

# Statistics and the British controversy about the effects of Joseph Lister's system of antisepsis for surgery, 1867–1890

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## Introduction

In his revisionist book *Bad medicine. Doctors doing harm since Hippocrates*, historian David Wootton<sup>1</sup> claims that, by introducing antisepsis with carbolic acid in surgery, Joseph Lister (1827–1912) was the first doctor to merge ‘science’ with the practice of medicine with resulting prolongation of life: ‘Lister thus begins the modern history of medicine, defined in terms of constant improvements in therapy grounded in developing scientific understanding, and it is striking that it is surgery’ (p. 227).

Wootton points out that Lister had a ‘scientific’ motive for doing what he did. Although it is true that he was convinced that ‘living bodies’ caused wound suppuration, in view of all the challenges he encountered (see below), he did not care too much about the theory underlying his method provided people used it.

Wootton refers to ‘Lister’s revolution in surgery’ (p. 229), but Worboys<sup>2</sup> had shown previously that there had been no surgical revolution caused by Listerism; rather this view was created by ‘Listerians’ after 1880 (p. 83). The alleged sequence antisepsis (Lister) to asepsis (Koch, German and Swiss surgeons) is probably too simplistic in that the latter can be retraced to the earlier ‘cleanliness-school’, which most surgeons combined in one way or another with antiseptic measures.

Wootton’s celebratory presentation of Lister’s work and its influence is quite common, but it is an oversimplification. In fact, there was considerable controversy about the effects of Lister’s system of antisepsis. As historians have pointed out, this fight was to last for over a decade, at least in Britain.<sup>3,4,55</sup> It can be followed in the annual meetings of the British Medical Association between 1867 and 1879 as well as in the medical press.<sup>5</sup> The arguments in this controversy have long been described and contextualised by historians with varying perspectives (see, for example, bibliographies in Nicolson<sup>6</sup> and Cartwright<sup>3</sup>). The theoretical principles underlying

Listerism, that is that germs caused wound diseases (infection, putrefaction, gangrene) were more readily accepted in continental Europe,<sup>2</sup> perhaps because carbolic acid had already been used to prevent and treat wound disease by Lemaire<sup>7</sup> in France and Bottini<sup>8</sup> in Italy.<sup>2,9</sup>

In this article, I focus on the type of evidence that was presented in Britain in support of, or against, the claims of beneficial effects of the antiseptic method. Of particular interest are the kinds and role of numerical data used. I begin by examining the nature of Lister’s initial reports, then reassess the short-term mortality statistics from his wards in the Glasgow Royal Infirmary (GRI) compared with those of all surgical wards of the Infirmary, and then consider a longer-term perspective. I raise questions about whether, how and why Lister selected his data. Finally, I look at the way important opponents of Lister’s claims presented their arguments. The results will shed light on the epistemic status of the evidence on which this momentous case in surgical history took place.

## Lister’s evidence of the effects of antisepsis: case reports

From 1867, Lister published a series of papers in *The Lancet* and the *British Medical Journal (BMJ)* in which he described ‘the principles and practice’ of his new ‘antiseptic system of treatment’ of surgical wounds. In the first paper of this series,<sup>10</sup> he supported his claims by presenting a series of 11 patients with compound fractures, only two of whom had died. This seemed an excellent result in its time. In the later publications, Lister continued developing the theoretical basis of antisepsis, that is, its foundation on the germ theory, and in consequence, he adapted his practical prescriptions. In this first and in the following articles, he illustrated theory and practice with lengthy descriptions of single cases.<sup>11–20</sup> Only two and a half years after he had published the first case series did he use statistics,<sup>21</sup>

which is why this paper has been selected for inclusion in the James Lind Library.

Lister's motive for this paper reads as follows: 'The antiseptic system of treatment has now been in operation sufficiently long to enable us to form a fair estimate of its influence upon the salubrity of an hospital'.

