Experimental phonetics in Britain, 1890–1940

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A thesis submitted for the degree of
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This thesis provides the first critical history of British developments in phonetic science from 1890 to the beginning of the Second World War. It draws on both published and unpublished documentary evidence, and on original digital analyses of contemporary images, experimental data, and sound recordings.

Experimental phonetics had diverse origins embracing medicine, physics and philology. A survey of the nineteenth century background shows that by 1890 significant British contributions in all three fields could have furnished the makings of a native approach to phonetics as an experimental science, but they failed to come together for a variety of bureaucratic, professional and personal reasons. Experimental phonetics—an academic fashion as much as a scientific specialism—was instead imported from Germany and France, and it had little continuity with British antecedents.

The study details the earliest British phonetics laboratories, their personnel, equipment, and research programmes, providing the first extensive account of the UCL laboratory, and bringing to light a forgotten 1930s laboratory in Newcastle. The major methods of empirical investigation of the period are scrutinised, rehabilitating long-neglected British origins.

The early work of Daniel Jones is extensively re-evaluated, establishing his scientific credentials, and the career of Stephen Jones, the first academic in Britain to earn a salary as an experimental phonetician, receives detailed treatment.

New light is thrown on many neglected figures, including W. A. Aikin, E. R. Edwards, John G. McKendrick, and Wilfred Perrett, while a detailed investigation of the work of Sir Richard Paget reveals the astonishing accuracy of his auditory analyses.

The study concludes with an account of the career of Robert Curry, the first recognisably modern and professional speech scientist to emerge in Britain.

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Introduction

1.1 The motivation for the present study

The present work arose from the author's decision around 2008 to take on the task of conserving a range of neglected artefacts—including books, papers, photographs, gramophone records and film—which came to light as the Department of Phonetics and Linguistics at University College London (UCL) prepared to move from the premises it had occupied for approximately 90 years at 21 Gordon Square, WC1. Much of the Department's early history, and especially the life and career of its founder and most significant member, Daniel Jones (1881–1967), had already been comprehensively researched by Beverley Collins (1938–2014) and reported in a series of publications from the late 1980s onwards—notably Collins and Mees (1999). That research had been done with the full cooperation of the Department and College, and Collins was instrumental in ensuring that much of the previously scattered material which he gathered for his research was directed into the UCL Library, where it is now held as the Daniel Jones Papers.

Nevertheless, from about 2006 a series of staff retirements, and the consequent room clearances, together with preparations for the removal from Gordon Square to newly-refurbished premises at Chandler House, revealed many further items which could not simply be added to the existing archive, either because they did not concern Daniel Jones directly or because they took a form which UCL Special Collections could not accept (such as film or gramophone records). It was evidently desirable to direct these items to the most appropriate archival homes, and that in turn entailed that the items had first to be researched and documented at least to a level that could engage the attention of the curators of major collections who were petitioned to accept them. Early efforts focused on a large collection of gramophone records of speech, 362 of which were accessioned into the British Library Sound Archive as the UCL Phonetics Collection in the spring of 2008. At about the same time, investigations on a collection of 89 remarkable lantern slides showed them to be lost work of the Japanese phonetician Tsutomu Chiba (1883–1959), an early alumnus of the UCL department. In 2010, restoration and digitization of the oldest film which had been recovered (from the 1920s) yielded the only known footage of a phonetic kymograph in operation, and what was later discovered to be the earliest extensive filmed record of British Sign Language. The range of artefacts to be considered was further augmented in 2010 when Stevie Russell, then the Librarian of the UCL Speech and Language Library, rescued 25 fileboxes of forgotten phonetics and linguistics material (referred to below as the 'Tracts') which were on the point of being discarded from the UCL Library, and brought them to the author's attention. The potential value and interest of the Tracts was soon established when one of the boxes was found to contain a hitherto unknown manuscript by Henry Sweet (1845–1912).

As a body, the material which was collected together, and the initial results of the investigations which it prompted, indicated that there was scope for an extended study of the early 'experimental' or 'scientific' work of the UCL Department to complement the work already done by Collins and Mees (1999) on the chiefly 'descriptive' and 'linguistic' approach associated with Daniel Jones himself.

1.2 Aims

Given the starting point outlined above, one approach would have been to focus specifically on the narrative history of the UCL Phonetics Laboratory from its inception in 1912. A brief account along these lines is given in Ashby, Faulker & Fourcin (2012). But the present work attempts rather more than that, by sketching the background of previous experimental work in Britain, and by providing accounts of the life and work of an number of figures who are only peripherally associated with the UCL Department—for example, Wilfrid Perrett, E. W. Scripture, Sir Richard Paget, and Robert Curry. Developments outside the academic context—for example, in technology and in subjects allied to medicine—are also covered. The work aims, therefore, to contribute to the history of a field of inquiry rather than of a department within a specific academic institution.

Furthermore, rather than merely establishing the historical chronicle of events, and evaluating the significance of early experimental work from the (supposedly superior) critical viewpoint of present-day speech science, an attempt has instead been made to put the early work into its educational, social and cultural contexts—and only then consider its relevance for the present.

1.3 The subject and scope of this study

In the present study, 'experimental phonetics' will mean the ensemble of devices, practices, techniques, assumptions and research questions which became a fashionable movement within academic phonetics from approximately 1890, and which was perceived as being distinct from, and sometimes in opposition to, the 'practical' or 'linguistic' phonetics of Henry Sweet, Paul Passy or Eduard Sievers.

The thesis focuses on the period 1890–1940. The starting date is the point at which ‘experimental phonetics’—in the sense of a movement or faction within academic phonetics—rather abruptly became a force to be reckoned with. The most significant single factor in this was the publication in 1891 of the prize-winning doctoral dissertation of the abbé Rousselot (1846–1924), which was quickly hailed as ‘epoch-making’ and seen as providing a new paradigm for phonetic research. For example, in a list of recent literature in his chapter on the speech organs, Streitberg (1896) describes Rousselot’s dissertation as ‘Muster experimenteller Phonetik—epochemachend’ (a paradigm for experimental phonetics—epoch-making). A compilation of numerous similar assessments from contemporary reviews of Rousselot is given by Breymann (1897: 54). As an example, the first in the list is ‘Bedeutend’ (momentous).

The finishing point in 1940 is also sharply defined. It corresponds both with the end of the career of Stephen Jones (the first person in Britain to hold a salaried post as an experimental phonetician), and the abrupt and prolonged interruption to academic phonetics in Britain brought by the Second World War. The third International Congress of Phonetic Sciences, held in Ghent in the summer of 1938, must have taken place in an atmosphere already charged with the threat of violent upheaval; the congresses were not to resume until 1961, by which time a greatly changed population of researchers was in place, and papers delivered to the congress were already reporting the application of digital computers to speech analysis.

1.4 Previous work

This is a work of history, and more specifically history of science. It has therefore been appropriate to some take notice of the extensive literature in historiography, and the historiography of science and technology in particular.

1.4.1 Meanings of 'history'

From the postmodernist critique of history (Jenkins 2003) one very important distinction has been kept in mind throughout: the distinction between (i) 'history' in the sense of all the events of the past, and (ii) 'history' in the sense of a written history (i.e., literally a 'story') which interprets a selection of past events in a particular way—a way that will be unavoidably influenced by the standpoint and purposes of the historian. The first kind of history is to be found in contemporary documents, surviving artefacts, and archives. In a sense, these provide the raw materials of history. They can be augmented by 'discoveries' when lost materials are brought to light, or when previously known materials are subjected to new forensic analysis, though they will always remain incomplete, since a total record of the past is unattainable. It is clear, too, that no accumulation of raw materials, however extensive, can amount to 'a history' of anything (in the second sense). Indeed, as the bulk of raw materials grows, so the need becomes ever greater for an interpretative narrative to order them and render them intelligible. Or more accurately, the scope becomes greater for interpretative *narratives* (plural) since the raw materials certainly do not lead inevitably to any one interpretation of them.

History in the second sense is always a matter of interpretation, evaluation, and criticism. It is looking for themes, influences, causes and effects, which are evidenced only indirectly in the raw materials—not to mention attitudes and motivations which participants did not always acknowledge, and of which they may well have been unconscious. There are no 'discoveries' to be made in this second kind of history, though of course there can be novel conjectures and interpretations.

Just as no amount of raw material adds up to 'a history', it is equally plain that histories in the second sense provide only a selective and incomplete picture of past figures, events, artefacts, etc.

Overall, the present work aims to make additions to history in both senses. In relation to the first sense, many new or neglected artefacts are identified, and there are numerous additions to the chronicle of events and the bibliographical record. In relation to the second sense of 'history' numerous new interpretations are brought forward.

The simple, but often overlooked, distinction between senses of the word 'history' is also a useful one to bear in mind when making critical evaluations of earlier contributions to the history of phonetics (or closely neighbouring fields). For instance, the extremely valuable compilations of Panconcelli-Calzia on the history of phonetics (1940; 1941) mentioned elsewhere in this study evidently wish to be seen as impartial catalogues or annals, and thus as contributions to history in its first sense. The title *Quellenatlas zur Geschichte der Phonetik* (1940) may be translated as something like 'Catalogue of sources for the history of phonetic science', while *Geschichtszahlen der Phonetik* (1941) may perhaps be rendered as 'Annals of phonetic science'. But the extent to which in reality these works do merely present 'facts', and avoid bias and evaluation, is debateable, as will be argued below.

1.4.2 History and sociology of science

No work on the history of a particular science can afford not to take notice of at least some major items among the extensive literature on the history of science in general, or ignore the widely accepted belief that the history of science and the philosophy of science are intertwined. At the same time, the field is so large that a non-specialist can know only a few representative titles. The most significant single contribution of modern times is Kuhn (1962), around which a large and continuing stream of discussion and criticism has grown. Many ideas from Kuhn's analysis (concepts such as 'paradigm', 'normal science', and 'puzzle'), as well as his insistence that science be studied in its social context, have indirectly influenced the approach taken in the present study, even where his terminology is not employed. Put into Kuhnian terms, the thesis effectively claims that experimental phonetics entered a period of 'normal science' from the late 1890s. Similarly, the period (1890–1940) chosen for this study might be viewed as owing its cohesion to the dominance of a particular 'paradigm' over those five decades.

Much has also been learned from the writings of Joseph Agassi, particularly Agassi (2008) which incorporates and extends his earlier (1963) work *Towards a historiography of science*. (It is acknowledged that Agassi, a harsh critic of Kuhn, might find it contradictory that inspiration has been drawn from both authors). Agassi's works on science and society (1981) and on science and culture (2003) have also been consulted. The suggestion of an implicit 'research project' for experimental phonetics from Rousselot onwards, and the perspectives adopted on citation practices, vanity publishing, pure vs. applied vs. basic research, and evaluation (peer review, competitive funding, etc.), all draw on ideas from Agassi.

As for the sociology of science, Kuhn and Agassi have been supplemented by Merton (1973), and simple background on sociology itself taken from Bruce (2000). For example, when the discussion touches on processes of professionalization and bureaucratization it makes straightforward use of classic ideas from Weber (as interpreted by Bruce 2000).

1.4.3 History of technology

Overall, what all of the sources and influences identified above have in common is that they encourage the study of scientific practice rather than of scientific method. In a similar way, the treatment of the history of speech research technology owes something to Edgerton (2006) in that instead of a list of 'pioneers' and inventions—designed, in Edgerton's phrase, for 'boys of all ages' (2006: ix)—attempts have been made to include information on the actual use to which technology was put, considering such factors as its manufacture, cost, commercial distribution, ease of operation, maintenance, longevity, and even the irrational loyalties which researchers sometimes evinced for particular types and brands.

1.4.4 'Whig' histories

A notorious type of failure in the writing of histories (in the second sense) is the production of so-called 'Whig histories'—that is, accounts which portray the past in terms of progress towards the (supposedly enlightened and superior) present. The term is due to Butterfield (1931), and originates in relation to political history, with the course of history presented (by 'Whig' historians) as continual 'progress' towards enlightenment, liberty and democracy. But since the notion of scientific progress is even more widespread, and more deeply and uncritically embedded, than any notions of political progress, and since histories of particular aspects of science are often written by scientists (or ex-scientists) rather than by professional historians who are alert to the pitfall, ‘Whig’ histories of science (and of technology and medicine) have abounded.

The need to avoid Whig-historical thinking has become a commonplace of works on the history of science, many recent examples of which open with sections of greater or lesser length emphasising aspects of that very point. Morus (2005: 5) explicitly refers to the dangers of ‘Whig history’ and claims that as a result of avoiding those dangers ‘the history of science has changed dramatically over the last quarter century’. Fara (2009: xiii) distances herself from ‘traditional books about science’s past’ and sets out to show ‘how science belongs to the real world of war, politics and business’.

But how far either of those two writers—or recent historians of science generally—have in fact totally escaped the danger, is open to debate. As Morus notes (2005: 5), the historian’s desire to present a ‘big picture’ and paint with a ‘broad brush’ is always pushing in the other direction. Butterfield himself went on to produce a history of modern science (1949/1957), which presumably exemplifies what he must have seen as a non-Whig approach (and which certainly deserves to be better known, in many respects anticipating Kuhn). But in its title *The origins of modern science* the very word ‘origins’ sounds an alarm to one on the lookout for signs of Whiggism.

1.4.5 Historiography of linguistics

Histories of aspects of linguistic science have certainly not been immune to Whiggism, especially since they are almost always produced by linguists who may have axes of their own to grind. For instance, one of the characteristic features of Whig histories as identified by Butterfield is abridgement: in the interests of ‘the big picture’, what appear from the modern viewpoint to have been ‘blind alleys’ of investigation are ignored, and thinkers who failed to establish a ‘school’ which has a useful relevance to current controversies tend to be passed over, regardless of how prominent they may have been in their own time. So, for example, although a study of Gardiner (1932) had apparently figured in the original doctoral dissertation on which Langendoen’s *The London school of linguistics* (1968) is based, it was dropped from the published version (1968: ix). A. H. Gardiner (1879–1963) may still be remembered as an Egyptologist, but his work in general linguistics has fallen into oblivion.

Meanwhile Anderson’s *Phonology in the twentieth century* (1985) furnishes a striking illustration of the point made above that histories (in the second sense) may be remarkably poor sources of information about the past: with its focus on ‘rules’ and ‘representations’ the work gives no hint of the impact of sound spectrography or the development of the acoustic theory of speech production. Neither ‘spectrum’ nor ‘formant’ are to be found in the index; the name of Gunnar Fant is mentioned only because he was a co-author of Roman Jakobson, and Kenneth Stevens similarly of Morris Halle.

1.4.6 Historiography of phonetics

The history of phonetics is still a relatively undeveloped field, while the historiography of phonetics (that is, the analytical and critical study of histories of phonetics) can barely be said to exist at all. Koerner’s ‘Historiography of phonetics: The state of the art’ (1993) is of great value, but occupies a mere seven pages. The ‘comprehensive’ combined bibliography which Koerner provides for the two fields together (which he terms 'Histor(iograph)y of phonetics') runs to just 16 pages more (Panconcelli-Calzia 1994: xxiii–xxxviii). Of these already small domains only tiny fractions deal specifically with experimental phonetics.

1.4.7 General principles for work in the history of linguistics and phonetics

The first two thirds of the twentieth century provide some excellent models for work in the history of phonetics in the relevant writings of J. R. Firth (1890–1960) and David Abercrombie (1909–1992) (e.g., Firth 1946; Abercrombie 1965, 1991. See also Kohler *et al*. 1981). In their close attention to original sources and efforts to understand the past on its own terms Firth and Abercrombie steer well clear of any Whig tendencies. Neither explicitly discusses the methods appropriate to the historian of linguistics, though this does not of course mean that they had not thought about the issues—and it should be remembered that Firth began as a historian rather than a linguist (Plug 2008).

The last third of the twentieth century saw a general growth of work in the history of linguistics—particularly, but not exclusively, in Britain. The subject earned recognition as an area of study in its own right, and began to be taught in universities. A textbook appeared (Robins 1967); journals were founded (*Historiographia Linguistica* began in 1974) and professional associations were formed (in Britain, the Henry Sweet Society was started in 1984). But none of these developments particularly favoured work on the history of phonetics—if anything, the reverse, since the emphasis was commonly on the history of 'theory'. The collocation 'linguistic thought' occurs repeatedly in the titles of books and articles produced during this period (e.g., Harris

1988). Two significant British historians of phonetics were certainly active in this phase: A. J. Kemp (1927–2012) and M. K. C. MacMahon (b. 1943), but they are not really products of it: their work connects rather with that of Firth and Abercrombie. Numerous items from their output are cited at relevant points in this thesis.

At the turn of the century, another textbook linked with this phase of work (Law 2003) concludes with advice on 'Becoming a historian of linguistics' (Law 2003: 276–283). This takes the form of very general principles concerning such matters as objectivity, the cultivation of close observation, and attention to all manner of ethical considerations. While it is hoped that the present thesis largely lives up to these worthy precepts, it has to be said that they do not constitute anything like a working methodology. The specific questions arising in the attempt to produce a history of phonetics in the digital age have had to be answered by trial and error in the writing of the thesis.

1.4.8 Directly relevant previous work

There is currently no extended study of the history of experimental phonetics in Britain, and very little published detail concerning individual contributors and their work. Two major sources on the history of experimental phonetics are the monographs of Panconcelli-Calzia (1940, 1941), which were republished in 1994 with an introductory chapter by Koerner, but the only British figure whom Panconcelli-Calzia mentions from the period after 1890 is Harold Atkinson. More recently, Tillmann (2006) gives an extremely valuable survey of the development of the field. Again, the focus is very much on work done in continental Europe. For the period under consideration, he too includes Atkinson and adds only one more British name, R. J. Lloyd.

The present work takes up the story in detail from 1890, but the lack of adequate previous studies applies equally to the earlier part of the nineteenth century, so that to set the scene it is necessary to begin by outlining some aspects of the whole nineteenth century background to phonetics in Britain. The author has written elsewhere that 'Modern phonetics began to take shape around 1850 at the three-way intersection of biomedical science (at first, mainly physiology), physical science (in the early days, chiefly acoustics) and linguistic science (at the time dominated by a comparative-historical paradigm' (Ashby & Ashby 2013: 198). As a consequence, any treatment of the background must deal with three different components. The relevant developments in British mathematics and physics took place earliest: the application of mathematics in the search for general models underlying the operation of apparently complex mechanisms and systems was in full swing by the 1820s, particularly in the university of Cambridge (Morus 2009: 33–35). These developments are therefore dealt with first in this sketch. Treatments of medical science and philology follow in sections 1.6 and 1.7.

1.5 Background: physics, mathematics and engineering

1.5.1 Robert Willis (1800–1875)

Few contributions to phonetic science can have been so often cited as Robert Willis's paper 'On the vowel sounds and on reed organ-pipes', though a very high proportion of the mentions appear to be derivative and superficial. The work itself has hitherto been subjected to surprisingly little critical evaluation or quantitative verification. A comprehensive account of Willis's life and career is given by Buchanan (2013). The account here adds a more technical assessment of his early contributions on speech, and traces the way they are reflected in later writers.

Willis presented his investigations on speech to the Cambridge Philosophical Society at three meetings (24 November 1828, 16 March 1829, and 18 May 1829). The Philosophical Society had been founded in 1819, and is described by Hall (1969: 1) as 'the first positive step taken in modern times towards the emergence of Cambridge University as a great centre for teaching and research in science'.

Material from the first two presentations formed 'On vowel sounds, and on reed organ-pipes', which appeared in volume three of the *Transactions* of the society (1830), while the third presentation became 'On the mechanism of the larynx', published in volume four (1833). Willis cites and acknowledges the previous work of Kratzenstein (1781)[1] and Kempelen (1791) directed at synthesizing vowels, and states (1830: 237) that he initially set out to replicate Kempelen's results using a funnel-shaped cavity, controlling its resonances by positioning a hand over or within the opening. For excitation he used a 'free' (i.e., non-beating) reed of the type used by Kratzenstein. He says that he succeeded in this replication, finding in addition that a sliding board over the opening could be used instead of the hand, and that use of a shallower funnel extended the range of qualities that could be obtained (1830: 238). But Willis expresses dissatisfaction with the work of both Kratzenstein and Kempelen on the grounds that neither had 'succeeded in deducing any general principles'. The resonators of Kratzenstein, for example, are 'of most grotesque and complicated figure, for which no reason is offered' (1830: 232).

Willis decided instead to explore the simplest possible resonator—a uniform circular tube (1.3 inches internal diameter and 1 ft 6 inches long), excited by a free reed fitted in a sliding piston so that the length of the resonator could be varied. Further sections of tube in a standard length of 1 ft 6 inches could be added by means of a socket fitting on each, up to a total possible length of 12 feet.

Willis reports that as the length of the tube is gradually increased (starting from zero) a succession of vowels is heard, which he represents 'I E A O U'. Beyond this, the percept becomes indeterminate, until at a certain length the vowels are heard again, in a sequence 'U O A E I…I E A O U' (see Figure 1.1).

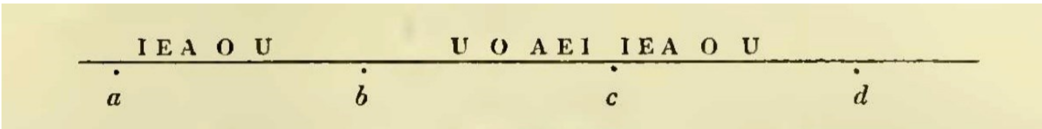

Figure 1.1 Willis's schematic representation of the relationship between vowel quality and resonator tube length (1830: 239).

He claims that the sequence repeats at intervals as the pipe is made longer and longer, though the vowels become 'less distinct in each successive cycle'. He says that the points *a b c*, etc., are spaced apart by half a wavelength of the reed frequency, and are thus at varying positions, depending on the reed employed, but the additional distances from *a*, *b* etc., to the vowel positions are fixed absolute values.

This enables him to give a table in which vowels are specified in terms of tube length—the range of vowels being now subdivided into specific English vowel qualities represented by keywords (Figure 1.2).

Table I.

I	See	.38 ?	g^{v}
E	Pet	.6	c^{v}
	Pay	1	d^{iv}
A	Paa	1.8	f'''
	Part	2.2	$d''b$
A^{0}	Paw	3.05	g''
	Nought	3.8	$e''b$
O	No	4.7	c''
U	But	Indefinite	
	Boot		

Figure 1.2 Willis's table of vowel qualities (1830: 243). The third column shows resonator tube length (in inches).

Willis comments:

> I have found this table as correct a general standard as 1 could well expect; for vowels, it must be considered, are not definite sounds, like the different harmonics of a note, but on the contrary glide into each other by almost imperceptible gradations, so that it becomes extremely difficult to find the exact length of pipe belonging to each, confused as we are by the difference of quality between the artificial and natural vowels. Future experiments, in more able hands than mine will, I trust, determine this matter with greater accuracy, and I should not even despair of their eventually furnishing philologists with a correct measure for the shades of difference in the pronunciation of the vowels by different nations.

The vowels produced by Willis were evidently not easy to identify when heard singly:

> ln repeating experiments of this kind, it must always be kept in mind, that the difference between the vowels, depends entirely upon contrast, and that they are therefore best distinguished by quick transitions from one to the other, and by not dwelling for any length of time upon any one of them.
>
> (1830: 234).

The only accounts of anything approaching a replication of Willis's experiments seem to be given by Paget (e.g., 1930: 17). Paget used a variable resonator belonging to Daniel Jones, which had a marginally smaller internal diameter but otherwise resembled the apparatus of Willis in all important respects. Paget reports being able to produce only a limited range of vowels, and of these the only one which corresponds fairly closely with the findings of Willis was an [ɑ] type vowel at about 2.25 inches.[2] There seems to have been no report of an attempt to replicate Willis's findings with increasingly longer tubes, and the mechanism by which a cycle of vowels could be repeated at intervals in the way Willis describes remains very obscure.

Calculations suggest that a uniform tube can only produce a reasonably vowel-like spectrum over a restricted range of lengths (Figure 1.3). As the tube is made substantially longer than the human vocal tract, more and more low-frequency resonances are crowded together. By the time the tube reaches 1 foot (approximately 305 mm) in length—only two-thirds of the way along the first section of Willis's extending tube—all the first four formants are already below 2 kHz.

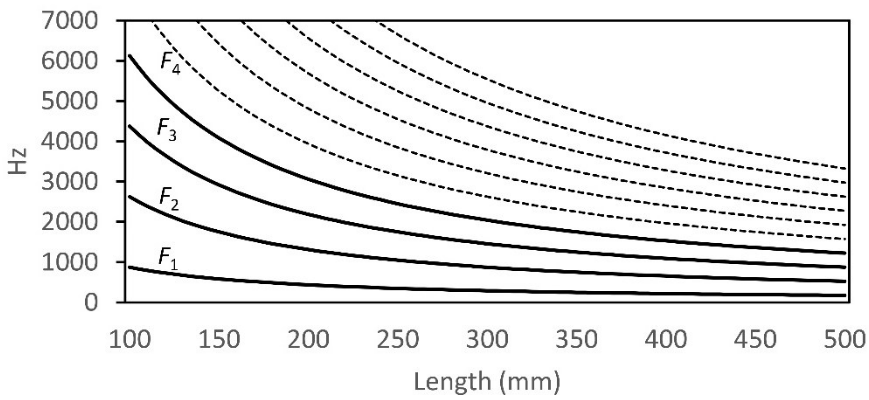

Figure 1.3 Theoretical standing-wave resonances of a uniform tube closed at one end for a range of lengths (calculated by the author). The velocity of sound has been taken as 350 m/s.

It seems likely that at short tube lengths the attention of Willis—and, after suitable priming, that of his listeners—was caught by a single component which probably corresponded fairly well to F_2 of an open vowel and could be swept suggestively in the general direction of [i] or [u] when the pipe length was varied, although the frequencies actually attained may not have been very accurate, and the spectrum as a whole must have been highly unnatural.

In many ways, Willis's lesser-known paper 'On the mechanism of the larynx' (1833) is even more remarkable than his work on vowels. It contains a detailed mechanical analysis of the actions of the various larynx muscles—which, it has to be remembered, had to be inferred from their functions, in the absence of direct observation in the living subject.

Weir and Mudry (2013: 131) give Willis a prominent place among the earliest pioneers of research into laryngeal physiology. They say of Willis ('a Cambridge mechanical engineer') that '[he]…discovered that the motion of the arytenoid cartilage was one of rotation. He thus defined the actions of the individual muscles of the larynx almost as they are understood today'.

Willis gives detailed descriptions of various working models of larynx function, and describes generating tones of controllable frequency from his 2-ligament analogues of the larynx. Disappointingly, there is no mention of whether he tried exciting his vowel tubes with the larynx model rather than a simple free reed.

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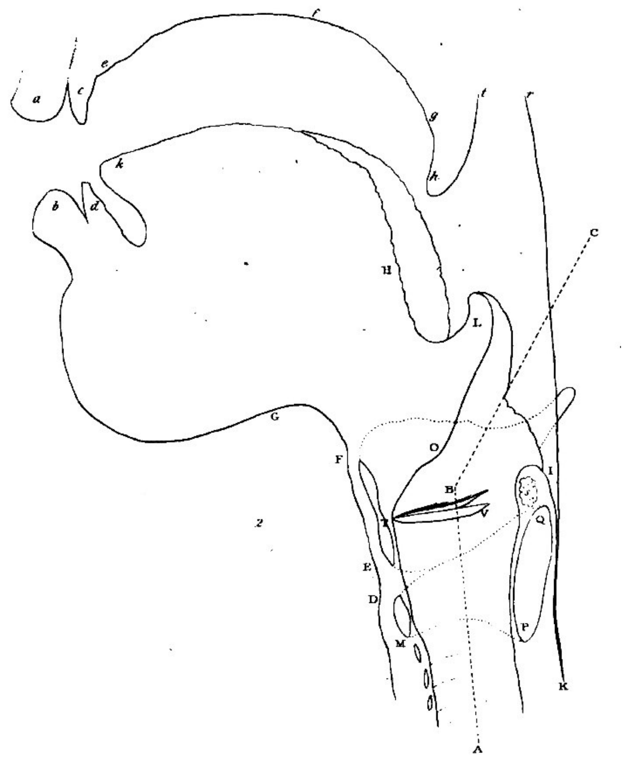


Figure 1.4 Mid-sagittal section of the vocal tract as drawn by Willis (1833: plate 22).

The paper is remarkable on other grounds too. It contains a wonderfully clear exposition of what we would today call the source-filter theory (1833: 324–325), and a superb stylised mid-sagittal section of the vocal tract, which must be among the earliest from any source (Figure 1.4). This, and the equally accurate and impressive larynx figures, can only have been drawn from dissections. Buchanan (2013: 47) infers that Willis ‘undertook his own anatomical studies’, but fails to make the obvious conjecture that he must surely have worked with William ‘Bone’ Clark (1788–1869) the Professor of Anatomy at Cambridge since 1817, a fellow member of the Cambridge Philosophical Society—and in fact his own brother-in-law.[3] That the two did work together is confirmed by a popular news item in a colonial publication: ‘He has in particular been engaged, with the help of Dr. Clark, the anatomical professor, in researches on the anatomy of the organs of articulation, and his dissections carried on for this purpose have led him to very remarkable results' (Anon 1833: 50). The article, evidently written with a good understanding of what Willis was attempting, and with some insider knowledge, concludes by hoping that Willis will proceed from vowels 'to construct a perfect talking machine' and that this will be coupled with its 'elder brother', by which is meant Babbage's calculating machine. It is extraordinary that a transient journalistic piece published in a tiny British colony in 1833 should have foreseen the possibility of using a speech synthesizer as an output device for a computer—something not realized in practice until about 130 years later.

Willis gave a course of lectures on 'Sound' at the Royal Institution in 1832, and 'Vowel Quality' and 'Human Larynx' are among the topics mentioned in the outline syllabus,[4] but he appears to have published no further work on speech after the early 1830s. The major part of his life's work was devoted to research into architectural history.

1.5.2 Sir John Herschel (1792–1871)

The earliest citation of Willis's work on vowels was published in the same year as the work itself (Herschel 1830). The authors must have consulted while their two MSS were in preparation, since Willis (1830) also has a reference to Herschel's article.[5]

Sir John Frederick William Herschel was one of the ablest and most celebrated scientists of the nineteenth century (Crowe 2004). In 1813 he was elected to membership of the Royal Society, was placed Senior Wrangler, and elected to a Fellowship at St John's College. In Crowe's words,

> During the 1820s John Herschel emerged as Britain's first modern physical scientist. Possessing not only exceptional mathematical expertise but also

> extensive knowledge of and experimental dexterity in such areas as chemistry, mechanics, optical science, acoustics, and electricity, not to mention his unrivalled command of matters astronomical, he was in many ways the model for future British physical scientists.

In the late 1820s, Herschel produced three ‘articles’ for the *Encyclopædia Metropolitana*. These were on ‘Physical astronomy’, ‘Light’ and ‘Sound’, and each was in fact an original book-length treatise rather than an ‘article’ in the modern sense. The one on ‘Sound’ is dated February 3 1830, and the volume of the *Encyclopædia* containing it was published later that year. Section 3 of the treatise (pp. 815–819), entitled ‘Of the Voice’, deals with speech, and a large proportion of this is a detailed and enthusiastically approving account of Willis’s experiments and findings. He describes Willis’s work as ‘curious and novel’, and ‘highly interesting’. Though Herschel was mainly resident in London by this date, it is not impossible that he might have been present at one or more of Willis’s original presentations in 1828–1829, but in any case he would have heard about the work very quickly. He was a Fellow of the Cambridge Philosophical Society from its inception and contributed no fewer than four papers to volume 1 of its *Transactions*, published in 1822.

Apart from the account of Willis, Herschel devotes much space to what he calls ‘an enumeration of our English elementary sounds’ (1830: 817)—that is, effectively an attempt to establish the phonemic inventory—and he arranges the sounds he identifies into a ‘scale’. Vowels, for instance, are in a numbered list 1–13, running from a close back rounded quality (*rude*, *wound*) to close front spread quality (*peep*, *leave*), and consonants are grouped into classes (sharp, flat, neutral and compound). At the conclusion of this list, he comments:

> We have here a scale of 13 simple vowels and 21 simple consonants, 33 in all, which are the fewest letters with which it is possible to write English. But on the other hand, with the addition of two or three more vowels, and as many consonants, making about 40 characters in all, every known language might probably be effectually reduced to writing, so as to preserve an exact correspondence between the writing and pronunciation; which would be one of the most valuable acquisitions not only to philologists but to mankind, facilitating the intercourse between nations, and laying the foundation of the first step towards a universal language, one of the great desiderata at which mankind ought to aim by common consent.
>
> (1830: 818)

Herschel thus seems to show a clear grasp of the phoneme concept, and to foresee the possibility of a universal phonetic alphabet.

