold stage, some of them also marked by the presence of cramps, in which the results of the treatment in question proved quite as satisfactory as were those experienced by the sea-sick lady just mentioned. Several of these cases were treated by different physicians, who have been good enough to send me reports of their experience of the method in question. Judging from the experience now had by myself and others of that method, I do not hesitate to express my conviction that if in all cases of diarrhœa, whether "infantile diarrhœa," "summer diarrhœa," or "diarrhœa premonitory of cholera," that method of treatment were promptly and properly made use of, the terrible fatality now consequent on that disorder would soon become a thing of the past.

The following passages in small type concerning the Treatment of Diarrhœa and Cholera according to my method are extracted from my book of "Cases of Diarrhœa and Cholera":—

In every case of both diarrhœa and cholera a thorough search for every removable exciting cause of the disease should be instituted, and if any such cause be found, it should of course be removed.

Simple diarrhœa, that is, uncomplicated with any marked fall of the temperature of the body, or with vomiting or cramps, should be treated by applying the spinal ice-bag continuously until the symptoms are overcome, and for some time afterwards at increasingly longer intervals. This will generally be all that is necessary to effect a cure. Indeed, in many slight cases the application of one bag of ice completely suffices to stop the flux and to prevent its return.

Severe diarrhœa, sometimes called choleraic diarrhœa or cholérine, is in fact a mild form of cholera, and should be treated exactly as that disease is directed to be treated in the following paragraphs.

Cholera.—The effective treatment of this formidable malady by my method implies that the physician practising it possesses accurate knowledge of the principles on which it is based, ability and tact to apply them correctly in each of the different cases—although presenting different physiological conditions differently combined, the exercise of the utmost possible watchfulness and untiring care, and the vigorous resolution to insure the doing of what needs to be done both *quickly* and *thoroughly*. While ice to the spine is duly applied, its action must be especially watched, and its application must be modified in accordance with the pathological changes observable. [The experience gained from the first trials of my method of treating cholera, strongly impressed each observer of those trials with this conviction. When, in 1865, Dr. Wiblin strongly commended my treatment, he added—"but all your injunctions must be strictly carried out." Dr. Lake observed, "as with all other remedies of power it requires to be used with discretion, and not continued too long." Dr. Griffin, after recovering from collapse his two cases which he afterwards lost at Freemantle, says that in consequence of their distance from him the application of the ice was continued too long. Mr. Bencraft, in his letter dated October 29, 1865, says—"I have no hesitation in recording my conviction that *if applied in anything like reasonable time*, the ice will save every life; *but I also see that it must be carefully watched*." Within the limits assigned to these directions, I cannot enter into the physiological and pathological reasons why ice applied along the spine of a cholera patient for a certain length of time may save life, and why if applied for a longer time it may endanger or even destroy it; I may observe here, however, that the same power which can reduce the amount of blood in the spinal cord and in the ganglia of the sympathetic from a state of hyperæmia to the normal state, may also be so used as to render those nervous centres anæmic, and that such a state is always attended with danger. It is therefore absolutely necessary to determine by careful observation of the symptoms when the ice has been applied long enough, when it needs to be re-applied, whether its application should be extended along the whole or along only a part of the spine, and if along only a part, which part.]

When about to apply the spinal ice-bag, it is of the utmost importance to select one of suitable size, in respect to both length and breadth. The spinal ice-bags 24 and 26 inches long are suitable for men, those 20 and 22 inches long for women, and, of course, those still shorter and narrower for children. The widths of the bags vary with their lengths. It is therefore necessary to secure one of right length in order to secure one of right width.

If the lungs are healthy, the ice may, as a general rule, be applied in the first instance along the whole spine, or rather—and *this is what I always mean by the phrase "the whole spine"*—from the upper part of the cervical to the middle of the lumbar region, and no lower. As soon as the whole of the ice in the bag is quite melted, the bag should be emptied, refilled with ice, and immediately re-applied. The applications should be continuous until the symptoms abate. If the circula-

tion in the head becomes thoroughly re-established first, the ice may then be omitted from the upper cell of the ice-bag. As soon as the vomiting ceases, and the chest and upper extremities have become warm again as a consequence of the re-establishment of their normal circulation, the ice may be restricted to the lower half of the spine until the alvine discharges cease also, and the lower extremities begin to become warm.

In cases in which collapse continues after both vomiting and purging have ceased, the medical attendant, guided by the special character of the symptoms in each case, must use the ice in such a manner as he judges most likely to subdue them. [At the Hôpital St.-Antoine, Paris, which I visited in 1865, there was an instructive case of this kind. An elderly woman, under the care of Dr. Buchet, had already been treated by the method he then practised, and which chiefly consisted in sponging the patient all over with water, and in then wrapping her up carefully, covering her with an abundance of blankets, and applying hot bottles to the limbs. The measures adopted wholly failed to recover the patient from collapse. She lay in a state of profound stupor: seemingly there was no cerebral action at all. The head was thrown backwards, the mouth was open; when addressed in a loud voice she gave no sign of consciousness; and she looked exactly as if dead. The head and upper extremities were as cold as those of a corpse; the lower extremities were, however, slightly warm. She was quite pulseless. There was neither vomiting nor purging, and no evidence of cramps. I applied ice to the upper half of the spine only, and altogether three bags of ice were used. The treatment began about ten o'clock in the morning, and was continued until about four o'clock in the afternoon. At this time, the temperature of the upper half of the body had increased very decidedly, the head became warm, the pulse perceptible, and the mental power had returned to such a degree that the woman answered questions intelligibly and easily. I restricted the ice to the upper half of the spine in this case for the following reasons:—First, because as the woman had been extremely drained by the discharges which had occurred in the previous stages of the disease, and therefore had but little blood in her body, I deemed it expedient in view of her peculiar condition to concentrate all the spasm-relaxing effects of the ice upon the head and chest; and second, because, inasmuch as there was some activity of circulation in the lower part of the body, it seemed to me that if I had placed ice along the whole spine, and had thus relaxed still further the arteries in the pelvis and lower extremities, the afflux of blood would have become still greater there, and my chances of recovering the cerebral circulation would have been less than they were by the method I actually pursued.]

