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Development of clinical probabilistic practice in Britain before Gavarret, part 2: British perspectives on prominent French researchers

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This article is in our series on probabilistic thinking and the evaluation of therapies, 1700–1900

Louis seen through British eyes

As we know, Louis contributed notably to two fields – to anatomo-clinical research and to the notion of numerical evaluation of therapies (see part 2/2 of this series).[a] He was influential in Britain in both areas – although British writers did not refrain from pointing to earlier British authors. When '*the uncertainty of medicine, and . . . the numerical method of Louis*' were the topic of a paper read at the London Medical Society on 16 November 1836, a discussant reminded the audience that this course of action '*did not originate with Louis. The late Mr. Alcock reported his cases numerically*' (*The Lancet*, 1836–37). In fact, Alcock had repeated his 1823 plan (see Part 3/1 of this series[a]) in his published *Lectures* (see Alcock,[1] pp. 61, 115). Another example was Thomas Hodgkin.

Quantification in anatomo-clinical research

When, in the 1820s, Thomas Hodgkin (b.1798), a Quaker, interrupted his studies in Edinburgh and spent over a year in Paris, he became so influenced by the exacting pathological and numerical approach of Louis that, back in London and in charge of the Department of Pathology at Guy's Hospital, he also started carrying out autopsies and collecting the reports. However, Louis's methodical numerical system was '*not altogether new or singular in his hands*', as Hodgkin rightly pointed out with reference to seemingly independent British work (Todd among others; see Part 3/1 of this series[a]) (Hodgkin,[2] pp. 1092–1093).

In 1834, Hodgkin lectured to the Physical Society of Guy's Hospital on the *Numerical method of conducting medical inquiries* (published 20 years later[2]). He deplored the conjectural state of medicine. This could only be overcome by strict adherence to precise descriptions of many clinical cases followed by equally precise inspections at autopsy among those ending fatally; to grouping and presenting them in tabular form for comparison and statistical analysis. This was Louis's extolled anatomo-clinical-research method for the description of disease (nosography) which was unquestionably useful, as in '*therapeutics, in which the assistance of the numerical system [was also] required*'. Foreign students understood this, while the French principally followed the ideology of Louis's elderly enemy, François-Joseph-Victor Broussais (b.1772): The much younger Hodgkin deployed the advantage of Louis's rigorous, objective approach at great length, culminating in caustically comparing it to Broussais's doctrinaire system:

> The unquestionable talent and powerful sarcasms of [this] author . . . under which many other systems and authors have seemed to give way, the physiological views upon which he so arrogantly plumes himself, and the authority of his name . . . make unitedly but a miserable figure when confronted with the counted facts of his accurate and statistical opponent. (Hodgkin,[2] p. 1093)

Remarkably, Hodgkin revealed his conscious mode of probabilistic thinking when concluding that, with respect to the past, the numerical system was,

> indeed, an invaluable method . . .; but with respect to the future, it appears to me to be rather an application of the doctrine of probabilities: this however, I do not advance as any objection to its application.

When publishing his lecture 20 years later, his motivation was not only to exhibit what the numerical system was, but also what it was not, '*and to what it cannot effect, and thus counteract a* cacoethes

numerandi [Hodgkin's italics], *or abuse of statistics, to which, as a statistician, I should object*' (p. 1090).

This was a British compromise, a warning, lest pure numerists should despise inquiries in which the *méthode* need not to be employed. For there were many cases, if '*carefully observed and detailed, possess very considerable value even when they stand alone*' (p. 1093).

This held for descriptions of pathology, Hodgkin's main subject, for which he referred to Louis before the Paris debates about evaluation of therapies took place, and Louis's respective work was published.

Evaluation of therapy

Louis's evaluative work and the Parisian debates were promptly received in Britain. As early as 1835, *The Lancet* reviewed his *Recherches sur les effets de la saignée* (Researches on the effects of bloodletting, 1835):

> The method adopted by M. Louis may be easily stated. If the [natural] mortality in pneumonia were known to be 25 in 100, and its mean duration twenty-one days, it would only be necessary to subject a considerable number of similar cases to a particular treatment, to count the deaths or recoveries, and to take the mean duration, in order to state in precise terms the modifying power of the treatment. The same process would serve to compare or contrast the effects of two systems of treatment . . . From data thus furnished the results could be calculated, thrown into tables, and readily compared. (*Lancet*,[3] 1834–35, pp. 84–85)