1.5.3 Sir Charles Wheatstone (1802–1875)

Charles Wheatstone was one of the leading electrical engineers of the mid nineteenth century (Bowers 2001). He was self-taught, lacking the formal mathematical training of Willis or Herschel, but in 1834 he became professor of experimental philosophy at King's College, London. He was an energetic and inventive experimenter. Ellis reports that he 'heard Willis's experiments repeated by Wheatstone' (Helmholtz 1885: 117), and in the mid-1830s Wheatstone also constructed an improved version of Kempelen's talking machine, which was still in working order when tried out nearly a century later by Paget (1930: 18–19). Many of his early experiments were on aspects of acoustics, though his only published work specifically on speech seems to be an article published in *The London and Westminster Review* in October 1837 but more easily accessible in Wheatstone's collected papers (Wheatstone 1879: 348–367). Wheatstone's article is untitled, and is presented in the guise of a review of Willis (1830).[6] Wheatstone says that his purpose is to give an account of 'the various attempts which have been made to imitate the articulations of speech', and he speaks of 'the united labours of the philosopher and the mechanician'—that is, in modern terms, the physicist and the engineer.

The first four pages of Wheatstone are devoted to a historical survey of deceptions in the form of talking heads, automata, etc. He then passes to the well-known story of the 1779 prize question from the Imperial Academy of St Petersburg concerning the nature of the vowel sounds, and gives an account of Kratzenstein's winning solution, and of Kempelen's researches at about the same time. This part of Wheatstone's paper corresponds to an expansion of material covered in the first two pages of Willis (1830).

Wheatstone then moves on to a general classification of vowel sounds and 'articulations' generally. He uses a two-dimensional vowel classification scheme (the two dimensions being tongue height and lip rounding), but this is confusingly presented in the form of a table in which Wheatstone's own scheme is mixed with Willis's measurements of tube length.

He then passes to a summary of Willis (1830), which is followed (pp. 358–360) by his discussion of 'multiple resonance'. The discussion is somewhat obscure, since 'multiple' seems to have at least two senses. On the one hand, he is referring to the fact that certain resonators will respond not only at their lowest resonant frequency, but at integral multiples of that frequency[7], and on the other hand he is referring to sound-sources (especially vibrating reeds) which are rich in harmonics. These can be picked out and heard as separate notes when reinforced by suitable resonators (including the mouth, suitably adjusted). So resonators have 'multiple' resonances (formants, in modern terms), but certain sound sources have ‘multiple’ components (harmonics).

He concludes this section of his discussion with ‘Thus it is evident that vowel qualities and multiple resonances are different forms of the same phenomena’ (p. 360). It is very hard to be certain exactly what he means by this, but taking into account the whole context, it can perhaps be paraphrased as the claim that ‘vowel qualities’ are a recognisable subset of those sounds which consist of multiple harmonics of a fundamental, sounding together.

Wheatstone then passes to a classification scheme for consonant sounds, and dwells at some length on the voiced-voiceless distinction (he employs the terms ‘sonant’ and ‘mute’). He points out that the provision of a single control selecting ‘voiceless’ or ‘vocal’ would halve the number of consonant keys required on a talking machine. The last three pages of the article return to the construction and control of talking machines resembling that of Kempelen.

Overall, it has to be said that Wheatstone’s article is uncertain in its aims, highly derivative in some respects, and in many places obscure. It appears, however (pp. 359–360) that Wheatstone tried the experiment of applying various adjustable acoustic resonators while he sustained a range of vowels and other speech sounds, and he thus anticipated in all essentials the analysis methods used by Helmholtz. What is regrettable is that Wheatstone gives no systematic account of his observations, and (apparently) made no attempt to quantify them.

1.5.4 George Biddell Airy (1801–1892)

Airy was another brilliant Cambridge mathematician, and an almost exact contemporary of Willis. He was Senior Wrangler in 1823, a Fellow of Trinity from 1824, and in quick succession Lucasian Professor of Mathematics and then Plumian Professor of Astronomy. He became Astronomer Royal in 1835 (Chapman 2004).

Airy was a prolific contributor to the *Transactions* of the Cambridge Philosophical Society around the time of Willis's work. Volume 3, which contains Willis's paper on vowels, has no fewer than five contributions from Airy, one of which, 'On certain conditions under which a perpetual motion is possible' (Vol 3: 369–372), is explicitly a mathematical analysis of an idea prompted by Willis's paper on the larynx: 'For this idea I am indebted to the admirable account of the organs of voice given by Mr Willis.'[8] Airy himself tells us that he was present at the original presentations made by Willis (1871: 261).

In 1868 Airy published a book with the title *On sound and atmospheric vibrations, with the mathematical elements of music. Designed for the use of students of the university.* A second edition appeared in 1871. The section devoted to speech (pp. 255–266) relies very heavily on Willis, and represents Willis as having solved a number of fundamental questions once and for all. For example, in his treatment of the larynx, Airy says:

> A tolerably clear idea was formed by anatomists, many years ago, of the nature and action of the organization in the human throat by which musical notes are produced. Several points of explanation, however, were wanting; these were supplied, perhaps finally, by Professor Willis, in a paper "On the Mechanism of the Larynx," published in the fourth volume of the Transactions of the Cambridge Philosophical Society, which may be regarded as a model of scientific and anatomical inquiry.
>
> (1871: 255)

It is a similar story with respect to vowels: ‘The great step of experimental explanation was made by Professor Willis in the Cambridge Transactions, Vol. III.; and nothing of importance has been added, till within a short time’ (1871: 260–261). What has been ‘added’ is the work of Helmholtz, treated in a short paragraph. Airy may be among the earliest to note an apparent incompatibility between the views of Willis and Helmholtz:

> But the distinct conclusion at which Professor Willis arrived, that, with different reed-notes, the difference in the linear measure of pipes required to produce different vowel-sounds must always be the same, seems to forbid the admission of similar mixtures of the upper harmonics with the fundamental tone as in all cases explaining the vowel-sounds.[9]
>
> (1871: 265)

Airy concludes his section on speech with the claim that an understanding of larynx vibration and of vowel production (the two topics selected by Willis) between them largely exhaust the subject: ‘This appears to be all that is required’ (1871: 265). He implies that there is nothing of importance to say about consonants: ‘On the production of consonants, we have little to remark.’ They are, he says, merely ways of beginning or ending vowels. ‘All these different modifications give rise to different consonants; but they do not appear to involve any distinct principle which requires attention here’ (1871: 266). In other words, speech does not present to Airy any worthwhile research questions other than the vibration of the larynx and the production of vowels.

1.5.5 Richard Potter (1799–1886)

In 1873 the Cambridge Philosophical Society heard another paper on vowels, ‘On the English sounds of the vowel-letters of the alphabet, on their production by instruments, and on the natural musical sequence of the vowel-sounds’ (Potter 1876). It was given by Richard Potter, who in his day had been Sixth Wrangler, and had later qualified in medicine. By 1873 he was retired from the chair of natural philosophy he had occupied at UCL and living in Cambridge (Cantor 2004). Interestingly, the paper mentions his experience as a university teacher (evidently the acoustics of vowels had long been on the syllabus), and confirms some of the limitations of Willis's apparatus:

> The author of the paper having had to lecture through many years on acoustics in the general course of experimental natural philosophy, using, amongst other acoustical apparatus, Kempelen's funnel-shaped instrument and Professor Willis's sliding tubes producing the vowel-sounds, found the sound of the English vowel I (i) not to be given by them.
>
> (1876: 306)

Potter's own method of producing vowels involved free reeds attached to openings in hollow rubber spheres, which were provided with an orifice to represent the lip opening. By deforming the rubber spheres appropriately, a whole range of vowels—including the elusive [i]—could, he claims, be produced. But the paper contains no diagram[10] or measurements, and the spheres, deformed in arbitrary and unquantified ways, are really a backwards step towards the 'grotesque and complicated' resonators of Kratzenstein.

1.5.6 James Clerk Maxwell (1831–1879)

The discussion which followed Potter's presentation was minuted, and it is extremely interesting to note that those asking questions included James Clerk Maxwell, the first Cavendish Professor of Physics. Maxwell's most celebrated work, the *Treatise on electricity and magnetism*, was published that same year (Maxwell 1873). 'Professor MAXWELL thought that these experiments could hardly be connected with those of Helmholz [*sic*][11] and Donders, as the vowel-sounds differed in different nations' (Potter 1876: 308). Two years later, Maxwell chose the topic 'The Telephone' for his

1878 Rede lecture (Maxwell 1890: 742–755) in the course of which he reveals that he was familiar with Visible Speech and the other work of Alexander Melville Bell (1819–1905), and knows of Ellis ('Mr Alexander J. Ellis, author of "The Essentials of Phonetics," a gentleman who has studied the whole theory of speech acoustically, philologically, and historically').

1.6 Background: medicine and allied areas

Contributions to nineteenth-century phonetics from within British medical science had got off to a very patchy start (MacMahon 1984b).[12] In Germany, development of phonetic science in the 1850s and 1860s owed a great deal to the direct involvement of physiologists, particularly Ernst Wilhelm von Brücke (1819–1892) and Carl Ludwig Merkel (1812–1876) (Kohler 1981). In Britain, by contrast, though physiology itself certainly flourished (O'Connor 1988; 1991),[13] no British physiologists—with the tardy exception of John Gray McKendrick (1841–1926), who is considered in Chapter 3—seem to have taken an interest in speech.

1.6.1 Laryngoscopy and laryngology

As MacMahon (1984b) notes, the medically-oriented strand of work on phonetics in Britain has always had a particular emphasis on the larynx. Laryngology developed strongly in Britain, particularly under the leadership of Morell Mackenzie (1837–1892), who founded a Hospital for Diseases of the Throat in London in 1865—the first of its kind in the world (Weir and Mudry 2013: 152). The key to the growth of the medical specialism of laryngology was the ability to view the larynx—layngoscopy. Morell Mackenzie had learned this in Budapest in 1859 from Johann N. Czermak[14]

(1828–1873), who in turn had been inspired by the account in a paper by García, which was read to the Royal Society in London in 1855 (García 1855).

Though Manuel Patricio Rodríguez García (1805-1906) was born in Spain, and then studied and worked in France, it is perhaps not unreasonable to claim him as effectively British (he was included in the *Dictionary of National Biography*, for example). He moved to London in 1848 and was appointed a professor of singing at the Royal Academy of Music. He lived and worked in London for the rest of his long life, and died there at the age of 101. García is commonly credited with the 'invention' of the laryngoscope, though by his own account it is not an invention at all, but 'a simple idea'. There are numerous precedents anyway for the use of a mirror in attempts to observe the larynx (Jahn & Blitzer 1996), though García seems to have been the first to make the technique work effectively, and was almost certainly the first person to observe his own larynx, and to apply the technique to the singing voice.

In his 1854 paper, he wrote convincingly and elegantly, in English,[15] about the simple observations he had been able to make. But remarkably the paper contains no illustrations, either of the instrument or of the larynx as revealed by it. García seems to have been content with his initial findings and to have done nothing further with the technique. Subsequent development of the technique, and exploration of its applications, was done by Czermak (Jankowsky 1996).

1.6.2 John Syer Bristowe (1827–1895)

Though only part of the knowledge was home-grown, by the 1870s the medical (and allied) professions of the English-speaking world,[16] display a good understanding of the general principles of speech production, including (effectively) what we would now call the source-filter theory, and at least some medical figures became very interested in language and speech, treating them as entirely legitimate fields of medical enquiry.

An example is the work of J. S. Bristowe. He was one of the most versatile and prominent figures in Victorian medicine, making meticulously-researched and critically-independent contributions to fields as diverse as epidemiology and neurology (Bynum 1994: 137–141; Pennington 2004). His textbook *A treatise on the theory and practice of medicine* went through seven editions between 1870 and 1890. Nevertheless, at the height of his fame, he included speech among his interests. He not only read the enormously popular Max Müller (1864), but also a range of sources at least as wide as Müller had himself consulted, citing 'Mr A. J. Ellis' and his 'various publications on phonetics'[17] as early as 1870 (Bristowe 1870a: 154). This was no dilettante interest, like the art and poetry he had dabbled in as a young man, but a central research concern he maintained over several decades. Invited to give the prestigious Lumleian lectures in 1879, he chose the subject 'The pathological relations of voice and speech'. They were published in parts in *The British Medical Journal* and then in book form (1880). Out of the three lectures, the second is concerned with motor control and the third with cerebral localisation—both predictably 'medical' topics. But a long section of the first lecture (Bristowe 1880: 12–53) is devoted to the analysis and classification of vowels and consonants—effectively, to articulatory phonetics. It is remarkable to think of Bristowe delivering the lectures, and interactively exploring the phonemic contrasts of his own spoken English before an audience of the assembled dignitaries of the Royal College of Physicians, or attempting to convince them that the paired 'long' and 'short' vowels of English (the vowels of *pit* and *peat*, for example) differ in quality as well as duration: ‘There is no doubt, however, I think, that they [i.e., the ‘short’ vowels] can all be pronounced independently, and prolonged in their pronunciation without merging into any other vowel’ (1880: 34). He concludes this section (1880: 51) with a table showing his own ‘physiological alphabet’ for consonant sounds, intended not merely for the sounds of English, but as a general scheme of all possibilities. This builds upon and extends his similar table from 10 years previously (1870a), and is remarkably complete in certain respects—he sets up, for example, not only a series of voiceless nasals but also a corresponding one of *aspirated* voiceless nasals.

Bristowe links his analysis with the acoustical investigations of Kempelen, Willis, and Wheatsone, and gives what must be one of the earliest discussions in English of the results obtained by Helmholtz. This was already present in Bristowe (1870a: 129–133), and predates Ellis’s first translation of Helmholtz by some five years. Bristowe made use of a French translation of Helmholtz which had appeared in 1868 with Georges Guéroult as the chief translator (Helmholtz 1868).

Bristowe’s publications on speech extend over many years, and cover a wide range, from clinical case reports (1870b) to practical advice on public speaking (1884: 848–850). His interest in speech also informs his medical textbook, which contains, for example, a detailed treatment of stammering. If Bristowe is viewed from the perspective of medical history, his contributions in other topics are so extensive that accounts of his life and work understandably find little or no space for his work on speech. But from the phonetic science viewpoint, it is strange that he has been comprehensively ignored and forgotten.

1.7 Background: philology

The development of British phonetics within the general field of philology and linguistic studies has been much better provided with historical treatments than corresponding developments within either physics or medicine (e.g., Firth 1946; Collins and Mees 1999: 455–471; Kemp 2001). But the existing accounts do tend to play down the interaction of linguistic work with the 'hard science' areas of physics and medicine, and since they often regard Sweet's work as the apex of 19-century British phonetics, they select from the work of his predecessors (such as Alexander Melville Bell and A. J. Ellis) only those features which anticipate aspects of Sweet's own work.

In fact both Bell and Ellis deserve detailed assessment in their own right. We will briefly take Ellis as an example, since, as has been seen in earlier sections, his work was known and respected by physicists and medics alike, while his death in 1890 places him exactly at the threshold of the period of detailed study attempted in this thesis.

1.7.1 Alexander John Ellis (1814–1890)

A full account of Ellis's career, including a complete bibliography of his enormous output, is given by Sanders (1977). In the present context, his most significant contribution—still of great value today—was his translation of Helmholtz's *On the sensations of tone as a physiological basis for the theory of music* (*Die Lehre von den Tonempfindungen als physiologische Grundlage für die Theorie der Musik*), particularly the second English edition of 1885, which represents the final (fourth) edition of Helmholtz's work. In fact, Ellis did much more than translate; most pages of the book have corrections and detailed annotations supplied by Ellis, while

Appendix XX, which runs to more than 120 pages, consists entirely of 'Additions by the translator'.

Ellis was yet another Cambridge-trained mathematician. He studied at Trinity under George Peacock[18], and in 1837 was placed Sixth Wrangler and elected a Fellow of the Cambridge Philosophical Society. He made prolific contributions to scholarship in the diverse fields of phonetics, philology, mathematics, and musicology. He was even a notable pioneer travel photographer, using the daguerreotype process (Gernsheim & Gernsheim 1971: 35).

While it is true that Ellis had *some* of the same aims and interests as his younger successor Henry Sweet, his view of the nature of phonetics, and particularly of its relationship with the physical sciences, was entirely different. As Kemp (2001: 1475) points out, Ellis 'defined phonetics as a branch of acoustics'.

> The science of Phonetics embraces all that portion of the general science of Acoustics, which relates to the sounds produced by the organs of speech; or, in the more limited sense in which we here propose to employ the term, that portion of Acoustics which relates to the Significant Sounds of Language.[19]
>
> (Ellis 1848: 1)

His first complete treatment of phonetics (Ellis 1845) quotes Willis, Wheatstone and Herschel at considerable length, and subjects them to incisive analysis. Perhaps the most remarkable of his later contributions—though apparently ignored by commentators—is Ellis (1874), where he outlines a programme for phonetic (particularly prosodic) investigation proceeding not from what is heard, or from transcribed symbols, but inductively from 'speech curves' (the term itself is his invention) obtained with a phonautograph (1874: 114). The procedural approach which the paper puts forward, and even the term 'speech curves', are curious foreshadowings of some aspects of the work of E. W. Scripture (see Chapter 8). When the phonograph became available a few years later, Ellis eagerly evaluated its potential, and realised that it was better source of the ‘speech curves’ than the phonautograph (1878a, 1878b).

Ellis retained his lively and informed interest in speech acoustics to the end of his life. Section M of Appendix XX in his translation of Helmholtz (Helmholtz 1885), contains detailed analysis of recently-published papers on the analysis and synthesis of vowels (Jenkin & Ewing 1878; Preece & Stroh 1878).

1.8 Background: summary

This brief survey of the nineteenth century background shows that by 1890 significant British contributions in all three fields could have furnished the makings of a native approach to phonetics as an experimental science. There was general agreement on what components such a science would comprise, and many of the key protagonists in the diverse fields knew and respected each other’s contributions. But as yet there was in Britain no professional or academic platform on which an independently defined field of study could be established. Experimental phonetics—an academic fashion as much as a scientific specialism—would instead be imported from Germany and France and it had little continuity with British antecedents.

1.9 Organisation of the study

Chapter 2, ‘Instruments and techniques’, deals with the five most widely used methods of investigation in the period under consideration (palatography, kymography, ‘mouth measurement', X-rays, and the display and analysis of waveforms), with particular attention to British contributions to their development.

Chapter 3 is concerned with the contributions of a number of figures whose work chiefly or wholly preceded 1912 (the year in which the UCL laboratory was set up). Among these, a fairly lengthy treatment is given to Daniel Jones's predecessor at UCL, Ernest R. Edwards, who was probably the first British academic phonetician to combine 'linguistic' and 'experimental' approaches in published work.

Chapter 4 deals with the British laboratories known to have been set up in the period under consideration, their personnel, equipment and research programmes.

Chapters 5 and 6 are devoted to the work of Daniel Jones and Stephen Jones respectively, while Chapter 7 deals with a group of other phoneticians who had a connection with the UCL laboratory.

Chapter 8 deals with figures who were not academic phoneticians like the group considered in Chapters 5–7, and are probably best classified instead as 'speech scientists': E. W. Scripture, Sir Richard Paget, and Robert Curry—whose book *The mechanism of the human voice* appeared in 1940, neatly coinciding with the end of the period of this study.

Chapter 9 presents a brief retrospective survey and indicates some possible directions for future work.

Note on abbreviations

Throughout this thesis, SJ = Stephen Jones and DJ = Daniel Jones. There is good precedent for this. Daniel Jones was generally known as 'D.J.' to his UCL colleagues, and often signed himself thus in his numerous contributions to *Le Maître Phonétique*; Stephen Jones appears as 'S.J.' in his first (1914) contribution to the same journal. Upper-case abbreviations are written without periods except in quotations, where the original style is generally kept.

Notes to Chapter 1

1 References to Kratzenstein's work variously give the date 1780 or 1781. The prize essay was presented in 1780, and an initial account appeared in *Acta Academiae Scientiarum Imperialis Petropolitanae* for that year; this was evidently the report known to Willis and Wheatstone. Publication as a separate monograph followed in 1781; for the present thesis the 1781 version has been used.

2 The F_1 of such a tube must have been in the region of 1500 Hz. But the corresponding vowel quality is hard to predict since neither Paget nor Willis give any information about the fundamental frequency of the reeds employed. There is thus no certainty that listeners would categorize the synthetic vowels as if they were those of an adult male. If the fundamental were rather high (in the female or child range), normalization could bias the listeners' expectations accordingly.

3 Willis's sister Mary was married to William Clark on 30 July 1827, and Willis (who was ordained) himself performed the ceremony (Buchanan 2013: 37). Clark's career is traced in Weatherall (2000) but there is no mention of Willis or of work on the larynx.

4 A copy dated 12 April 1832 is in the Hume Tracts, a collection of pamphlets preserved at UCL. Stable URL: http://www.jstor.org/stable/60212131
Accessed: 09-Feb-2016

5 In a footnote in Willis (1830: 243).

6 The article is catalogued as a review by Kennedy (1927). Two other works (Kempelen 1791 and Kratzenstein 1780) are also listed at the head of Wheatsone's article, as if they were also being reviewed—though the periods of 46 and 57 years respectively between publication and his 'review' are extraordinary.

7 There are some odd features of Wheatstone's discussion. On p. 359 he seems to say that a cavity can be made to sound the octave of an applied tuning fork. In principle, this should not happen. The resonator will only sound the octave if the second harmonic is present in the excitation—which in the case of a tuning fork, it should not be. If Wheatstone obtained such an effect in practice, it would seem that the fork may have been mishandled in a way that produced significant second harmonic output (cf. Rossing *et al.* 1992).

[8] This paper by Airy thus appeared in print *before* Willis's larynx paper which inspired it.

[9] It is, however, uncertain that Helmholtz actually did hold the position on vowel quality that Airy (and many others) attribute to him. Rayleigh (1894: 472–473) gives his opinion that what Helmholtz says indicates that he held the same view as Willis.

[10] The drawing given by Paget (1930: 21) is evidently his own interpretation of the description given by Potter.

[11] At this period, papers in the *Proceedings* are evidently summaries produced by amanuenses. Maxwell himself would hardly have misspelt the name.

[12] I am grateful to Professor MacMahon for kindly supplying the (unpublished) full text (1984b) of this paper. A résumé was published as (1984a).

[13] The appointment of William Sharpey (1802–1880) at University College London in 1836 as Britain's first full-time Professor of Physiology is 'usually regarded as the beginning of English physiology' (O'Connor 1988: 72).

[14] The spelling **Czermak** will be used in this thesis, as widely in the nineteenth century (e.g., by Sweet) and since. A spelling **Czermák** is also common. The Czech spelling is **Čermák**. Strictly, **á** indicates a long vowel, but the short quantity seems to be usual in English, and also in German (e.g., in Mangold 1990).

[15] The paper was presented to the Royal Society not by García himself but by Dr William Sharpey (Professor of Physiology at University College London—see above). Though there is no particular reason for doubt, it is not known for certain whether the elegant English of the paper is entirely García's own work. It is always possible that Sharpey or others edited or translated it. His other publications are mainly in French, while he was generally referred to with the title 'Signor'.

[16] This conflation of Britain and America is supported by Bynum (1994: xiii) when he writes '… before the last third of the century, American doctors were more consumers than producers of scientific ideas'. Even Kingsley (1879) quotes only European—chiefly British—sources, and writes as if his (American) audience will as a matter of course be familiar with British authorities such as Bristowe.

[17] Ellis published so many similar and overlapping contributions that citing him concisely has always been a problem.

[18] Peacock (1791–1858) was among a group of Cambridge students, others including Herschel and Charles Babbage (1791–1871), who met during 1812–1813 as the Analytical Society (Enros 1983), a precursor of the Cambridge Philosophical Society. It was Peacock who originally suggested to Airy that he should write his textbook on sound. Peacock was also the biographer of Thomas Young (Peacock 1855).

[19] This is a transliteration from the 'phonotypic' spelling in which the whole work is printed.

2

Instruments and techniques

2.1 Categories of instrument

The list of instruments employed in the phonetics laboratory eventually became a very long one. Rousselot (1897) and Scripture (1902) already describe dozens, and experimenters and instrument-makers went on adding to the number. A large German collection is preserved at the Technical University of Dresden, which amalgamates collections from Berlin, Hamburg, and elsewhere. The 'catalogue' of this (Mehnert 2012)—in fact a detailed critical account with extensive bibliography—runs to several hundred items.

But many of these devices perform ancillary functions, easing the process of measurement or analysis (for example, planimeters, travelling microscopes, and mechanical harmonic analysers). It is very doubtful how many of these expensive accessories were ever used in Britain.

In the period under consideration, instruments could in broad terms be used:

1. To image the vocal tract, either in whole or in part (as in palatography, mouth-measurement, conventional photography, or X-rays). The resulting images were usually static, though some of the techniques permit extension to dynamic imaging.
2. To register air flow and pressure, and the movements of speech organs, using chiefly mechanical transducers (kymography).

3. To display or record the acoustic waveform (which might then be analysed in some fashion). Techniques to estimate voice fundamental frequency and vowel resonance frequencies form a subdivision within in this category.

This chapter reviews in turn the origins of palatography, mouth-measurement, X-rays, kymography and waveform-recording (oscillography), with particular attention to British contributions to their evolution, and to the earliest examples of use in Britain.

2.2 Palatography

2.2.1 Previous accounts

There are several published summaries of the development of palatography (for example, Hagelin 1889: 1–3; Rousselot 1897: 57ff; Scripture 1902: 296ff; Panconcelli-Calzia 1940/1994: 55–56; Moses 1940; Witting 1953; Abercrombie 1957; Keller 1971: 5–50), though these accounts are not in agreement in all details of chronology and bibliographical particulars. For that reason the primary literature has been re-examined for the present study, and a new presentation is attempted of the chronology and of the relationships among the early contributions.

All are agreed that the first announcement of the technique we now know as palatography[1] was made by an English dentist, James Oakley Coles, in 1872 (Coles 1872a).

2.2.2 James Oakley Coles (1845–1906)

A brief account of Coles's life and work is given by Lindsay (1955), and this can be supplemented from the civil record and other sources. He was born in London in 1845, apprenticed as a dentist (the usual method of training at the time), and went into dental practice at 81 Wimpole Street. He was elected a member of the Odontological Society of Great Britain in 1867, and served as either Council member or Honorary Secretary of the society in every year from 1874–1881. He began to publish on dental topics in 1868 at the age of twenty-three. After being extraordinarily prolific in research, publication and teaching, he retired from dentistry at the age of 40 in 1886 and began a second equally energetic career in the Church, holding a succession of parishes, and being made an Honorary Canon of Canterbury in 1899. He died on Christmas Day 1906 at the age of 61; an obituary appeared in *The Times* but it focuses entirely on his Church career.

Coles published his material on speech at least twice. He presented his results in a paper read before the Odontological Society of Great Britain on February 5th, 1872. The paper, with 26 illustrations, was included in the annual *Transactions* of the society (Coles 1872a). But essentially the same text (and a single sample illustration) appeared in the *British Medical Journal* for 17 February (Coles 1872b)—less than two weeks after the original presentation, and perhaps indicating that Coles was eager to establish priority. Publication in yet a third form as ‘a complete monograph on the subject’ (Coles 1872b: 182) was promised, and announced by his publishers under the title *The mechanism of speech* (Coles 1872c: facing title page), though no copies have been located, and it seems likely that the monograph never materialised.

The method of investigation devised by Coles was direct palatography using a mixture of flour and gum applied to all the upper surfaces of the mouth—teeth, hard and soft palate (he doesn’t mention the upper lip, but his results suggest that he might have coated that too). After articulating a single ‘letter’, he observed the removal of flour from the upper surface and its transfer to the tongue. These patterns he recorded in red on pre-printed sheets containing upper and lower plan views prepared from plaster casts of the upper and lower surfaces of his mouth (the latter made with the tongue 'in a state of repose'), together with a front view of a standard (slightly open and spread) lip position. In fact, therefore, he was making simultaneous palatograms and linguagrams. Although he did not draw different lip positions, he indicated lip action by colouring the standard lip diagram selectively: the lower lip is shown red for labiodentals (with matching indication on the upper teeth), both lips are red for bilabials, and the corners of the two lips are red for rounded sounds (Coles's 'O' and 'U') indicating partial closure. He also attempted to determine whether the velum was open or closed: 'One point of the utmost practical importance to be determined was, as to when there was complete separation of the nares from the mouth by means of the soft palate' (1872b: 182). He did this not primarily by observing articulation, but by detecting airflow from the nose revealed by the misting of a polished knife blade positioned below the nostrils (the 'Czermak mirror test', which is still in use). In short, he was attempting to give a complete characterisation of the roles of the articulators for each sound, though in interpreting Coles's representations it has to be remembered that the articulators are seen after they have been returned to a standard position to match the printed blanks on which the observations are recorded.

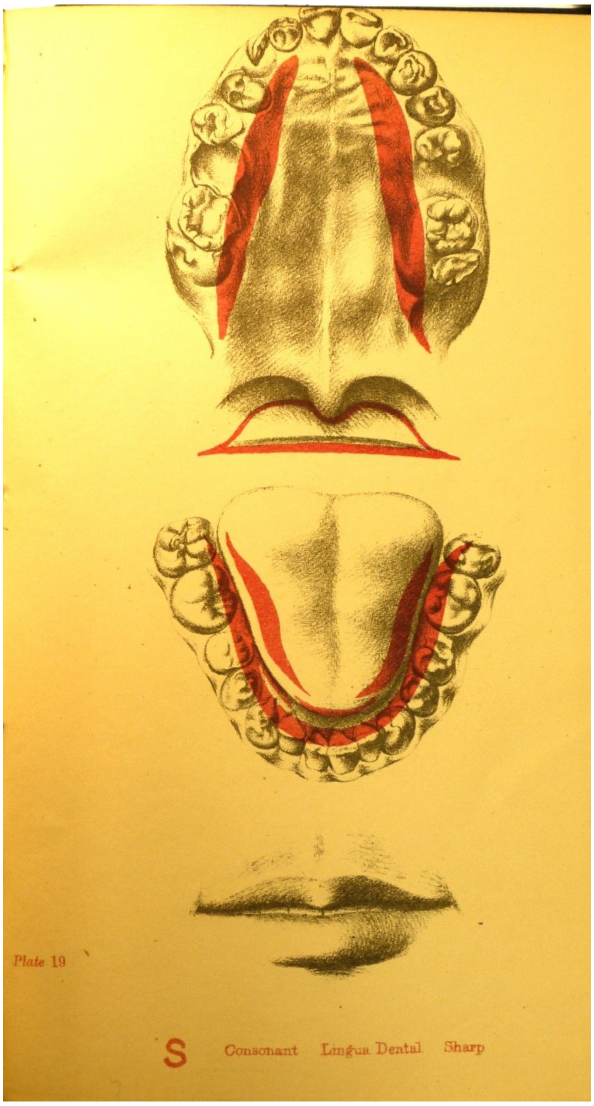


Figure 2.1 Plate 19 from Coles (1872a), showing his palatogram and linguagram for 'S'.