Allay the extreme thirst during the stage of collapse by non-stimulating fluids. It is well to give the patient the choice of several beverages, and indeed to vary them as he may feel inclined, for it is of the utmost importance to secure the co-operation of mental influences, which are no insignificant aid in allaying sickness. Good beef-tea,

temptingly flavoured; arrowroot made with water, flavoured, *but only flavoured*, with brandy, and made thin, so that it may be easily drunk; *weak* tea and coffee, with but little milk while sickness still continues; lemonade and barley-water may each be given alternately or successively with advantage.

If the purging should subside before the vomiting, injections of beef-tea, or of arrowroot, or of a mixture of both, should be given frequently.

Strychnia, opium, and all drugs which act as nervine stimulants, should be scrupulously avoided. And this remark applies to *strong* solutions of coffee and tea.

It is impossible to state too emphatically that *in cases of cholera under treatment by my method the life of each patient depends in great measure on the nurse in attendance.* THE REMEDIAL POWER OF THE SPINAL ICE-BAG CAN ONLY BE EXERTED WHILE IT IS RIGHTLY APPLIED. IT MUST BE KEPT EXACTLY ALONG THE CENTRE OF THE SPINE: IF IT IS NOT IT WILL DO HARM, AND HAD BETTER NOT BE USED AT ALL. The only method of keeping it in its place which does not involve incessant watching on the part of the nurse, is that of employing the "Ice-bag Jacket," described at page 33 of my volume on "Diarrhoea and Cholera;" and even then in cases of extreme restlessness slight readjustment may be necessary occasionally. To insure the proper application of the bag; to keep each of its cells, when all are used, duly filled, and duly replenished, when needful, with ice; to keep the surface of the patient clean and dry; and duly to attend to the other and various wants of even one patient—all this is a considerable task, and one demanding for its adequate fulfilment considerable intelligence, a resolute will, and a strong sense of duty. I am painfully aware how difficult it is to secure these qualities in attendants on the sick, and how strong in the eyes of many will seem the objection to my method that its successful practice necessitates not only first-class nurses, but many more of them in proportion to the number of patients whom they attend than are usually provided in public hospitals. To this objection I can only reply: no easy method of curing cholera is yet known; drugs have been proved useless: and it behoves all whom the matter may concern, to ask themselves, "Is the life of the patient in question worth saving at the cost of providing the conditions mentioned?" If the answer is affirmative—then, as in cases of cholera, life is destroyed very swiftly, it will be well to insure those conditions not only completely, but *promptly*: an hour, half an hour, nay, a few minutes, lost in delay may seal the fate of the sufferer whose life might have been saved by *swift* as well as judicious action.

In my work on Cholera (p. 182 *et seq.*) I have advised the application of heat over the surface of the body generally, simultaneously with the application of ice along the spine, as well as the free use of hot drinks during the algide stage, and I have adduced what seemed to me strong reasons for these recom-

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mentations; but since the date (1866) when the second edition of that work was published, my opinions on this matter, notwithstanding the seeming force of the reasons for those recommendations, has gradually become so far modified that I now feel by no means sure that it is expedient to act upon them. After long and anxious consideration of the subject, it appears to me that, bearing in mind the small amount of our knowledge and experience in the matter, we should act most judiciously in allowing ourselves to be guided by the instincts of our patients. Now it is certain that when in a state of collapse they crave an abundance of cold, or iced, water, and are glad to have lumps of ice in the mouth; and some physiological and pathological considerations seem to supply valid reasons why it is expedient to satisfy their longings: if the stomach be thus converted into a cold, or iced, water bag, the cold is brought into more immediate contact than it can be otherwise with the great accumulation of nervous centres, called the semi-lunar ganglion or solar plexus (which is behind the stomach), and therefore can exert a more direct influence than the spinal ice-bag can do in relaxing the arteries distributed to the abdominal and pelvic viscera. Moreover, seeing that the intestines are, as I have shown, in a state of intensely active congestion, bordering on inflammation, the temperature of the lowest part of them, and probably of the whole, being abnormally high, we have in this fact an emphatic indication that cold drinks, while supremely grateful to the patients, are likely to exert a beneficial influence. A case was communicated to the Paris Academy of Medicine, September 16, 1884, in which sucking lumps of ice, and the use of iced-water enemata, were attended with strikingly satisfactory results.

