

research focuses on evidence found through methods that are biased toward standardised, 'blind'-able methods to collect data by aggregating human participants. These methods do not allow CAM practitioners to evaluate humans as individuals with unique histories, practices, health experiences, and cultural and religious backgrounds and expectations, all of which can influence a subject's response to treatment.

For the *JAMA–Archives* issues that focus on CAM, these and other differences related to assessment were overlooked: CAM manuscripts were peer reviewed by the same review standards as scientific and biomedical manuscripts. Therefore, for the *JAMA–Archives* issues, the journal was metaphorically forcing a square peg (CAM) into a round hole (biomedicine); CAM does not fit randomised, controlled, blinded methods for trials that create evidence-based medicine well if at all.

*Bounding Biomedicine* left me examining my own perspective on biomedicine and CAM as well as on evidence-based research and medicine and on research methods that require blinding, randomisation, control, placebos and aggregation. At times, I struggled with in-text references to authors whom I did not know, and at other times, the creative approach to reporting seemed to create redundancy. But the author kept me engaged with her logical progression and analysis. At one point, I wrote a note in the margin: 'Derkatch is asking questions that anyone assessing medicine, research, and rhetoric should be asking.' She encourages the reader's critical thinking with her tone; she prods her readers to define 'medicine' and thus expand their own perspective on care, wellness, illness, disease, evidence and medical rhetoric. Perhaps by transparently addressing the pre-existing boundaries of biomedicine, practitioners can bridge the differences and build respect and resolution between the overlapping fields of biomedicine and CAM.

My copy of the book currently has 100 sticky notes in the margins with questions, comments and notes. I anticipate that I will reread this book again and again before I reflect and answer my own questions. I am also using the full and well-rounded bibliography to find related readings for myself and my students. The text should be introduced to graduate students as well as researchers and practitioners in science and the humanities. Like Segal's *Health and the Rhetoric of Medicine* and Mol's *The Body Multiple*, Derkatch's *Bounding Biomedicine* is destined to become a muse for medical and scientific rhetoricians.

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**Miguel García-Sancho**, *Biology, Computing, and the History of Molecular Sequencing: From Proteins to DNA, 1945–2000* (Science, Technology and Medicine in Modern History) xv + 242 pp., illus., apps., bibl., index (New York: Palgrave Macmillan, 2012), and paperback in 2015. \$35.00, ISBN: 1137543329.

This review is of the newly published paperback edition of the book, which was originally published in hardback in 2012. Miguel García-Sancho traces the history of molecular sequencing by primarily following the path of Frederic Sanger's work on protein, RNA and DNA sequencing and its subsequent use and alteration in DNA sequencing machines. García-Sancho traces the work on linear sequences through the historical contexts of biochemistry, molecular biology, the human genome project and biocomputing.

García-Sancho utilises – and occasionally critiques – previous work on the history of molecular biology, eg. by Horace Judson and Michel Morange; the specific British work of Soraya de Chadarevian; the human genome project by Robert Cook-Deegan;

work on sequences and databases, eg. by Bruno Strasser and Joel Hagen; and finally, biocomputing, by eg., Joseph November. García-Sancho's 2012 book was in production at the same time as Hallam Stevens's *Life Out of Sequence* (University of Chicago Press, 2013); neither refers to the other but the two books nicely complement each other. They make some of the same points about the change in the nature of work in biology with the advent of computerised databases to store sequences and algorithms to analyse them. Stevens is US-centred. García-Sancho concentrates on sequencing work in the UK: first – where Sanger was located – the Biochemistry Department of the University of Cambridge, the Laboratory of Molecular Biology (LMB) in Cambridge and, then, the formation of the European Molecular Biology Laboratory (EMBL) to store and analyse sequences.

Part I, 'Emergence: Frederick Sanger's Pioneering Techniques' begins with the early twentieth-century history of protein chemistry and the context within which Sanger began his career. In the 1950s, Sanger developed techniques for sequencing proteins and used them to sequence insulin for which he received his first Nobel Prize in 1958. Sanger was, according to García-Sancho, one of those who 'created...the concept of sequence' (p. 34). With the advent of molecular biology and the influence of Francis Crick and Sidney Brenner, Sanger shifted to RNA and DNA sequencing, joining the newly formed LMB in 1962. Nonetheless, Sanger always viewed his work as within the discipline of biochemistry. Hence García-Sancho emphasises that the 'phenomenon of molecularization' cannot be explained solely with the development of molecular biology.

Part II, 'Mechanisation-1: Computing and the Automation of Sequence Reconstruction' follows the changing use of computers from structure analysis in crystallography to sequence storage and analysis in biochemistry and molecular biology. The 'form of work' (a historiographical category explicitly utilised by García-Sancho) shifted with the introduction of databases for storing sequences, algorithms for analysing them, the personal computer in the laboratory and more centralised working groups.