Coles took great care over the preparation and reproduction of his plates, and at their best they do give useful information about articulations. An example is seen in Figure 2.1, which shows Coles's representation of 'S'. The grooved articulation is clearly seen on both the palatogram and the linguagram, and the firmly closed velum is also indicated. But, as others have pointed out, Coles was no phonetician and appears to have confused the sounds of English with the names of the letters of the alphabet (Carruthers 1900: 239, Panconcelli-Calzia 1940: 55). As Carruthers explains,

> The author's method is valuable, rather than the results he gained; for unfortunately he did not observe the distinction between a letter and a simple sound, nor apparently appreciate the difference between the name of a letter and its sound-value, and consequently scarcely any of his experiments represent simple sounds.
>
> (1900: 239)

For ‘S’, he probably pronounced the syllable [es], and so, as it happened, obtained a satisfactory palatogram of [s]. But in other cases, of course, his results were misleading. The pictures he obtained for ‘A’, ‘E’ and ‘I’ represent not three different vowels, but the similar close front tongue position shared by [iː] and the end points of the diphthongs [eɪ] and [aɪ]. For ‘G’, he no doubt pronounced and analysed the syllable [dʒiː], thus obtaining a picture, and a resulting description, which bear no relation to the sound [g], and quite unlike his own results for ‘K’.[2]

Pronouncing the names of letters is not the only problem with Coles's representations. Because he is looking at what are essentially ‘word palatograms’ (Firth 1948), his representations commonly contain signs of accidental modifications and accompaniments that are not essential features of the target sounds. Thus, for ‘B’ he shows the expected lip contact, but also a broad area of tongue closure against the palate which must presumably have occurred during the closed phase of [b] in this particular pronunciation of [biː]. As a result, the description he arrives at is ‘Lingua-Palato-Labial’. Similarly, when he describes all of ‘B’, ‘P’, ‘D’ and ‘T’ as having the nares only ‘partially shut off’ he must presumably be taking account of nasal air flow which he detected during the accompanying vowel or in the rest positions which preceded or followed the articulation of the relevant syllables.

Coles's lack of awareness concerning the differences between speech and writing might suggest that he had little, if any, familiarity with earlier work on speech—and indeed he does not cite any previous work or written sources. He says 'I could find no book that gave such information as was necessary' (1872b: 181). It would be wrong, however, to believe that he was a sort of amateur pioneer, working in a vacuum. By 1872, if not earlier, he was 'Dental surgeon to the Hospital for Diseases of the Throat' (Coles 1872c: title page). This put him in contact with Morell Mackenzie, the founder of British laryngology (Weir & Mudry 2013: 151–153), who was a pupil and friend of Czermak. Coles cites a personal contact with Czermak 'some two years since' as the source for his test of nasality. An extant thank-you letter from Czermak to Mackenzie[3], dated 5th January 1870, confirms that Czermak had been on a short visit to London at Mackenzie's invitation around Christmas 1869/70—exactly the right period to match Coles's account. At the time he was conducting this research, therefore, Coles was in touch with at least two of the leading medically-oriented speech scientists of his day. Besides, many features of the articulatory classification scheme he employs—for instance Latin labels such as 'Lingua-Dental'—must have been derived from some source.[4]

The description of the indirect method of palatography, using an artificial palate, followed very soon afterwards in Kingsley (1879). This was similarly the work of a dentist, the American Norman William Kingsley (1829–1913)[5]. There is no mention of Coles in this first report of Kingsley.

Early references to Coles's work include Rosapelly (1876: 113–114), Gavarret (1877: 402) and Grützner (1879: 204–221). Grützner used a form of direct palatography to investigate his own productions of certain consonant sounds (he gives palatograms of [l], [r] [s] and [ʃ] sounds). His method was to paint the tongue liberally with red or black ink, and then record the contact pattern transferred to the palate, using mirrors to obtain the view. He claims (footnote on page 204) to have arrived at his method independently of Coles. He unhesitatingly cedes the precedence to Coles, while pointing out that some of Coles's results are baffling. He is puzzled by the extensive tongue-palate contact in Coles's 'B', for example. The following year, Techmer (1880) printed a total of 17 original palatograms illustrating various articulations.[6] He painted the tongue with a flour-gum paste (*Mehl-Gummi-Brei*), which was coloured with ink. His method thus combines elements from both Grützner and Coles. He explains that he had learnt of Coles's work from Gavarret (1877).

Awareness of the palatographic method entered the linguistic-phonetic mainstream via the second edition of Sievers (1881: 46).[7] He calls the method 'very ingenious' (*die sehr sinnreiche Färbungsmethode*), and says that it represents a significant advance in the accurate determination of points of articulation (*einen sehr wesentlichen Fortschritt in der genaueren Bestimmung der Articulationsstellen*), though he does not in fact explain how the method works, or reproduce any diagrams.

The Coles-Grützner-Techmer approach was employed in a further work that was to have a lasting impact, Lenz (1887). Panconcelli-Calzia (1940: 56) implies that Lenz's work on palatal consonants was the first application of the palatographic method to a linguistic-phonetic research question, though it should be pointed out that Lenz's palatograms are not of palatals in real languages, but rather his own performances of target positions established to match his own descriptive framework.

In 1887, Kingsley effectively re-published his 1879 paper[8] on indirect palatography, but this time in a linguistics journal, Techmer's *Internationale*

Zeitschrift für Allgemeine Sprachwissenschaft. Coles is added to Kingsley's references in this second version. One may wonder whether Kingsley added the reference himself, or at the prompting of his editor. Techmer certainly interfered to the extent of adding a (direct) palatogram of his own in a footnote, to correct an apparent error in Kingsley.

It is of great interest that 1887 saw the earliest publications of Rousselot.[9] By far the most significant of these is his 'Introduction à l'étude des patois', a flagship manifesto piece of more than 20 pages which opens the first number of the journal *Revue des patois gallo-romans*. This was written before Rousselot adopted instrumental methods, and describes investigations that rely on the ear of the observer. Only two 'experimental' techniques are mentioned at all, and they are only called upon to settle cases where the ear may be in doubt. One is an application of Coles's method in order to differentiate the true palatals [ʎ] and [ɲ] from the sequences [li] and [nj]:

> Pour ces quatre articulations, la langue est collée au palais. Mais, elle touche sur toute sa largeur pour *l̯* et *n̯*, par les bords seulement pour *ly* et *ny*. On peut s'en assurer en badigeonnant le palais avec un mélange de farine et d'eau gommée: les points de contact s'impriment sur la langue.[10]
>
> (1887 : 8)

The other is the mirror test for nasal airflow (p. 16), due to Czermak, though used and described by Coles (1872a). Rousselot (1887) does not mention Coles (or Czermak) by name, but it is an intriguing possibility that the work of Coles might have been the initial trigger for Rousselot's conversion to a wholly experimental approach.

2.2.3 Samuel William Carruthers (1866–1962)

Coles had only one direct follower, S. W. Carruthers (1866–1962), a medical practitioner who completed a thesis with the title 'A contribution to the study of articulate speech' which was published (in three parts) in the *Edinburgh Medical Journal* (Carruthers 1900).

Samuel William Carruthers was born in London in 1866. His father, William Carruthers (1830–1922) was a notable botanist who became a Fellow of the Royal Society in 1871 and was Keeper of Botany at the British Museum (afterwards the Natural History Museum) from 1871–1895. He operated a seed-testing laboratory at the family home, served as President of the Biological Section of the British Association, was consulting botanist to the Royal Agricultural Society, and active in the Linneaen Society, the Geological Society, and Royal Microscopical Society, so Samuel and his younger brother John (b. 1869) must have grown up in an intensely scientific milieu. John became a botanist like his father but Samuel studied medicine in Edinburgh, gaining his MD in 1899. While a student in Edinburgh, Carruthers would have attended the lectures of John Wyllie (see Chapter 3). By 1901 Carruthers was established as a GP in Norwood, in South East London, and spent his working life there. He has a small number of publications on various medical topics, though apparently no others on speech. He died in 1962 at the age of 96.

Carruthers gives an account of his attempts to refine the method (1900: 239–241). He found that the flour and gum mixture used by Coles did not give clear and satisfactory results. He experimented with charcoal mixed with water, with glycerin, or with gum, and tried applying the mixture either to the palate or to the tongue. He eventually came to the conclusion that the clearest palate images were obtained by spraying the palate with finely powdered charcoal, and the best results on the tongue were obtained when the palate was painted with charcoal and gum. In fact, all his diagrams are composites over a variable and unspecified number of repetitions, possibly utilising more than one variation of the technique.

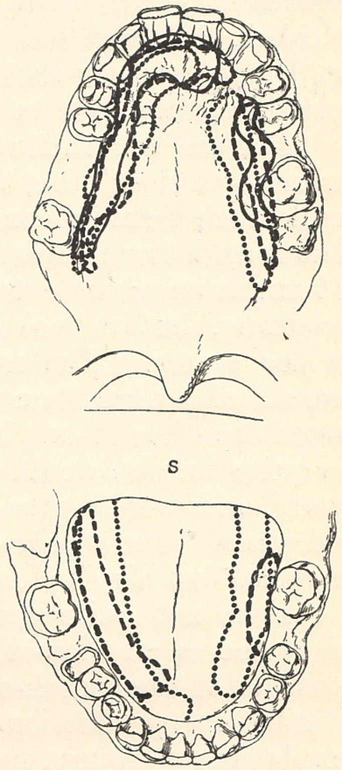


FIG. 1.
. . . . . Palate painted.
- - - - - Palate sprayed.
——— Tongue painted.

Figure 2.2 Carruthers' comparison of the contact areas for his own [s] as revealed by 3 variations of technique. His grooved articulations were markedly angled to the left.

Rather than attempting to view his own palate with mirrors, Carruthers relied on an assistant (his father) to make the drawings. There can be little doubt that the greater difficulty involved in recording the results from direct palatography must have been one of the factors that favoured the predominance of indirect palatography from about 1890 onwards. The artificial palate, once removed from the mouth, can be observed and drawn at leisure, or placed in an episcope and projected onto a screen. In principle, photography could have been applied in either kind of palatography, though in practice it seems not to have been widely used. The practical difficulties of framing, focusing at close range, and providing adequate illumination would all have been considerable. Cost must also have been a factor. Witting (1953: 58) identifies Hagelin (1889) as the first to apply photography in indirect palatography.[11] Photography began to be used in conjunction with direct palatography only in the 1950s (Witting 1953; Abercrombie 1957).

Carruthers seems to be unique among early palatographic studies in systematically exploring different procedures, and acknowledging the fairly considerable variations in the findings they produced. Like Coles, he also presents his results on drawings which represent his own (very imperfect) dentition. We can see, for instance, that overall he retained only 6 of his 12 molars. By contrast, both Grützner and Techmer present their results on diagrams which show a suspiciously perfect maxillary arch of 16 teeth, and one may suspect that their diagrams are somewhat idealised.

Carruthers rejected the assumption that sounds in voiced/voiceless pairs would necessarily have identical contact patterns, and believed that his own results showed consistent differences, usually a larger contact area for the voiced member of a pair (Figure 2.3). Since Carruthers himself was the sole speaker, probably not much weight can be attached to this finding. More reliable is the consistent evidence that certain of his own articulations are markedly asymmetrical. In particular, the groove in his [s] and [z] articulations is not central on the incisors, but towards the left canine tooth.

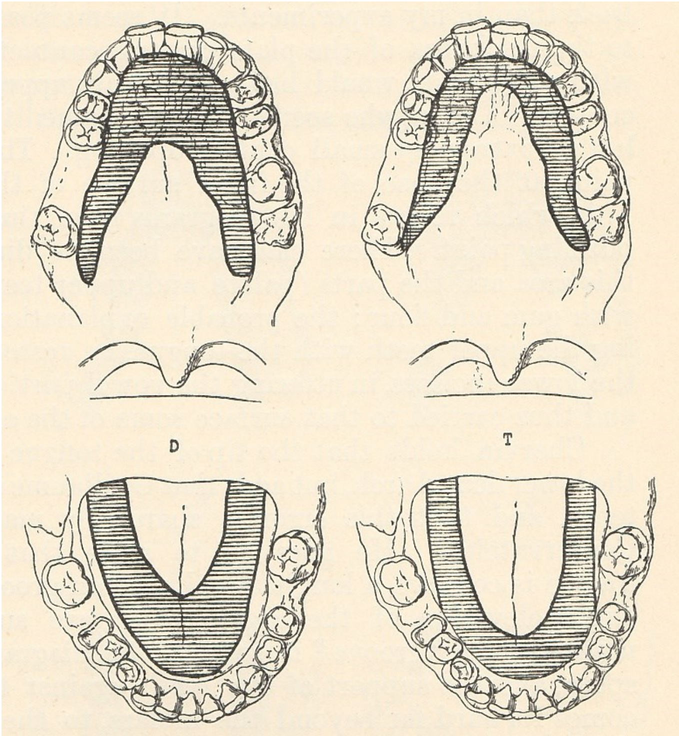


Figure 2.3 Carruthers' palatograms and linguagrams of his own 'D' and 'T' articulations, indicating considerable differences in contact area (1900: 343).

The palatography practised from around 1900 by British phoneticians such as E. R. Edwards and Daniel Jones was based entirely on the Continental techniques described by Rousselot and Scripture, owing nothing directly to Coles, or indeed to Carruthers. The work of Carruthers was at least nominally known to DJ, since offprints of the three parts are included in his Bound Offprints 'Miscellaneous 2'. It was also known to Noël-Armfield, who added an extensive account of it to his *General Phonetics* from the third (1924) edition, acknowledging direct help and advice he received from Carruthers. Much later, Abercrombie invited Carruthers to Edinburgh to see the revival of direct palatography then in progress at the Edinburgh laboratory (Abercrombie 1957). Otherwise, despite its great interest, the work has been conspicuously absent from the historiographical and bibliographical record. Carruthers' study is interesting not only for the palatographic method, but also for the general account of speech production it incorporates, and his classification scheme for vowels.

2.3 Mouth measurement

2.3.1 Mid-sagittal sections

Anatomically detailed mid-sagittal sections through the vocal tract proliferated in the nineteenth century, as in the example from von Luschka (1868) reproduced as Figure 2.4. Of course such figures represent the vocal tract observed in a lifeless condition.

From the 1850s there begin to appear mid-sagittal sections showing—as Scripture sceptically puts it—what 'observers … considered to be the positions of the tongue during speech sounds' (1902: 327). The earliest may have been those of Brücke (1856), which are sketchy in many details, though the defects are masked to some extent by their small size on the printed page. An example is shown in Figure 2.5 alongside a penny coin (diameter 20.3 mm). The earlier of Merkel's two works (1857) contains mid-sagittal sections for vowels only.

Figure 2.4 A midsagittal section from an anatomical work (von Luschka 1868) used (with implied symbolic significance) as the frontispiece of a book on Indo-Germanic philology. (The first edition of 1897 is similar).

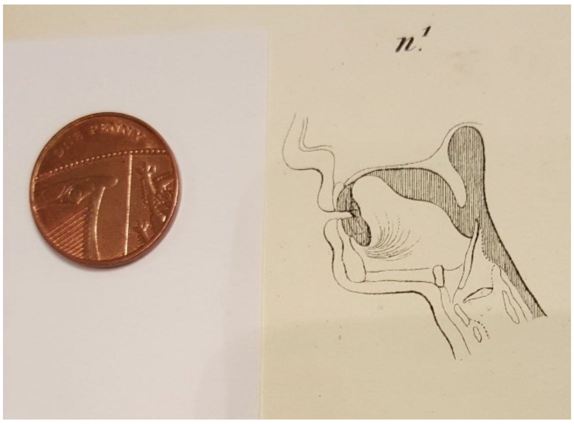


Figure 2.5 Midsaggital section for [n], Brücke (1856) (approximately actual size).

In the 1860s significant additions to the range of published diagrams came from Britain. Müller (1864) contains some 21 such figures, illustrating both vowels and consonants. Müller does not specify how the diagrams were obtained, but comparison of corresponding examples leaves little doubt that Brücke must have been an important source (see Figure 2.6).

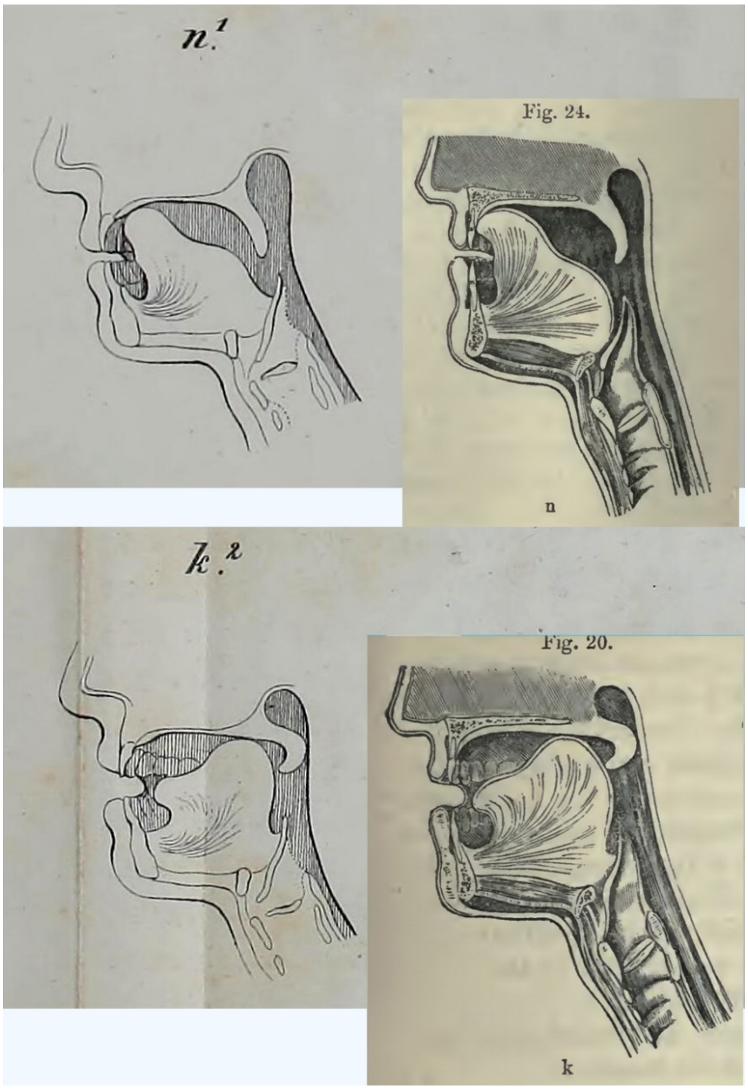


Figure 2.6 A composite image showing corresponding midsagittal sections from (left) Brücke (1856), and (right) Müller (1864), scaled to approximately equal size for comparison. Upper row [n], lower row [k].

The number of articulations illustrated by Max Müller is greater than that given by Brücke, and the anatomical drawing is corrected somewhat (the hard palate is shown more accurately, for example). In both sets of figures, continuity of shading seems to indicate that the oesophagus is open and in communication with the oral cavity—perhaps the result of basing that part of the drawing on a mid-sagittal section from a cadaver, where the sphincteric closure of the oesophagus would have ceased.

Bell (1867) gives a systematic array of outlines corresponding with his classification scheme for speech sounds. Bell's diagrams are considerably more schematic, and may be the earliest to use arrows to indicate airflow; they do not include the open oesophagus seen in Müller and Brücke. But all three of Brücke, Müller and Bell agree on a feature which has subsequently fallen out of use in mid-sagittal diagrams—the representation of the nasopharynx as a clearly defined cavity at the head of the pharynx.

Bell had an unorthodox view concerning the way that nasality is regulated, believing that the nasal airway is not shut by closing the soft palate against the rear phayrnx wall, but instead by moving it so that its upper surface closes the two choanae (the 'inner nostrils' which link the nasaopharynx to the nasal cavity) (cf. Eijkman 1926). For this reason, Bell's representations of entirely oral sounds such as [t] or [s] appear show the velum in a position we now equate with 'open'. Nasal airflow is, however, supposed to be blocked, even though the nasopharynx remains coupled to the oral cavity. Sweet did not share Bell's ideas on nasality but this did not prevent him from recommending the reader to consult Bell if diagrams were wanted (1877: xi). It is remarkable that Sweet's own writings on phonetics do not anywhere contain a single diagram—not even a diagram reproduced from another source.

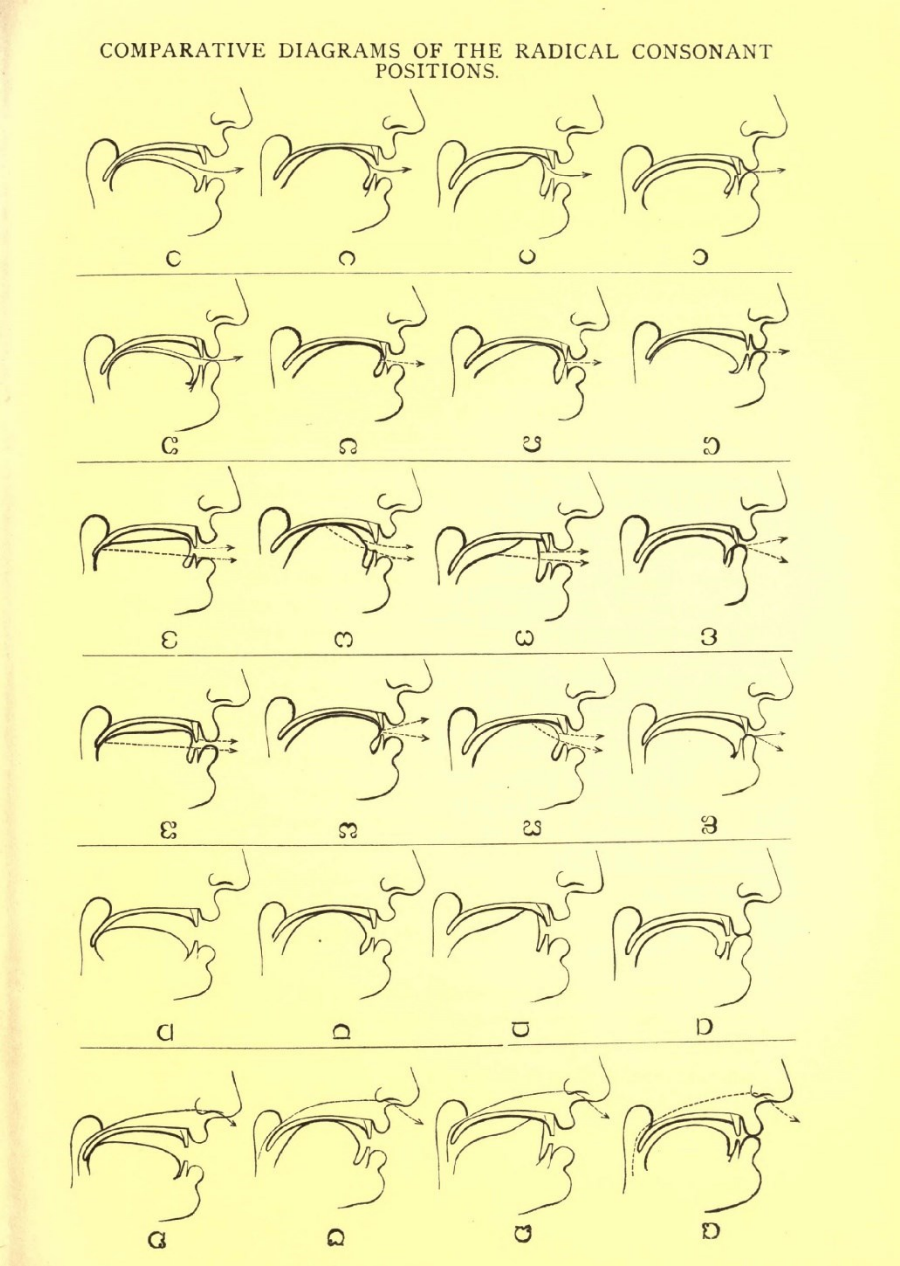


Figure 2.7 Schematic midsagittal diagrams given by Bell (1867). The third image from left in the bottom row represents [n]; the one directly above it represents [t], but is shown with what appears to be the same (open) velum position as for [n].

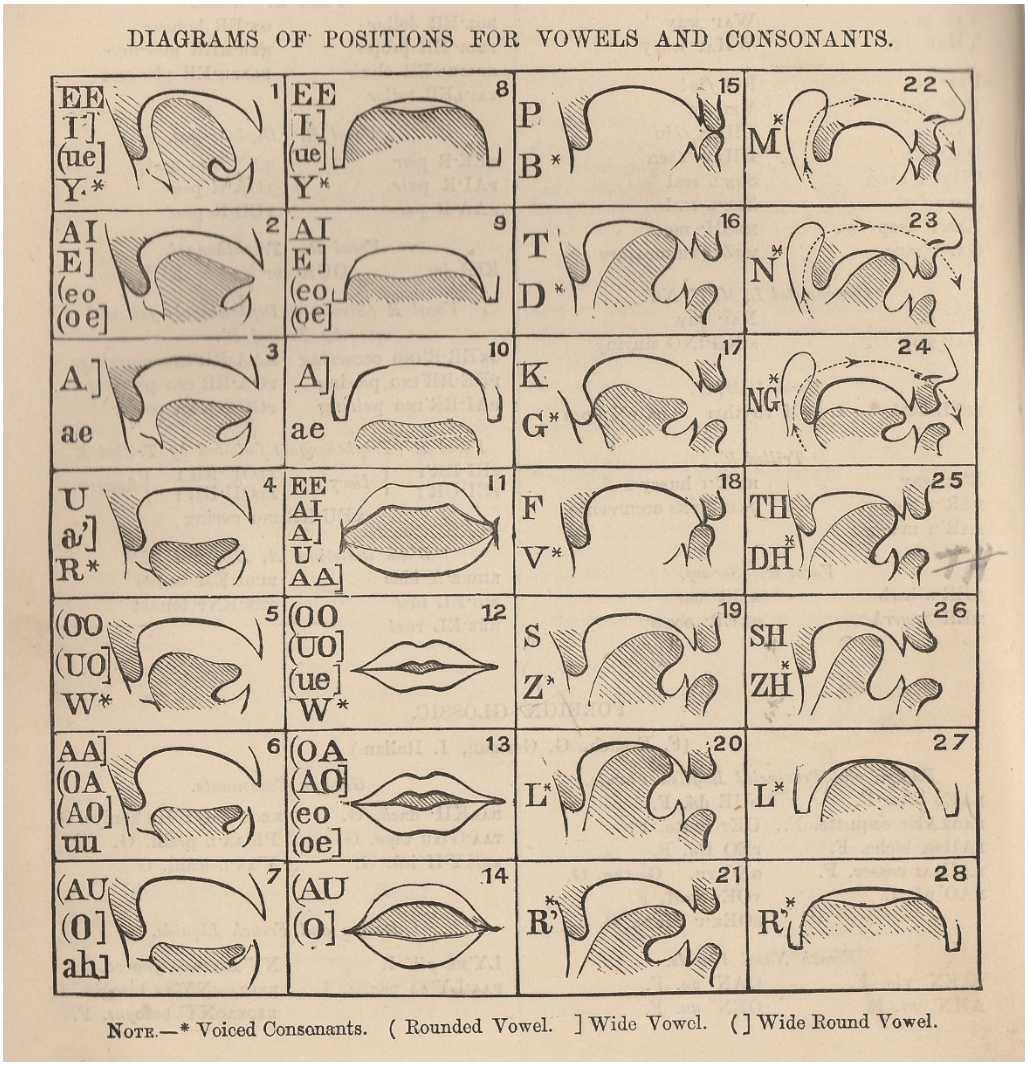


Figure 2.8 Diagrams from Ellis (1877)

Ellis (1877) gives a selection of diagrams which are simplified to the point of being grotesque caricatures (Figure 2.8), commenting 'These are merely diagrams, not complete drawings of the vocal organs. They are intended to show roughly the positions of the tongue with regard to the palate, teeth, and uvula, and the position of the lips with respect to each other' (p. 15). Everything superfluous is omitted. For example the vowels in cross-section (Diagrams 1–7) are shown without the lips, since Diagrams 11–14 separately show the four lip positions to be combined with the tongue outlines of column one, while the labials (Diagrams 15, 18 and 22) 'omit the tongue as its position … is determined by that due to the following sound'.

Thereafter, the history of mid-sagittal sections is divided between those which are unabashedly diagrammatic and those which aspire to realism. But the 'realism' is often a matter of accuracy in overall proportions, such as can be guided by an anatomical preparation. The problem addressed in the various methods of 'mouth measurement' is the determination of realistic articulator positions in the course of sound production in the living subject.

2.3.2 Early probing methods

The possibility of measuring the configuration of the mouth for the production of particular speech sounds is at least hinted at by Helmont (1667). The frontispiece of the work (Figure 2.9) shows the author seated in front of a mirror, estimating with callipers a measurement (perhaps the lip opening) on his own mouth reflected in the mirror.

Helmont certainly says that would be possible for someone 'to learn and teach human speech through the various configurations of the tongue and mouth' (Coudert & Corse 2007: xv). He claimed to have accomplished this himself with the deaf.

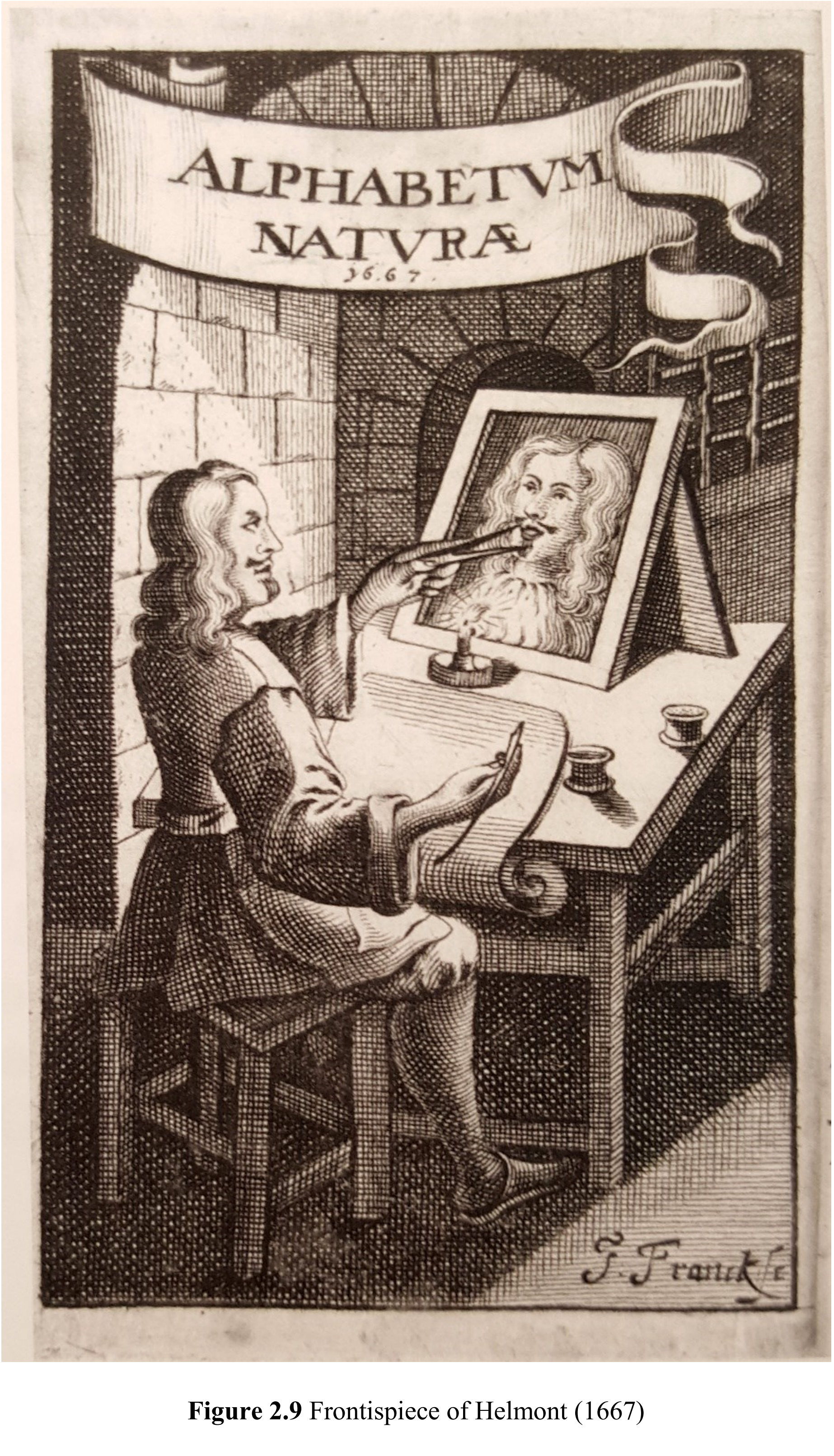


Figure 2.9 Frontispiece of Helmont (1667)

In the ‘philosophical notes’ which follow his poem *The Temple of Nature* (pub. 1803), Erasmus Darwin (1731–1802) gives a fairly detailed account of his attempts to estimate vocal tract configurations for vowels:

> I have found more difficulty in analyzing the vowels than the other letters; as the apertures, through which they are modulated, do not close; and it was therefore less easy to ascertain exactly, in what part of the mouth they were modulated; but recollecting that those parts of the mouth must be more ready to use for the purpose of forming the vowels, which were in the habit of being exerted in forming the other letters; I rolled up some tin foil into cylinders about the size of my finger; and speaking the vowels separately through them, found by the impressions made on them, in what part of the mouth each of the-vowels was formed with somewhat greater accuracy, but not so as perfectly to satisfy myself.
>
> (1803: 119)

An interesting feature of Darwin’s method is that it was possible to speak ‘though’ the tubes and thus obtain an impression from a sustained natural-sounding vowel. In this respect it is superior to the later method devised by Grandgent, whose transverse cardboard ovals, inserted on wires, must have largely blocked the channel they were intended to measure.[12]

The vocal tract configurations shown on the diagrams of Brücke and Merkel, and indeed those produced in Britain around the same time by Müller and Bell, were not based on any systematic sets of measurements. Atkinson (1897: 14) provides a survey and a critique of the sources of information they must have relied on. They include informed guesswork (the ‘method … of probability’), examination with the laryngoscopic mirror, and probing with the finger or with implements made of wire or whalebone.