Cholera is chiefly a disease of hot climates, and when it is developed in temperate climates it is so during the months when the temperature is at its maximum. Now, seeing what a potent factor in the production of the disease solar heat is, I have been led to think that a part of the rational treatment of cholera may consist in placing the patient in conditions, so far as heat is concerned, as opposite as practicable to those which have induced the disease; therefore, seeing that during cholera epidemics the air, being generally very warm, is saturated with aqueous vapour, the excess of which renders breathing difficult, I have entertained the idea that by placing patients while in choleraic collapse in chambers, the atmospheric temperature of which has been reduced artificially to 60° or 50° Fahrenheit (or even lower according to the dictates of experience), we should, probably, assist powerfully in their recovery. Placing them in hot air baths, as was done at Scutari (but without the simultaneous use of the spinal ice-bag) failed signally; and I hope some one at least of the

large hospitals will establish cold air rooms in which the experiment in question may be fairly tried. As is well known, there is now no practical difficulty, except that of expense, in supplying such rooms, and therefore in thoroughly testing their therapeutical value as aids in the treatment of cholera. I believe they would be comparatively valueless without the simultaneous and persistent use of the spinal ice-bag; but seeing how *extremely* contracted the lungs are during collapse, and how the patients struggle for breath, I am led to think that cold used in the form of cold air chambers, while ice is being applied to the spine, may prove extremely helpful in the treatment of choleraic collapse.

Of course such cold air chambers could only be had in hospitals; but when they are not available cold can be applied externally in the form of cold affusions, followed by vigorous dry friction over the whole surface of the body several times a day. The use of cold as here suggested receives strong countenance from the experience of Dr. W. C. Seaman, Deputy Inspector-General of Hospitals, who says, "I was advised by the late Deputy Inspector-General of Hospitals, Dr. Archibald Stewart, to try the cold affusion which, he said, had been used with success in Bengal." He followed the advice in 1859, and he says, "certainly with some success, inasmuch as the cramps were much relieved by it, and reaction to some extent obtained." In 1865 theoretical considerations caused him to add to this treatment the administration of ten or fifteen grains of calomel at the beginning of it, and he seems to think this was helpful; but while not endorsing his opinion on this point, I am grateful to him for repeating the experiment of the cold affusions, and especially for reporting the result, which certainly seems to have been satisfactory. He says, "The patient almost invariably felt relief; the cramps were mitigated, and sometimes in cases of collapse the pulse could now be just felt. In about five minutes the bath was repeated as before, after which, even in the worst cases, the pulse could now generally be felt; the skin became warmer, and the patient would beg for another bath. With reaction, vomiting and purging gradually ceased. The cold affusion was used at least five or six times in each case; and, as a rule, after each application of it the pulse grew stronger and all symptoms improved."* In seeming confirmation of Dr. Seaman's experience, I am able to cite that of one of my patients, an English clergyman, who, when in Berlin some time ago, suffered from diarrhoea. He plunged head-foremost into a cold bath and then remained in it up to the neck during twenty minutes. He assured me that the diarrhoea quickly and com-

* "Cholera, its Etiology and Treatment." By W. C. Seaman, M.D. A letter published in the *Lancet*, July 19, 1884.

pletely ceased, and that he felt only "*the greatest comfort*" from the treatment. He has often treated threats of diarrhoea in like manner and with like success.

In my opinion the question whether it is wisest to apply heat or cold to the surface of the body generally while ice is being applied to the spine during collapse, is by no means settled. There seems *à priori* strong reasons in favour of both plans: those in favour of heat are fully stated in my book, and my patients recovered while the heat was used; those in favour of cold I have just given, and now it remains for experience on a large scale to dispel all doubt on the subject. Those physicians who have the opportunity of trying each plan have ample assurance in the facts I have mentioned that the use of cold in the manner described is neither dangerous nor disagreeable to the patients. It is especially worthy of remark that Dr. Seaman's choleraic patients "*would beg for another bath,*" and that my clerical friend suffering from diarrhoea "*felt only the greatest comfort*" from his cold plunge.

The foregoing directions wholly relate to the treatment of the incipient and algide stages of cholera. A large number of patients whose lives are prolonged through these stages sink during the period of reaction, the fever, feverishness, or local congestions of which need the most studious attention, in order that they may be prevented or remedied by the judicious use of heat to the appropriate part of the spine. I have already suggested that in cases where cerebral or pulmonary reaction has become established while vomiting, purging, or coldness of the lower parts of the body persists, the ice should be omitted from the upper segments of the spine. But cases occur in which purging, at all events, as well as coldness of the lower extremities, co-exists with a full reaction, merging into slight or severe congestion of the brain, and sometimes, though less frequently, of the lungs. In these cases it may not suffice merely to omit the ice from the upper half of the spine, but it may be necessary, while still controlling the purging by retaining the ice in the lumbar and lower dorsal regions, and while, by its agency, relaxing the vasic spasms of the lower extremities, to effect some contraction of the cerebral and perhaps pulmonary blood-vessels by the application of heat along the upper third, or upper half of the spine. To do this effectually, and at the same time not to overdo it, calls for the utmost discrimination and care—discrimination with reference to the controlling force needed, and care with respect to the temperature of the water used, and the length of time during which it is applied. The changes in the circulation which may be induced by the spinal water-bag are much more rapid than those inducible by the spinal ice-bag, therefore the action of the former must be watched even more carefully than that of the latter. If heat be applied in order to prevent or subdue cerebral congestion, the forehead and pupils of the patient must be scrupulously examined every

few minutes in order to ascertain whether the temperature of the former is falling, and whether the size of the latter is increasing: if so, the bag should be immediately removed, even though it may be desirable to apply it again a short time afterwards. The condition of the lungs may be ascertained not only by the feelings of the patient, by the sputa, and by the degree of ease with which he breathes, but of course much more surely by auscultation; and the use of heat must be determined by the symptoms. If there should be no special but only a general feverishness, or even a decided reactionary fever, this condition may be controlled by the application of heat along the whole spine. It may be stated as a general rule, that in proportion to the gentleness and slow graduation with which the fever, feverishness, or local congestion is controlled, the more satisfactory will be the result, and the less the chance of relapse. If, for example, in order to diminish cerebral hyperæmia, water of a high temperature in the spinal water-bag be applied along the upper third of the spine, the head will, in many cases, become rapidly and extremely, and it may be, in some cases of cholera, fatally cold. After such rapid and extreme contraction of the cerebral arteries, and the necessarily sudden removal of the spinal water-bag, there is danger of vigorous reaction. I advise that the temperature of the water used in these cases be the lowest consistent with slowly attaining the desired end. The vascular system of many patients may be influenced by water at 105° Fahr. It will, however, be found most available at 110°, and may range, particularly if the patient's body be well clothed with fat, to 115°, or even higher.