What evidence did Lister adduce to present this 'fair estimate'? He began by claiming that the antiseptic system of treatment had transformed the wards until recently under his care at the GRI. Since he had developed it, they had turned 'from some of the most unhealthy in the kingdom into models of healthiness'. He then gave a vivid account of the nature of these wards. They were on the ground floor, a foul drain had been underneath. Along one side there was a sepulchre and one end of the building was "conterminous with the old Cathedral churchyard, which is of large size and much used, and in which the system of 'pit burial' of paupers has hitherto prevailed." Lister quoted a recent report from *The Lancet* that 'five thousand bodies were lying in pits, holding eighty each, in a state of decomposition, around the infirmary'. He concluded his detailed description of overcrowding, stench, etc. as follows:

I have said enough to show that the wards at my disposal have been sufficiently trying for any system of surgical treatment. Yet... [he continued again to use the example of compound fractures], since I began to treat compound fractures on the antiseptic system, while no intern treatment has been used, I have not had pyaemia in a single instance, although I have had in all thirty-two cases – six in the forearm, five in the arm, eighteen in the leg, and three in the thigh.

These figures did not include severe cases demanding immediate amputation. But, thanks to his new system, he claimed, amputations had now become rare if the system was correctly followed. For 'a loose and trifling style of "giving the treatment a trial" swells the death rate at once of compound fractures and of amputations'. Such were Lister's first – still implicit – comparisons.

### Lister's evidence of the effects of antiseptics: statistics

Explicit comparison using numbers followed in the second part of the 1870 paper, in which he presented the mortality associated with amputations before and after the introduction of his antiseptic system:<sup>21</sup> before the antiseptic period, there were 16 deaths

among 35 cases (or one death in every 2½ cases); during the antiseptic period, there were six deaths among 40 cases (one death in every 6⅔ cases). This was a reduction from about 45% to about 15%, or of two-thirds.

Lister was aware of the element of chance when he wrote that:

these numbers are, no doubt, too small for a satisfactory statistical comparison [but explained it away saying that] when the details are considered, they are highly valuable with reference to the question we are considering. This is especially the case with amputation in the upper limb, where neither injuries requiring primary amputation nor the operations involve, as a general rule, much loss of blood or shock to the system.

Death in these cases would therefore be a consequence of 'the wound assuming unhealthy characters'. (Note that Lister avoided the term 'wound infection'.) The relevant numbers followed in a statistical table (see below).<sup>21</sup>

Lister went on to compare all the figures from the table in the text, analysing the circumstances and causes of death in each case and period. He further pointed out at length the striking contrast in the occurrence of pyaemia and of hospital gangrene, again giving the details of each case. He concluded 'We have seen that a degree of salubrity equal to that of the best private houses has been attained in peculiarly unhealthy wards of a very large hospital, by simply enforcing strict attention to the antiseptic principle'.

For Lister, this conclusion was obvious in the light of the figures he had presented. This was not the case for his critics, however, nor did it convince sceptics. They distinguished two lines of criticism – local and temporal. Both have also been considered by historians.

### Lister's statistics in the context of the GRI

Lister's numbers, which related to the wards for which he was responsible in the GRI, were doubted by some of his contemporaries. Hamilton<sup>22</sup> has studied the records of the whole GRI, in particular the Annual Reports, and was able to calculate the annual death rates. He did this for cases of compound fracture and for primary amputations of the upper limb (excluding amputations of fingers and at the wrist). He chose these for the same reasons Lister had done. We can compare these amputation data with Lister's own figures. The results, as percentages, are shown in Table 1.

**Table 1.** Death rates following amputations of the upper limb (excluding amputations of fingers or at the wrist) in the Glasgow Royal Infirmary.