To make this plain, the reviewer adduced '*a few examples, selected from several others now before us*': There was the report by Sir James McGrigor, Director (Inspector) General of the British Army's Medical Service, about different treatments for syphilis. Indeed, McGrigor (b.1771), when still head of Wellington's Army Medical Corps during the Napoleonic wars, had developed a statistical bent, drawn up regular reports and had stimulated others to do so. Since 1815, McGrigor had drawn on thousands of cases treated by his subordinates (Tröhler,[4] pp. 100, 104, 108–110). So, it was easy for *The Lancet*'s reviewer to criticise Louis's data compared to McGrigor's arrangements. The number of patients studied by Louis was too small, he held, and '*this distinguished pathologist falls into some errors*' by not taking account of patients' ages: by reference to British life-tables, it had become clear that mortality varied by age, and this might have accounted for the differences observed by Louis.

Furthermore, Louis had not presented his data in tabular form – and *The Lancet*'s reviewer made up for that by dressing them up in two tables! Of course, he aimed '*to separate facts from opinions, dissipate the scepticism* (sic!) *which some have entertained concerning the utility of medicine, and raise it to the rank of an exact science*' – just as had been formulated in Paris (*Lancet*,[3] pp. 85, 87).

A few weeks later another review of an earlier work by Louis was also published in *The Lancet*. It was on the *Pathological Researches on Phthisis*, Louis's first field of activity so much admired by young Hodgkin. It had meanwhile been translated into English by Charles Cowan. The editor was

> glad of a new opportunity of drawing attention to the subject [and hoped] to prosecute and promulgate the numerical doctrine with effect in this country. [It was] the method of weighing medical facts and establishing medical principles, by counting and comparing them, and registering quantities, majorities, and minorities. (Lancet,[5] 1834–35, p. 292)

And it implied probabilistic thinking, albeit in the unconscious mode.

The rest of the article consisted of an extensive quotation from the introduction by Charles Cowan (b.1806), the British translator. Cowan was a well-educated young physician. He held an Edinburgh MD and a bachelor *ès-lettres* of the Sorbonne in Paris. He had spent nearly four years in the hospitals of Paris; and he was personally acquainted with Louis, having followed his clinical rounds and assisted in his post-mortem room. First, he drew attention to the principles of observation according to that famous 17th-century Englishman, Thomas Sydenham (b.1624). Cowan then went on once again to emphasise the need for large numbers of cases, to be presented in tables. He also stressed the need to consider patients' ages, sex, the severity of disease, its natural course, and the characteristics of the epidemic at a given time when submitting '*all* [. . .] *facts to the unerring test of arithmetical analysis*'. All these efforts were to be made in order not to overlook fallacies, particularly in therapeutics: '*No part of medical knowledge is more in want of a rigorous method of investigation [. . .]*'. One had to overcome preconceived ideas and selection bias to obtain the necessary fair comparisons. Put in one sentence:

> It is not our intention, in advocating the numerical method, to conceal for a moment its difficulties; these are great and numerous, but at the same time they can never form any solid argument against its utility,

> though they will necessarily curtail the number of its disciples. (pp. 295–296)

What sober, yet sensible and farsighted words! McGrigor's and Cowan's reviews illustrate the culture of numerical accountability, unconsciously linked to probabilistic reasoning, that *The Lancet* propagandised. It can further be illustrated by yet another 'home grown' British contribution a few years later.

Probabilistic reasoning in 19th-century British clinics

William A. Guy (b.1810), a young London-educated physician with a Cambridge BM, described the state-of-the-art of medical epistemology from a British point of view in a 22-page article entitled *About the value of the numerical method as applied to science but especially to physiology and medicine*.[6] Like Hodgkin, he had studied on the continent, in Heidelberg and especially in Paris (whence he was well aware of Louis's work). He had become interested in statistics, and he wrote for one of the first issues of the *Journal of the Statistical Society of London*.