It must be well understood that the application of heat along the spine is capable of producing vomiting and purging after they have been thoroughly subdued by means of ice, and of causing the body again to become cold. It is impossible, therefore, to overstate the importance of the advice to use these agents only when they are absolutely needed, and to watch their effects with the utmost possible care. The intensity of the cold or heat which is applied, and the length of time it is used, must be judiciously determined and modified according to the exigencies of each case.

The chief things required by patients who have fairly passed through the algide and reactionary stages of cholera are good nursing and good nourishment. No medicine is absolutely necessary, unless to meet some special symptom; but I am of opinion that a gentle tonic is desirable, and that that tonic should consist of the citrate of iron and quinine. I cannot conclude these directions more appropriately than in the following words of Sir Ranald Martin, for it is impossible to over-estimate their importance:—"In cholera, in common with the last stages of violent fevers and dysenteries—as, in fact, in all cases of great exhaustion—the patient must always owe much to the horizontal position, and to careful and unremitting nursing. The most careful nursing and the most attentive watching of the patient are both of the utmost importance in this disease; and so easily is the balance of circulation fatally overturned, that a strict

attention to the recumbent posture is absolutely necessary to success. In no other diseases are these simple matters of so great importance to be attended to, and in the disease under special notice I have seen many a life apparently lost from inattention to them."

So far as I am aware, 41 cases of cholera have been partially or wholly treated by means of the spinal ice-bag; but of these 14 were so inadequately or so improperly treated that whoever is intent on forming a just estimate of the value of the remedial method in question must ignore them. Any one can verify the correctness of this assertion, for each of these 14 cases is reported in my book entitled "*Cases of Diarrhœa and Cholera*," where the numbers prefixed to them are as follows:—25, 27 to 32, 34 to 38, 40 and 41. But even of these 14 cases, defective as the treatment was, *eight were recovered from collapse*.

Of the 24 cases in which a cure was effected, 5 were in state of partial collapse, and 16 were in complete collapse, several being quite pulseless, so that of the whole of the cases 5 were recovered from partial and 26 from complete collapse. The important significance of this statement will be at once understood by all who bear in mind the fact already adverted to—viz., that no drug yet discovered exerts any appreciable power in rescuing patients from the state of choleraic collapse.

Dr. Griffin, of Southampton, who had the care of several of the patients above referred to, summed up the characteristic features of the treatment in question in the following pithy sentence:—"*It stops cramps, vomiting, and purging; it makes the patients warm, and it prolongs life.*" My discovery of the power of exerting a controlling influence over the nutrition and functions of all parts of the body, and my demonstration that in man, and in all animals having a highly differentiated nervous system, diseases, whether called structural or functional, of any part, are really, as a general rule, symptoms or expressions of disorder in that system, reveal and explain the as yet seeming wonder—viz., the stopping of cramps, vomiting, and purging, and the regeneration of animal heat by the application of ice along the spine, as well as the fact now thoroughly established and apparently not less wonderful—viz., that the reactionary fever which in a certain proportion of cases follows the collapse of cholera, is most effectively subdued by the appropriate application of heat along the same region. The more this method of treatment (not of cholera only, but of diseases generally), as well as the physiological principles on which it is based, is studied, the more thoroughly will it become recognized as truly scientific, and therefore as the dawn of a new era in pathology and therapeutics.

The general adoption of this treatment will necessarily be slow, because (1) the great body of the medical profession is still prac-

tically unacquainted with it, and only partially or vaguely acquainted with the neuro-physiology which underlies it; and (2) because people generally are horrified by a method of treatment which, in those cases in which it does not prescribe the application of heat, prescribes the application of cold, by means of ice, along some part or the whole of the spine. People will only cease to be thus horrified in proportion as they are taught so much of neuro-physiology as will enable them to understand the vaso-motor (artery-contracting) functions of the great sympathetic ("frigorific") nerve, and therefore to understand the seeming paradox, but really simple truth—viz., that precisely those persons who suffer most from bodily cold are most needing treatment by the application of ice along the spine, may be most benefited by it, and may be rendered warm meanwhile. While this knowledge is growing, it will be chiefly those diseases for which there is no other known remedy, or none commanding trust, that will be treated according to the scientific principles and method here indicated: such diseases pre-eminently are epilepsy and the whole group of convulsive affections, the hitherto incontrollable vomiting often associated with pregnancy, seasickness, and especially cholera of all grades of intensity. Of all diseases with which I am familiar, cholera is precisely the one in which the power of cold applied along the spine is most astonishingly manifested and is most astonishingly rapid in its action. This truth is attested in the following extracts from the reports of cases:—

CASE I.—The patient "agonized with cramps," suffering from extreme difficulty of breathing, and extremely anxious and restless, was laid on a spinal ice-bag, "*and within five minutes she was in a placid sleep.*"

CASE II.—Before treatment cramps were occurring continually; the patient "had not had five minutes' sleep at a time for them." She was purged "every ten minutes or oftener." Her skin was cool, her head decidedly cold. The spinal ice-bag was first applied at 11.45 A.M. She was immediately soothed. At 2.45 P.M. the cramps were reported to be much lessened, and during the three hours' interval the bowels had been moved but twice. Before 7.15 P.M. of the same day the cramps had wholly vanished, and she had become "very comfortably warm all over."