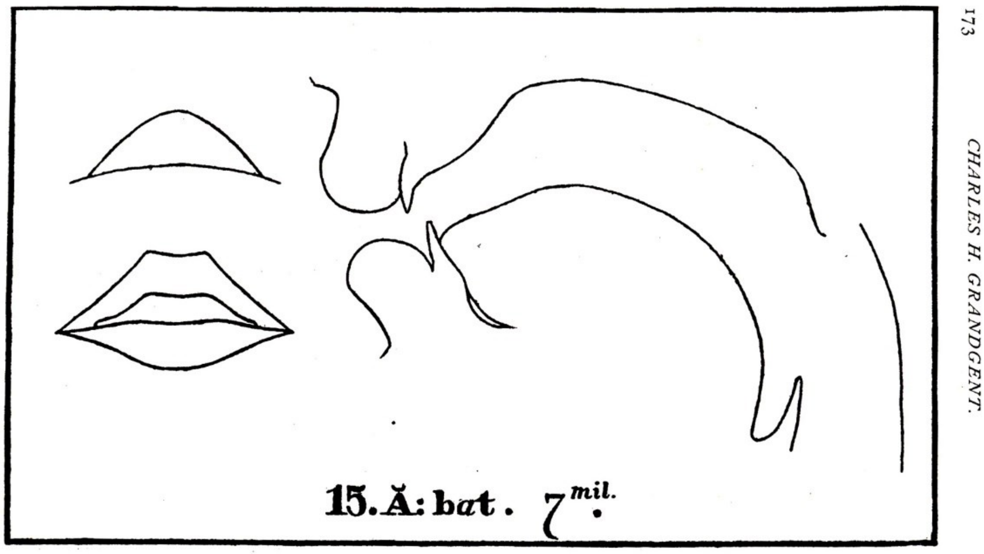


Figure 2.10 A typical figure from Grandgent, showing his TRAP vowel (1890: 173). '7 mil.' indicates jaw opening (in mm), relative to the fully-closed position. In the original, each drawing fills a page, and is probably life-size. The frame is 165 x 95 mm.

In 1890 the American Charles Hall Grandgent (1862–1939) published his 'Vowel measurements'. He used a set of cardboard ovals in graded sizes, each fixed at right angles on the end of a wire probe. The positions along the vocal tract at which ovals of different sizes just fitted the channel were found by marking the wire at the incisors and transferring the measurement to a pre-prepared drawing. In this way the outline could be laboriously built up.

He had presented his results at a convention of the Modern Language Association of America in December 1889, and a report of the meeting, with a summary of Grandgent's measurement method, appeared in *Le Maître Phonétique* (Spanhoofd 1890). Grandgent joined the IPA in the same year.[13] This 'linguistic' context, and Grandgent's activities as a language teacher and Romance philologist, may have earned him more attention from linguistically-inclined phoneticians than he would have gained had he been only an experimentalist. Grandgent's 'Vowel measurements' and a related work *German and English sounds*, published in book form in 1892, were widely cited for a number of years. Sweet was still maintaining that Grandgent had obtained 'the best results hitherto as regards the vowel positions' nearly twenty years after they had first been published (1911: 459). It should be noted, however, that Grandgent's measurements focus on the body of the tongue. He could not, for instance, measure the position of the rear pharynx wall for each vowel *as it was being produced,* but had to shift to an open mouth position to attempt it. This probably accounts for the wide pharynx opening shown in Grandgent's TRAP (Figure 2.10), which is very implausible for a vowel of this type.

2.3.3 H. W. Atkinson (1868–1946)

It was an English schoolmaster, Harold Waring Atkinson, who devised and manufactured the most ingenious and precise probing instrument of all, Atkinson's Mouth Measurer.

The outlines of Atkinson's biography can be gained from the *Alumni Cantabrigienses* (Venn 1922) and an obituary (Munson 1947). He was born in Beckenham, Kent, and attended Merchant Taylors' School. He was at Trinity College Cambridge 1887–1890, where he was Classical and Greek Testament Prizeman, and gained his BA in 1890. He studied German and chemistry at Marburg from 1891 to 1892, and was then Assistant Master at Dean Close School, Cheltenham (1892–1895). He returned to the Continent for a period of study at the University of Paris in 1896. After a succession of positions teaching classics, science, and modern languages in English schools, he went to South Africa in 1902 where he was Head Master of the Boys' High School, Pretoria. He returned to Britain in 1906, but seems not to have held any further full-time post, devoting himself to a variety of educational and welfare activities. Venn lists: ‘Occasional Examiner to Civil Service Commissioners. Examiner in French for the College of Preceptors. A member of the Executive Committee of the Modern Languages Association. Hon. Librarian to the British Prisoners of War Book Scheme (Educational)’. As Munson (1947) makes clear, he was also active in the Temperance movement. He was made an MBE in 1919.

Atkinson was evidently inventive, ingenious and dextrous.[14] He shows an informed and practical interest in the properties of materials, whether glass, metals and alloys, casting materials or adhesives. He himself made the prototypes of all his various mouth measurement devices.

We know that during his stay in Marburg (1891–1892), Atkinson was in contact with Viëtor, since it was he who drew Atkinson’s attention to the recently published work of Grandgent (1892). By 1897 Atkinson and Grandgent were in correspondence (see below), and Grandgent sent Atkinson a copy of his earlier work from 1890.

It is also clear that before Atkinson published anything on his methods, he was in communication with Rousselot. This was apparently not long before the latter completed the first volume of his *Principes*. It is quite likely, therefore, that the two met during Atkinson’s Paris stay in 1896.

Atkinson published two related papers on mouth measurement and the results he obtained for vowels (1897, 1899). Though he is chiefly associated with the eponymous ‘Mouth Measurer’ which was eventually manufactured and sold commercially, his 1899 paper gives a detailed account of an earlier method he had developed and discarded, while the testimony of Rousselot adds a third method, which is apparently not mentioned at all in Atkinson’s own publications:

> M. Harold W. Atkinson vient de me communiquer une méthode qui est à la fois plus sûre et plus facile. Il prend la position de sa langue au moyen d’un ruban de

> godiva (p. 5)[15] ramolli dans l'eau bouillante, qu'il enfonce aussi avant que possible dans la bouche et qu'il fixe aux dents supérieures. Quand le godiva est durci, il le retire, et, l'ajustant sur les dents d'un moulage en creux de sa voûte palatine scié dans le sens de la longueur, il a du même coup la courbure du palais et celle de la langue. Il ne reste plus qu'à les fixer l'une et l'autre en les découpant dans une feuille de papier fort ou d'ébonite. Le temps un peu long que demande le godiva pour se durcir dans la bouche peut être de beaucoup diminué et même réduit à un instant, si l'on dirige dessus un jet d'eau froide. Par ce moyen, l'expérience deviendrait même plus exacte, car le godiva insuffisamment durci s'infléchit, et, comme le point de repère est loin de l'extrémité du ruban, le moindre abaissement donnerait une erreur considérable. Au lieu d'un simple ruban, M. Atkinson emploie encore une lame de godiva de la largeur de la langue, et obtient ainsi la représentation totale de la courbure supérieure de cet organe. Il pourrait encore prendre à la fois le moulage de la langue et de la voûte palatine, et toute chance d'erreur serait écartée.
>
> (Rousselot 1897: 277)

The example is interesting, since it may be the sole case where Rousselot reports on a recent technique of British origin. Scripture (1902: 330) also gives an account of this technique and similarly attributes it to Atkinson.[16] The 'ribbon' technique was taken up by Rousselot's nephew and associate Laclotte, and applied in an ambitious attempt to elucidate the phonetic basis of a consonant alternation in Ancient Greek (Laclotte 1899). Laclotte gives a detailed specification of the method of working, and arrived at some plausible tongue profiles (see Figure 2.11).

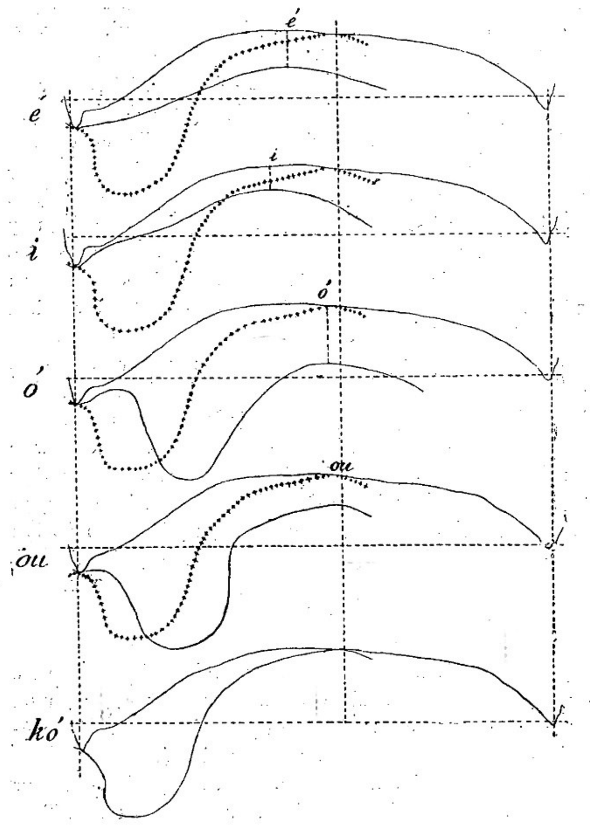


Figure 2.11 Tongue profiles obtained by Laclotte (1899) using the ‘godiva ribbon’ method attributed to Atkinson.

There are some resemblances between this ‘godiva ribbon’ method and the technique which Atkinson himself describes as being used for his first investigations (1899: 494–499). A small plate of thin nickel is cut and bent so as to fit the front surface and biting edge of the upper frontal incisors. Attached to this is a nickel wire extending backwards along the centre of the tongue. By a process of trial and error the user gradually bends the wire into shape with pliers so that at the point of maximum raising the tongue just fits into the profile defined by the wire. A similar method had apparently been tried independently by Grandgent (Atkinson 1897: 15).

The method must have been laborious, though Atkinson was able to use it (in conjunction with palatography) to obtain tongue contours for both vowels and consonants (See Figure 2.12). He even believed that comparison of his vowel contours made in Germany and after return to England showed accommodation to the linguistic context (that observations were made in Germany indicates that Atkinson was using this nickel-wire method at least as early as 1892).

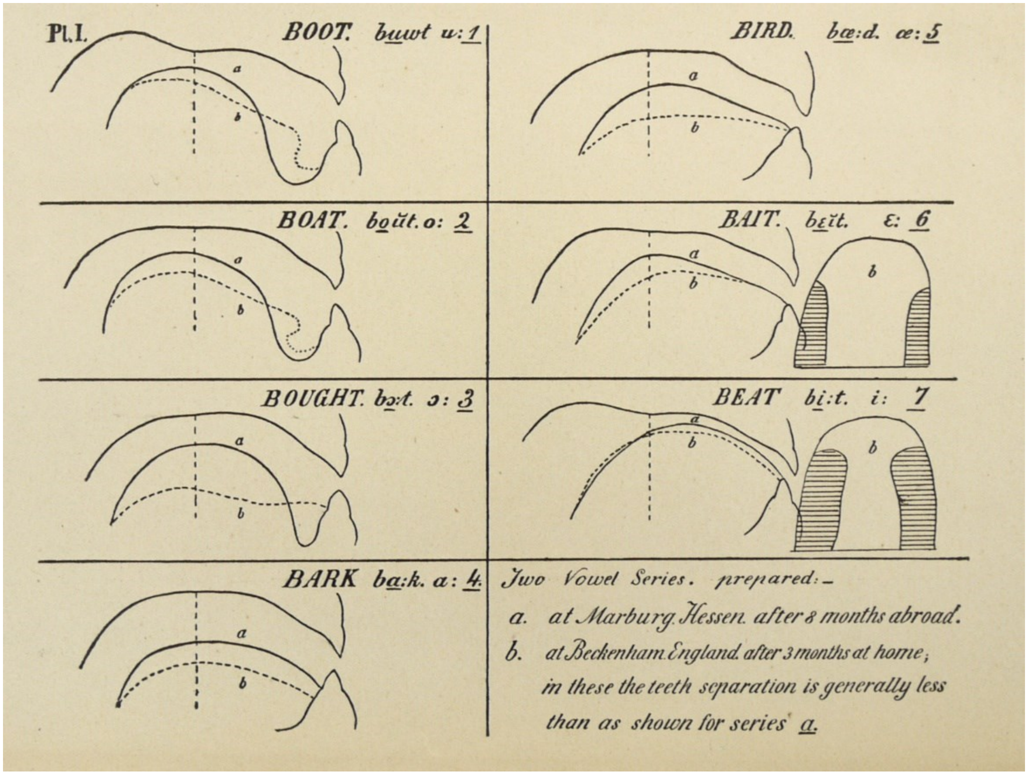


Figure 2.12 Results from Atkinson's nickel wire method (1899: plate I, detail).

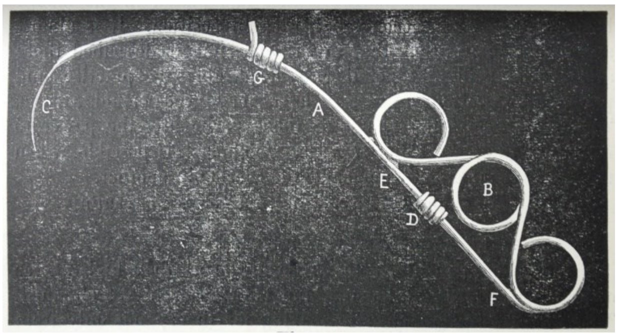


Figure 2.13 Atkinson's Mouth Measurer (1897: Fig. 4), showing labelling of parts. The tooth stop *G* is the earlier (single-wire) pattern.[17] (The blotchiness of the black background is a feature of the original illustration.)

The final method devised by Atkinson was an instrument described as early as 1897, and later sold in almost exactly the same form as 'Atkinson's Mouth Measurer'. See Figure 2.13. Atkinson used the same description in his two papers (1897: 15–16) and (1899: 500–501):

> In a tube *A* slides a fine wire of which the end appears at *C* protruded from the tube. The other end bent at a right angle and gripped between the coils of the helix *D* slides in a slot extending from *E* to *F* on the side towards these letters. On *D* being drawn back to *F* the wire is completely withdrawn within the tube; the distance to *E* gives a protruding length of 4,5 mm.[18] B is a three-ringed handle. Through the rings are placed the first, second, and third fingers; the thumb is then able to slide *D* up and down. *G*, the tooth-stop, is moveable; it is so made and bent that on turning it with projecting point down—i.e., to the inside of the curve, it slides freely, while in the position shown it grips firmly on the tube.
>
> Fig. 5 shows the position *A* which it takes in the mouth. The tooth-stop (Fig. 4 *G* or Fig. 5 *E*) is here omitted. It would beat *e*, the pointer of it lying in the valley of the two front teeth, the tube touching the edge of the teeth at *a* and the hard palate at *b* (or if reaching further into the mouth at *c*, or at both points). The wire is then protruded until it touches the tongue. Taken out of the mouth the instrument is applied to a facsimile section in plaster of the central portion of hard palate with teeth, as shown by the outline *d a b c*, and the point of the wire *D* marked on the paper. By the aid of the tooth-grip Fig.5 *G* the tube assumes a slightly lower position in the mouth; and the grip Fig. 5 *F* puts it in the position shown at *B* for measuring the front cavities of the mouth in back vowels. The dotted outline *C* shows a modification of the instrument for soft palate measurements. In this the wire comes out upwards.

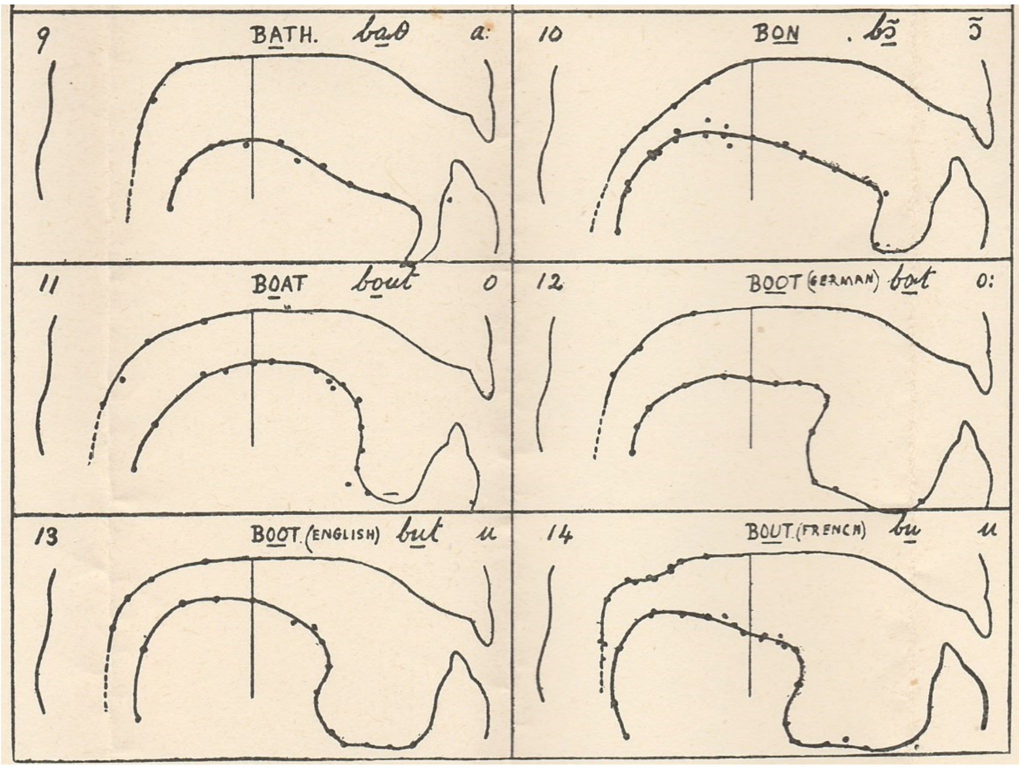


Figure 2.14 Outlines for certain English, French and German vowels, as obtained by Atkinson (1897).[19]

Figure 2.14 provides a sample of Atkinson's results (he seems to have published only 14 such outlines in total). For the most part the plotted points lie impressively close to a smooth curve. Also revealed is the ability of the device to probe around corners, and to obtain measurements not only from the surface of the tongue, but also from the soft palate—and even *beneath* the tongue tip.

Nevertheless, Atkinson (1899) concludes with a number of somewhat pessimistic observations, which perhaps explain why he seems to have done nothing further with his methods at that stage. (1) Though any one measurement system may appear 'accurate' in the sense of giving repeatable results, different methods give different results. (2) A speaker probably has undetected slight variations of pronunciation, which produce different results. (3) A person may have two (or more) relatively distinct methods of producing identical sounds. (4) Differences in tongue shape (for example, in similar vowels across languages) affect the whole profile and are much more complex than can be expressed with simple labels such as ‘advanced’, ‘raised’. Atkinson favours a coordinate system of measurement. (5) Mouths of different shape (for example high palates as opposed to low palates) will require different tongue contours to achieve comparable sounds, so how (if at all) can these individual differences be taken into account systematically? He also points out that the inconveniences of the techniques available mean that data can only be gathered in small quantities, and from cooperative (phonetician) subjects.

These objections apply not only to his own mouth-measurement techniques, but quite generally to many systems of vocal-tract imaging, especially those which capture static positions.

The only modern attempt to evaluate the mouth measurer appears to be Keller (1971). She obtained tongue outlines for three of her own vowels, of a generally very rough appearance, and including conspicuously different outlines for [æ] traced on different occasions. Of course she was less practised then Atkinson, who had been working with the device and its predecessors for several years. She gives a list of the obvious drawbacks of the device (laborious in use, difficulty in reproducing the position of the device relative to the teeth and palate, insensitivity of the tongue surface to contact with the probing wire, need to reproduce and sustain the same articulatory position repeatedly). She concludes ‘there is nothing this instrument can do which cannot be done better by some other technique.’ This is not altogether fair. Until the arrival of ultrasonic tongue imaging (UTI) there was no easily available, safe, and relatively inexpensive way of getting articulatory data comparable with what the mouth measurer can in principle provide.

Some 13 years separate the development of the Mouth Measurer and the first indication that it was being manufactured for sale. Though Scripture (1902) gives particulars of suppliers for much of the apparatus he describes, his account of the Mouth Measurer says nothing about the device being commercially available. The Mouth Measurer as sold is shown in Figure 2.15. The price of 8 shillings (1910) corresponds to something of the order of £240 at 2015 prices, depending on what index of comparison is used.[20] The device came with an instruction sheet which looks as if it was meant to double as promotional literature, since it carries testimonials from DJ, Bernard MacDonald, D. L. Savory, and even Sweet ('Mr. Atkinson's apparatus is, as far as my knowledge goes, the best that has yet been devised for determining objectively the position of the tongue in forming different speech sounds.'). Unsurprisingly, the sheet does *not* reproduce a negative testimonial that Atkinson himself had written 13 years previously:

> I believe it is the most exact [method] up to the present time; but simple though it seems as here described, the process of measurement of the tongue and palate is not an affair of pure and unalloyed pleasure. On the contrary, I have found it toilsome and trying to the nerves.
>
> (1897: 20)

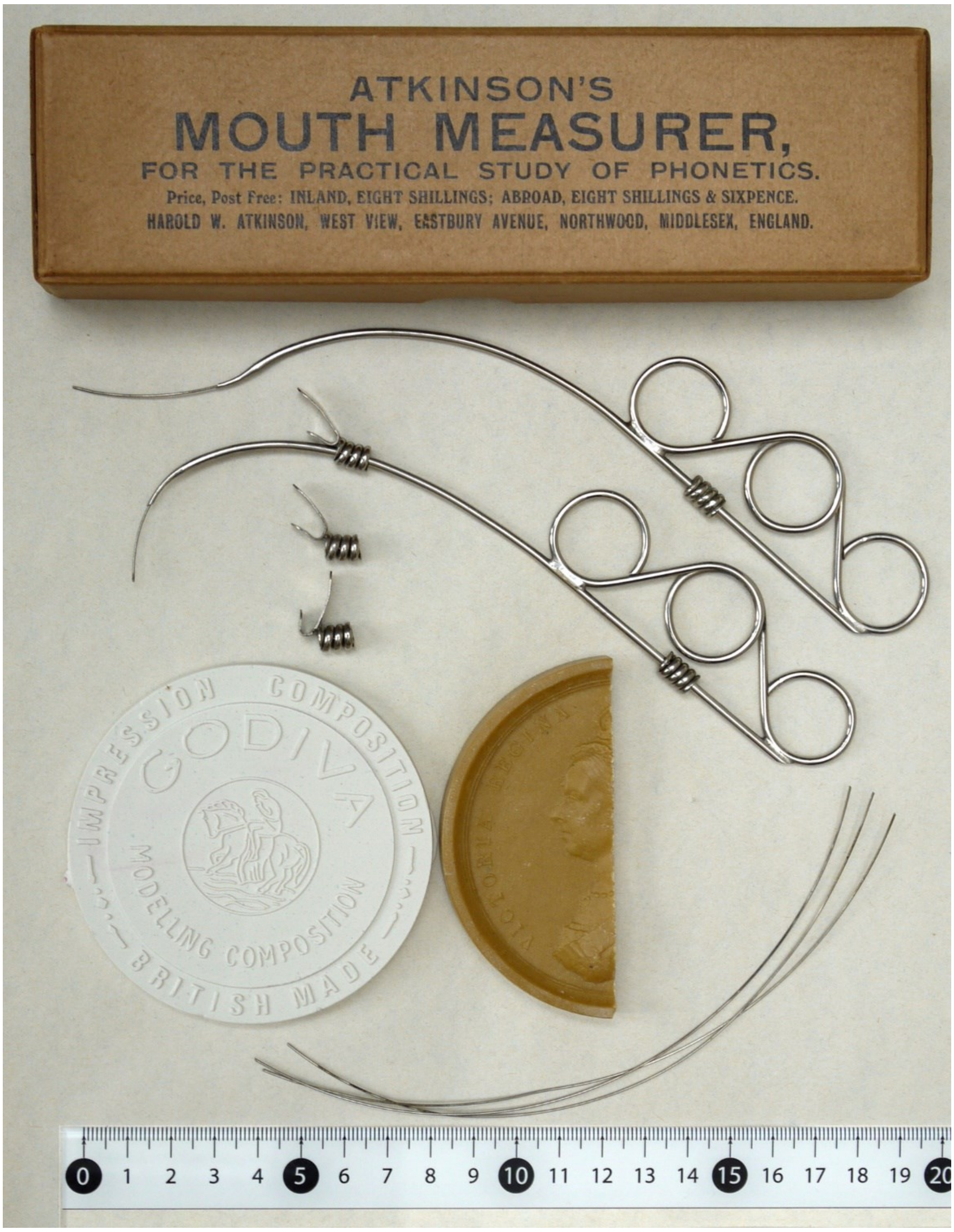


Figure 2.15 Atkinson's Mouth Measurer as supplied in its box. The instruments were in pairs. In one (upper) the probe wire emerges with an upward curl (towards the palate), and in the other downwards (towards the tongue surface). A selection of tooth stops was supplied with differing angles; they can be further tailored easily by bending. The half disc is exactly 1 oz. of modelling compound, supplied for making a (partial) cast of the palate. It is the same diameter as the modern disc of Godiva shown next to it and precisely half the weight. Replacement probe wires were also included.

2.4 Kymography

Throughout the period 1890–1940, the large drum of the kymograph was the most conspicuous piece of machinery visible in any phonetics laboratory and indeed was almost an emblem of the whole enterprise. It certainly formed the impressive focal point for many laboratory photographs.

But the drum of the kymograph is in reality the least significant feature of the device, being simply the recording method. Daniel Jones (1917) deals dismissively with this part of the apparatus, speaking of ‘a revolving drum covered with smoked paper, or some similar contrivance’. He clearly understood that the capabilities and limitations of the machine were in fact determined by the characteristics of the transducers connected to it—the mask into which one spoke, the capsule which was held against the larynx, and (especially) the so-called ‘tambours’, small drum-like pressure transducers, equipped with rubber membranes whose movements actuated the marking levers.

The history of the kymograph is thus not really the history of one instrument, but of a range of devices and methods typical of the era when the smoked paper and drum were the usual means of recording. The same articulatory, aerodynamic and acoustic measures were later obtained with other apparatus, and indeed continue to be collected to the present day. But with the replacement of the recording method (first by the pen reorder and subsequently by digital acquisition), the terms ‘kymograph’ and ‘kymography’ have fallen out of use.

The kymograph (German *Kymographion*, formed from Greek elements meaning ‘wave writer’) was developed for physiological and medical research by Carl Ludwig (1816–1895) and was widely used in this role from the mid nineteenth century. Ludwig is generally credited with the ‘invention’ of the device, although as detailed by Hoff & Geddes (1959), there are antecedents extending back at least to the seventeenth century. They identify the essential components of the system as:

> (a) the concept of self-registration of natural phenomena, (b) the clock-work-driven cylinder on which to inscribe the record of these events, (c) the smoked surface which so greatly reduced friction and made possible the greatest faithfulness in reproduction, and (d) the simultaneous registration of tuning forks or other timing devices.

Many of these components have British origins. The use of a recording drum, the inscription of the movements of a vibrating body, and the registration of small intervals of time, can all be traced to Thomas Young:

> By means of this instrument we may measure, without difficulty, the frequency, of the vibrations of sounding bodies, by connecting them with a point, which will describe an undulated path on the roller. These vibrations may also serve in a very simple manner for the measurement of the minutest intervals of time; for if a body, of which the vibrations are of a certain degree of frequency, be caused to vibrate during the revolution of an axis, and to mark its vibrations on a roller, the traces will serve as a correct index of the time occupied by any part of a revolution, and the motion of any other body may be very accurately compared with the number of alternations marked, in the same time, by the vibrating body.
>
> (1807, Vol. 1, p. 191)

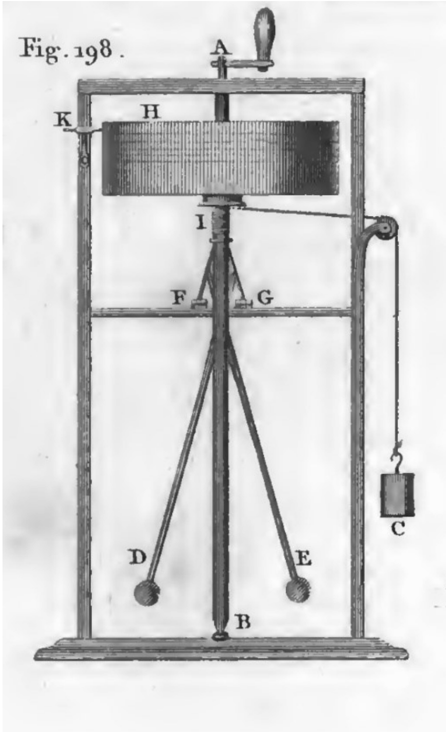


Figure 2.16 Young's 'chronometer' for recording events on a rotating drum. *D*, *E*, *F* and *G* constitute a speed regulator. The marking point is at *K*. The drum *H* descends during rotation as the thread *I* is unwound beneath it, producing helical traces.

Young's drawing of such a device is reproduced as Figure 2.16.[21] The French physicist Jean-Marie Constant Duhamel (1797–1872) may have been the first to use the technique of marking on a smoked surface (Hoff & Geddes 1959: 14).

Almost all of the other developments and refinements in kymographic technique as it was used in speech research can be attributed to Étienne-Jules Marey (1830–1904). An account of Marey's life is given by Braun (1994: 1–7). A brilliant student, he trained in medicine, but practised for less than a year in 1861 before turning to full time research on the physiology of movement, at first in his privately-funded laboratory but by 1867 at the Collège de France. Marey (1868) already contains a comprehensive account of the techniques of kymography (including practical details of the smoking of the drum and the varnishing of completed traces).

Ludwig's kymograph was unlike those later used in phonetic applications in that it used a direct mechanical link from what was being recorded (for instance, a manometer) rather than an indirect coupling accomplished by air pressure in tambours. Marey's first device—the sphygmograph—also depended on a physical linkage between the subject's skin and the recording point.

The tambour—arguably the most significant development in kymography as applied to speech—was devised by Marey at least as early as 1861 (Braun 1994: 18). At first, the tambours were used in pairs and operated as pneumatic relays to transfer motion from one point to another. The subsequent development of the tambour for speech purposes has two stages: first the use of a single tambour as a transducer for air pressure variations led to it by a tube (e.g., from a nostril), and later the realisation that the membrane of a tambour was akin to that of the phonoautograph and could respond in some measure to acoustic vibrations as well as to slow pressure variations.

The application of the kymograph to speech research is generally traced to a report by Rosapelly (1876). Charles-Léopold Rosapelly (1843–1919) was a physician who had already published physiological research which made use of the kymograph (Rosapelly 1873). According to his own account of events, a delegation of the Société de Linguistique de Paris approached Marey early in 1875 with the specific question whether the graphic methods which Marey had developed and was applying to movement of all kinds could be applied to the complex movements of speech. Marey was of the opinion that they could, and entrusted an investigation of the possibilities to Rosapelly, who collaborated with Louis Havet (1849–1925), a linguist chosen by the Société (Rosapelly 1876: 109–112).

