Sir Thomas Lewis: his impact on American cardiology

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At the beginning of the century, Thomas Lewis had just begun his formal university education at Cardiff. On this side of the Atlantic, the American Physiological Society had reached maturity but the bulk of medical education was in the commercial medical schools, where basic science languished. The young Lewis was early attracted toward a career in classical physiology, worked in the laboratory of E H Starling, and certainly believed in the scientific life exemplified by Starling. 1 Soon, however, Lewis engaged in what was to be a lifelong evangelic campaign to establish clinical science as a discipline, separate and definably different, from both physiology and medical practice. In doing so, however, he repeatedly reaffirmed his respect and affection for those two traditional professions. 2 His scientific and social independence probably presaged his lack of active participation in the great physiological congresses of the twenties, Edinburgh (1923), Stockholm (1926), and Boston (1929). His conviction of the need of an entirely separate breed of medical persons was disturbing to many in the profession, and Sir John McMichael recently has penned a strong contrary opinion regarding the desirability of such a group. 3 I suspect that Sir John may well have been miffed by certain of the Lewis contentions for example “Efficient medical practitioners are not scientists”. 4

I shall deliberately present a partisan view of the impact of Sir Thomas Lewis on cardiology in the United States and my assertions will be biased by the opinions of a number of my senior mentors. As a recent medical graduate in early 1939, I did attend several of Lewis’s clinics, and I can still vividly recall two cases presented at the time of my first visit: for example, one, a young man, with mitral valve disease and a recent popliteal embolism, and the other, a woman, with atrial fibrillation and probably thyrotoxicosis. These cases constituted the background which allowed me to hear Sir Thomas expound on the nature of atrial fibrillation; the value of digitalis in patients with rheumatic heart disease in atrial fibrillation contrasted to those with heart failure and regular rhythm; and how physicians probably made the correct diagnosis of mitral stenosis, by the sound complexes, like recognising the presence of a dog by hearing its bark, rather than with self deception believing to have identified the accurate timing of the murmur. I was impressed by the challenges, and I think they were intended to be just that. I believe that his views, admittedly, literally axiomatic, have been frequently misrepresented as absolutes, by some of the present generation of American cardiologists. These have derided his dogmatism instead of analysing the purpose of his questions. (Parenthetically, I continued to use digitalis in patients with regular rhythm following the dictates of my American mentors.) I was surprised and discomfited on being singled out at the end of his clinic to be asked about American cardiology—but he was kindly disposed toward my ignorance. I remember one item of advice he gave to me (in paraphrase): “If you ever have an idea, and it has possessed you, don’t go early to an expert, he will tell you it is impossible, throwing cold water over your project”.

While I believe that reminiscences are frequently embellished, or slanted by faulty memories, there are recollections 5 on record which state that Lewis was introduced to the string galvanometer in Einthoven’s laboratory by an American (George Fahr) and, when Lewis’s own instrument was being installed and made operative in the basement of University College, another American was his helper (Alfred Cohn). These were the beginnings of American contacts and within a decade, a veritable flood-tide of Americans was visiting him, some becoming active workers in his laboratory. One of these was Paul White who entered the periphery of the Lewis arena in 1913, and after a few months he was “recognised” and became an active worker. 6 Indeed, with Lewis and Meakins, White carried out studies on atrial activation with, incidentally, conclusions contrary to that of Meek and Eyster in this country, initiating a long continuing controversy. Dr White remained a close friend of Dr Lewis and his family. As a pleasant, gregarious, somewhat egocentric clinician, however, I suspect that he did not share the...
same convictions as Lewis regarding clinical science as an independent vocation. Lewis in the preface of his book Pain mentioned his pleasure in acknowledging the help he received from Paul White in review of the manuscript.

The journal Heart was launched in 1909 with Lewis as editor and unquestionably the dominant force. It is noteworthy that two Americans were listed among the editorial consultants, A W Hewlett of the University of Michigan and G N Stewart of Cleveland. It must have relevance to Lewis's plans for the journal that he was also able to list on the title page of the first issue 19 Americans, of the total of 44 editorial “collaborators”. These Americans were widely dispersed geographically, and were of varied disciplines; for example, clinicians, surgeons, physiologists, and pharmacologists. Hirschfelder of Johns Hopkins was one of these. How this connection was established is not known to me, but Hirschfelder had published a good paper on experimental atrial fibrillation in 1908 and had been active in recording electrocardiograms with Lewellys Barker in 1909 and 1910. Originally a Californian, Hirschfelder became “the first cardiologist” at Johns Hopkins before coming to the University of Minnesota as a professor of pharmacology. It is interesting also that Barker wrote the first American “in depth” review of electrocardiography. This appeared in Johns Hopkins Bulletin in 1910. It was as late as 1915 that an electrocardiograph was installed in Minnesota, reportedly, after the acting professor of medicine, S Marx White, visited Lewis’s clinic.