Year	Whole Infirmary <sup>22</sup> Death rate (%)	Lister's wards <sup>18-21</sup>	
		<i>n</i>	Death rate (%)
1864	25	7	57
1865	9	No data	
1866	35	5	60
1867	60	3	0
1878	59	3	0
1869	34	6	17

Hamilton does not give the absolute numbers of operations performed by all the surgeons at the GRI. Lister, as we can see, did very few. It is clear by aggregating the data from the three years before (1864–1866) and the three years after (1867–1869) the introduction of antisepsis that there are two striking differences. Before antisepsis, the death rates were higher in Lister's wards than in the whole GRI. They fell strikingly with the introduction of his methods, while they rose considerably in the whole GRI. However, Lister's figures become much less convincing when seen in the context of the overall results of the Infirmary.

In 1865, there had been a very low overall mortality rate (9%) among upper limb amputees in the GRI, but Lister gave no data for his wards, claiming that 'the hospital records are unfortunately imperfect' for this year. This seems like a pretext for not presenting data which would result in a less striking apparent effect of antisepsis. After all, had he not himself suggested that 'statistics can be made to prove anything or nothing'.<sup>3</sup>

### Lister's statistics seen in a longer-term perspective

Extrapolating from these data, Hamilton suggests that Lister had probably selected the initial 11 cases presented in his 1867 paper, a suggestion supported when one looks, as Hamilton did, at death rates further back in time. Between 1861 and 1864, the annual case fatality rate associated with upper limb amputations at the Infirmary was 23, 50, 12 and 15%, respectively; in the mid 1850s it had risen to peaks of 65–75%, but it had been much lower (0–30%) around 1845. These figures reflect the cycles of hospital epidemics. These variations, well known to

contemporaries, were among the reasons for rebutting Lister's claims for his system.

Of course these data concerned only amputations, and only of the upper limb; an effect of the antiseptic system could (and can) also be seen in the results of treatment of all compound fractures, or in the decreasing rate of amputations needed, and in the death rate in the practice of all four surgeons in the GRI. The latter, Lister 'rejoice[d] to find [...] during the three years of the antiseptic period, has been less by fully one-fifth than during the *five* (my italics) previous years'.<sup>19</sup> Indeed, Lister had been in charge of his wards at the Infirmary since 1861. In yet another paper in 1870, this time in the *BMJ*, he seems to have stated that 'in the *six* years (my italics) before antisepsis, between 40 and 45 per cent of his cases of amputation and excision of joints died' in the GRI (quoted from Cartwright<sup>3</sup> (p. 83); I was unable to find this statement in the reference from Lister indicated by Cartwright, i.e. Lister<sup>20</sup>). Be that as it may, Lister obviously had records of earlier years. Why then did he choose to give the death rates in his amputation statistics beginning only from 1864? Was it again to make his claim look more impressive?

Taking this longer-term perspective sheds yet more light on the effects of Lister's antiseptic treatment, raising doubts about whether the reduced mortality was actually due to his treatment. Traditional historical accounts often refer to Lister's antiseptic system as a 'revolution', that is, a rapid and complete change. But the results in some pre-antiseptic years had been as good as those under the new treatment. Notably, however, just before Lister started his carbolic dressings, death rates had been very high. His contemporaries had a sense of this periodicity. Some actually did explicit numerical comparisons, such as those presented above. They were interpreted either for or against Lister, depending on the type of operation and situation to which they referred (see below).

The absence of an antiseptic revolution, at least in Glasgow, also becomes clear when one examines death rates associated with amputations of the upper limb going forward to 1900. These rates had decreased to around 15% by 1895 (with occasional peaks up to 37% between 1873 and 1875). To be sure, there was improvement, but it was slow, not sudden. And '(t)he start of the period of improvement coincides not only with Lister's introduction of the carbolic acid regimen, but also with the onset of relief for the mid-century social crisis . . .'<sup>22</sup> (p. 39) This can be judged by the decrease both of typhus deaths in Glasgow and hospital admissions for common, non-varicose ulcers. The former is widely regarded as an index of social conditions, the latter as an index of nutritional deficiencies. Finally, the concomitant rise

in real wages in the UK reflects an improvement in living standards. Taken together, these features would be consistent with improved host defence against infection.