They evidently worked very quickly, and Rosapelly (1876) describes a sophisticated arrangement for three-channel recording of speech activity, representing larynx vibration, lip action and nasal airflow. They designed a device to track lip movements, linked pneumatically to a recording tambour, and also devised what amounts to a make-and-break electrical accelerometer to detect larynx vibration. They had wanted also to record tongue movements, and the paper anticipates the possibility of dynamic palatography (p. 124) utilising multiple palate sensors linked pneumatically or electrically to a recording device, though this was not realised in practice at this early stage.

The results are presented in a striking tableau (reproduced here as Figure 2.17) which effectively decomposes the sounds they selected for study into their component features.

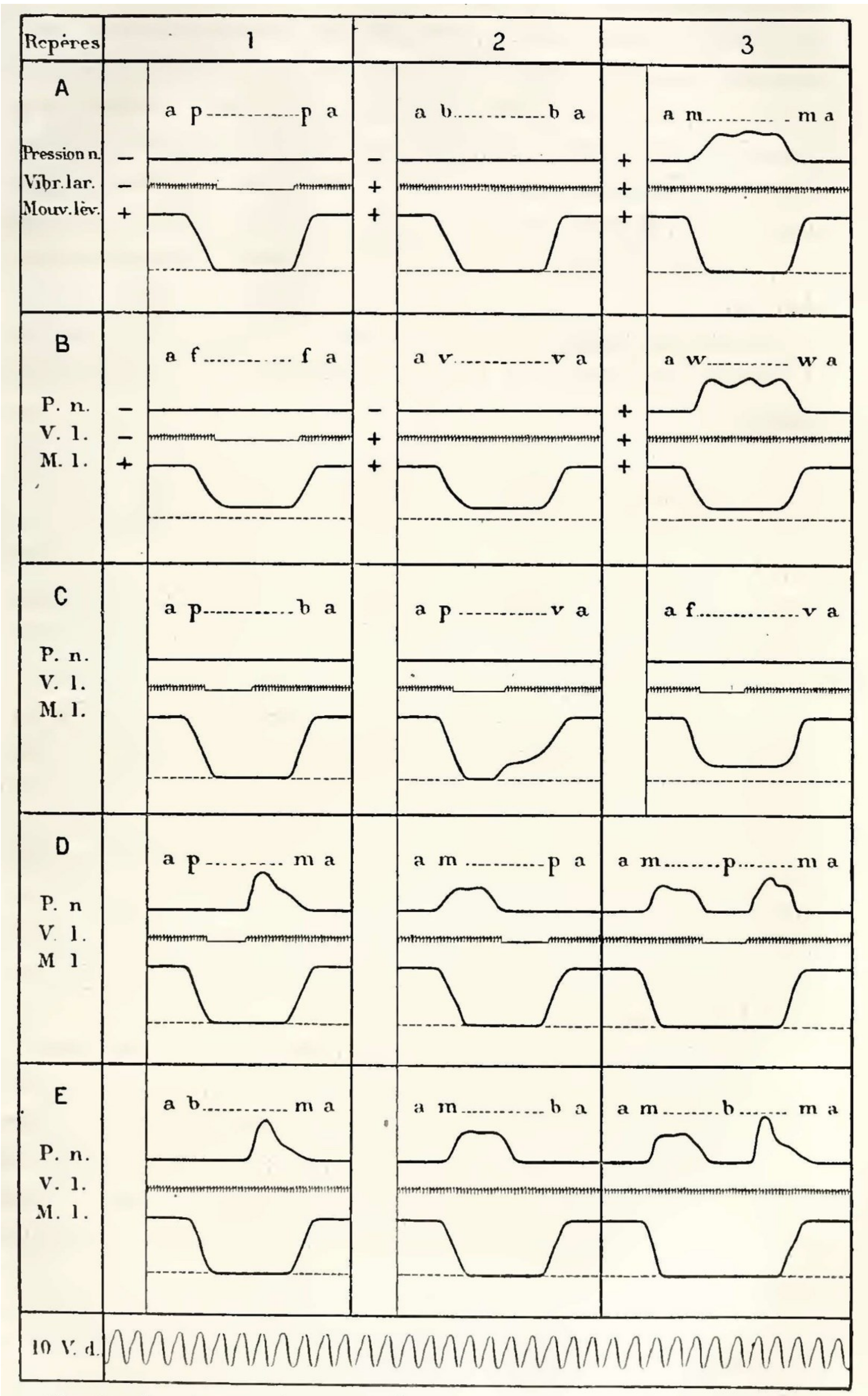


Figure 2.17 Kymograms published by Rosapelly (1876). The timing trace (bottom) was made with a 10 Hz tuning fork.

The most characteristic and widely used kymographic procedure—particularly in Britain—became the recording of a 'mouth' channel from a mask-like mouthpiece. This channel, as will be shown below, corresponds to a somewhat variable mixture of the acoustic signal with a measure of airflow from the mouth. This is the one channel used by Edwards (1903), the one most commonly represented and discussed by Daniel Jones (see Figure 2.18), and the single channel that Scripture continued to use in many papers published into the 1930s.

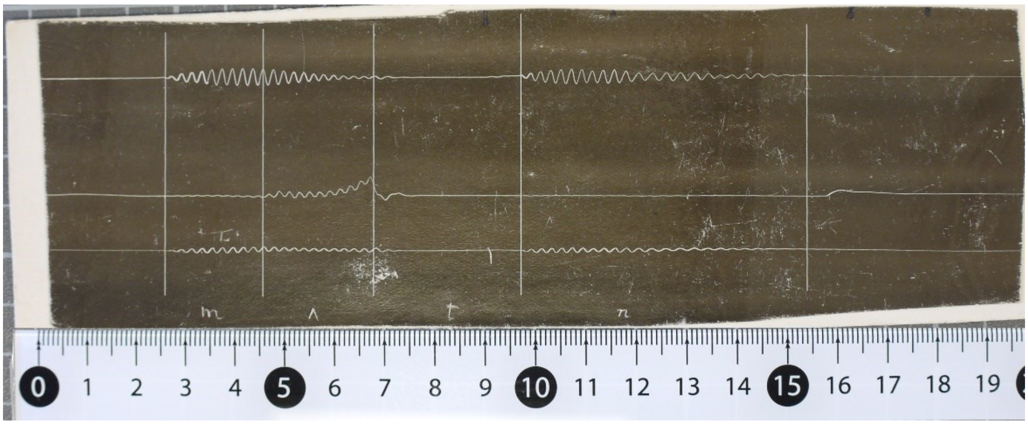


Figure 2.18 A 3 channel kymogram by DJ of the word *mutton*, photographed from the original (in the Daniel Jones papers). From top: nose, mouth, larynx (the standard order followed at UCL). A figure prepared from this kymogram appears in DJ's *Outline* (1918: 178).

The 'mouth' channel, however, was not among the early techniques. It is not used by Rosapelly (1876) (he actually recorded no representation of the acoustic speech waveform at all: the signal from his larynx accelerometer would have been a clipped wave of constant amplitude). Rousselot added a speech signal, but at first he used an electrical method. A mouthpiece led to a carbon microphone, which drove an electromagnetic marker of his own design (1891: 16). But Rousselot makes hardly any use of the resulting curves. The same device is also covered in the *Principes*

(1897: 127–131). It is likely that a very limited frequency range was achieved, and that both the microphone and electromagnetic marker introduced considerable distortion.

The first to report the use of a Marey tambour to detect acoustic vibration in the speech wave may have been Viëtor. He published a short note in *Le Maître Phonétique* which can be translated as follows:

> I have now had the opportunity to examine graphical representations of vocal fold vibration in all types of speech-sound, using Philip Wagner's[22] apparatus—that is to say, a kymograph with a tambour of Marey type. Voiceless sounds have a smooth line, voiced sounds wavy curves. The indication is clearly visible on the airflow curve. No additional apparatus is needed. You just replace the hose that has a funnel with a short piece of rubber tubing that passes between the lips while one is speaking. In all other ways, the apparatus is used exactly as by Wagner or Rousselot. Low pitch voice vibrations are seen very easily, and even at higher pitches vocal fold vibration is clearly visible to the naked eye. The number of vibrations per second defines the pitch, of course. The method makes it possible to determine whether glide sounds are voiced or voiceless.
>
> (1893: 147)[23]

'No additional apparatus is needed' probably refers to the complex arrangement of carbon-microphone and electromagnetic marker used by Rousselot. This note drew a somewhat hostile and condescending response from Rousselot, again in the pages of *Le Maître Phonétique*. Viëtor went on to publish a more detailed account in *Phonetische Studien* (1894), adding also an account of his investigations with palatography.

When Rousselot returns to the question of registering voice frequencies with a tambour (1897: 81–86), he is rather more generous towards Viëtor, and even reproduces one of Viëtor's figures (1897: 83), but is still careful to state that his own experiments preceded Viëtor's (he gives a precise date of 4th April 1886 for the starting point), and also to insist that Viëtor did not really understand the reason for his success.

Viëtor believed the key was his use of a tube passing into the mouth, but Rousselot thought the likely cause was the favourable condition and adjustment of the tambour Viëtor happened to use and that this was probably entirely accidental.

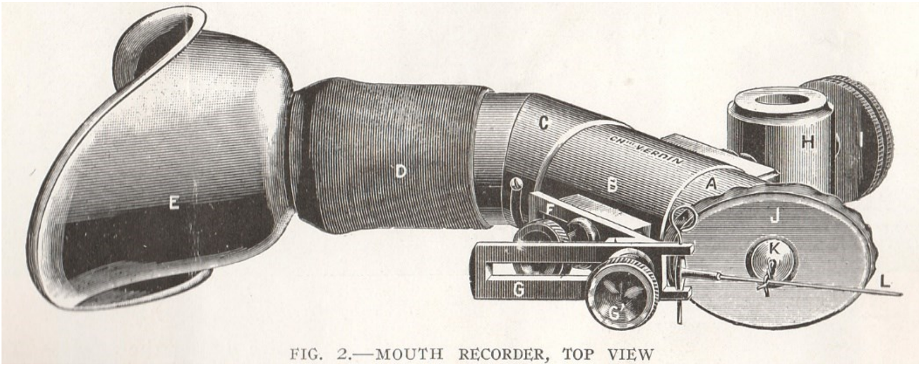


Figure 2.19 Rousselot's mouth tambour ('l'oreille inscriptrice'). Photographed from DJ's copy of Scripture (1913b). The source is probably the catalogue of the maker, Charles Verdin. Scripture calls it a 'mouth recorder'.

Rousselot went on to develop a special mouth tambour, and published a separate account of this device (1899). It drew on his experience of the behaviour of different sizes and types of membrane, but was developed largely by trial and error. Rousselot termed it 'l'oreille inscriptrice' (the writing ear), since some aspects of its design were copied from the human ear (Figure 2.19). The oval shape of the membrane, and its oblique orientation across the tube, supposedly imitate the tympanic membrane. In some respects, the sensitive tambours developed for speech by the British firm of C. F. Palmer appear to be modelled upon this device.

Although the general practice became that of adjusting a tambour in a compromise fashion so that it responded both to gross pressure variations and to acoustic vibration, Rousselot was well aware that the requirements for tambours to register the two quantities were different. Experiments were made with double sided tambours, having two diaphragms of different characteristics, and also with use of a Y-piece to lead the mouth air to two tambours of different characteristics working in parallel. Interestingly, one of the photographs in Curry (1934) shows this latter arrangement very clearly, though the text does not appear to contain any related discussion (See Figure 2.20).

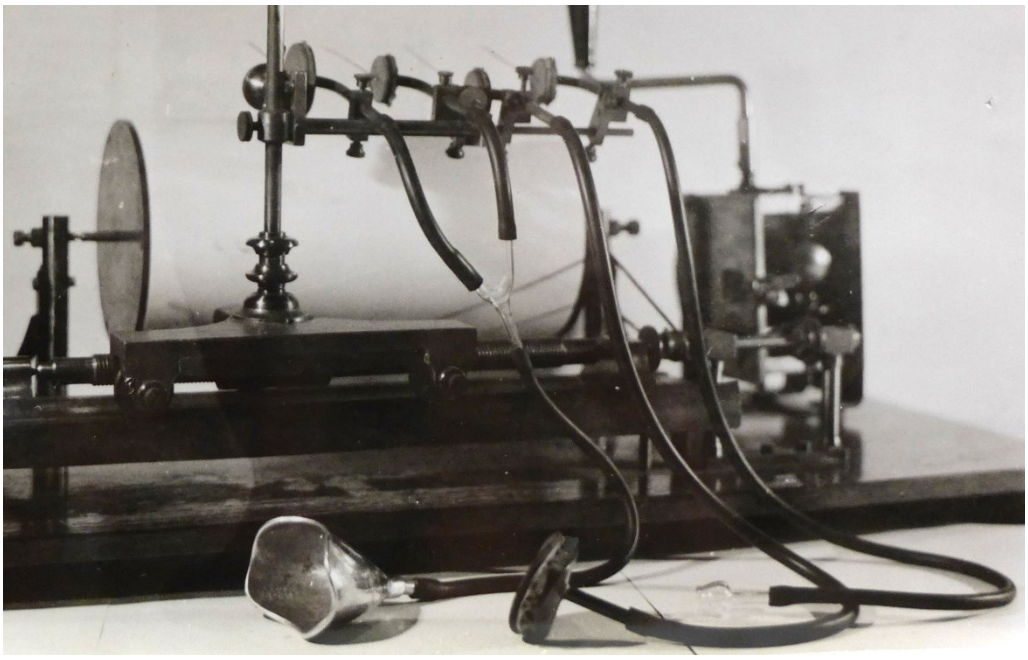


Figure 2.20 The kymograph used by Robert Curry (1934a: 10, detail). The tube from the mouthpiece leads to a Y-piece made from glass. The left branch leads to a larger diameter tambour, and the right to a smaller one.

The question remains of exactly what the ‘mouth’ channel represents. As suggested already, the result is in effect an unsystematic blend of aerodynamic and acoustic properties, though in the contemporary literature the two are not adequately distinguished. Rousselot writes ‘La parole nous présente deux ordres de phénomènes à inscrire: la parole elle-même et les mouvements organiques qui la produisent’ (Speech presents us with two different kinds of phenomena to be recorded graphically: speech itself, and the movements of the organs which produce it’) (1897: 79). This separates the physical activity of speech production from ‘speech itself’ (*la parole elle-même*), but appears to imply that the latter includes both the aerodynamic and the acoustic results of speech movements. A mask over the mouth collects air flow, as well as acting as the input to an acoustic speaking tube. Air flowing into the device will raise the pressure inside, and unless air can also escape, pressure will continue to rise as the device is effectively inflated. If air *can* escape, pressure inside the device will rise according to the resistance to flow offered by the means of escape, and the time-varying pressure in the system will be a measure of the rate of flow through the device. There are only scattered indications in the early literature of the need to provide systematically for the escape of air. Scripture says ‘A tambour with a mouth-piece communicating with the external air may be used to indicate the rate of expenditure’ (1902: 220). In his discussion of factors which favour the registration of vibration, Rousselot (1897: 132) mentions making a small hole in the side of the tambour (‘Aussi, un petit trou pratiqué sur les bords du tambour favorise-t-il l'inscription’). In practice, however, air probably escaped irregularly around the edge of the mask, and at intervals via the nose when the velum was opened.

There are in principle three possibilities, which are illustrated diagrammatically in Figure 2.21. (1) Either the input transducer is such that there is no net flow into the device, or any air which does enter may escape through a resistance sufficiently small as to produce no elevation of pressure. Both result in the registration of only the alternating component of the signal. (2) Air may escape through a significant but approximately constant resistance, so that the alternating component is superimposed on an indication of flow. (3) There may be a (short-term) sealing of the system, so that the alternating component is superimposed on a steadily rising contained pressure.

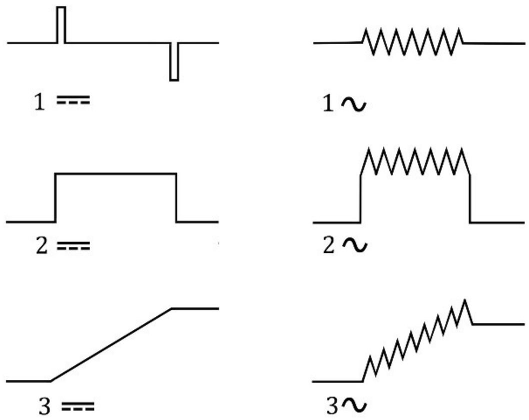


Figure 2.21 Theoretical response of a tambour in the three numbered conditions identified in the text (rows 1–3) to an interval of uniform steady flow (left column) or of alternating (voiced) flow (right column).

In Figures 2.22 and 2.23, each of these theoretical possibilities is illustrated with a real example from a single published study (Kanehiro 1933), showing how the venting of air must have fluctuated erratically in practice, even whilst the same apparatus and procedures were in use. (Similar examples would be easy to multiply from other sources).

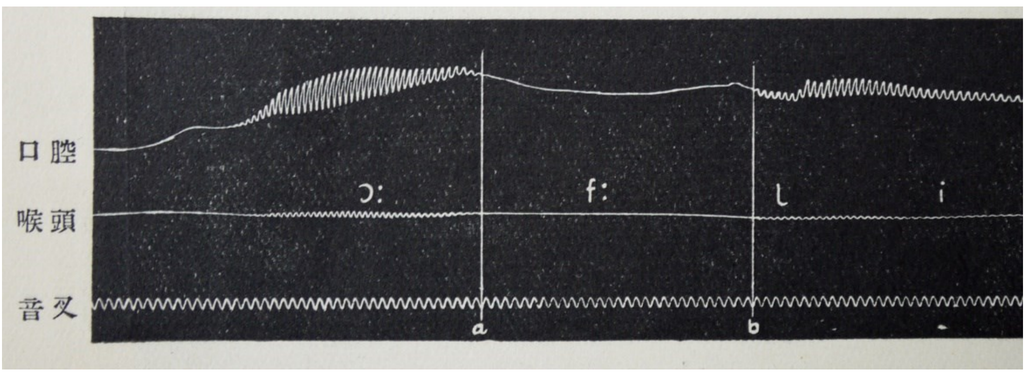


Figure 2.22 Kymogram of *awfully*, appearing to show progressive inflation of the mouth tambour during the first segment. From top: mouth, larynx, timing track. (Kanehiro 1933: 254, detail).

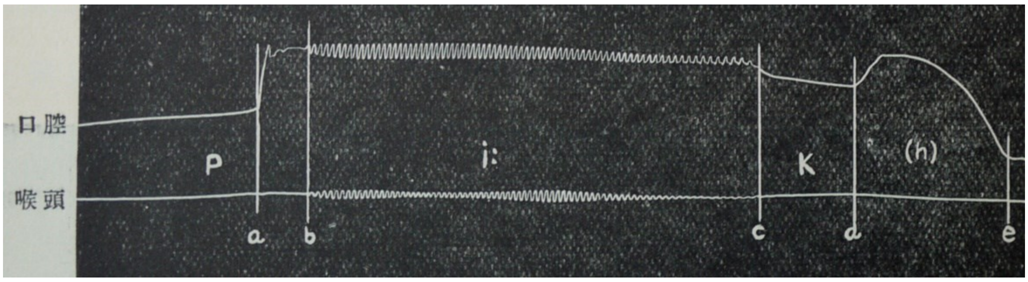


Figure 2.23 Kymogram of *peak*. From top: mouth, larynx. At the release of [p], the beginning of voiceless flow is indicated by an upward step change in pressure. When vocal fold vibration begins, it is superimposed on the elevated level (Kanehiro 1933: 150, detail). Both here and in Figure 2.22, the larynx trace is nearly pure a.c.

It would have been possible to produce more consistent behaviour for the ‘mouth’ channel,[24] but no systematic analysis seems to have been attempted. Stetson perhaps comes close:

> The technic of Rousselot was followed for the tracings of pressure just outside the mouth. A firm rubber mask was fitted about the mouth, an escape valve of some type was provided near the mask, and the pressure was led to a 5 cm. tambour of very thin, taut rubber like the tambour used for the tracings of the pressure in the mouth. The pressures were regulated by changing the escape valve in the line from mask to tambour.
>
> (1928: 23–24)

In an extraordinary autobiographical piece, Scripture confesses that he doesn’t really understand:

> For thirty years I applied the graphic method of making speech inscriptions (point recording on a moving smoked surface) with a receiver held closely over the mouth to problems of various kinds. One day I realized that I did not know the nature of what I was recording. This led to an understanding that physical speech consists of two parts. The familiar one that we hear consists of the waves that are propagated outward from the speaker. These consist of minute vibrations of the air particles, and they may be appropriately termed the microphonic factors of speech. The other part consists of the currents and puffs of air that issue from the mouth and nose and die away immediately in front of the face. These mass movements of air constitute the macrophonic factors of speech. They produce the microphonic speech. Macrophonic speech is what is registered by the graphic method of Rousselot. This method gives valuable information that can be

> obtained in no other way. It is the basis of most of the work done in experimental phonetics.
>
> (1936a: 347)

He seems to be close to making explicit a distinction between acoustic and aerodynamic aspects of speech. But the phrase ‘the currents and puffs of air’ falls far short of separating even such basic ideas as *flow* and *pressure.*

A clear understanding of the difference between the aerodynamic and acoustic aspects of speech is, however, to be found much earlier in the little-known British work of W. H. Barlow, which actually preceded that of Rosapelly.

2.4.1 William Henry Barlow (1812–1902)

Barlow was a structural and railway engineer, responsible, among many other achievements, for the 240-foot span roof at St Pancras station in London, regarded as ‘one of the triumphs of Victorian structural engineering’ (Chrimes 2004). Alongside his engineering design work, Barlow published numerous scientific papers, and he was elected a member of the Royal Society in 1850.

His 1874 paper, delivered to the Royal Society in April of that year,[25] describes an instrument to record the ‘pneumatic actions’ which accompany speech. It consists of a mouthpiece connected to a membrane diaphragm, which in turn moves an aluminium lever carrying a brush fed with ink. This records on a strip of paper ‘in the same manner as that employed in telegraphy’ (devices such as the siphon recorder, which marked telegraph messages in the form of undulating curves inked on moving paper tape were by then the norm).

Barlow shows a unique understanding of the relationship between flow and pressure in a mouthpiece:

> To provide for the escape of the air passing through the instrument, a small orifice is made in the side of the tube of the speaking-trumpet, so that the pressure exerted upon the membrane and its spring is that due to the difference arising from the quantity of air forced into the trumpet and that which can be delivered through the orifice in a given time.

Barlow's device thus consists of a pressure sensor in a mask provided with an escape of fixed resistance, and hence it measures oral flow.

Barlow examined both isolated words and stretches of connected speech. A sample of output is reproduced as Figure 2.24 (interestingly, though inking on paper tape would produce a black-on-white trace, the 1874 paper uses white-on-black reproduction).

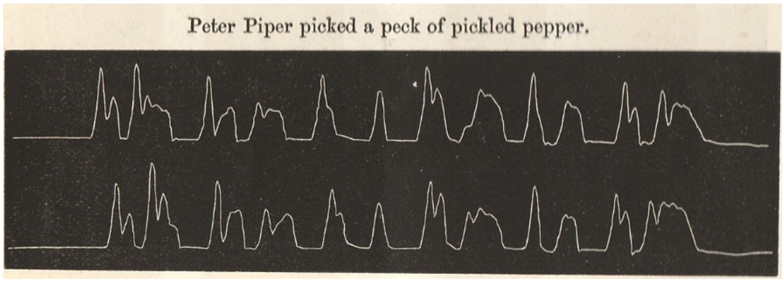


Figure 2.24 Two repetitions of *Peter Piper picked a peck of pickled pepper* as registered by Barlow's logograph (1874: 282, detail; actual size as printed)

Barlow works out average rate of expenditure of air in speaking (p. 279) and his speaking rate, which he determines as 'rather more than four syllables per second, including stops' (it is plain that by 'stops' he means pauses). Without stops (pauses), he obtains 5 to 6 syllables per second.

Barlow notes variation in the length of certain segments within a sequence, according to context, and also observes and illustrates—probably for the first time—what would now be called 'polysyllabic shortening' or 'rhythmic clipping' (White & Turk 2010; Wells 2008: 155). See Figure 2.25.

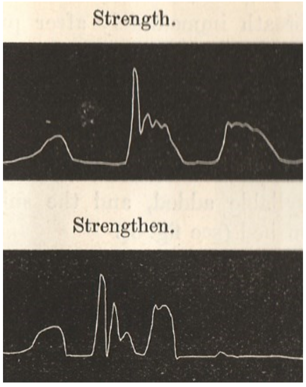


Figure 2.25 Comparison of *strength* and *strengthen* (Barlow 1874: 282, detail).

Since Barlow's logograph will give zero output for nasal consonants, the added [n] of *strengthen* will not add a further visible excursion to the curve; hence the three periods of flow—representing [s], [re] and [θ] respectively—must correspond in the two curves and the time compression in *strengthen* is clearly evident.

This earlier paper contains no treatment of vowels, and there is no mention of vocal fold vibration or voice. The results show only relatively smooth flow curves. In its first form his device seems to have been particularly responsive to rapid flow at the release of plosives. He distinguishes (p. 283) between words where relatively low rates of flow continue for an extended time (he calls these 'Words of quantity'—none have plosives) and those showing spikes with high rate of flow ('Words of Intensity'—all have plosives).

Barlow's later paper (1880, but delivered in 1878) describes the same device, now calling it the 'logograph', and gives a diagram of its construction (reproduced as Figure 2.26).

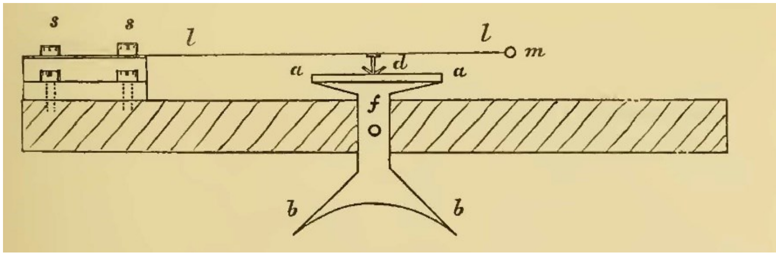


Figure 2.26 Schematic section through Barlow's logograph (1878: 153). A sprung lever *l l,* carrying a marker *m*, is linked by stud *d* to a rubber membrane stretched across *a a*. The mouthpiece is *b b*. The orifice *f* is the crucial airflow vent. Barlow's earlier paper tells us that the membrane *a a* was 2.5 inches in diameter (1874: 277).

In this 1878 paper the results are reproduced as black on white. Here he takes a more systematic approach, analysing the phases of an intervocalic plosive and attempting to characterise the appearance of various other classes of consonant (pp. 155–159). He also describes different arrangements of the membrane and marking lever required for vowel sounds, and observes that vowels show vibration, but one vowel cannot be distinguished from another on the basis of the waveform: 'The vibratory action in the Logograph appears to depend mainly on the pitch of the fundamental note, and to be but little affected by those harmonics and their compounds which have been shown by Helmholtz to govern the differences in vowel sounds'. He was aware that the membrane and lever combination must exhibit resonance, and says that by singing a scale, one can discover the resonance, and then phonate at that pitch to get maximum effect: 'the best exhibition of vowel action is obtained by speaking in a monotone upon such note when discovered by trial'.

Whereas the earlier paper has no references whatever to previous work, the later paper mentions the names of Helmholtz, (Max) Müller, Clavigero,[26] König, and Eduard-Léon Scott (inventor of the phonautograph), and refers to Walker's pronouncing dictionary. Some of Barlow's phonetic terminology is explicitly credited to Müller (presumably Müller's *Lectures on the science of language*).

He makes a comparison with the 'indicator diagram' used by Watt to study the action of a steam engine—precisely the same source of inspiration mentioned by Ludwig (Bud & Warner 1998: 344). No link with Marey has yet come to light, though the work of Marey was certainly known in Britain. The journal *Nature* alone has more than 20 mentions of his name in the years 1870-1873, and it seems improbable that Barlow can have been entirely unaware of Marey's work. A lengthy obituary of Barlow (Anon 1903) makes only brief mention of his work on speech, and throws no further light on the matter.

Though the work of Rosapelly was the foundation of all later kymographic work on speech, the fact remains that the earliest 'kymograms' of speech appear to be those published by Barlow in 1874, though of course these were of a single channel, and obtained with somewhat different apparatus.

Barlow's work on speech has not been entirely forgotten but it seems not to have been properly understood and appreciated. Chrimes (2004) is wrong in saying that the logograph 'recorded sound waves graphically'. Barlow is very clear that he is concerned with the 'pneumatic' action accompanying speech—that is, with aerodynamics rather than acoustics. Rousselot also seems to have misunderstood Barlow's purpose. He too treats the Logograph as an acoustic waveform-recording instrument, dealing with it in the same section of the *Principes* as the manometric flame and the phonautograph (1897: 112–113).

Panconcelli-Calzia (1940: 51) correctly credits Barlow with having published the first kymograms of speech, and reproduces some examples, but comments that the device 'was far from sensitive' (*der Apparat weit davon enfernt war, empfindlich zu sein*). This seems unfairly harsh, applied to a pioneering effort. He may perhaps mean that it failed to register any acoustic (larynx) vibration, but if so, his further comment that the 1878 results are 'just as bad' (*ebenso schlecht*) is hard to understand, since in some cases the later results do show larynx vibration.

2.5 X-rays

Wilhelm Conrad von Roentgen announced his discovery of X-rays in December 1895. News of the discovery spread quickly. The apparatus needed to produce the new rays was existing technology and widely available, so that in a matter of weeks X-rays were being demonstrated and investigated in numerous centres around the world. All the essential components had in fact originated in the British Isles: the Crookes tube (Crookes 1879), the induction coil with interrupter, generating many kilovolts from a low-voltage supply (Callan 1837), and the dry plate photographic process (Maddox 1871).

The German laryngologist Max Scheier (1864–1921)[27] is generally credited with having first pointed out the potential value of X-rays in speech research (Scheier 1897), though his method was direct observation on a fluorescent screen, and he seems to have published no photographs.

2.5.1 John Macintyre (1857–1928)

What may be the earliest surviving X-ray image of any part of the vocal tract (Figure 2.27) was made by the Glasgow laryngologist John Macintyre as early as April 1896 (Macintyre 1897).

A brief account of Macintyre's life and career is given by Calder (2001: 2–4). Macintyre demonstrated X-rays to the Glasgow Philosophical Society in February

1896, and by March had obtained permission to set up at the Glasgow Royal Infirmary what was certainly the first clinical X-ray department in the world.

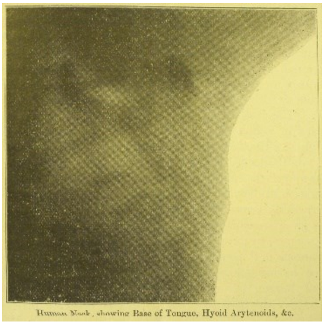


Figure 2.27 Lateral X-ray of the base of the tongue and larynx area (Macintyre 1897: 49). (Photographed from the dot-screen image as published).

The earliest X-rays revealed bones, and metallic foreign bodies such as bullets, but numerous potential applications depended upon the imaging of soft tissues. Macintyre achieved some success in this, and attempted to determine the conditions which favoured it (such as particular adjustments of the Crookes tube). According to Hull (2004), this aroused the interest of Roentgen, who wrote to Macintyre requesting details of his technique as early as March 1896.

Macintyre's place alongside Scheier as an early advocate of X-rays in laryngology is now recognised (Weber 2001), but unlike Scheier, Macintyre does not seem to have angled any of his copious output specifically towards a linguistic audience. So far, no evidence has come to light of contact between Macintyre and the world of phonetics, either in Britain or on the Continent. Probably it is the lack of that connection that has led to his being overlooked in the history of speech research.

2.5.2 Appearance of early X-rays

The earliest X-ray image of the whole vocal tract which the author has been able to locate is reproduced as Figure 2.28 (Scheppegrell 1898: 368).[28] In the late 1890s, phoneticians would generally have viewed X-rays of the speech organs in the form of live images on a fluoroscope screen rather than in photographs, but this still image from 1898 is probably reasonably representative of what they might have seen.

Figure 2.28 Very early lateral X-ray of the head (by Scheppegrell, 1898).