CASE III.—At 4 P.M. the patient was in complete collapse: "lips blue, whole surface of body cold, cramps violent, rice-water purging and vomiting." The spinal ice-bag was then ordered to be applied continuously. In six hours afterwards, when she was next visited, she was "decidedly improved,

markedly warmer, the cramps and sickness much lessened; *the cramps only recurred when the ice had melted and the spinal ice-bag had become warm.* The next morning the lips were red, the whole surface of the body quite warm, the cramps had recurred only once, *and again only when the bag had been allowed to become warm*; bowels moved but three times in the twelve hours; vomited only once.

CASE IV.—At midnight the girl was in complete collapse; “rice-water stools; purging and vomiting almost incessant; very cold all over, tongue especially so; pulse almost wholly imperceptible; very bad cramps; countenance deathlike.” Treatment was begun immediately. At nine o’clock the next morning she was already much better; she had vomited but once; was warm all over; the pulse was very distinct, 116; there were no cramps at all, and her countenance was “immensely improved.”

CASE V.—At 11 A.M. the man presented all the symptoms characteristic of choleraic collapse. A spinal ice-bag was then applied; within an hour “a favourable change in him was observable;” he was already becoming warm; and at 9 P.M. of the same day, though still sick, he was “much better, warm all over, a capital pulse, no cramps.”

CASE VII.—“A worse case of cholera,” says the reporter, Mr. Peterson, who had seen many in India (where this occurred), he had “never seen.” At 11 A.M. the man was suffering from “incessant vomiting and purging;” his extremities were “quite cold;” his pulse was “scarcely perceptible;” his voice “almost entirely gone;” he “was in great agony, and appeared to be sinking fast.” Ice was applied along the spine, and within half an hour “I could perceive,” says Mr. Peterson, a “change for the better. The cramps began to lessen visibly, and the involuntary purging to stop. He never vomited once after I applied the ice. I was much surprised, about half-past twelve, to find the pulse gradually getting stronger, and the legs and arms warmer. The warmth went on increasing until, at half-past one, his body, except where the ice was, became hot as in fever.”

CASE VIII.—At 9 A.M. the girl was completely collapsed, and was quite pulseless. The spinal ice-bag was applied continuously, and by 2 P.M. of the same day reaction was perfectly established, “a good pulse” being observable.

CASE IX.—At 1.30 P.M. the girl was suffering from vomiting, purging, and cramps, and was collapsed and livid. The spinal ice-bag was applied continuously; the cramps and vomiting speedily ceased. “By 5 A.M. the next day the purging had also ceased, and on the same evening she had “a good pulse,” and was “warm all over.”

CASE X.—At 10 A.M. the girl was suffering from vomiting, purging, and cramps, was quite collapsed, and quite pulseless. The spinal ice-bag was applied at that time, and continuously until 4 P.M., “when the countenance had become much less choleraic, and the vomiting, purging, and cramps were already greatly subdued.”

CASE XI.—The woman was attacked suddenly in my presence. She became deathly pale, strikingly cold, the lips turned livid, and the sweat exuding in large drops over her face and upper extremities. Her head was cold and she was quite pulseless. *In about five minutes after the spinal ice-bag was applied*, “her pulse became distinctly perceptible, slight colour returned to her face, and in a few minutes more she said, ‘I am better.’”

CASE XXIV.—“Was apparently hopeless.” The man was delirious owing to uræmic poisoning, caused by the prolonged arrest of the action of the kidneys. Within half an hour of the first application of the spinal ice-bag the patient was relieved, and, beginning at once to improve, continued to do so steadily until he had completely recovered.

That the foregoing summary of cases and results is not a pardonable exaggeration of facts due to the fervid enthusiasm of the writer, will probably be held proven by evidence to which I shall now venture to refer. Nearly the whole of the cases in question occurred at Southampton during the epidemic there in 1865 and 1866. Those medical men in Southampton who adopted or witnessed the treatment in question were neither using a remedy nor observing the effects of one originated by themselves, therefore their judgment of the value of that treatment was not likely to be either obscured or perverted. Now, at my request, they were good enough, in 1865, to express that judgment in letters from which the following passages are extracted:—

“I have now seen and treated, with Mr. Bencraft and Dr. Cheeseman, six cases of cholera, in the stage of collapse; and what I have witnessed and noted in these cases justifies me in stating that your treatment is superior to any other that I have hitherto seen practised or pursued by myself or others.

“Were I seized with cholera I should give your mode of treatment a preference; indeed, I would submit to no other.”—JOHN WIBLIN, *Physician*.

“The five cases in which I had the good fortune of witnessing the application of the spinal ice-bags by you, have sufficiently convinced me of their utility in rousing the patients from collapse and removing the algide symptoms of cholera.”—G.

CHEESEMAN, *Physician and District Medical Officer to the Southampton Incorporation.*

"Dr. Chapman's treatment of cholera proved itself a remedy of very considerable power, restoring the heat, relieving the cramps, checking the vomiting and purging. Its use was followed by reaction from collapse, even in cases where the patient was quite pulseless.