### The evidence of Lister's opponents

In 1868 (that is, a year after Lister's first publications on the subject), R. Lawson Tait (1845–1899), a young ovariologist of the pre-antiseptic 'cleanliness-and-cold-water-school', from Wakefield, published one of the papers that started the debate about Lister's system. He reported tersely that '...in twelve compound fractures, where there existed a probability of union without suppuration, the only cases where suppuration did occur were two in which I employed the acid paste exactly as recommended by Mr. Lister'.<sup>23</sup>

In 1868–1869, reports about the use of 'the carbolic treatment' in the 10 major London (teaching) hospitals were published in *The Lancet* by 13 surgeons. Their reactions were divided. They varied between enthusiastic support of the method to reports that it had been abandoned, considered useless or meddling. Some preferred zinc chloride dressing. Their 'good trials' were presented as single cases and in vague terms without numbers. There were editorials in the *BMJ* and in *Medical Times & Gazette*.<sup>3</sup>

At the 1869 meeting of the British Medical Association in Leeds, the local senior surgeon Thomas Nunneley (1809–1870) launched a violent attack upon antiseptics. He outed himself as ultra-conservative, not just by denying the germ-theory of wound infections but also by misquoting the experience of local colleagues who had had some success with Lister's methods. As Lister pointed out in his reply in the *BMJ*, Nunneley 'dogmatically oppose[s] a treatment which he so little understands and which, by his own admission, he has not tried'.<sup>16</sup>

Lister's 1870 paper was thus an answer to criticisms rooted in preconceived opinions or in experience, the vagueness of which was not realised by the authors. Lister had now presented more facts than before and had used statistics to present them in a concise way. Soon his opponents would do so, too.

Other British surgeons, pre- and post-Lister, also had theories. They continued attempting to prevent and control gangrene and wound sepsis with various chemicals, or, like Lawson Tait and his followers, with rigorous cleanliness. They held that both ways were less aggressive than Lister's carbolic acid. Others, for instance Rudolf Krönlein in Zürich, Switzerland, claimed comparable successes by simply exposing the wounds to the salubrious air of their environment.<sup>24</sup> These approaches were also much

less cumbersome, expensive and time-consuming than antiseptics using carbolic acid sprays, and thus better suited to busy practitioners and in emergency cases.

Statistics were used to suggest that cleanliness was more successful than Listerism in preventing death. For instance George Callender, at St. Bartholomew's Hospital, London, made his case in 1873 on the basis of 200 operations.<sup>4</sup> Lawson Tait, by now a successful hospital surgeon in Birmingham, was a particularly keen statistician. Throughout the 1880s, he repeatedly published case series and charts on tens,<sup>25,26</sup> hundreds<sup>26–30</sup> and even of a thousand operations.<sup>31,32</sup> The mortality rates in his cases were always lower than those published by Listerians, for example, only three deaths in a series of '[o]ne hundred cases of ovariectomy performed without any of the Listerian details'.<sup>29</sup> But such numbers were constantly contested in statistical reports by supporters of Lister, for example, from Newcastle-upon-Tyne and Glasgow.<sup>4</sup> In 1890, Tait was still challenging Lister to provide adequate statistics of his results.<sup>33,34</sup> Lister politely ignored him. In contrast, to Tait, Lister did not relish controversy.<sup>2</sup>

### The unanswered quest for adequate statistics

In 1879, William Savory, already quite a prominent London surgeon (he was to become President of the Royal College of Surgeons of England 1885–1888), accused Lister of still not having produced comparative statistics to underpin his claim that his approach was better than others.<sup>35</sup> Leading British surgeons, particularly in London, were still at that time waiting for clear proof that antiseptics was an improvement. Obviously they were looking for numerical proof, yet, as we have seen, 'Lister refused on the grounds that statistics can be made to prove anything or nothing',<sup>3</sup> (p. 100) and continued to rely on case histories. Again and again *The Lancet* suggested that one ward should be set aside in a teaching hospital, where antiseptics might be exclusively used and a comparison made. This was an old idea.<sup>36</sup> Yet nobody seems to have taken it up in precisely this way. So, finally, Lister had a point when he said that one could prove anything with statistics – that is statistics as he himself, and most contemporaries used them. But – and this is important – he did nothing to improve this state of affairs.