Features such as the tongue or soft palate which were barely more than faint shadows on the X-ray photographs of the day would perhaps have been somewhat easier to recognise when seen in motion, but even so, it is evident that definition was severely limited. Jespersen encountered X-rays in 1897:

> An exhibition was being held in Leipzig, where for the first time I saw X-ray pictures, e.g. of the bones of my hand. This was something quite new, and when after my return I heard that the municipal hospital in Copenhagen had acquired an apparatus, I was several times allowed to do experiments with it. My idea was that it might be used to investigate tongue-positions for vowels and the like,

> but the stage of the technique at that time did not make it possible to obtain anything from it of value for phonetics.
>
> (Jespersen 1995: 107)

X-rays get no more than a mere mention in Scripture (1902). His chapter on 'Tongue positions and movements' ends with what seems like a lame afterthought: 'The tongue in action may be observed by means of the Röntgen rays' (1902: 337). As yet, there was no more to report.

2.5.3 Application to speech

Macmillan & Keleman (1952) give an extremely valuable account of the early application of X-rays to speech research (though their account makes no mention of either Macintyre or Daniel Jones). The main problem remained that of enhancing the contrast so that soft tissues could be photographed. Various techniques were evolved, but in general terms they may be divided into (1) the introduction onto the tongue (and sometimes elsewhere) of radio-opaque markers—metal plates or chains, and (2) the coating of the soft tissue surfaces with a paste or powder (generally based on barium compounds or bismuth). Fixation of the head, essential if accurate measurements are to be made from the resulting images, was not at first universal, though some of the earliest studies did indeed make use of it.

In the early years of the twentieth century, the X-ray study best known to phoneticians was probably that of Meyer (1907). He published vocal-tract outlines for a range of sounds, which were certainly employed in teaching by Daniel Jones (see Chapter 5). Meyer used a ladder of thin metal plates on the tongue, though he failed to stabilize the subject's head position.

Some twenty years were to elapse between Macintyre's 1896 image of the larynx and the first serious use of X-rays for speech research in Britain. During this time radiography became established as a medical specialism, and both apparatus and techniques underwent considerable refinement. A good idea of British hospital practice around the time of WW1 can be gained from the textbook of Knox (1917).

2.5.4 Barclay & Nelson

The earliest British work in point of time was probably that done in Manchester by Barclay and Nelson, though it was not to be published until some years later (Barclay & Nelson 1922). See Figure 2.29. Alfred Ernest Barclay (1876–1949) was 'one of the oldest pioneers in radiology in England', who became 'well-known and highly esteemed throughout the world' (Edling 1949). His prolific teaching and research career included posts in both Cambridge and Oxford. Conditions during his time in Manchester are described in Elwood & Tuxford (1984: 127–130).

Of 99 research publications listed in the bibliography given by Edling (1949), this paper from 1922 is, however, the only one concerned with speech. His co-author William Nelson OBE (1862–1926) was an influential figure in the education of the deaf, and by this time Headmaster of the Royal Schools for the Deaf in Manchester. Like Barclay, he seems to have no other publications specifically concerned with speech. His role in the work was probably that of one experienced in teaching speech.

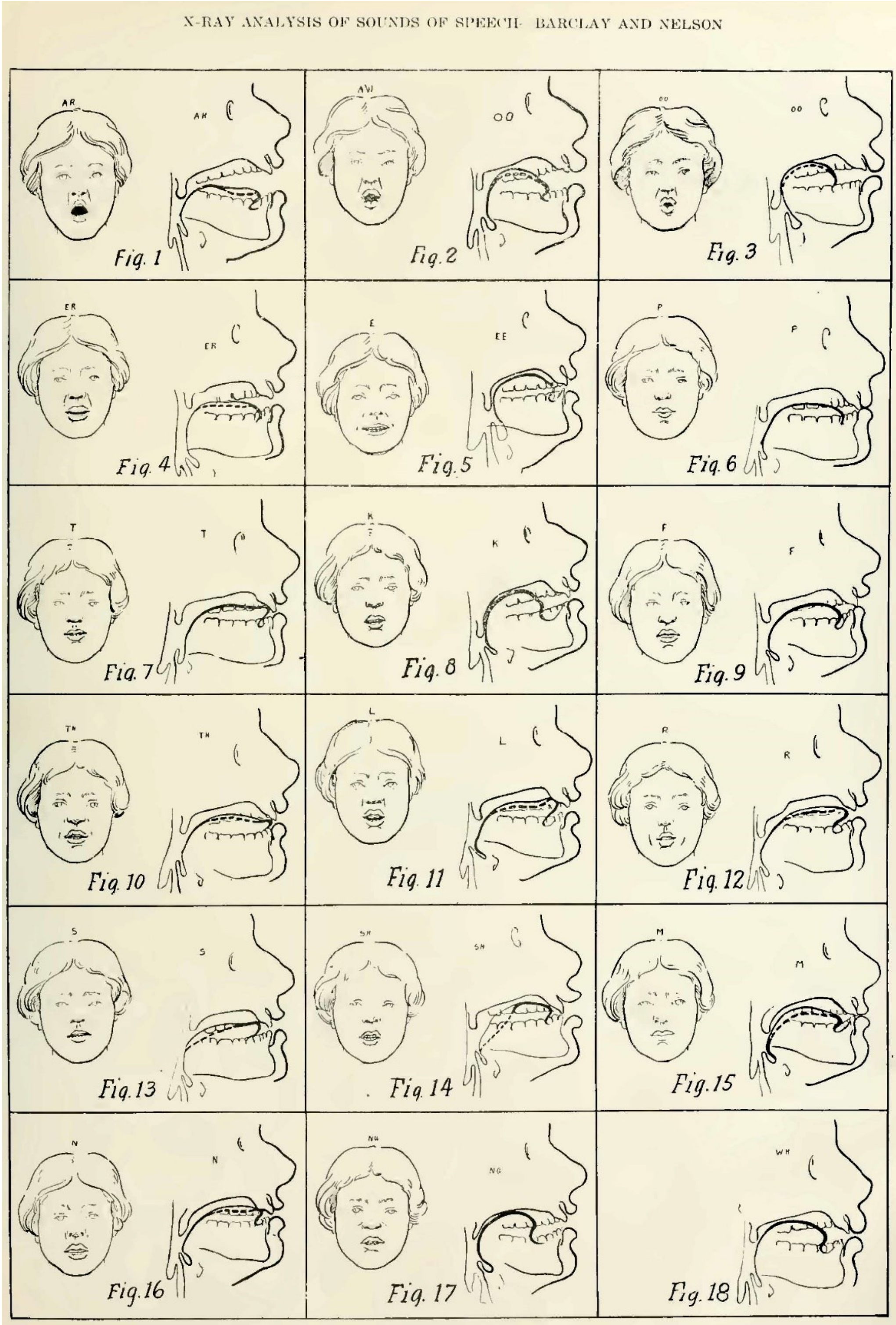


Figure 2.29 Chart of English sounds prepared by Barclay and Nelson from X-rays made in the period 1914–1918 (1922: 179)

The motivation for Barclay and Nelson's study was the treatment of 'functional aphonia' in war casualties. For this, they found the existing charts of articulation then in use 'not satisfactory' because they were based on impressionistic data. They believed that more accurate data (obtained from X-rays) would improve treatment.

Barclay and Nelson enhanced the contrast of their images by painting a mixture of bismuth carbonate and Vaseline on the mid-line of both tongue and soft palate; and also puffed fine bismuth powder on to the surface of the tongue and pharynx. The single photograph reproduced in the paper (for an unidentified sound) certainly shows a very dark outline on the tongue blade.

They used two subjects (teachers from the Royal Schools), and examined a range of 18 sounds. The vowels were [ɑː ɔː uː ɜː iː]. The omission of front vowels [e] and [æ] is unexplained, but may have been on account of the relative openness of the articulations, which makes the tongue position fairly visible anyway. Barclay and Nelson evidently assumed that it was unnecessary to examine both members of voiced-voiceless pairs of consonants, the total set examined being [p t k f θ l ɹ s ʃ m n ŋ w]. They claimed to find little variation between the tokens from their two speakers. They selected the 'best' plate in each case, and outlines were drawn by an artist, who added a front view with lip position. This appears to show a female speaker, though Barclay and Nelson do not specify whether their speakers were male or female. The paper includes brief verbal descriptions of the articulatory configuration for each sound.

The study attempted by Barclay and Nelson was relatively ambitious in scope, and the very brief and belated report which they made in 1922 is explained as resulting partly from the work being 'put aside' until they were able to get better images for publication, and partly from the loss of all the papers relating to the subject (they happened to be among items taken in an opportunistic theft from Barclay's car).

Barclay and Nelson do not say whether their results were ever applied as intended in remediation or teaching, or if the hoped-for benefits of greater accuracy were realised in practice. The premise of their work—that greater accuracy of representation will lead to more successful learning—has been widely shared, but seems inherently questionable. It has never in fact been investigated whether sounds can be learned from *any* kind of diagram, still less whether anything but the grossest features could be of use. How is the student imagined as adjusting his or her vocal tract so as to match a model pattern in detail? Still, the gallery of images given by Barclay and Nelson belongs to an extensive genre, and the preparation of such model images was to remain one of the motivations for X-ray studies for several decades.

Barclay was present when Stephen Jones delivered his 1929 talk 'Radiography and pronunciation'. In the discussion following that paper, he praised the quality of the images shown by SJ and somewhat deprecatingly referred to his own earlier work.

2.5.5 DJ's X-rays of 1917

Other than 'during the war', Barclay and Nelson do not specify the date of their original study. The work done by Daniel Jones, Stephen Jones and H. Trevelyan George was first published by DJ early in 1917, and the celebrated Cardinal Vowel photographs were taken in January of that year, though it is unknown whether work might have been in progress before that (see Chapter Five). A number of other sounds were photographed at the same time.

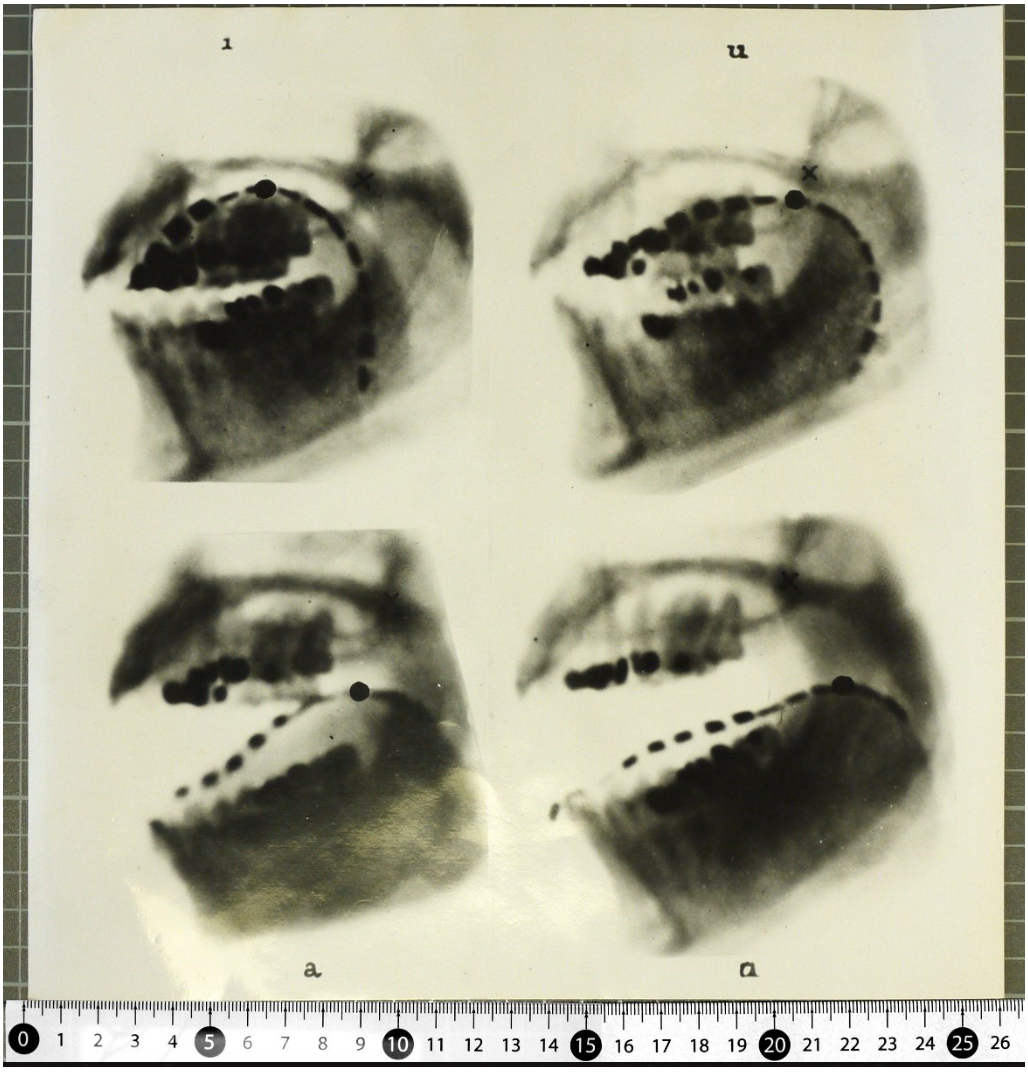


Figure 2.30 X-ray print of the Cardinal Vowels (1917) (Daniel Jones Papers)

What survives is a silver gelatin print approximately 264 mm × 260 mm (10.4 in × 10.2 in), with a collage of four juxtaposed positive images showing Daniel Jones's articulation of the Cardinal Vowels 1, 4, 5 and 8. Collins & Mees (1999: opposite p. 159) call these 'The original X-ray photographs of the Cardinal Vowels'. The 'originals', however, would be in the form of four separate X-ray negatives on glass plates. Early reproductions of the Cardinal Vowel images (e.g. Jones 1917a) show them separate (not in the form of a collage). It is probable, then, that the four-image composite was prepared at a date later than 1917. It might indeed have been made as late as 1950 (the date of its first publication) though DJ's notes indicate that the frontispiece was first envisaged at some point between 1922 and 1932.

An attempt has been made to fix the date of the print from the paper it is printed on, taking note of information in Messier (2010). It was examined by UV illumination (on 4 June 2014), along with control samples of photographic papers from a range of dates. The paper was found not to contain optical brighteners. The paper does not have any manufacturer's markings on the rear. These findings are inconclusive, being consistent with any date from 1920 to 1950.

The measurements reported in Chapter 6 were made on a photographic copy made by the author with a resolution that provides about 7.44 pixels per mm of the original (about 189 dpi). The resolution achieved is equivalent to improving the familiar frontispiece of the *Outline* to approximately 450 dpi, while simultaneously removing the dot screen pattern used for book reproduction.

Because subsequent interest has largely been focused on the Cardinal Vowel system itself, the technical quality and early date of these photographs has previously been overlooked. For their time, they are of high quality—as good as, or better than, any other X-rays of speech sounds published until then.

DJ (1917a) tells us that the X-rays were taken by 'Dr. H. Trevelyan George', and the frontispiece to the eighth (1958) and later editions of the *Outline of English Phonetics* adds the further information that the photographs were taken in January 1917. Beyond this, nothing has hitherto been known about 'H. Trevelyan George', but is possible to piece together an outline of his life.

2.5.6 Howard Trevelyan George (1873–1929)

Trevelyan George was born in 1873 in ‘Hirwain’ (now usually Hirwaun) a village in South Wales to the north of Aberdare and about 7 miles west of Merthyr Tydfil. In the 1881 census, his father John Evan George is described as a ‘chemist and druggist’. He was educated at Cardiff College School, and then at the University College of South Wales. He was admitted at Gonville and Caius College, Cambridge, in October 1894, and took the Natural Science Tripos in 1897. He gained the LRCP and MRCP in 1904 and was registered as a medical practitioner in the same year. By 1911, described as a ‘physician and surgeon’ he was living in Ampthill Square, St Pancras. In 1913 he was elected a member of the Royal Institution.[29] He was variously ‘Clinical Assistant’ and ‘Senior Chief Assistant’ in the X-ray Department at St Bartholomew's Hospital. He published a number of short papers on radiographic topics in the years 1916–1918. In 1919 he is among those listed as being elected to the British Association of Radiology and Physiotherapy (founded in 1917), and has made a donation of £25.[30]

It is unknown why the UCL phoneticians made their X-rays at St Bartholomew’s Hospital, rather than University College’s own hospital or the Middlesex Hospital, which is also close nearby. Perhaps there was a prior South Wales connection between H. Trevelyan George and SJ. They were close contemporaries, and both were alumni of University College Cardiff. Another possible connection could be via the Royal Institution (both H. Trevelyan George and DJ were members). In whatever way the association may have come about, H. Trevelyan George would seem to have had unique qualifications for the task. The three publications which have been located (1916a; 1916b; 1918) all deal with X-ray imaging of the head, and specifically with the making of accurate measurements. For example, the opening paragraph of (1916b) is:

> The little device described in this article is one for ensuring a true lateral view of the head, or in other words, for ensuring that the median sagittal plane of the head shall be accurately perpendicular to the vertical line drawn through the focal point on the anticathode.

He was also engaged in research that remained unpublished, as we learn from an internal report on the management of the X-ray department at St Bartholomew's, written by H. Trevelyan George in late 1917.[31] He had been asked to take over as Temporary Head of the X-ray Department, and the report contains his proposals for dealing with the under-resourcing and under-staffing that were severely affecting the department in the midst of the war.

> At this point I think I ought to explain my own position in regard to attendance at the Dept, during the first half of the week, viz. Mondays and Tuesdays. For some time past I have been engaged on Research work for the Ministry of Munitions. The object of the research is the improvement of the manufacture of British made X-ray tubes. I think I am correct in stating that the research is considered to be a most important one in view of the enormous number of X-ray tubes now required, and in view of the very poor quality of the present British X-ray tubes as compared with the German and American ones. The laboratory in which I work is in Piccadilly and is equipped with a complete plant for exhausting X-ray tubes. As there are only two of us engaged on this work in the laboratory, I do not think that I should be justified in giving the work up completely, but as I am not paid for the work, I do not anticipate any difficulty if I decided to cut down to some extent my times of attendance at the laboratory - which have been all day on Mondays, Tuesdays and Wednesdays. I may mention, in passing, that most of the X-ray tubes which we prepare in the laboratory are sent later on in the week to me at St. Bartholomew's to be tested in actual Hospital practice, and I think the Hospital has been spared some expense in the purchase of X-ray tubes by this arrangement.

The clinical use of experimental X-ray tubes just days after they had been made is a clear illustration of a point made by Keevil (2012: 1520): 'The speed with which basic research findings could affect clinical practice more than 100 years ago far outstrips present efforts at translational medicine.' In a later section of the report, it also becomes evident that he had a private laboratory as his disposal:

> I thoroughly understand the need for strict economy in such an expensive department as the X-ray Dept.,[32] and I should make it a point of not ordering any apparatus except that actually necessary for carrying on the work of the Dept, in an efficient manner. I have for many years possessed a Physical laboratory of my own, and I hope that the experience I have gained in it will prove of use in the choice of apparatus for the X-ray Dept.

And from one of his publications (1916a) we further learn that he was also a Fellow of the Physical Society (of London). Overall, therefore, it seems that H. Trevelyan George was not simply a Natural Sciences graduate who had subsequently turned to medicine, but rather a physicist who was also medically qualified. In short, he was in effect a medical physicist—or 'hospital physicist'—before the name existed, and at a time when the role itself was just coming into being. It is generally claimed that Sidney Russ (1879–1963) was the first 'hospital physicist', being appointed to the Middlesex Hospital in 1913 (Hessenbruch 2004). Russ (his affiliation given as 'Physicist to the Middlesex Hospital') was in fact one of the editors of the journal *Archives of Radiology and Electrotherapy* in which Trevelyan George's papers were published. It seems very likely that the two men must have been acquainted.

Though Trevelyan George was certainly well qualified to collaborate on X-ray studies of speech, how and why he undertook the work at all in the midst of the war remains mysterious. His 1917 report makes clear that the X-ray Department was understaffed and short of resources—to the extent that he recommended closing the department for up to two days a week, and permitting unqualified staff to make X-rays when necessary. He was personally dealing with an average of 46 patients per day. Stocks of consumable supplies were very low. Against this background, DJ's research cannot have seemed very pressing. Unlike the work done by Barclay & Nelson, it had no declared purpose for the rehabilitation of war casualties. Of course, it might have turned out to have some value in speech remediation, but the only application for phonetics that Jones mentions in publications of this period is the teaching of foreign languages. In fact, as Collins & Mees point out, DJ remained remarkably aloof from the war (1999: 136–163). And, as is discussed in Chapter Five, having published the X-rays in 1917 and 1918, DJ then did nothing further with them for 40 years.

As for Trevelyan George, for ten years following the end of the war the trail goes cold, and his activities are unknown. He died in January 1929 at the age of 55, leaving the unusually large amount of almost £53,000 (several millions of pounds in present day values).

2.6 Waveforms and signal analysis

One of the most striking differences between the speech research of the period 1890–1940 and that of the present day is the extent of the use made of the acoustic speech signal. Data gathered from recordings, and subjected to acoustic analysis, accounts for an overwhelming proportion of current work. But the equipment required for this—relatively inexpensive high-quality microphones, portable recording equipment, and digital computers with analog-to-digital converters—have become available only within the last 30–40 years. In the period under consideration, few researchers had access to a satisfactory means of capturing and displaying the acoustic signal, and still fewer to methods of analysing acoustic signals beyond a basic level.

A detailed account of the seemingly innumerable methods devised over the years for displaying and recording waveforms is given by Phillips (1987), but even that extensive survey is incomplete, since from the specialised phonetics literature it is not difficult to add further items which escaped his notice.

Following the classification of Phillips, we can distinguish membrane methods and electrical methods. Membrane methods, which are acoustic and mechanical, can in turn be divided into those which produce a direct record (such as the phonautograph), and those where a phonograph or gramophone recording intervenes.

2.6.1 The phonautograph

The phonautograph (Pantalony 2009: 41–45) was the earliest device which plotted a wave representing airborne sound impinging on a membrane. It was announced in 1859, and in the same year samples of its output were exhibited in Britain. It was almost immediately put to important use in speech research by the Dutch physiologist Franz Cornelius Donders (1818–1889) who published a 'preliminary note' (*Vorläufige Notiz*) on the analysis of vowels in 1864, in which he leapt directly to the conclusion that the timbre of vowels is determined by 'overtones' of absolute frequency (i.e., to use a later term, *formants*), and not by harmonics of the fundamental. Donders (1864) does not reproduce any waveforms, but some are included in Donders (1870), which may well qualify as the earliest work on speech to be illustrated with acoustic waveforms.

In Britain, A. J. Ellis (1874) sets out a whole programme for the description of speech, beginning from phonautograph tracings. Ellis says (1874: 115 fn) that he has been able to examine ‘many phonautographic drawings’ thanks to Sir Charles Wheastone, but it is unclear whether this means that Wheatstone had access to a device and was able to make tracings (e.g., of Ellis’s own speech), or simply that Wheatsone had been sent many examples from other workers. Certainly Ellis does not reproduce any in his paper, and Wheatstone himself appears not to have published any. Ellis does, however, refer to the examples published by Donders (1870) and Pisko (1865).

When the kymograph is set up so that the mouth channel will record aspects of the acoustic waveform of speech (as was generally the case from about 1900 onwards), it becomes effectively a phonautograph. It is noteworthy that Scripture, who published many papers using a single-channel kymograph in this mode, commonly refers to the device as a phonautograph. Used this way, the kymograph has most of the defects of the original phonautograph, and may even be inferior.

2.6.2 Manometric flames

As early as 1862, dissatisfaction with the phonautograph led Koenig to develop his manometric flame apparatus (Pantalony 2009: 58). Pressure variations impinge on a membrane, which modulates the flow of gas to a small burner. The height of the resulting flame supposedly follows the applied pressure variations without inertia, and when the flame is viewed in a rotating mirror drum (a device invented by Wheatstone), a sort of approximation to the acoustic waveform is observed. The manometric flame was widely employed, at least as a demonstration experiment, and an example is known to have belonged to the UCL phonetics laboratory.

Koenig published pictures of the appearance of the flame for various vowels, but these were drawings only. Various attempts were made to photograph the flames, culminating in Nichols and Merritt (1898), who were able to produce photographic records of short stretches of connected speech, but only after several years of research spent developing highly luminous gas mixtures and an elaborate electrically-driven camera which exposed film 3 inches wide at rates up to 1 metre per second. Even then, their records leave much to be desired, and their method was plainly too complex and expensive to become a routine laboratory technique. There seem to have been no British attempts to photograph the flames.

2.6.3 Jenkin & Ewing

From 1878 and throughout the period covered by this study, another widely used means of obtaining the speech waveform was from phonograph and gramophone recordings. An important early contribution was work done in Edinburgh by two British physicists and engineers, Henry Charles Fleeming [ˈflemɪŋ] Jenkin (1833–1885) and Sir James Alfred Ewing (1855–1935). The lives and careers of both men are extensively documented (Stevenson 1887; Cookson & Hempstead 2000; Bates 1946), though both were so versatile and productive that their work in speech gets barely a mention from biographers.

Edison announced the principle of the phonograph in November 1877, and *Scientific American* for December 22 1877 has a drawing, and describes the well-known mode of operation using tin foil on a cylinder with grooves. The idea of examining the appearance of the groove (either microscopically or by mechanical enlargement) must have occurred independently to many investigators. Within months, Alfred Marshall Mayer (1836–1897) incorporated an account of the phonograph into a popular book on sound (Mayer 1878). An expanded version of the section of the book devoted to the phonograph appeared first in *Nature* in April 1878, and includes a waveform for a vowel [a], produced by enlarging the pattern from the groove with a lever, and also compares this with the appearance of the groove itself and the response of a manometric flame.

The phonograph is a simple machine, as all the early accounts point out. 'The new invention is purely mechanical—no electricity is involved ... It is a simple affair of vibrating plates, thrown into vibration by the human voice,' said *Scientific American* No doubt numerous copies were made within a very short time in suitable workshops.

According to Stevenson (1887: 229–232), Jenkin and Ewing constructed two excellent working examples of the phonograph on the basis of nothing more than the verbal description published in *The Times* (the report appeared on January 17, 1878). Their phonograph (Jenkin & Ewing 1878) differs from Edison's in having a second (high-speed) shaft carrying a flywheel, with a belt drive to the cylinder, giving better speed regulation. Instead of the metal plate diaphragm used by Edison, they used a stiff paper cone compliantly mounted in an oiled silk surround—an arrangement remarkably similar to that in the modern moving-coil loudspeaker.[33] Jenkin & Ewing began to publish short notes on results obtained in speech research with a phonograph from March 1878 (reporting, for example, the effects of reversing the speech or altering the playback speed). But they also constructed a well-engineered device using levers to magnify the profile of the groove by a factor of 400.The resulting waveforms were recorded on moving paper tape by means of a frictionless electrostatic inking arrangement. By July 1878, Jenkin and Ewing published a substantial report, running to 30 pages of text and 7 detailed plates, in which they not only describe their methods and reproduce the resulting waveforms, but also subject the waveforms to quantitative harmonic analysis, and review their findings in relation to contemporary vowel theories.

The full significance of Jenkin and Ewing (1878) does not seem to have been appreciated. The opening paragraphs of their paper are:

> The permanent record obtained with Mr T. A. Edison's phonograph has afforded a new opportunity of investigating the nature of spoken sounds, and the method which this invention placed at our disposal appears to us to possess several important advantages.
>
> This method consists in obtaining a magnified transcript on paper of the indentations impressed by spoken vowels on the tinfoil of the phonograph, and then subjecting the periodic wave-forms thus obtained to harmonic analysis.
>
> The curves as drawn in ink on paper represent to a large scale the surface of a longitudinal section of the tinfoil made along the centre of the furrow impressed by the pricker of the phonograph. The forms impressed on the tinfoil depend essentially on the movement which a particle of air performs when the given sound is being uttered, and the harmonic constituents of each period of the continuous wave-form indicate the relative proportions in which the prime tone and its harmonics are present in the sound.
>
> We thus obtain what may be called a harmonic analysis of the vowel sounds.
>
> (1878: 745)

Because this concept is now so familiar as the very basis of all work in acoustic analysis of speech, it is easy to overlook the originality of the statement. They appear to be formulating clearly for the first time the insight that quantitative harmonic analysis of a waveform will yield an analysis of the sound itself.

They employed 6 male speakers, who sang sustained vowels (evidently the GOOSE vowel and a monophthongal version of GOAT) on a range of pitches. Figure 2.31 shows a detail from one of their figures and illustrates their method of working.

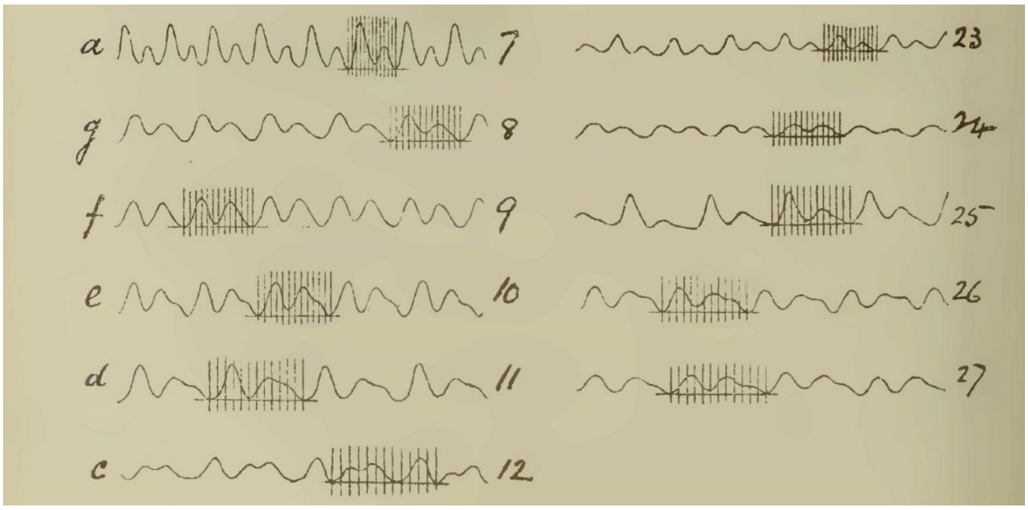


Figure 2.31 Vowel waveforms from Jenkin and Ewing (1878).

The curves in the left column are some of the 'o' vowels from Voice 1, and those on the right from Voice 2. The numbers at the right of each curve identify individual tokens. The letter to the left indicates the pitch on which the vowel was sung. Thus token 12 was sung by Voice 1 on the C below middle C, i.e., 128 Hz. Once the enlarged waves were plotted, a tangent was drawn between corresponding minima, and the period thus identified was divided into 12 equal portions. The height of the curve was determined at each of the 12 points and the harmonic analysis computed manually. The general solution had been worked out by Peter Guthrie Tait (1831–1901), professor of Natural Philosophy in Edinburgh, and the calculations, though laborious, essentially involve methodically inserting the measured values of the 12 ordinates into a series of multiplications. It will be evident that in modern terms, the signal is being sampled at a rate equal to 12 times the fundamental frequency. Thus for token 12, the sampling rate achieved was 1536 Hz, and the resulting bandwidth of the analysis is 0–768 Hz. This is a severe limitation, but it should be remembered that this is the first ever attempt along these lines, and later workers using the same manual methods never managed to improve very greatly on this.

Unsurprisingly, Jenkin and Ewing conclude that most of the energy in the signal is in the lowest harmonics—a problem made worse by the no doubt very restricted frequency range of the recording apparatus, and the imprecision in measurement which will tend to obscure components present at small amplitude.[34] At this period, and well into the 20th century, there was a general underestimation of the necessary bandwidth for speech.

In the closing lines of their report, Jenkin and Ewing envisage an extension to vowel synthesis. Their apparatus would be used in reverse, to transfer any wave supplied as a large pattern into small indentations in the phonograph foil. The resulting recording could then be replayed, enabling synthetic vowels to be heard. According to Cookson & Hempstead (2000: 180), in 1879 Jenkin applied for a grant of £50 to pursue the synthesis proposal, but was unsuccessful. It may be relevant that work on vowel synthesis along very similar lines had already been published by Preece and Stroh (1878). Neither Jenkin nor Ewing appear to have done any further work on speech.