"The treatment appears to have the great advantage of producing reaction from the state of collapse, leaving the patient free from the very large quantities of medicines which in other modes of cure so fatally hamper the treatment of the secondary fever."—G. A. LAKE, M.D., *Surgeon to the Royal South Hants Infirmary, &c., &c.*

"The power [of the treatment in question] to relieve the vomiting, purging, and cramps is almost marvellous, and its influence over the circulation, in restoring heat to the surface of the body, and in bringing back the pulse where nearly, and in some cases entirely, gone, must be seen in order to be properly appreciated.

"For my own part, I have such entire confidence in the method of treatment that, should any more cases of cholera come under my care, I shall without hesitation trust to it alone; and were I to be attacked with cholera I would insist on being, treated entirely by your method."—HENRY BENCRAFT, M.R.C.S.A., L.S.A., *Medical Officer to the Southampton Work-house.*

These extracts describe results which, in the present state of medical science, can be produced by no other means than those in question; and it is to be hoped, indeed it is evident, that when the people become acquainted with these facts, or with others like to them, and when, therefore, their horror at the idea of ice applied along the spine will have passed away, they will wish to profit by the lesson they have learnt, and will probably begin to do so by saving the thousands of young lives which, otherwise, will continue to be destroyed yearly by "summer" or "infantile" diarrhoea.

Having reviewed the results of the treatment which is the direct logical outcome of the pathological principles sketched in the first part of these pages, and having adduced the important evidence concerning these results from the several medical observers of them, whose names are given above, I appeal to all competent and impartial judges to pronounce whether, inasmuch as ice applied along the spine "stops the cramps, vomiting, and purging, makes the patients warm," and produces "reaction

from collapse, even in cases where the patient was quite pulseless," its action does or does not constitute a practical verification of the doctrine I have propounded—viz., that "the *proximate* cause of all the phenomena of cholera [before the stage of reaction] is hyperæmia, with consequent excessive action, of the spinal cord and of the sympathetic nervous system." What is the physiological action of ice? It is powerfully sedative. Obviously, therefore, if the exertion of a powerfully sedative influence over the spinal cord and collateral ganglia of the sympathetic nerve abolishes the phenomena of cholera, it must do so by lessening the circulation of blood in, and, consequently, the energy of, those nervous centres. But if it does this in cases of cholera, then it follows that the essential condition precedent of recovery from the disease is diminution of circulation in, and consequent energy of, those nervous centres, and therefore that the proximate cause of the disease (however that cause may have been established) consists in "hyperæmia of the spinal cord and of the sympathetic nervous system."

Moreover, if the symptoms were the consequence of a blood-poison, or of a microbe in the intestines, they, surely, could not be subdued with the rapidity exemplified in many of the cases above referred to by the use of a purely dynamic agent—the application of ice along the spine. It would, in my opinion, be just as rational to ascribe the several phenomena of sea-sickness to one or the other of those two alleged causes, as it is to maintain that either of them is capable of producing the multiform symptoms of cholera. As I have already said, the analogy between the two diseases is strikingly great. There are various predisposing causes, both of sea-sickness and of cholera, undoubtedly inherent in different individuals—otherwise of, say, 100 persons placed in the same locality and under the same circumstances in a cholera-stricken city, and of 100 persons on board the same ship, the whole would either become victims of cholera and of sea-sickness respectively, or the whole would escape. But we know that as a matter of fact, only a certain proportion suffer, and these in different degrees, while the rest are not affected at all. The ultimate cause of sea-sickness—motion—operates alike on the whole 100 passengers; but a certain number resist its influence. And in like manner the ultimate cause of cholera—whether thermal, electric, or (as I believe is most generally the case) thermo-electric, each of which is a correlative of motion and convertible into it—operates alike on all within the several foci of its energy; just as a certain number resist the influence of motion and remain free from sea-sickness, so a certain number resist the influence of heat or electricity, and, though dwelling in a cholera district, remain proof against cholera. The essential

likeness to each other of these two diseases is still more strikingly attested by the fact that they are both curable by one and the same method. Now, seeing that in very severe cases of sea-sickness, nearly all the symptoms of cholera, including even that of sudden onset, are manifested; seeing that the cause of sea-sickness is motion, and that the ultimate causes of cholera are heat and electricity, which, as physicists teach us, are modes of motion; seeing that the personal predisposition to each disease is analagous; seeing that each is a disorder of the nervous system; seeing that each is curable by acting in one and the same way on that system; and, seeing that we are absolutely certain that one of them, sea-sickness, is not the result of a blood poison, is not the result of a microbe, and is not contagious, we must, I think, conclude that the other is not less free from these characteristics, and not less a purely dynamic product than is sea-sickness itself.

I have now laid before my readers a view of the nature, causes, and treatment of cholera, which differs fundamentally from that generally accepted, and indeed from each and all previously promulgated. This distinctive feature may, perhaps, be regarded as *primæ-faciæ* evidence by no means in its favour, and might be justly so regarded were not the pre-existing doctrines and practices mutually irreconcilable, or were any one of them capable of offering explanations, at once intelligible and consistent with each other, of the whole phenomena of cholera, and also capable of suggesting a method of treatment at once scientific and successful. But, as a matter of fact, the whole literature of cholera, exceptions excepted, is a literature of chaos: a chaos of conflicting doctrines, which severally seem capable of explaining some of the phenomena of the disease, but which fail signally to explain the whole; and consequently a chaos, not less confounding, but far more dangerous, of conflicting practices. On the other hand, the doctrine now submitted to the reader offers a simple and clear explanation of every symptom and every characteristic of cholera, and though these symptoms and characteristics are, in the aggregate, very numerous, and therefore necessitate numerous explanations, yet the whole of these are absolutely accordant with each other. While cholera is shown to be essentially a disease of the nervous system, the genesis of every symptom of the disease, or its mode of origin in that system is distinctly demonstrated; though the factors in the creation of that special condition of the nervous system, out of which they arise, are numerous and manifold, the hypothesis in question assigns to each predisposing or exciting cause its appropriate place and function, and fully reveals its special mode of action as a co-operative agent. The increase of