Guy's analysis was original in that it based its strong advocacy of the numerical method on the history of *science*. '*A very obvious and most important application of statistical investigation is as a test of the truth of theories*'. The idea of hypothesis-testing with the help of quantification was an immense epistemological step. Unconsciously it implied probabilistic reasoning. With reference to astronomy and chemistry he continued: '*The certainty of a science is exactly proportional to the extent to which it admits of the application of numbers*'. Numerical probability became the natural substitute when numerical certainty was not available (Guy,[6] pp. 30–31, 34, 37). Now, this was obviously the case in medicine, with its variable quantities and events. And so, medicine was also amenable to perfection through adoption of the numerical method as had been the case in astronomy and chemistry. Indeed, he diagnosed '*. . . a growing disposition to apply calculation to the phenomena of life, . . . as one of the characteristics of the age in which we live*' (p. 35).

To start with, Guy referred to Bisset Hawkins's *Elements of Medical Statistics* (1829). It was the only reference to a statistical work he gave. From there he developed a methodological hint, namely '*a best rule [. . .] for ascertaining whether the observations which we have collected are sufficiently numerous to yield a true average*' (pp. 32–33). For medicine this was obvious in vital statistics and nosography (description of diseases). And then he continued:

> . . . as to the action of remedies, and the relative advantage of different modes of treatment – nothing can determine these but an accurate numerical comparison of their fatality and duration under the several methods of treatment proposed. (p. 40)

This statement was comparative, quantifying and consciously, yet informally, probabilistic. But as a current example of formal probabilistic thinking, Guy referred to the trustworthy estimate of the benefit of vaccination against smallpox, and of the extent of protection it gave (p. 46). This was the example in which a calculus of probabilities had been used 60 years earlier to show the utility of inoculation of smallpox. Numbers added precision to words of doubtful meaning. Yet Guy admitted that the application of this method to individual cases was limited. It was again the group-versus-single-case issue: Medical practice needed 'tact' – what the French referred to as *art médical*. But, on a closer look, this 'tact' was nothing more than '*a rough calculation of chances in which all the elements of calculation are rapidly seized and accurately estimated*'. This was conscious probabilistic reasoning. Guy saw this as '*common sense to men in ordinary affairs of life*' (p. 44) and we may recall here Laplace's 'common sense reduced to numbers'.

Although Guy could have taken these arguments from 18th-century Scottish doctors, or literally – yet improbably – from John Gregory (who has been dealt with in part 1 about Britain in this series),[a] his qualification of the numerical method was new. In his words, it was

> to supply the want of tact by furnishing the inexperienced with accurate calculations of the *probable* event of different diseases, and the *probable* [my italics] consequences of different modes of treatment. These calculations supply but one element for the solution of the problem, for they apply only to cases of average severity. (p. 42)

In other words, they applied to the average of a group. And the important practical consequence of this apparent limit was entirely new, too: these calculations '*leave to the physician the task of ascertaining all the circumstances in which any particular case departs from the average severity*': This was clearly probabilistic thinking (pp. 42–43). And it was not obstinately doctrinaire, but pragmatically realistic.

Thus, Guy's definition of the numerical method differed from Louis's or Trousseau's. His was the method of averages (p. 32). These essentials being specified, he had neither the intention to discuss the numerous errors with which a statistician could be

charged, nor to '*defend him and his method against the objections as well as the ridicule of his opponents*'. For the errors were precisely those to which the results of common observation, expressed in common language, were equally exposed (p. 43). Indeed, at the outset of his reflections, he had enumerated the necessity, the difficulties and pitfalls of precise observation, of grouping of comparable facts in tables, of large numbers etc. (pp. 31–35).

As the French clinicians had done, Guy distinguished medicine considered as a *science* from medicine considered as an *art*. Yet the latter was medicine's disadvantage, for as a practical art it must necessarily remain imperfect, whereas nothing could hamper it from attaining a high degree of perfection as a science. Calculation was the necessary feature of a science, or inversely: '*Rob science of calculations and we degrade it to an art*'. He concluded optimistically '*. . . we may rest assured that every addition made to our science will be [also] a gain to our art*' (pp. 45–46).

All these prevarications about probabilism condensed into a clear-cut statement in a 21-page review of Gavarret's *Principes Généraux de Statistique Médicale* (1840) published in 1841 by *The British and Foreign Medical Review*. Because the review mixed the contents of Gavarret's book with an input very much taken from Guy, I believe that Guy wrote it.