In Part III, 'Mechanisation-2: The Sequencer and the Automation of Sequence Construction', García-Sancho compares work on the development and commercialisation of sequencing machines in the UK and the US. Different values and attitudes toward the 'academic-industrial complexes' account for the eventual success of the American Leroy Hood's sequencing machine. Its use 'triggered a shift in sequencing from a human-led to a mechanised form of work' (p. 117).

Readers interested in the history of bioinformatics will also want to consult a recent open source series on the 'roots of bioinformatics', edited by David Searls ('The Roots of Bioinformatics', *PLoS Computational Biology* 6, 6 (2010): e1000809. doi:[10.1371/journal.pcbi.1000809](https://doi.org/10.1371/journal.pcbi.1000809)).

García-Sancho might have compared sequencing methods in the US. He just mentions the Maxam-Gilbert sequencing method in passing, with no comparison or analysis as to why Sanger's method became more widely used, both by people and those who altered it when building sequencing machines. Anecdotally, a molecular biologist once told me that she had run a sample using both techniques; the Sanger method took less time.

García-Sancho uses the term 'information' in several different ways, without distinguishing them. When he mentions 'qualifications to the central dogma of molecular biology' (p. 192, n. 14), he might have quoted Francis Crick's definition of information when Crick first stated the Central Dogma: 'Information means here the *precise* determination of sequence, either of bases in the nucleic acid or of amino acid residues in the protein' (Crick 1958, 'On Protein Synthesis', in *Symposium of the Society of Experimental Biology* 12, p. 153). In this sense, information involves a mechanism

operating to produce a linear sequence. A different non-semantic sense of ‘information’ is that used in assessing the accuracy of the transmission of electrical signals (García-Sancho, p. 65), which Crick claims did not influence him. A final sense of ‘information’ is that used in the later chapters to refer to any item entered into and processed by a computer program.

In sum, readers seeking an intellectual and institutional biography of Sanger’s work on sequencing or the early UK perspective on databases, algorithms and machines for sequencings will find it in García-Sancho’s book.

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**C. F. Goodey**, *Learning Disability and Inclusion Phobia: Past, Present, Future* (Oxon, and New York: Routledge, 2016), pp. viii, 178, £79.00, hardback, £25.49 kindle, ISBN: 978-0-415-82200-8 (hbk) and 978-0-203-55665-8 (ebk).

In his book, Goodey Goodey makes the point that the term ‘learning disability’ is not a fixed, stable concept, but one which has changed and evolved over time. He states his aim as being to not only deconstruct the concept of a learning disability but also to ‘reconstruct the concept as something else entirely’ (p. 2). He argues that learning disabilities are as a consequence of an inclusion phobia which he defines as, a condition which has existed since time immemorial resulting in the identifying and scapegoating of outgroups. He states therefore that social inclusion should be our target in both social and political action.

He argues that although we think about ‘learning disabilities’ as being fixed and permanent, a learning disability is in fact a label which shifts over time. For example, he discusses how a previous label applied to this group, ‘feeble-minded’ would have included many groups of people (such as unmarried mothers) who would not be included within our current concept of a learning disability. Goodey cites many examples of inclusion phobia in action, including ‘mild’ phobias such the person in the bank who talks to the member of a person’s support staff rather than the person being supported by the member of staff, tracing this concept through history.

In a recent commentary on Goodey’s paper on why we should study the history of learning disability, Bradshaw and McGill point out that staff can also be instigators of social exclusion.<sup>1</sup> In this research, the regard for residents was one dimension which emerged from their evaluation of staff culture. This dimension focused on the extent to which people with learning disabilities were seen as being fundamentally different, or as the authors describe in the paper ‘not like us’. This seems to be a very clear illustration of Goodey’s inclusion phobia.

He argues that this concept of inclusion phobia pre-exists the label of learning disability. He suggests that classifying a human group by their intellectual abilities is the current method by which this inclusion phobia is manifested. He argues that this representation of status by reference to an internal characteristic of ‘intelligence’ is recent.

<sup>1</sup> C.F. Goodey, ‘Why study the history of learning disability?’, *Tizard Learning Disability Review* 20, (2015): 3–10; J. Bradshaw and P. McGill, ‘Commentary on “Why study the history of learning disability?”’ *Tizard Learning Disability Review* 20 (2015): 11–14; C. Bigby, M. Knox J. Beadle-Brown, T. Clement and J. Mansell, ‘Uncovering dimensions of informal culture in underperforming group homes for people with severe intellectual disabilities’, *Intellectual and Developmental Disabilities* 50 (2012): 452–67.