heat immediately before death, the increase and persistence of heat as well as the remarkable changes in the colour and aspect of the visage and general surface of the body, together with the not infrequent movements of the limbs after death, are simply and satisfactorily explained by the same hypothesis. It renders completely intelligible the established fact that cholera often arises suddenly in numerous localities, more or less distant from each other, at one and the same time, and that occasionally it ceases no less suddenly. While recognizing, and fully accounting for, the facts out of which the doctrines of a cholera-poison, of cholera-germs, of cholera-bacilli, and, therefore, of the contagiousness of cholera have arisen, the hypothesis now propounded at once repudiates and confutes those doctrines, and at the same time, by proving that cholera is a disorder the nature of which is exclusively dynamic, also proves that it cannot be contagious. In doing this it fully justifies Anglo-Indian physicians who, holding the disease to be non-contagious, treat patients suffering from it in hospital-wards along with those afflicted with other diseases; it explains the general immunity from cholera of the attendants of cholera patients, as well as of students who dissect cholera corpses; it explains why, notwithstanding that animals are really liable to cholera as human beings are, Dr. Koch could not induce the disease in them, however much he fed them with his comma-shaped microbes or with portions of intestines containing them; it also explains why quarantine in all its vexatious and misery-inflicting forms, the disinfection of travellers, and of goods and letters *in transitu* are utterly useless and indefensible as means of protection against cholera; it shows that the summer diarrhœa of Europe, the cholera infantum of the United States of America, the diarrhœa premonitory of cholera, cholera *nostras*, and "Asiatic" cholera are all mere varieties or grades of intensity of one and the same disease, that they are all alike due to the same *proximate* cause, and are controllable in one and the same way; and finally, it dictates a method of treatment which is approved alike by medical science and by common sense, which is thoroughly practicable, which is sanctioned by the experience of every physician who has adopted it, and which has been proved to be successful to a degree surpassing that of every other kind of treatment which has hitherto been tried.

JOHN CHAPMAN, M.D.

CONTEMPORARY LITERATURE.

THEOLOGY.

THE Hibbert Lectures of 1884¹ are somewhat disappointing. Dr. Albert Réville treats his subject in too hurried and superficial a manner, owing partly to his gliding over dangerous places. In fact, he warns us (p. 5) that he "retains his own very marked preferences." He writes more as a religious person—a doctor of divinity, as he is—than as an unshackled critic of religions. Then the arrangement is rambling; there are repetitions; the subject is not well in hand. Add to this, the vague and slightly pompous manner of the preacher, who puts both facts and theories in the same authoritative condescending way, as though he were talking to children, and it must be said that this year's Lectures are by no means in the first rank.

Still the compilation is of much interest. We are brought face to face with two religions, developed in absolute independence of each other, and of all others of which we have records; and preserving their autonomy, just as Polynesia has preserved to this day the pre-historic mythology of civilized mankind. It cannot be too clearly understood that the severance of the Mexicans and Peruvians from the rest of the earth, up to their discovery by Cortes and Pizarro, was as complete as if they had been inhabitants of another planet. All attempts to prove an earlier intercourse with Europe or Asia have been failures, and the legend of the Apostle Thomas evangelizing America is acknowledged to be as baseless an illusion as Von Humboldt's theory that Buddhism must at some remote period or other have penetrated into Mexico.

Both religions have their objective development in Nature-worship, the predominant form of which was the adoration of the eternal and omnipresent Sun. We have in Mexico a god of rain and fecundity, called "the nourisher," who dwelt in the mountains, whence he sent his numerous offspring, the clouds. Lightning and thunder were among his attributes. There were also the cloud-serpent, whose name, Mixcoatl, survives as that of the waterspout; and the god of the winds, especially of the east fertilizing Atlantic wind, one of whose names, Hurakan, may have given us our word "hurricane," which certainly was brought by the Spaniard from the west. The cross surmounted his temples, and was a symbol of the cardinal points.²

¹ "The Native Religions of Mexico and Peru." By A. Réville, D.D., Professor of the Science of Religion at the Collège de France. Williams & Norgate.

² An instance of M. A. Réville's haste is his specially attributing the Mexican cross to the temples of Quetzalcoatl the feathered serpent, the wind-god, on page 38; and to Tlaloc the rain-god, on page 72. But he then drops this inconvenient cross question, and never mentions it again.