A British view on Gavarret

Considering the independent British tradition of unconscious mode of probabilistic thinking, it is instructive to consider how Gavarret's book was reviewed. The text provides a kind of theoretical standard of probabilistic reasoning after 1840.[7,8]

The long introduction advocating the NUMERICAL METHOD (author's upper case) repeated at length Guy's arguments about the status of medicine within the sciences, and their history. It also referred to Todd's tabular analysis (described in Part 3/1 of this series[a]) as a development of Baconian precepts. But then the review developed two novelties: the clarification of definitions and the inclusion of the calculus of probabilities when one reads:

> The numerical method is sometimes erroneously regarded as a mere substitution of figures for words. Against this mistake Gavarret strongly protests, and with good reason, though th[is] mere substitution is [already] a great improvement in our scientific methods, seeing that figures admit of strict comparison which words do not (p. 13). [According to Gavarret] medical statistics, or as we prefer to call it, the numerical method, is "la théorie des grands nombres," the application of the calculus of probabilities to the science of the physician, "le complément le plus indispensable de la méthode expérimentale". (pp. 16–17)

This terminological clarification was deemed necessary to specify that the use of numbers as a research tool differed from the historical meaning of statistics as the science of the state (p. 12). The calculus of probabilities supplied a method to determine the limits of error of our observations, a method, for instance, to specify the limits of confidence of a difference between two treatments (p. 19). For 'the medical brethren as are conversant with the mathematics', the formula was included in the review (p. 20). Examples followed, among others some from Louis, with the reviewer's appropriate critique of his having studied too few cases (pp. 19–20). And he continued:

> If even Louis [. . .] lies open to censure, what shall we say of the majority of his followers, and in what terms shall we speak of those who still persist [. . .] in drawing important conclusions from one or two scattered and not comparable facts. (p. 18)

What was to be done? One could just repeat:

> Once more, then, the SCIENCE OF MEDICINE wants facts – comparable facts – numerous facts: well observed, carefully arranged, minutely classified, and acutely analyzed. Her language must be the language of figures; her test, the calculus of probabilities; her example, the most perfect and exact among the sciences of observation and experimentation [i.e. astronomy and chemistry, respectively]. (p. 21)

This was certainly a truly remarkable passage, an example of the proverbial British pragmatism reuniting the perennial quest for dogmatic certainty with the proposed yet practically unattainable model of probability under review to form a practical *modus vivendi*.

Traditions persist

If only these words had been taken to heart! Yet the application of formal probabilistic procedures was hampered, within medicine, by ignorance, many inherent difficulties (for example in collecting comparable cases), confidence in presupposed certainty of technological innovations, and socio-culturally, by hierarchical authoritarian structures within medicine. Despite the methodological insights into the

possible sources of errors described, many but not all doctors continued to compare small groups, often incomparable, to select cases, to fail to take account of the natural course of diseases, to fall into the *post hoc-ergo-propter-hoc* fallacy (for instance in the appreciation of homeopathy), even to cheat. Other doctors were more satisfactory.[9–12]

Interestingly enough (as I have shown above), Hodgkin saw no necessity to change anything in his text, when re-producing his lecture 20 years later,[2] rather he had a new motive. He was in fact no clinician, but he had become a leading British pathologist by then: Hodgkin's disease is still known today.

Likewise, Guy's Croonian Lectures of 1860 bore a title similar to his 1839 paper (described above), namely, *The numerical method and its applications to the science and art of medicine*.[13] By now engaged in forensic medicine and public health statistics, Guy had noticed the current of the times: He had read Gavarret as I concluded from its review in the *British and Foreign Medical Review* and therefore now emphasised the importance of calculations in the 'theory of probabilities [his italics], *and in that numerical method which may be said [to be] one of its principal branches, and certainly its most important one*' (p. 331). He drew on Gavarret's '*mathematical formulae for calculating the limits of possible error attaching to any given number of facts*' – criticising again Louis for his insufficient numbers. But if in practice one stuck to Gavarret's

> . . . ordeal of a mathematical formula, we shall be driven, if not for state purposes where we can command almost unlimited supplies of facts, [. . .] to forego the use of the numerical method altogether. So, he asked:
>
> Is there not, in the absence of certainties, at least a fair probability, that the average results of even a small number of facts may be entitled to confidence?[. . .] is there not some escape from the very disagreeable dilemma of being obliged to reject all average results except those derived from one thousand, two thousand, three thousand facts? (p. 469)

Yes, there was one. Namely when the difference of averages was either none or uniformly very marked, or '*when we have to do with the apostles [. . .] of wild medical heresies*'. Then '*a heavy battery of facts and figures*' was not always required to demolish a hypothesis (p. 554). He offers as an example four sentences quoted from his friend Thomas Graham Balfour (*1813). These describe a controlled trial to test the claim that belladonna reduced the likelihood of developing scarlet fever. 'To prevent the implication of selection', Balfour took 151 boys in a military orphanage and assigned them alternately either to receive (n = 76) or not to receive (n = 75) belladonna. Two boys in each group developed scarlet fever. Balfour properly cautioned against inferring on the basis of such small numbers that belladonna had no prophylactic effect (Balfour, 1854), whereas for Guy this evidence was sufficient!