It is true that both *The Lancet* and the *BMJ* published letters under subheadings such as 'on the fallacies of statistics', or 'scepticism' and 'asepticism'. Lawson Tait also contributed.<sup>37,38</sup> Yet his own published statistics did not live up to his standards. In



1882, he read a paper at the Surgical Society of Ireland with the promising title 'An experimental research on the value of Listerism in abdominal surgery'. Unfortunately the printed report in the *BMJ* – or Tait's address – was confusing.<sup>39</sup> It said:

The most valuable information was to be obtained, not by noting the ratio of deaths, but by observing under what mode of treatment the recoveries were most easy, even, rapid, and uncomplicated. For this purpose he [Tait] exhibited comprehensive charts of a large number of cases treated by various methods, showing temperature, pulse, duration, etc, of the cases in each group. Those treated by complete Listerism gave the worst results. This [prospective] investigation lasted over two years.

The summary (there was no other publication on these 'experiments') went on to report on Tait's work with various dilutions of carbolic acid solutions up to pure water. These were used as a spray (Group 1), to soak sponges used to clean wounds (Group 2) and in other (unspecified) details of the operation (Group 3). The result was that '[t]he recoveries improved in ease as the carbolic acid was reduced in strength', and the best was pure water. 'Tait therefore gave up the use of carbolic acid altogether'. No numbers were given in this summary, but they appear to have been in the charts presented at the meeting.

The controversy among surgeons was in fact about two points: first, the theories of wound infection and sepsis; second, whether, various other antiseptics – fresh air, hospital hygiene and/or simple cleanliness during operations – were just as effective as Lister's complicated, ever changing and not ready-to-hand regimen. As to the first issue, even Lister ended up with an attitude of 'never mind the theory, try the practice'. As for addressing the second issue, Tait wrote at least 13 papers between 1872 and 1890, on the evaluation of antisepsis wherein statistics played a role. Also, in the discussions they provoked, arguments about the use of statistics were put forward. All these issues are summarised in the Appendix (Tables 2 to 5).

Table 2 shows that the need for statistics had been recognised since the early 1880s.<sup>27,30,33,34,40–44</sup>

Table 3 groups certain requirements necessary for statistics to be valuable, which were stressed by Haward,<sup>45</sup> Callender,<sup>46</sup> Greenfield,<sup>47</sup> Keith,<sup>48</sup> Savory<sup>35</sup> and Tait.<sup>25–30,32,37–39</sup>

Table 4 shows that readers were repeatedly urged to be aware of some fallacies.<sup>25,27,28,35,48,49</sup> The most remarkable of these insights remain Tait's 1890 recognition of the irrelevance of most contemporary evaluative research.

Table 5 refers to Tait's proposals for methodological advance. Equally noteworthy is his rejection of the arguments, offered by others, against planned (that is, prospective) experiments.<sup>33,34</sup>

Despite the understanding of the kind of evidence needed – comparative statistics of at least two parallel groups in comparable conditions, and the ways to obtain these – nothing of the kind was done, except, maybe, by Tait himself. But his large scale experiments were published only in abstract form. There were comparisons, but they were with historical controls. These were criticisable – and criticised – because hospital hygiene had been much improved, and operative technique had changed concurrently with the introduction of the ever changing Listerian antiseptic regimens. It was therefore impossible to sort out the extent to which each of these was responsible for the observed decrease in operative mortality.