No further original work seems to have been done in Britain on the plotting of waveforms from phonograph recordings for almost 20 years, though work continued elsewhere (a review is given by McKendrick 1897: 209). Lloyd, for example, worked with curves obtained by others. In the meantime, the phonograph itself saw some (rather slow) development, and wax cylinders replaced tinfoil as the recording medium. In the 1890s, McKendrick, (see Chapter Three) developed at least two versions of an instrument for obtaining waveforms from wax cylinder phonograph recordings, though unlike Jenkin and Ewing he did not attempt to analyse the resulting traces or apply them to any clear research question. When the gramophone appeared, similar methods of plotting a waveform from the groove were applied, most significantly by Scripture, though McKendrick also published short reports. The plotting of waveforms from gramophone recordings continued at UCL into the 1930s, using a commercially produced device made by Lioret (see Chapter Four).

2.6.4 The Duddell oscillograph

In 1893 the French engineer and physicist André-Eugène Blondel (1863–1938) worked out the concept of an electrical oscillograph capable of a very wide frequency response in which the moving coil of a galvanometer was reduced to a single ‘bifilar’ loop of wire, taking the form of two conductors held under tension and very close together between the poles of a powerful magnet. Deflection was indicated by a beam of light reflected from a tiny mirror fixed to the two conductors, and the movement of the whole vibrator assembly (including the mirror) was damped by being immersed in an oil of suitable viscosity. The principle was taken up and greatly developed by the British engineer and physicist William Du Bois Duddell (1872–1917), who introduced numerous improvements and turned the device into a practical commercial instrument. For a period of no less than 50 years from 1898 onwards, the Cambridge Scientific Instrument Company manufactured and sold various models of ‘Duddell oscillograph’ (Cattermole & Wolfe 1987: 219). From the beginning, the Duddell oscillograph approached or matched in performance the oscillograph eventually used at Bell

Laboratories for the work described in Fletcher (1929). A permanent record was captured on rapidly-moving photographic film.

It does not seem to have been preciously noticed that both Duddell himself and a number of other British engineers published early accounts of the application of the oscillograph to speech, or that the engineering community in Britain formulated and attempted to address many fundamental questions concerning the transmission of speech several years before the concerted effort began at Bell Laboratories. Duddell published oscillograms of various speech sounds in 1906 and 1907 (1907, 1909). Similar results were published by Cohen and Shepherd (1907).

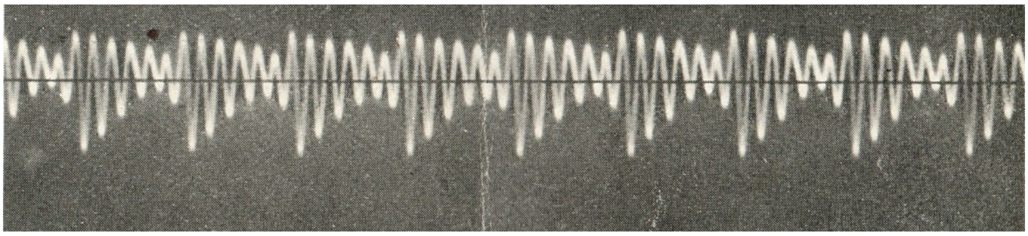


Figure 2.32 Vowel in [mɑː] (Duddell 1906 [1909]: Figure 1, detail). The figures he showed in 1906 are positive prints.

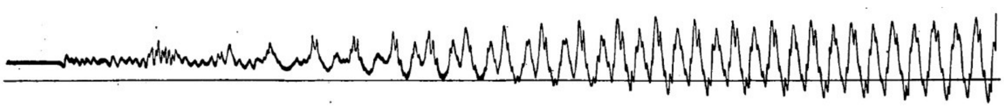


Figure 2.33 First part of [kʰiː], showing release of the plosive, as published by Duddell (1907: 546, detail). The straight line has probably been marked by a second oscillograph channel which was left idle. The 1907 figures are reproduced as high-contrast negatives.

Nevertheless, as the remarkably good waveforms published by Duddell (Figures 2.32 and 2.33) and others show very clearly, it was already apparent that the combination of a microphone and high-frequency oscillograph offered the best hope for investigation of the speech wave. Exactly that conclusion was reached at the time in a short piece in *Nature* by Edwin Edser (1866–1932), Head of the Physics

Department at Goldsmiths' College in the University of London, who was the author of several textbooks on various aspects of physics and calculus (Edser 1909). He compares the various methods available—plotting or displaying waveforms obtained from phonograph recordings, direct acoustic membrane methods, and the use of a microphone and Duddell oscillograph—and concludes that the 'best curves' are given by the oscillograph.

The Duddell oscillograph was a low-impedance device (offering a resistive load of about 10 ohms), and at this date the only practical input source for speech was a carbon microphone in circuit with a battery. Both Duddell and Cohen & Shepherd (1907) mention use of the 'solid back' transmitter, which was then a standard pattern used in telephones (Abbott 1904: 144–220).

Campbell (1907) attempted to measure the frequency response of 'an ordinary telephone transmitter' using an organ pipe of adjustable pitch as the signal source, and revealed two major resonances centred around 500 Hz and 1 kHz. He commented 'In view of the fact that the transmitters and receivers ordinarily used have these strong resonance points at frequencies well within the working range, it seems surprising that speech can be transmitted so well' (1907: 534). An even worse defect of the carbon telephone microphone was nonlinearity (Oksanen & Välimäki 2011), which adds distortion in the form of numerous intermodulation products.

But even with the imperfect microphones then available, the superiority of the oscillograph should have been clear. In fact, carbon microphones were to be improved very considerably for applications other than telephony, and the Marconi-Reisz design, having a frequency range extending to 7 kHz, was retained in use for broadcasting by the BBC until as late as 1935 (Read 1984).

Despite its eminent suitability, the Duddell oscillograph was not taken up by those engaged in experimental phonetic research in Britain. This signals a failure of connection between the worlds of electrical engineering and phonetics. It is evident from the remarks in Cohen and Shepherd (1907) and from the discussion which followed (e.g., Kingsbury 1907) that the engineers had no knowledge of contemporary phonetics. Cost must also have been a factor. At the time of its introduction, the oscillograph with drum camera cost £79 (Cattermole & Rolfe 1987: 53), an amount equivalent to at least £7,936 in 2015. The oscillograph also required ancillary electrical equipment and a photographic darkroom, processing equipment and chemicals, plus relevant technical skill. It is evident from the petty cash book that even in the 1920s the UCL laboratory was paying to have accumulators charged commercially, an indication that the laboratory had little or nothing in the way of electrical apparatus and test equipment. There is no clear indication that the pre-war UCL laboratory had any photographic facilities at all, and again the petty cash book shows payments made to outside firms for relatively simple photographic jobs such the preparation of lantern slides.

Notes to Chapter 2

1 The term ‘palatogram’ was introduced—as a conscious neologism—by Atkinson (1897: 495), while ‘palatography’ seems to have been added by Scripture (1902). By then, the technique itself was thirty years old, and had previously been referred to with a range of different names. The complementary term ‘linguagram’, may have been introduced by Noël-Armfield (1924). OED gives Scripture (1902) as the first source for ‘palatography’ and ‘palatogram’. The term ‘linguagram’ has no entry in OED.

2 There are thus 26 plates in total, essentially one for each letter of the alphabet. Though Coles did not include ‘W’, he added ‘CH’. It is not certain that Coles simply pronounced the name of a letter (as opposed to performing a properly isolated sound) in every case. His palatogram for ‘H’, for example, certainly does not represent the pronunciation [eɪtʃ], since no closure is indicated. It resembles instead a vowel-like articulation, similar to what he gives for ‘E’. Possibly he pronounced [hiː] Similarly, Carruthers (1900: 344) points out that Coles cannot have pronounced [zed] for ‘Z’, since the central groove is visible. Maybe he pronounced [ziː].

3 Wellcome Library MS.5722

4 Such terms have a long history in English before Coles. According to OED, the first use of ‘Lingua-Dental’, for example, is in Wilkins (1668).

5 For brief biographical information about Kingsley, see Carey (2000).

6 They are in Plates 3 and 4 (on unnumbered fold-out pages) at the back of the ‘Atlas’ part of his work; the account of his method is on page 30 of the main text.

7 In later editions the note is retained, but updated to include Kingsley. Sievers hyphenates ‘Oakley-Coles’ and does not cite Coles’s work directly. It appears he may have relied on Grützner and Techmer.

8 The two papers use the same 36 engravings in almost exactly the same order. The later paper drops a section on pathology, illustrated with two further figures, which had been part of the 1878 paper.

9 A bibliography of Rousselot’s publications in given by Millet (1925: 141–143).

10 ‘For these four articulations the tongue is in contact with the palate. But it touches across its full width for [ʎ] and [ɲ], and at the edges only for [lj] and [nj]. One can prove this by coating the palate with a mixture of flour and gum solution. The points of contact print themselves on to the tongue’.

11 Hagelin deserves credit as one of the earliest studies using indirect palatography. He obtained his data from palates made for a number of native-speaker subjects (Paul Passy was among them) and was thus probably the first to collect genuine palatographic fieldwork data. Far from praising this aspect of Hagelin’s work, Rousselot (1897: 54) bizarrely criticizes him for making his work dependent on other people!

[12] In fact it is hard to understand why Grandgent did not simply form oval loops at the ends of his wires. They would have been simpler to make (he doesn't specify how he managed to attach the cardboard ovals at right angles to the wire), and would not have obstructed the airflow.

[13] He became a member of Council in 1908. From 1929 until his death he was Vice-President.

[14] Both Venn and Munson mention his invention of 'Atkinson's optical black', a non-reflective coating for the interiors of optical instruments, but further details of this have proved elusive.

[15] By 'godiva' Rousselot is referring to a proprietary re-useable dental impression material ('Godiva' in English). A similar composition is still on sale under the same name.

[16] Scripture's account is probably derived from those of Rousselot and Laclotte. In Scripture's version, the strip of 'Godiva' inexplicably becomes a strip of vulcanised rubber.

[17] The design of the tooth stop was changed between the original description and the device as commercially produced. In place of the single wire which fitted in the valley between the incisors, Atkinson returned to the idea of a thin metal plate folded around the edge of the teeth. A range of tooth stops supplied with the instrument had this plate at varying angles.

[18] The 1897 text reads '4.5 mm' and the 1899 text has '4,5 mm', though the actual distance is 45 mm (4.5 cm). The source of the trouble is probably Fig. II of the earlier account which gives the dimension as '4.5 cmm'. Scripture (1902: 331) reproduces the error, writing '4.5 ^{mm}'.

[19] Photograph is a detail from a sheet headed 'Tongue positions of vowel sounds' included in the box with the Mouth Measurer as sold. The sheet is evidently based on Figure V of Atkinson (1897) but is not an unmodified reprint: Atkinson removed potentially confusing alternative soft palate positions shown in detail on the original figure.

[20] The tools at www.measuringworth.com have been used, and a comparison based on GDP per capita.

[21] It appears on Plate XV (the plates are at the end of the volume).

[22] Probably a reference to Wagner (1891). Philipp Wagner was an active member of the IPA between 1888 and 1914. His 1891 paper in *Phonetische Studien* (delivered as a talk in May 1890) on the uses of the kymograph and the phonograph for phonetics gives him a claim to be among the earliest to recognise the value of adding experimental techniques to the existing field of phonetics.

[23] The original article is in phonetically transcribed German.

[24] Both the 'nose' and 'larynx' signals, when these were used along with that from the mouth, were obtained in ways that favoured the recording of the alternating component, with a fairly steady baseline. The nose signal came from a tube inserted into one nostril (the so-called 'nasal olive' being nothing but a hollow plug to hold it in place). Since the other nostril generally remained open, air entering the nasal

cavity had a ready means of escape though a constant resistance. When air enters the nasal cavity through the open velum, air pressure in the nose will rise, and the variation of pressure over time is a measure of nasal flow. This is indeed the quantity shown by Rosapelly (1876). Rosapelly's smooth curves show gross flow, but the later practice became that of using a recording tambour sensitive to the vibratory component. Among early writers Wagner (1891) mentions blocking the other nostril, though this does not seem to have been general practice.

The 'larynx' signal was picked up from the vibrating skin surface just above the larynx, and might more accurately be thought of as a 'throat stethoscope' signal. Originally, a hollow cup was pressed against the skin. In later versions the sensor has a membrane across its opening (Rousselot 1897: 97). In either arrangement, there is no net flow into the device and pickup is entirely acoustic.

[25] An exact reprint also appeared in *Popular Science Review*, 13, 278–288, though there the title is given as 'The logograph, or writing by the voice'.

[26] Evidently Francisco Javier (Saverio) Clavijero (Clavigero) (1731-1787).

[27] Some biographical information about Scheier is given in Pagel (1901: 1491–1492); the year of his death has been taken from Kütterer (2005: 385).

[28] (Friedrich) William Scheppegrell (1860-1928) was born in Hanover. His parents emigrated to the United States when he was a child, and it was there that he completed his education and qualified in medicine. Like Macintyre, he already combined the specialisms of laryngology and medical electricity before the advent of X-rays. (*Encyclopedia of American Biography*, Vol.11, 1901, p. 547).

[29] Royal Institution archive; his election took place on 4 March 1913.

[30] *Archives of Radiology and Electrotherapy* **25** (1919), 62–64. The donation of £25 appears relatively large. Most donations listed are in the range 1–5 guineas. Only Sir James Mackenzie Davidson is listed as having given more (£100).

[31] St Bartholomew's Hospital Archives SBHB/RD/1/2, 'Temporary management of the X-ray Department', [1917]. I am grateful to St Bartholomew's Hospital Archives for supplying a copy of the report, and giving permission to quote from it.

[32] He mentions that each X-ray tube (they had a limited useful life) cost £10–£12 (equivalent to about £600 at 2015 prices).

[33] Preece & Stroh (1878) also used a paper cone, mounting theirs on a rubber membrane. Their report post-dates and acknowledges Jenkin and Ewing (1878).

[34] Measurements were made to one two-hundredth of an inch, the least significant digit being visually estimated. But since the overall height of the curve is not indicated, it appears impossible to estimate the bit depth of the sampling. It is probably of the order of 5 or 6 bits at best.

3

Before the UCL laboratory

This chapter is concerned with a number of British figures who were active in the period 1890–1912, before the establishment of the first experimental phonetics laboratory in Britain. They all published at least some of their work during the initial heyday of 'experimental phonetics', though they have a range of different relations with the concept, and diverse attitudes towards it. By no means all of them would have regarded themselves as phoneticians at all; in fact, they variously represent all three of the strands identified in Chapter 1 (physics, medicine, philology). They are considered in order of their year of birth.

3.1 John Gray McKendrick (1841–1926)[1]

Though his work specifically in speech has never been systematically surveyed, the general story of McKendrick's life and work is well documented (Paton 1926). He was born in Aberdeen, and orphaned as a young child, but despite poverty and hardships managed to gain a good education, qualifying in medicine in 1864. Alongside medical practice, he continued to study a range of scientific topics, gradually moving into physiological research and teaching. On a visit to European laboratories, he met among others du Bois Reymond, Helmholtz, Ludwig, and Koenig. He was appointed to the Regius Professorship of Physiology in Glasgow in 1876, a position he held for 30 years until his retirement in 1906. In 1881, he was elected Fullerian Professor at the Royal

Institution in London, lecturing there for two years, and became a Fellow of the Royal Society in 1884.

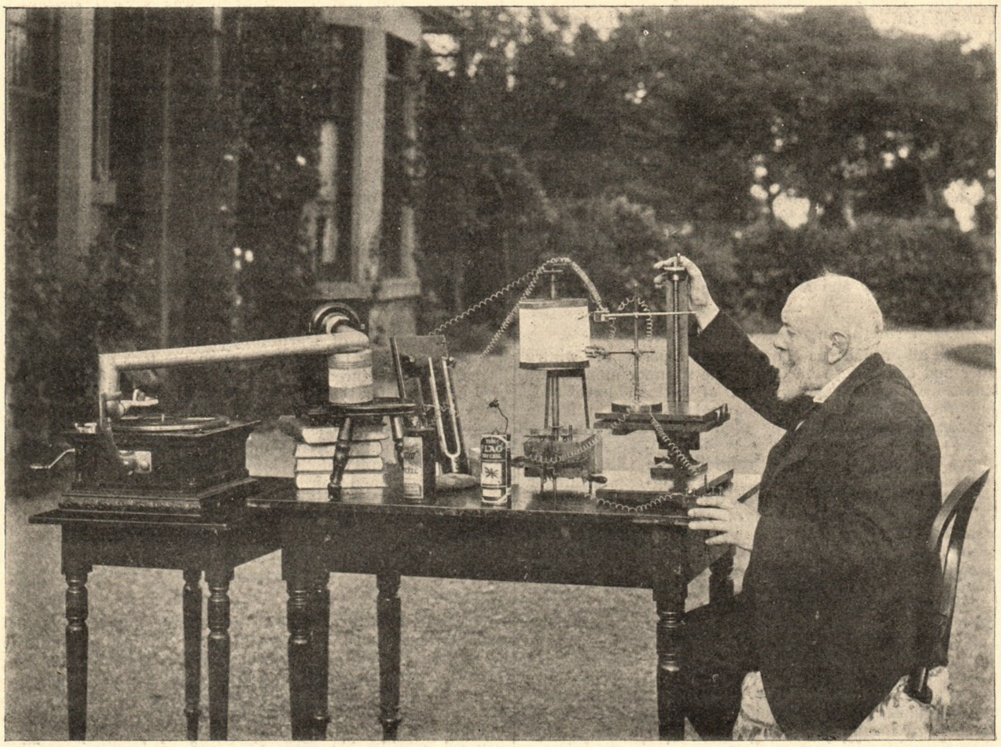


FIG. 1.—Arrangement of apparatus. Gramophone to the left. Observe the tin tube carrying the sound waves to the microphone. The operator is on the right controlling the movable stage. The upper electromagnetic marker adjusted to the cylinder (revolving drum) registers the vibrations of the 1/100th per second tuning fork seen in the middle of the table. The lower electromagnetic marker writes the vibrations of the sound waves as these act on the microphone. While it is registering the lower marker gives forth the sounds played by the gramophone.

Figure 3.1 McKendrick, in retirement, with his arrangement for real-time plotting of waveforms from a gramophone (1909a). The electrical arrangement, of carbon microphone and improvised electromagnetic marker, is no different in principle from what Rousselot had described in 1891, and almost certainly had all the same deficiencies. Far better results would have been obtained by simply connecting the microphone to a Duddell oscillograph (see Chapter 2).

McKendrick was interested particularly in the senses, working first on vision, and then increasingly on hearing and the mechanism of the cochlea. This led him to experiment with the phonograph and telephone. During the 1880s and 1890s, McKendrick clearly kept abreast of developments in experimental phonetics, and published two substantial surveys: one, jointly authored with Alexander Gray, as a chapter in a textbook of physiology edited by Schaefer (McKendrick & Gray 1900), and the other in *Nature* (McKendrick 1901).[2] These provide what seem to be the earliest detailed accounts in

English of the development and contemporary state of (much of) the field—the surprising omissions being all methods concerned with production: palatography, 'mouth-mapping' or X-rays. McKendrick's summaries are still valuable today for the comprehensive bibliographical references and excellent diagrams. Together with the bibliography of Breymann (1897)—which McKendrick also knew and cites—they would have provided anyone embarking on the subject around 1900 with a superb entrée.[3]

Paton, his successor at Glasgow, says of McKendrick that 'Teaching rather than research was his forte' (1926: xvii), pointing out that he entered the field of physiology at the relatively late age of 30, and never had any training in the methods of experimental science. On the other hand he was a popular and entertaining lecturer whose ingenious demonstrations 'always delighted the spectators'. Much the same evaluation can be applied to McKendrick's writings on speech. Superbly clear, well-illustrated, and informed by wide reading and punctilious scholarship, they are nevertheless largely restricted to reporting the work of others. It isn't that McKendrick was unoriginal—on the contrary, his writings contain numerous 'bright ideas'. The problem rather is that none of the ideas are developed and pursued with clear research goals in mind. As an example, an early publication (McKendrick 1878b) sees him outlining, in the space of a few hundred words, such novelties as a carbon-microphone transducer used to amplify phonograph playback, a lightweight compliant playback system using a Marey tambour to avoid destroying the impressions on the tinfoil recording, and a contact microphone on the throat ('attached to the throat by an india-rubber band … the faintest trill or whisper is audible' (1878b: 240). Later, he tried cutaneous stimulation as a way of presenting sound (such as music) to the deaf

(1897: 212–213), and proposed establishing an archive of recorded sound for the benefit of linguistic science (1901: 259). Any one of these ideas, systematically explored and properly developed, might have given him a prominent place in the history of speech research. But he seems to have gone no further than suggestions, or rapidly-improvised demonstrations of concept. His experimental work on speech gives an impression of playful tinkering rather than clearly directed research—an impression one receives even more strongly from the numerous notes he published in *Nature* after his retirement in 1906 (see Figure 3.1).

McKendrick's interests expanded to include matters of symbolisation: he deals with Jespersen's analphabetic notation (1901: 189), and taught himself Visible Speech. For a few years at the beginning of the twentieth century, McKendrick was the closest approach to a spokesman for phonetics within the British scientific establishment. This was particularly so in the pages of *Nature*, to which he contributed several reviews of books on phonetic topics (e.g., 1909b, 1910, 1911a).

Despite his British medical training and background, he does not have much connection with the native tradition of medical input to phonetic studies. McKendrick's orientation was instead very much a Continental one. He greatly admired Helmholtz, and published a life of him (1899a). But he was certainly acquainted with Lloyd. A connection with another of the figures treated in this chapter is that among the book reviews McKendrick contributed to *Nature* is one of Aikin (1910).

3.2 Lord Rayleigh (John William Strutt, 1842–1919)

Chapter 1 traced the contributions to speech research made by Cambridge mathematical physicists as far as James Clerk Maxwell. Rayleigh succeeded Maxwell as Cavendish Professor of Physics in 1879—and towards the end of his life he completes the connection of Cambridge physics with the phonetics of DJ's era, since it was he who presided at DJ's discourse at the Royal Institution in 1917.[4]

There is an extensive literature on Rayleigh's life and scientific achievements. A brief summary is provided by Gavroglu (2004). Acoustics was just one of numerous scientific fields in which Rayleigh made fundamental contributions. His Nobel Prize (1904) was awarded for the discovery of the element argon.

On its appearance, Rayleigh's T*heory of Sound* (2 vols., 1877–1878) quickly became the definitive mathematical treatment of acoustics. The first edition appears to contain nothing specifically concerned with speech, but volume 2 of the second edition (1896) adds a long section (pp. 470–478) surveying the issues in the acoustics of vowels and the reviewing contemporary work. Like Herschel and Airy before him, Rayleigh gives a prominent place to the work of Willis, quoting him at length (pp. 470–472), and saying 'It will be seen that so far as general principles are concerned, he left little to be effected by his successors' (1896: 471).

Rayleigh describes Helmholtz as 'following Wheatstone', and proceeds to an examination of whether there is a real difference between their view and the 'inharmonic' theory associated with Willis. He points out '...at first sight it may appear essentially different from, or even inconsistent with, the account of the matter given by Willis', but after a very clear and non-mathematical explanation (pp. 472–473), he concludes: 'From these considerations it will be seen that both ways of regarding the subject are legitimate and not inconsistent with one another.' Nevertheless, this was to be a matter on which a great deal of ink continued to be needlessly spilt—and which remained one of Scripture's hobbyhorses until the 1940s. Rayleigh concludes:

> Since the publication of Helmholtz's treatise the question has been much discussed whether a given vowel is characterized by the prominence of partials of given *order* (the relative pitch theory), or by the prominence of partials of given *pitch* (the fixed pitch theory), and every possible conclusion has been advocated. We have seen that Willis decided the question, without even expressly formulating it, in favour of the fixed pitch theory. Helmholtz himself, if not very explicitly, appeared to hold the same opinion, perhaps more on *a priori* grounds than as the result of experiment. If indeed, as has usually been assumed by writers on phonetics, a particular vowel quality is associated with a given oral configuration, the question is scarcely an open one.

Again, we see authority accorded to Willis. He 'decided the question', which 'is scarcely an open one'.

Rayleigh's reading of the recent literature was clearly extensive. He cites various contributions from Auerbach, Edison, Alexander Graham Bell, Jenkin and Ewing, Hermann,[5] Lloyd, Preece and Stroh, and Pipping,[6] with a brief mention for McKendrick. Hermann in particular is treated at some length (pp. 474–476). Rayleigh devotes space to the relative merits of laborious computation of the Fourier analysis and direct measurement of periodicities in the waveform. It is also evident that Rayleigh performed some auditory analysis of vowels himself (p. 477), using an acoustic resonator which he tuned by placing a finger over its opening. By listening in this way he confirmed that the first harmonic is strong in live speech examples of vowels, but largely absent in the phonograph recordings of his day. He does not seem to have published these investigations in greater detail.

Among Rayleigh's minor papers, most of which were reprinted in the 6 volumes of his *Scientific papers* (1900–1920), are several of great phonetic interest which seem to have lain neglected. In 1908 Rayleigh published a paper on 'The pitch of sibilants', and in 1913 opened a debate on the subject in *Nature* which prompted the experimental psychologist E.B. Titchener[7] to make a series of ingenious perceptual experiments using high-pitched whistles. Rayleigh in turn made similar experiments trying diverse sound sources. Unlike many observers at the time, Rayleigh correctly deduced that sounds such as [f] and [s] have spectra extending to at least 10 kHz. His method of accurately measuring such high frequencies is characteristically simple and brilliant, requiring only a small sensitive (gas) flame and a piece of board. He used the sensitive flame to detect nodes in a standing wave pattern reflected from the board. From the measured positions of a series of nodes, he determined the (relatively short) wavelength, and thus in turn the frequency.

Rayleigh concludes the section on vowels in his *Theory of Sound*—and indeed it stands at the conclusion of the whole two-volume treatise—with the words:

> In the account here given of the vowel question it has only been possible to touch upon a few of the more general aspects of it. The reader who wishes to form a judgment upon controverted points and to pursue the subject into detail must consult the original writings of recent workers, among whom may be specially mentioned Hermann, Pipping, and Lloyd. The field is an attractive one; but those who would work in it need to be well equipped, both on the physical and on the phonetic side.
>
> (1896: 478)

There could scarcely have been a more positive and hopeful ending to the book, or one that held out more clearly the prospect of equal collaboration between physicists and phoneticians.

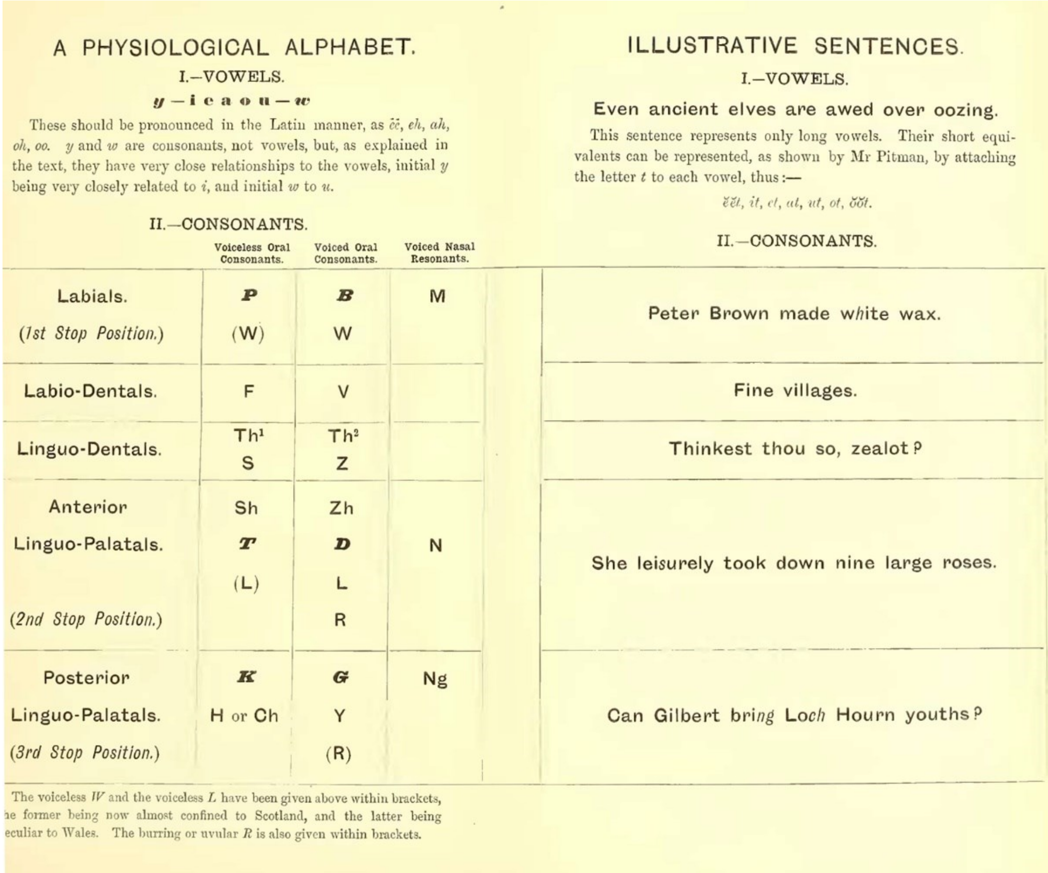
3.3 John Wyllie (1844–1916)

An account of Wyllie's life, and the context in which he worked, is given in MacMahon (1984c). That work leaves some scope for the further exploration of the phonetic science component in Wyllie's work, and that is what is attempted here.

Wyllie is located very firmly within the medical strand of British phonetic science. He continues the tradition seen in Bristowe (see Chapter 1), and Wyllie's own students (he was an influential teacher) were to carry this tradition well into the twentieth century.

Wyllie's work on speech is *The disorders of speech* (1894). This is a substantial textbook of over 500 pages. As is usual within the nineteenth-century medical strand, his interests are not narrowly specialized, but range over the whole field including laryngeal physiology, normal speech production, stammering, cleft palate and other organic disorders, voice problems (including those of psychogenic origin), speech acquisition, and deafness. Somewhat more than half of the book—the whole of the third and final part—is concerned with 'Speech in its relations to diseases of the nervous system', and much of that with various categories of aphasia (though Wyllie is not even mentioned in Levelt 2013). To this already comprehensive mixture, Wyllie doesn't hesitate to add discussions of topics whose 'relations … to practical medicine are somewhat remote', justifying their inclusion because 'the facts brought out in the study ... are of great scientific value' (1894: 149). These include, for example, a sketch of various schools of philology ('that noble science'), the possible imitative and onomatopoeic origins of language, types of language (agglutinative, inflexional, etc.), 'phonetic decay', affectations and vulgarisms, and 'the Shibboleths of modern English society', where he covers speech habits which still provoke debate today: *w*–for–*r*, *h*-

dropping, intrusive *r*, and *g*-dropping (1894: 150–168).[8] In short, Wyllie had a wide range of linguistic interests, and evidently encouraged his students to share them.



A PHYSIOLOGICAL ALPHABET.

I.—VOWELS.

y — i e a o u — *w*

These should be pronounced in the Latin manner, as *ēē*, *eh*, *ah*, *oh*, *oo*. *y* and *w* are consonants, not vowels, but, as explained in the text, they have very close relationships to the vowels, initial *y* being very closely related to *i*, and initial *w* to *u*.

II.—CONSONANTS.

ILLUSTRATIVE SENTENCES.

I.—VOWELS.

Even ancient elves are awed over oozing.

This sentence represents only long vowels. Their short equivalents can be represented, as shown by Mr Pitman, by attaching the letter *t* to each vowel, thus:—

ĕĕt, it, et, at, ut, ot, ŏŏt.

II.—CONSONANTS.

	Voiceless Oral Consonants.	Voiced Oral Consonants.	Voiced Nasal Resonants.	
Labials. (*1st Stop Position.*)	***P*** (W)	***B*** W	M	Peter Brown made w*h*ite wax.
Labio-Dentals.	F	V		Fine villages.
Linguo-Dentals.	Th¹ S	Th² Z		Thinkest thou so, zealot?
Anterior Linguo-Palatals. (*2nd Stop Position.*)	Sh ***T*** (L)	Zh ***D*** L R	N	She lei*s*urely took down nine large roses.
Posterior Linguo-Palatals. (*3rd Stop Position.*)	***K*** H or Ch	***G*** Y (R)	Ng	Can Gilbert bri*ng* Lo*ch* Hourn youths?