Guy concluded his lecture series by recounting Ambroise Paré's amputation trial of 1536 (Paré, 1575) as 'an unconscious illustration of the value of that numerical method which I have endeavoured to set before you to explain, and to vindicate . . .' (Guy,[13] p. 597).

Outlook

Guy's special position becomes clear if we see him in the context of a well-known clinician of his age, Andrew Whyte Barclay (b.1817). This Edinburgh and later Cambridge MD lectured to students for twenty years, till 1882, on pathology and therapeutics at St. George's Hospital in London. Thus, he was a man of the establishment. His attitude towards the methodological aspect of medical research may be derived from his Lumleian Lectures for 1864 which bore the title *Medical errors . . . Fallacies . . . of the inductive method of reasoning to the science of medicine*.[14] They were all about the ways to find laws of cause-effect by experimentation in the Baconian tradition. Yes, large case collections, tabulated in statistical form, were better than to trust in memory, and there might even be a way to induce a causal relationship (p. 17). But they were simply used for calculating averages, and thence for deducing erroneous assertions '*of the curative power of a remedy*', which was anyway not applicable to individual cases (pp. 57–58).

Barclay shrewdly analysed the therapeutic inquiries then issued recently by the British Medical Association on therapies of pneumonia (with and without bleeding), non-syphilitic psoriasis, tapeworm and scarlatina. He quoted huge accumulated Parisian and London statistics (not deemed necessary), Dr. Balfour's controlled trial and even William Guy, without any details. He did this from the standpoint of principles of logic without entering in the ways of thinking behind them. No kind of probability was mentioned, let alone the calculus. Rather he repeated a typically paternalistic hackneyed saying:

> It is much to be hoped that scientific medicine may ere long be delivered from this, the oldest, the most obstinate, the most universal fallacy . . . the most

> constant theme of logicians of all times-[namely] the post hoc ergo propter hoc [Barclay's italics].

But how? Here he quietened down. Barclay's were state-of-the art lectures, overall critical, full of warnings, but without a vision for advancement:

> The numerical method has not yet been applied to any great extent in therapeutical [sic!] inquiries. The difficulties attending its employment are so great, and the method itself so open to fallacy, that the results are not likely to be very available for scientific purposes. (pp. 116, 119–120)

This attitude, which he repeated, less outspokenly, 17 years later in his Harveian Oration to the Royal College of Physicians (Barclay, 1881), was not constructive, yet it may very well have been representative of the views of a great majority of physicians:

> [The] curative power of a remedy [was asserted] because in ten, twenty or even a hundred cases recovery followed its administration; and yet this is what is commonly meant when experience is appealed to. (Barclay,[11] p. 119)

Under these circumstances, for the few who thought about methodological issues, a numerical method was, for the time being, the solution of the complicated problems of day-to-day clinical practice. Insofar as Guy, for instance, had found some well-designed and cautiously interpreted trials, (as judged by contemporary insights), he confirmed a genuine British tradition of enlightened pragmatism,[15] whereas the French mathematical tradition, sophisticated formally by Gavarret, was only theoretically valuable.

The general situation in practical therapeutics was one of *laisser-faire*. But German authors would not leave things there. As soon as Gavarret's book had been edited in German in 1844, young clinicians faced its theoretical and practical difficulties. They developed his mathematical concepts and applied them in practice.

(To be continued)

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Supplementary File: The references listed below are chosen as essential to the reading of the article. However, the full list of primary and secondary references is available online both on the Journal's website as supplementary material, and with the original publication at https://www.jameslindlibrary.org/articles/probabilistic-thinking-and-the-evaluation-of-therapies-1700-1900/.

Note

1. The series on probabilistic thinking and the evaluation of therapies, 1700-1900, will appear as separate articles in forthcoming issues of the JRSM.

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