### A longstanding controversy: theory versus practice

In the background of the prolonged argument described here was the longstanding question, revived after the 18th century, of what science meant.<sup>50,51</sup> Was it theory supported by laboratory work as Lister would, implicitly, believe?<sup>15,17</sup> Or was it empirical observation in the clinic expressed by numbers, as Tait would claim? It seems that German and Swiss surgeons in particular found it easier to integrate new ideas that were based on laboratory science than their British colleagues.<sup>52</sup> This may be one reason why Listerism was more readily accepted in continental Europe than in Britain.<sup>2,3</sup> On the other hand, what is now called 'evaluation science' appears not yet to have been conceived as an entity by British surgeons. Bull commented on this many years ago in his MD thesis<sup>53</sup> and in an article based on it.<sup>54</sup>

In 1870 [Lister] published his statistics for amputations and compared 33 cases before the use of antiseptics with 40 cases treated by the new method. He showed a mortality of 43 per cent in the former and 15 per cent in the latter but was diffident about drawing conclusions, saying 'These numbers are, no doubt, too small for a satisfactory statistical comparison'. Our comment now might be that the numbers are not at fault for the chi square test shows them to be highly significant; what is more open to question is the adequacy of the comparison with previous experience since so many relevant factors such as selection of cases for operation must have changed. Had it been possible, a careful comparative trial of rival methods at this stage might have

prevented the bitter and profitless controversy which raged for many years on the subject of the importance and technique of prevention of infection at operation. (p. 234)

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## Appendix. Arguments about the use of statistics to evaluate antiseptics, 1870s–1890

### Table 2. Generalities.<sup>27,30,33,34,40–44</sup>

#### Against

1. Popular belief: 'Statistics can be made to prove anything'. 1880.
2. Disbelief in hospital statistics. 1881.
3. 'One crucial example [is] worth more than any amount of statistics'. 1881.

#### Neutral

'No rough aggregate of operative results would be of the slightest use for any purpose whatever': Statistics alone seldom prove anything. Interpretation depends on perspective. Circumstances must be considered. 4x: All around 1880.

#### Pro

1. Contrary to 'general impressions', statistics make 'substantiated claims'. They bring 'proof having mathematical exactitude', provided they fulfil certain requirements (Table 3) and avoid fallacies (Table 4). 1880.
2. Results are the key arguments in the debate. 2 times: 1883, 1890.
3. Hospitals which do not publish their results are 'beginning to be regarded with doubt'. 2 times: 1890.

### Table 3. Requirements for useful mortality statistics.<sup>25–30,32,37–39</sup>

1. Compare (the comparable). 9 times: 1870s, around 1880, 1890
2. Include all cases, successful and unsuccessful. 5 times: 1870s, around 1880, 1890
3. Record trustfully (best: by independent person). 2 times: 1879.
4. Present 'in as condensed form as possible': tables, charts. 5 times: 1880s, 1890.
5. Define terms clearly. 2 times: 1881, 1883.
6. 'The laws of statistics': Use large numbers-'equal distribution of errors if any'. 2 times: 1880, 1890.
7. Do not select your cases in a trial in regard of statistics. 2 times: 1883, 1888.

**Table 4. The fallacies of statistics.**<sup>25,27,28,35,48,49</sup>

1. Historical controls: When two (or more) variables have changed. 3 times: around 1880.
  2. *Post hoc-ergo propter hoc* argument. 1879.
  3. Inconsistencies between statistics and analysis of individual cases. 1890.
2. 'How utterly futile our present research is', where 'everybody [is] treating everything in every kind of way'. Therefore: plan 'deliberate and logical [prospective] experiments' with well-defined groups to allow meaningful comparison. 1890.
  3. Argument against such a plan: It 'hampers liberty of action'. But 'our present liberty is not wise; indeed it is not liberty at all, but licence'. 1890

**Table 5. Improvements needed.**<sup>33,34</sup>

1. 'Better system of working', a 'logical plan in recording and classifying' and 'careful publication of surgical results'. 1890.