In Peru we find star-worship; Venus the page of the sun, the Pleiades next in veneration, and the other stars revered as the moon's maids of honour. Comets, too, as in modern Europe, foreboded the wrath of the gods, and stones were worshipped, perhaps for meteoric reasons. But all this is ancillary to the central figure of the pantheon, which in Mexico counts 260 deities. The whole lives of the Mexicans and Peruvians were steeped and enveloped as truly in the worship as in the light of the Sun. He was animated and personified, and represented independently in both countries by a round human head encircled by diverging rays and flames, precisely what has long since descended in Europe to comic portraiture, except where it still survives in the "glory," the halo, of sacred Christian pictures.³ His great Mexican feasts were three: in May, at the return of flowers; in August, when the fruits of his influence had been gathered; and in December, when he reaches the lowest expression of his power—in the figurative language of mythology, "dies"—to arise and return again, like Osiris, Adonis, Atys, and the host of other solar transferees.⁴ The winter sun, called the "shining mirror," and brother of the summer sun or "humming-bird," was then worshipped. Peru, on the other hand, in its southern hemisphere, had its festival of the diminished, but thenceforth growing, sun in June. The spring feast was in September; and the third, the harvest festival, was in May. Our country parsons still keep up these feasts; and ladies who deck the churches little think they are but perpetuating the pious duties of the virgins of the Sun. There was a fourth feast in Peru, that of the height of summer, the festival of Power, when the thunder-god was joined in adoration with the supreme being. Eclipses were the cause of great terror, and were regarded as self-made; the Sun hiding his face from his people in his anger.⁵ Then the fire-god, "the lord Fire," "the old god," no doubt an outcome of Sun-worship, was venerated in every house. The supernal fire which had become contaminated by contact with earthly things, was extinguished once a year, and brought down anew from its heavenly source, by the friction of wood. At every meal "the first libation and the first morsel of bread were consecrated to him."⁶ Gold was sacred; perhaps, from its colour, looked upon as a part of the substance of the sun. A golden disk held

³ It is worthy of note that similar projecting golden rays surround the circular glazed disk of the "monstrance" in which the wafer, when converted into the deity Jesus by the priest's repetition of a formula, is displayed above the altar for worship in Roman Christian churches.

⁴ Bonwick's "Egyptian Belief," 1878, pp. vi., 169. A. S. Murray's "Manual of Mythology," 1876, p. 86.

⁵ A common idea in the Old Testament. "Your sins have hid his face from you," Is. lix. 2; "Thou hast hid thy face from us because of our iniquities," Is. lxiv. 7; "For all whose wickedness I have hid my face from this city," Jer. xxxiii. 5; and others too numerous to quote.

⁶ It was related of the Red Indians of Virginia, when discovered in the same century, that "before their dinners and suppers the better sort will take the first bit and cast it in the fire; which is all the grace they are known to use" (Captain John Smith's Works, in Arber's "English Scholar's Library," 1884, p. 77).

the human face already described. In the temples of the Sun, which opened to the east,⁷ the altar was surmounted by the golden disk of the Sun, so placed as to reflect his first morning rays, and give a self-produced effulgent image of the divinity in his holy of holies.⁸ The nuggets of gold found in the mountain soil were the tears of the sun, according to Peruvian folk-lore. Silver was equally sacred to the Moon, the sister of the Sun, and she had a disk of silver in her adjacent temple. Japanese lacquer-artists to the present day represent the moon by an inserted solid disk of polished silver. The Mexican priests never cut their hair; wearing the hair long was a distinctive sign of the favourites of the Sun, and was reserved in Peru for the Inca, his family, and the most exalted nobles. Here, as in the golden locks of Phœbus, the fancy portraits of Christ, the flowing hair of Kephalos, and the long hair of Hercules and Samson, we have the rays of the sun, which are shorn at its wintry downfall, but grow again in spring. The glowing humming-bird was the divine messenger of the Sun, and gave its name to, or took its name from, the summer sun. The Aztec tongue called it "sunbeam," and "Sun's hair." Like the dove of the holy spirit,⁹ our swallow, the plover among the Latin races, and the crow of the Red-skins, it was an emissary of the Sun, coming with the return of the propitious season.¹⁰ Eventually the humming-bird miraculously became incarnate in a pious widow, he

⁷ Even village houses generally looked eastward, so that their occupants might salute the supreme god as soon as he arose. The Roman Christian priest says mass facing the east, the congregation hearing it in a similar attitude; and we have had enough of late years in England of ritualistic wrangles about "the eastward position;" those who object to the priest turning his back on the people showing as complete an ignorance of the origin and significance of the practice as their opponents.

⁸ See note 3, *ante*. The parallel with Japanese Shintō would be startling if it were not a mere unit in the infinitude of such analogies of Sun-worship still surviving all over the globe; which owe their unconnected identity to the simple but all-important fact that they everywhere grew up in human minds out of the reverence for this one supreme and omnipresent god. In the penetralia of the Shintō temple at Ise is preserved and worshipped the circular mirror which was brought down from the Sun-goddess by her sublime grandson Hiko-ho no Ninigi no Mikoto, first divine sovereign of Japan, when he descended from heaven. These mirrors must be considerably more recent in the history of the races which adopted them than the discovery and working of metals; they imply a progress in manufacture and arts which should be measured by vast stretches of time. We find, for instance, the rekindling of the sacred fire in Peru after the arrest of the sun in the winter solstice, by means of a concave mirror, which brought the holy flames directly from the Sun-god. It may be noted as another odd coincidence that in Mexico the statue of Tezcatlipoca, or "Shining-Mirror," the Sun of the cold and sterile season, held a crystal mirror in which all the actions of men were reflected. He was dreaded as a stern god of judgment. In Japanese Buddhism there is the *jō-hari* mirror in hell, which reflects, for those who have to gaze on it, the good and evil deeds of life.

⁹ Matt. iii. 16, 17; Mark i. 10, 11; Luke iii. 22; John i. 32, 33. Ernest de Bunsen points out that the Samaritans had a brazen fiery dove. (The Holy Ghost also descends in the form of tongues of fire, Acts ii. 3.) Even in the Polynesian islands a bird is the emblem of the spirit of God.

¹⁰ The wren superstitions among the Celtic peasantry are also worthy of note.