The early issues of Heart had many articles, of which either the chief author or a coauthor were Americans. These persons were later to be among the leaders in American medicine who were interested in heart disease, for example Levine, Cohn, White, Stroud, Wedd, Feil, Master, and Marvin.

I was curious to explore any relations of Lewis and Sir William Osler. I found Lewis had scant notices in the writings of Osler or in his biography. I sensed that Osler had a benevolent paternalistic attitude toward Lewis reflected in his praise of the work in the Army Hospital at Colchester. From Frank Wilson’s writings, the most intimate friendship established by Lewis with any American was that cemented with him. Apparently their personalities were compatible; their attitudes toward the clinical users of the electrocardiograph and their scientific visions were similar. (Maybe later they shared mutual chagrin in their role in the mix-up in the nomenclature of the bundle-branch blocks, an error early appreciated by George Fahr in Minnesota). Their association in ornithological pursuits started in early 1918; from Wilson’s account they were able to develop an amazing detachment from the sickening horror of the late months of the 1914–18 war. Without functional cardiac disorders coming to their hospital, they were professionally idle and wandered the countryside looking for bird life. The adulation by Wilson for Lewis was strongly voiced and when Lewis came to the University of Michigan for an honorary degree in 1922 they were able to reinstitute bird watching and photography. This was the year of a Lewis American tour and he gave his important exposition concerning “limited potential differences” as the basis of the electrocardiogram in both Pittsburgh and San Francisco.

Wilson’s praise of Lewis’s contributions continued. In his introductory remarks, addressing the International Conference of Physicians (1947), he stated: “Thomas Lewis contributed more than any other man to the development of the principles and methods of analysis upon which the interpretation of the electrocardiogram is founded”. In a foreword to a paper by colleagues (1953) he equated Lewis’s concept of ventricular excitation, expounded as the theory of limited potential differences as amounting “in essence, to what was subsequently referred to as the dipole hypothesis”. I feel in accord with such an assertion but the crucial point is a generous definition of the phrase “in essence”.

That Lewis and Wilson were kindred souls is supported by some mutual characteristics, for example times of detachment and apparent coldness to visitors, a ferocious editing of papers and on occasion an acidulous tongue. In a scathing denunciation of an alleged misuse of the electrocardiograph by inadequately trained physicians Wilson wrote (1951): “Now there is one (electrocardiograph) in almost any village of any size and there are comparatively few people who are not in greater danger of their peace and happiness destroyed by erroneous diagnosis of cardiac abnormality based on faulty interpretation of an electrocardiogram than of being injured or killed by an atomic bomb”. I believe such a statement was more intended to be jocular than critical and really foreign to Wilson’s character. This downgrading of the performance of general practitioners seems to echo Lewis’s remarks, when addressing the British Medical Association in 1919, he advised his audience, “to become expert in the use of simple means, rather than indifferent manipulators or puzzled worshippers of instruments wholly or partially beyond general comprehension”.

Another American connection to Lewis that has a scientific poignancy relates to the saga of W H Craib, a story well told by Pruitt. An engineer first, then a graduate of Guy’s Hospital Medical School, he was surprised, by his own telling, in having a Rockefeller research fellowship tendered to him in 1925. He went to Johns Hopkins Hospital and there, he first heard of Lewis’s already classic book, The Graphic Registration of the Heart Beat. He worked under the aegis of E P Carter and Cowles Andrus, and, probably with some aid from the physicist, R Canfield, I believe that he
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personally evolved a clear revolutionary theory of the genesis of the electrocardiogram based on a concept of myocardial excitation through sequential doublets (or dipoles). (Incidentally, Carter had worked on University College hospital records showing intraventricular blocks five years previously.) Craib reported having had a terse, quasi-sympathetic commendation from Wilson but the alleged conversation has been suspect. Craib is even less charitable in his report of encounters with Williams, Einthoven, and Adrian. Wiggers' mentions that Craib's paper at the Stockholm conference (1926) was one of the outstanding ones. Craib had, however, apparently developed feelings of rejection, and to a large sense seems, from the research viewpoint, to have been a victim of limited finances and space, together with the scientific reorientation of Lewis's laboratory. He left England to return to South Africa, and from his story seems to have harboured a sense of injustice, expressed with some humour, but which continued to rankle within him, with Janus-like reaction toward Lewis. His autobiographical account is written in large part in a dialogue format. His recall of conversations should be honoured as having some basic veracity but the phrases used must be regarded as largely fictional or a highly individualised rendering of the facts.

Unfortunately the Craib memoirs have initiated controversy regarding priority in the conception of the dipole theory. The appearance of Craib on the programme of the American Society of Clinical Investigation has not been documented and the admission by Wilson that Craib had "scooped" him, has been branded as false by some of the Wilson disciples. The Craib claims were an affront to Wilson's daughter (recently deceased), who reacted strongly to what she sensed was the charge of plagiarism and a slur on her father's veracity. I think the matter should rest with praise (in excelsis) for both Wilson and Craib. Differences of opinion will persist and I disagree with the implication in the phrases of McMichael, "his (Craib's) doublet theory became the dipole theory in the hands of F N Wilson".

Among persons developing an interest in clinical electrocardiography in America were some who started with basic projects, for instance, Carl Wiggers, who was early interested in the relation between electrical excitation and the contraction process. His book on electrocardiography, published in 1929, was dedicated to Einthoven, and it dwelt at length on the physical nature of galvonometers. He extolled the works of Lewis at a rather modest level. Wiggers gives an account of being introduced to Lewis while visiting in Starling's laboratory (1923) and later having been entertained by Sir Thomas and Lady Lewis.

Visualising the two men, one smallish, tense, and rather impatient, and the other largish, relaxed, and verbose (and probably with a cigar), one can understand, despite mutual respect, that they might not be congenial associates. Thus, Wiggers' statement, "If Lewis had a sense of humour, I never found it". The development of different types of galvonometer with their introduction into the United States has been outlined in detail both by Wiggers and by Burch, and the contributions of H B Williams in this country, emphasised in these accounts, testify to early independent electrocardiographic developments in progress in the United States outside any dominating influence of the "Lewis school".

When Lewis reoriented his research goals, consistent with his vision of his "clinical science" away from electrocardiography, it has been alleged that he may have thought electrophysiological science, as being either too advanced technically, or too practical for him. For his change in main interests, he has been scorned by some, yet acclaimed by other like minds. I am aware that a group at the Mayo Clinic, headed by George Brown and Edgard Allen in particular, beginning to probe the riddles of the peripheral circulation and of hypertension, often referred to Lewis as a leader in the field. Relevant to these activities, was the formation of a special section on the "Peripheral Circulation" in the American Heart Association in 1935.

H M Marvin who was on a Guggenheim fellowship studied aspects of control of the peripheral circulation in 1926 with Lewis. As president of the American Heart Association he wrote the first editorial when the new journal Circulation was launched in 1950. The studies by Lewis of peripheral circulatory control and of the mediators of pain apparently merited the attention of Charles Best, my first professor of physiology in Canada (1930), who instructed my class at length in the triple skin response to histamine and of an "H" substance, underlying pain. Another bridge of Lewis to the American scene is reflected by the choice of the University College laboratories by Eugene Landis, also when on a Guggenheim fellowship. After years of impaling capillaries in amphibia, in London he was able to puncture human capillaries. After return to the United States he became Professor of Physiology at Harvard. Still another linkage of the Lewis influence to American medical education may be seen in the career of H C Bazett who, while at Oxford, used many of Lewis's records in the study on the QT interval (1920) and shortly thereafter came to Philadelphia as professor of physiology.

It is overtly impossible to identify all the Lewis influences on the work of the Americans who had visited him or who had read his many papers. I doubt if any of them could escape from having been stimulated one way or another by him.

The Mechanism and Graphic Registration of the Heart Beat will remain the outstanding Lewis contribution
and will have a permanent place on the cardiologists' bookshelf. Lewis's concept of an independent clinical science will persist as an item for discussion and perhaps a stimulus for clinical research careers. While Sir Thomas will probably remain an enigmatic and forbidding figure to many, what he accomplished is incontrovertibly established. He was recognised as a leader and the details of his life as given by Pickering's "In Memoriam" portrays him as such.25 Americans can claim that one from their midst, Frank N Wilson, was perhaps his closest friend. I think that many Americans will have envied the succinctness and clarity of Lewis's writing and his penetrating insight into the existing cardiac problems of his day, and tolerated his errors. They may have been even prevented from undue theorising, remembering his admonition that "the purity of a science is to be judged by the paucity of recorded hypothesis".24 Currently we have heard another Englishman supporting such a viewpoint (Medawar) stating, "I cannot give any scientist of any age better advice than this. "The intensity of the conviction that a hypothesis is true has no bearing on whether it is true or not."25 In crepusculum, what cardiologist has not had his or her emotions and aspirations stirred up by the eloquent invocation for Heart . . .?"

"To know the pump, which Harvey revealed to us, more intimately; to understand more definitely the forces which regulate its action, and the mechanism by which it distributes the constant stream of blood, bathing and feeding the tissues of the body; to determine the causes which impair the circulatory functions; to see more clearly the means of prevention and the remedies which palliate, where injury to the heart or vessels is concerned; such is the scope of this Journal.26"

References

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