The voiceless *W* and the voiceless *L* have been given above within brackets, the former being now almost confined to Scotland, and the latter being peculiar to Wales. The burring or uvular *R* is also given within brackets.

Figure 3.2 Wyllie's 'physiological alphabet' (1894). The illustrative sentences are ingeniously contrived so that the sounds are exemplified in the same order as their arrangement in the alphabet (e.g., 'Peter Brown made white wax' has [p b m ʍ w], and so on).

His 'physiological alphabet' is a prominent part of the work that is brought to the reader's immediate attention on a fold-out opposite the title page (see Figure 3.2). By 'physiological' in this context he means the same thing as Bristowe before him (see Chapter 1)—i.e., organised rationally so as to make plain the correlations of voicing, place and manner among consonant sounds. Wyllie gives a brief history of previous physiological and phonetic alphabets (pp. 4–5). But he does not attempt to answer the whole of the 'physiological question' posed by Müller, which was 'Which are the principal sounds that can he formed with our organs of speech, and therefore may be expected to occur in any of the dead or living dialects of mankind?' (1854: 2). Wyllie restricts himself to the sounds of English, only filling what would otherwise be empty cells of the taxonomy when the relevant sounds are to be found within the British Isles. In this way he adds what he symbolises as '(L)', 'Ch' and '(R)' – that is, [ɫ], [x] and [ʀ] (perhaps also [ʁ]—the explanation is not quite clear). There is no question of adding to the alphabet any categories which are not required for English (such as Bristowe's voiceless nasals), even though his (rather amateurish) survey of sounds in other languages (p. 170) mentions both Arabic 'gutturals' (presumably pharyngeals) and Hottentot clicks.

The chief use of the alphabet was apparently in the treatment of stammering. Wyllie believed that the cause of stammering was a failure of coordination between the phonatory and the articulatory mechanisms.[9] The patient was to be helped by giving him an insight into the timing and preparation of the adjustments required in forming a particular word-initial sound (usually a consonant). Though Wyllie was therefore interested in the timing of vowel onsets, and the alternation between vowels and consonants in the stream of speech, the precise articulatory configurations of vowels did not greatly concern him. Perhaps for this reason, the treatment of vowels in his alphabet is perfunctory and unsatisfactory—somehow, they are to be represented by *i*, *e*, *a*, *o*, *u* pronounced 'in the Latin manner'. The details, he says, are 'not of sufficient importance to be here described in detail' (p. 8). This is an interesting reversal of Airy's view that there was 'little to remark' about consonants—see Chapter 1.

Wyllie doesn't merely tabulate his alphabet, but devotes about 10 pages to leading the reader through the classification scheme, making observations on pronunciations to be heard in English, and to some extent in other languages, and contrasting it with conventional spelling. Whereas many other physiological alphabets served only to represent individual sounds, Wyllie does give some thought to the possibility of transcribing connected speech. On pages 145–148 he envisages an extension of his alphabet as a means of teaching the deaf, elaborates his scheme of vowel description somewhat, and gives a short specimen of transcription (a sentence 'spelt phonetically').

Wyllie shows that he has a good understanding of what would now be termed Voice Onset Time, and of aerodynamic differences between voiced and voiceless stops. A footnote on page 10 explains:

> On careful experiment, I find that for the production of B it is not absolutely necessary that the air accumulating behind the barrier should be voice-laden. All that is absolutely necessary is that the voice should be emitted as soon as the air. In the pronunciation of an initial P, the air begins to escape just an instant before the voice. This can be made evident if a house-key be held before the mouth as if for whistling, and Pa and Ba be pronounced over it. And, no doubt, the same distinctions exist between T and D, and between K and G.

In the same footnote, he adds:

> … the pressure of air behind the barrier is greater in the voiceless than in the voiced explosives. In the voiceless explosives the pressure of the air in the mouth is the same as in the trachea but in the voiced explosives the air meets with some amount of obstruction a the glottis, owing to the approximation of the vocal cords in phonation; and this obstruction in the larynx causes the air pressure to be less in the oral cavity than in the trachea.

Further remarks make plain that he understood, among other things, co-articulatory variation in the place of articulation of velar consonants (p. 12), the possibility of ‘tip-up’ and ‘tip-down’ coronal sounds (p. 13), and phonation on ingressive flow (p. 18), which he terms ‘drawback phonation’.

In short, he was an acute observer, and probably a naturally talented phonetician. It seems likely that a student who followed Wyllie’s lectures would have derived at least some of the benefits normally associated with a practical training in phonetics—for example, the cultivation of an analytical approach to speech, and the breaking down of prejudices based on the written language. Wyllie does use the word ‘phonetic’—first on page 4, and thereafter at numerous places in the book—though, significantly, never once the noun ‘phonetics’, suggesting that he was somewhat out of touch with contemporary developments. The most recent phonetician that he cites is Ellis—his source seems to have been Ellis’s *Pronunciation for singers* (1877)—and there is no mention of Sweet. Equally, there is no indication that Sweet knew anything of Wyllie. It is interesting to wonder whether the famously intolerant Sweet might have overlooked Wyllie’s amateurish philology (pp. 150–161) and his disregard of vowels (‘not of sufficient importance’ p. 8), and recognised the genuine phonetic insights in Wyllie’s work.

Wyllie has a vague mention of palatography (though he does not use that term), which he has learned about directly from Kingsley (1887), a paper which ‘demonstrates in an ingenious way the parts of the palate, etc., with which the tongue is brought in contact during pronunciation of certain consonants’ (p. 5). There is certainly no indication that Wyllie knew anything about Continental work in experimental phonetics, which, in terms of volume of output, was at its height around the very time he was writing. Wyllie gives a somewhat simplified account of the acoustics of vowels (pp. 6–7), referring to the piano experiment (in which a vowel is sung into a piano while the sustain pedal is held down). He mentions Willis, Wheatstone and Helmholtz but does not give any detail concerning their findings. The acoustics of the vocal tract also enter into his discussion of 'dysphonia clericorum' (or 'clergyman's sore throat'), which he identifies as a disorder of the pharynx (not of the larynx). Translating from Schech (1890), he says:

> The wall of the Pharynx forms the most important reflector for the sound-waves streaming out of the Larynx. Here, and against the Velum Palati, they first strike. If this wall is not smooth, but knotty and uneven, from granulations and hypertrophies, 'sound shadows' must be formed; just as an uneven mirror surface can give only a distorted image of a body placed before it. Owing to the faulty reflection, the tone thus suffers loss: it is weakened; it does not carry far; or, what is the same thing, it loses its metal.
>
> (1894: 71–72)

Wyllie was not alone among nineteenth-century writers in imagining sound emerging like a stream of rays from the larynx, and travelling along the vocal tract towards the lips by specular reflection from its internal surfaces. Diagrams of vowel positions given by Merkel (1857) appear to illustrate the very same concept (Figure 3.3).

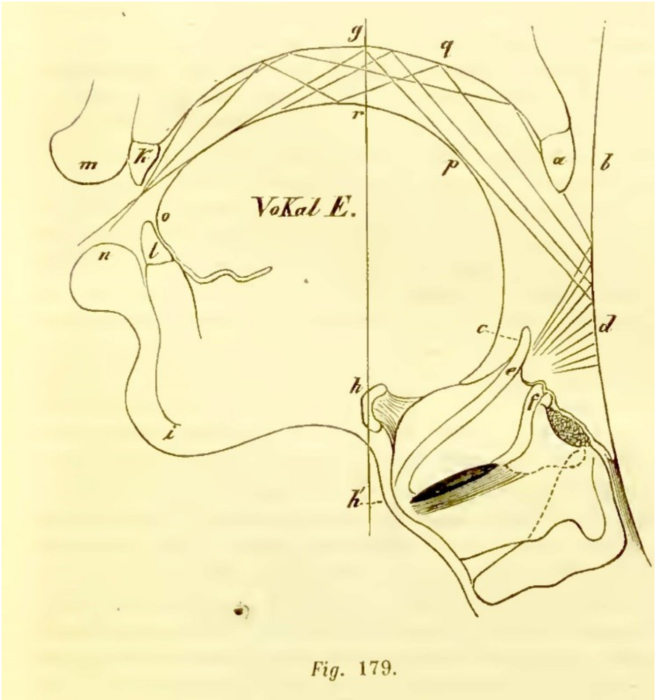


Figure 3.3 Merkel's representation of the vocal tract in the production of a vowel 'E' (1857: 790). A diverging beam is shown emerging from the larynx and being reflected from the inner surfaces of the vocal tract as if from a mirror, in the way that the paths of light rays are traced through an optical instrument such as a telescope. Merkel represents the other vowels in a similar fashion.

Wyllie had certainly read very widely, even if he was not entirely up-to-date in all the fields he tried to cover. But his accounts of philology, anthropology, and to some extent physics, appear self-taught and amateurish. He makes use of books intended for the common reader. During his discussion of whisper (p. 33ff), it becomes plain that he has not seen Brücke in the original, but knows of Brücke's view from Meyer's *Organs of speech* (1883), a book which appeared in the International Scientific Series, a publishing venture specifically 'directed to a popular reading audience' (Howsam 2000: 188).

Wyllie is just one representative of what had grown, by the end of the nineteenth century, to be a voluminous medical literature in Britain concerned with speech (cf. Rockey 1980). He has been selected here because the date of publication of his book (1894) places him in a defining position at the beginning of the period covered in this study and because of the long-lasting influence within British medical approaches to speech that his teaching role in Edinburgh brought him: he is cited as ‘still well worthy of study’ as late as 1935 (Seth & Guthrie 1935).[10] There is little doubt that if a native ‘experimental phonetics’ had come into being in the 1890s from the plentiful but disparate components within British phonetic science, Wyllie would have been qualified to play a full part in it.

3.4 Henry Sweet (1845–1912)

There is currently no extended study of Sweet’s life and work, but a detailed short account, and many references, are given by MacMahon (2004). Also relevant are Jankowsky (1999), Kemp (1995, 2001) and Momma (2012). By the beginning of the period covered in this study, Sweet was universally recognised as the leading phonetician of his day. He was President of the International Phonetic Association from 1887 until his death.

Sweet had made his name in phonetics with the *Handbook of phonetics* (1877). Though it contains no quantitative or experimental work, it is not openly hostile towards such approaches, and in fact the Preface contains a knowledgeable and balanced summary of (mainly German) work which had preceded the *Handbook*:

> Until within the last few years phonetics was hardly recognised as a science in this country, and it is to Germany that we owe the first attempt to construct a general system of sounds on a physiological basis—E. Brücke's *Grundzüge der Physiologie der Sprachlaute* (2nd ed., Wien, 1876). The investigation of the mechanism of the glottis in producing speech-sounds received a great impulse from the use of the laryngoscope, first introduced by Garcia, whose investigations were continued in Germany with brilliant success by Czermak, Merkel, and

> others. The latter, in his *Anthropophonik* (1856), and in the shorter and more convenient *Physiologie der menschlichen Sprache* (Leipzig, 1866), has accumulated a mass of details on the physiology of the vocal organs which for fulness (*sic*) and accuracy stands quite alone. The purely acoustic investigation of speech-sounds begun by Donders in Holland, and carried out more in detail by Helmholz in his celebrated work *Die Lehre von den Tonempfindungen*, seems likely to have a very important influence on the progress of phonetics. The main results of German investigation have lately been summed up in a most masterly manner and in a moderate compass by Ed. Sievers in his *Grundzüge der Lautphysiologie* (Leipzig, 1876), which has almost entirely superseded the older work of Brücke.
>
> (1877: vi)

But there are already signs of Sweet's quarrel with experimental phonetics—and with German experimentalists in particular. The generous passage just quoted is followed immediately by 'The fact that the majority of those who have worked at phonetics in Germany have been physiologists and physicists rather than practical linguists, naturally accounts both for the merits and the defects of the German school' (1877: vii–viii).

Sweet published a second treatment of general phonetics, *A primer of phonetics*, in 1890. Based around Sweet's development of Bell's Visible Speech, it focuses upon 'the art of producing speech-sounds and recognising them by ear' (1890: 1), and avoids all mention of experimental methods. But a late work, *The sounds of English* (1908), expounds Sweet's contention that there is an 'antagonism' between practical linguistic phonetics and instrumental phonetics:

> At present there is a natural and indeed, unavoidable antagonism between the practical linguistic phonetician and the physico-mathematical instrumental phonetician. The qualifications and training required on both sides are so opposed to each other, and each of these branches of research makes such imperious demands on the time and energy of its votaries, that

> it is difficult to see how any one investigator can combine them. […] It cannot be too often repeated that instrumental phonetics is, strictly speaking, not phonetics at all. It only supplies materials which are useless till they have been tested and accepted from the linguistic phonetician's point of view. The final arbiter in all phonetic questions is the trained ear of a practical phonetician. Differences which cannot be perceived by the ear—and many of the results of instrumental phonetics are of this character—must be ignored; and what contradicts a trained ear cannot be accepted. And it must not be forgotten that the utility of instrumental phonetics as a means of research does not necessarily imply a corresponding utility as a help in acquiring a practical mastery of sounds which, as we have seen, is the only sound foundation of the science. As yet, instrumental phonetics, so far from being a help in the practical study of sounds, has been rather a hindrance, by diverting the learner's attention from that patient cultivation of the organic and acoustic sense which is the indispensable basis.
>
> (1908: 109-110)

The negative assessment of instrumental phonetics is reproduced almost verbatim in Sweet’s long article ‘Phonetics’ in the 11th edition of the *Encyclopædia Britannica* (1911: 458–467).

To a degree, the criticisms raised by Sweet are well-informed and reasonable. He knew Scripture’s *Elements of experimental phonetics* (1902), and includes it—with a caveat—in the bibliography of Sweet (1908): ‘A convenient summary of the methods and literature of instrumental phonetics will be found in E. W. Scripture, *Elements of Experimental Phonetics* (New York, 1902), although this work is from the linguistic phonetician's point of view inadequate.’ (He does not specify in what way it is inadequate).

In relation to palatography, he observes (1908: 108) ‘This method is limited in its application, and its results are often doubtful and ambiguous’, adding in 1911 that palatograms are ‘much vaguer than they appear in the published drawings of them’. He expresses disappointment, too, with X-rays; the results hitherto are ‘too vague to be of much use’ (1908: 107). The 1911 article confirms that Sweet had probably observed X-ray images on a fluoroscope screen rather than as photographs: ‘…the shadow of the tongue projected on the screen is too indistinct to be of any use’ (1911: 459).

Further criticisms raised by Sweet concern the undigested and unhelpful presentation of large volumes of instrumental data (1908: 108-109), and the possible interference with a speaker’s patterns by even the thinnest artificial palate (1911: 459), or when required to speak ‘into a funnel with his mouth full of apparatus’ (1908: 109).

But Sweet’s criticisms were also partly fuelled by hostility and quarrelsomeness. As MacMahon says (2004): ‘He was exceedingly sharp with his tongue (and pen) to colleagues with whose views he did not agree’, and Jespersen recalled that ‘…towards the close of his life Sweet had almost a knack of falling out with other linguistic scholars, so that several who had formerly been good friends of his would have nothing at all to do with him’ (1995: 98). He adds ‘...it could be embarrassing to listen to him constantly running down other scholars.’

A major component of Sweet’s disagreements with his German contemporaries Moritz Trautmann (1842–1920) and Friedrich Techmner (1843–1891) was an acrimonious dispute over vowel description, with Sweet championing an articulatory scheme derived from Bell, but the Germans favouring auditory classification within a vowel triangle. Matters were made worse when Sievers changed sides in the second edition of his *Grundzüge der Phonetik* (1881), in which he acknowledges Bell’s *Visible Speech* and Ellis’s *Early English Pronunciation* as the two works ‘which have founded modern phonetics'.[11] This drew an immediate attack from Trautmann, which in turn was quickly countered by Sweet. Then in an 1882 Report on Phonetics to the Philological Society[12] Sweet goes over the matter in detail, and adds an unfavourable review of Techmer (1880). He pours scorn and condescension on both: 'The reckless, almost boyish, conceit of Trautmann's tone has certainly excited more amusement than indignation among his adversaries, but is nevertheless to be deplored…he has made a bad beginning to his phonetic career.'

Sweet's remarks on Techmer are worth quoting at length:

> Techmer, in his *Phonetik*, has also gone a way of his own, but what that way really is, or what his object was in publishing this elaborate and expensive work, I am unable to say. The book consists of a mass of anatomical details, many of which have scarcely the remotest bearing on phonetics, with remarks on acoustics, psychology, the origin of language, and other general questions, together with a mass of undigested quotations from the most incongruous authorities. The author's views on phonetics proper are expressed in the vaguest and most abstract way, and he has added little or nothing to our knowledge of the actual sounds of language. Not a single key-word is given to explain what sound the author means by 'open e', &c. Nor is there any clear definition of the author's standpoint compared with that of his predecessors. Although the work no doubt contains many hints which may be useful to specialists, it is an entire failure as a guide to general phonetics.

There is some truth in this criticism. Techmer certainly does not succeed in establishing the relevance of all the detail he assembles from anatomy and other fields—and similar objections can be raised against many a 'speech science' textbook published since. But Techmer's work does have real merits, too (see Chapter 2 for his contributions to palatography).

Interestingly, Sweet's quarrel with Trautmann did not prevent him from including Trautmann's *Die Sprachlaute* (1884) in the select bibliography of 'the most important general works' in his *Britannica* article (1911: 467).

3.5 Richard John Lloyd (1846–1906)

An extensive and comprehensive treatment of the life and career of Lloyd, and a full bibliography of his writings, is given by MacMahon (2007). The present account attempts only to add some technical detail to the analysis of Lloyd's methods and results.

Lloyd's views on vowels are contained in more than a dozen publications covering a period of about 10 years (1890–1900). The papers overlap considerably in content, have similar titles and similar stated aims, and do not at first sight show any clear line of development, so that it is difficult to select any one paper as definitively representing Lloyd's position, or even two or three giving snapshots of an evolving view at different times in the decade. Eight of the papers form an extended series published between 1892 and 1897 in the journal *Phonetische Studien* under the title 'Speech sounds. Their nature and causation', with a consecutive numbering of sections throughout. To simplify the discussion, these 8 papers are here referred to as 'S1'–'S8', the full references being given in a note.[13] MacMahon summarises these papers systematically (2007: 301–311), though even with guidance from that summary they do not make straightforward reading.

Lloyd began from a general analysis of the state of affairs in the vocal tract for the production of a vowel. He visualised this as a bottle with a neck of varying length and form, for which two parts he adopts the terms 'chamber' and 'porch', the chamber being in the pharynx and the porch in the mouth, near the tongue constriction. On general grounds, he expects two resonances, but does not imagine that the two parts of the system act as entirely separate resonators. One resonance will be due to the whole cavity made up of porch and chamber together, and a second contributed chiefly by the porch.

Lloyd divides vowels into an *i*-group, an *a*-group and a *u*-group (S1: 266). The first type has a simple tubular porch, the second has a 'bulging' or 'vaulted' porch, which is considerably wider at the front than at the back, while the third has a 'bulbiform' or 'bulbous' porch, the rear of which is delimited by the raised tongue, and the front delimited by narrowing at the lips. He points out that the three types, and the progression of subsidiary vowels within each type, represent a progressive decrease in porch resonance throughout (S1: 266).

He modelled this configuration with a real bottle (or in some instances a glass cylinder), closed with a cork, adding the porch for the simplest *i*-type configuration by a rolled paper tube of adjustable bore and length and fitted to the cork. He applied noise excitation, which he generated by blowing air through a tube packed with broken glass pieces to generate turbulence. The resulting small device could be held close to the ear for judgement. The process was then one of analysis-by-synthesis:

1. The starting conditions for the porch + chamber model were based on very approximate articulatory data for a given vowel, obtained by visual examination, probing, and estimation. It should be remembered that Lloyd was working before the results of Grandgent or Atkinson were published.
2. The model was then blown with noise excitation and repeatedly adjusted to find the best auditory match with the intended vowel.

3. The dimensions of the model in its optimal adjustment were used to calculate two theoretical resonance frequencies, using existing empirical formulae for resonators of the appropriate forms—e.g. a formula given by Rayleigh for long-necked flasks (Rayleigh 1878. Vol. 2, p. 173).
4. Auditory judgement was employed to place the two resonances of the model on a musical scale. (To assist judging, the model can be excited in different ways so as temporarily to render one or the other resonance more prominent. For instance, blowing across the end of the paper tube will excite chiefly the porch resonance).
5. The calculated resonance frequencies—suspected to be somewhat approximate, but unlikely to be in error by a whole octave—could now be used to ensure that the judged auditory pitches were within the correct octave.

Reduced to its essentials the method is revealed as a very ingenious process of successive approximation. Starting with initial data that is barely better than guesswork, it blends two stages of auditory judgement with calculation and modelling in an attempt to arrive at quantitative estimates of resonance frequencies.

But it is hard to point to any results where Lloyd appears to have made the method work particularly well. For example, his Table 1 (S2: 43), concerned with a vowel of type [ɪ], places F_1 much too low (in the region of 120 Hz), and F_2 considerably too high (in the neighbourhood of 3.5 kHz). Throughout his first series of experiments, Lloyd continued to find very low values for the lower resonance, and this leads him to entertain the mistaken hypothesis that ‘the inner resonance is tuned to the vocal chords' and is substantially constant (S3: 204), even though he is aware (p. 205) that this runs counter to evidence from Helmholtz and A. G. Bell.

By S2, Lloyd had convinced himself that what matters in the specification of a vowel is not the resonance frequencies themselves, but the ratio between them, which he terms the 'radical ratio'. By manipulating the formulae he was using to compute the resonances of his model, he derived a method of directly calculating the radical ratio of a particular model configuration without first determining the resonance frequencies themselves, and from Table 3 (S2: 56) onwards he often cites radical ratios without specifying any actual frequencies, with the consequence that his results begin to be very difficult to follow and to assess. In S4 he takes a further precarious step in asserting that the best exemplars of the major vowel types ('cardinal vowels' in Lloyd's idiosyncratic terminology) are those in which the radical ratio is a prime number: ' … every prime radical ratio is associated with a vowel sound of more than ordinary impressiveness and individuality' (S4: 276).

In the course of S4, Lloyd abruptly begins to take notice of the recently published work of other investigators, remarking

> The year 1890 has been signalized by the publication of two remarkable series of researches in which it has been attempted, with markedly increased success, to analyse the vowels by phonographic means. The primary aim of both sets of experiments is the same: it is to cause the vibrations of a membrane or plate, against which a vowel sound is uttered, to transcribe themselves legibly, in a more or less magnified form, upon the surface of an evenly rotating cylinder, or other moving surface. The apparatus by which this is done is sometimes called a phonautograph. (S4: 295)

The researches he means are those of Pipping (1890) and Ludimar Hermann.[14] Lloyd tabulates and compares some of their results in his Table 9 (S4: 305), where he attempts to align their findings with his own claims concerning the radical ratios of the vowels in question—which they plainly do not match at all well, though Lloyd casts about for partial correspondences and excuses. In the case of vowels of general type [i], for example, Pipping and Hermann report ranges of very plausible F_2 values which agree at around 2300 Hz. But this is much too low for Lloyd's 'radical ratio' of 41 to 43. (In fact Pipping's results would yield a radical ratio of about 8). Lloyd is driven to blame defects in the equipment used by Pipping and Hermann: 'We cannot yet think that *i* has been successfully analysed … We conclude that the instrument employed did not fully record the highest vibrations of the vowel: they were beyond its "field of view"' (S4: 305). In the remaining sections Lloyd continues to be unwilling to take the empirical findings of Pipping and Hermann at face value; again he insists on interpreting them in the light of preconceptions derived from his model. In S6 he arrives at an estimate for F_2 of a vowel of type [ɛ] which at 1370 Hz is very comparable with those provided by Pipping and Hermann. But since he believes the radical ratio of such a vowel will be 11, he searches (in vain) for signs of an implied F_1 of 125 Hz or so 'but neither Hermann's nor Pipping's curves enable us to trace any such influence. We remain without evidence …' (S6: 140).

It is probably impossible to determine in any detail where Lloyd went astray with his (potentially promising) models, and thus arrived at unrealistic radical ratios which in turn prejudiced his assessment of the work of Pipping and Hermann. It seems likely, however, that a major part of the problem lay in his F_1 estimates. The lower resonance of his models was probably much too low in frequency, and thus the vowel spectra he produced can seldom have been realistic overall, though this deficiency was probably masked to some degree by the use of aperiodic excitation and the implicit perceptual comparison with *whispered* vowels which this may have triggered.

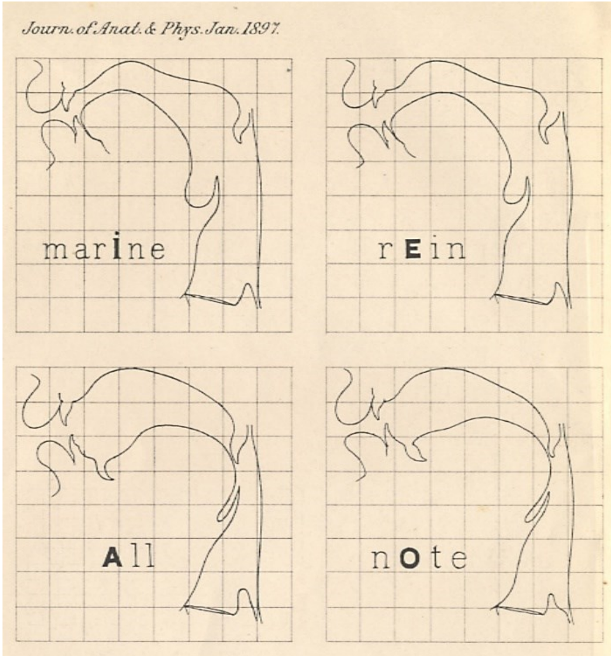


Figure 3.4 Midsagittal sections for vowels published by Lloyd in 1897.

It appears that Lloyd's knowledge of literature in the field was developing as his writing progressed. In S5 we find the first of several mentions of Grandgent, and it is evident that Lloyd and Grandgent either met or exchanged letters. Later he mentions '[t]he ingenious method of internal measurement described by Mr. Grandgent', though this had

> … failed to indicate the position of the uvula. This was owing, I am told, to the tactile insensibility of that organ. But Mr. Grandgent has since succeeded in making an arrangement of light and mirrors which disclosed the position of the uvula in all his normal vowel articulations and he has favoured me with tracings for all the "back" vowels.
>
> (S7: 265)

In the period 1896–1898, Lloyd published a number of vocal tract outlines for both vowels and consonants (see Figure 3.4). They are beautifully clear, though of uncertain origin and dubious accuracy (the 8 × 8 grid they appear on suggests a degree of precision which is probably spurious). Lloyd does not specify how they were obtained, but the second part of Lloyd (1891e)[15] dealing with ‘an improved method of measuring articulations’ refers to the recently published results of Grandgent and again suggests (pp. 146–147) that Lloyd was in correspondence with Grandgent. The Tracts contain 4 offprints of papers from Lloyd spanning 1896–1899 which had been in the possession of Harold W Atkinson, inventor of the Mouth Measurer. Three are inscribed from Lloyd to Atkinson, indicating that the two men were in contact in those years. Both served (1898–1899) on the Phonetic Sub-Committee of the Modern Language Association (of England); another member of that committee was Walter Rippmann [Ripman] (1869–1947), who was to include Lloyd’s diagrams in successive editions of his *English Phonetics* (it remained in print until at least 1957). Atkinson published his own results in 1897–1898, and he too was in contact with Grandgent (see Chapter 2). It seems very likely that Lloyd obtained his vocal tract outlines using Grandgent’s method, probably with at least some of Atkinson’s improvements, which were being developed at precisely the time Lloyd and Atkinson were in contact. It is ironic that while Lloyd seems never to have published a single diagram illustrating his own vowel models, these late vocal tract diagrams, which have very little connection with his most original ideas about speech, have become the most visible and lasting part of Lloyd’s output.

Some of Lloyd’s insights are brilliant, and he writes well in the sense that individual sentences or paragraphs can be clear and persuasive, but the large scale organisation leaves a great deal to be desired. The impression is of a project begun, interrupted by numerous wearisome digressions, modified while in progress, and eventually left unfinished and effectively abandoned. Even the last of the eight papers in the series concludes with ‘To be continued’ (S8: 24)—although it never was. Another characteristic of Lloyd, especially in these *Phonetische Studien* papers, where he seems to have been under no length restrictions or editorial pressures, is the laborious rebuttal of every apparent exception or difficulty. These are often ingeniously turned around so that they become evidence *for*, rather than *against*, the case that Lloyd is advancing. But the cumulative effect of all this special pleading is to weaken his argument.

In his later papers on vowels, Lloyd becomes mainly a reporter and interpreter of results obtained by others who had laboratories and instruments at their disposal (Lloyd seems never to have had any apparatus beyond bottles, corks, and bits of cardboard). He must have realised that his own work dating from around 1890 was superseded—by the end of the decade, he refers to ‘my early work’ (1897a: 242). Mentions of his ‘radical ratio’ become less frequent, though they do not entirely disappear. Lloyd also made some attempts to analyse the spectra of voiceless fricative consonants (1899i; 1900e), where the task is not in principle too different from the analysis of noise-excited vowel spectra which he had begun ten years earlier, but without conspicuous success (cf. the work of Rayleigh described above).

Although Lloyd was certainly read by some prominent contemporaries, and got into detailed controversies with Hugo Pipping (1864–1944) and Felix Auerbach (1856–1933), we may doubt how many people since then have had the patience to read through his work in any detail. It is noteworthy that the account given by Paget (1930: 22–24) is wrong in several respects, and looks as if it may be based on the reading of just one or two (late) papers of Lloyd’s. For instance, the table of resonances which Paget reproduces (p. 23) does not in fact show Lloyd's own results as Paget implies, but results *complied and tabulated* by Lloyd (1897a: 249) from the publications of Pipping, Hermann and Jan Boeke (1874–1956). And when Paget says 'Lloyd appears to have experimented with models … [but] … no record appears to have been published' (1930: 24), he betrays that he cannot have studied any of Lloyd's earlier papers, in which experimentation with models is essentially the *sole* method employed.

3.6 William Arthur Aikin (1857–1939)

For much of the twentieth century, W. A. Aikin was a widely-cited authority on all matters concerned with the voice, in the fields of music, dramatic performance, and—to some extent—phonetics. His book *The voice* appeared in 1900, was considerably revised in 1910, and then remained in use and in print for many decades, being issued in a 'new edition' as late as 1967. Nevertheless, there appears to have been hitherto no critical assessment of Aikin, and even the basic details of his biography have been unknown. He merits a very brief mention in O'Connor (1991: 539), who notes the lack of a biography.[16] Though obituary tributes appeared in both *The Times* (Colyer 1939) and *The Musical Times* (Anon 1939), neither gives many particulars of Aikin's life, and the following account has been assembled from the civil record, *The Medical Directory*, and a variety of other sources.

He was born in London into a medical family. His father, grandfather, and great-grandfather were all medically qualified, as was his eldest brother. He was educated at Wellington College, and trained in medicine at Guy's hospital. He gained his MRCS in 1884, and in 1886 added the LRCP (Lond.), and the Licentiate in Midwifery from the Rotunda Hospital, Dublin. He completed his medical education in 1887 with the degree of MD awarded by the University of Brussels. This last does not imply a period of residence and study in Belgium, nor indeed any facility in the French language. Over a number of years, the Brussels degree provided British medics with a relatively inexpensive route to the 'much coveted title of M.D.' (Leonard 1903: 1037) that required attendance only for the period of 7 to 9 days devoted to the examinations—which were conducted in English. Distance coaching (by post) was offered. (Pocock 1881; Leonard 1903; Henchley 1903).

Aikin was elected as a member of the Laryngological Society of London in 1893 (its inaugural year).

In 1900 the first version of his book *The voice* appeared. It already outlines all the four main themes which recur throughout Aikin's work:

1. Management of the breath in speaking and singing.
2. The 'resonator'—i.e., the cavities of vocal tract, especially their use in vowel production. He establishes a quasi-musical 'vowel scale'.
3. The 'vibrator'—i.e., the larynx.
4. A prescriptive concern with all three of the aspects above, with a view to training public performance of song or speech.

His later publications take up these various themes, and add a certain amount of detail, but do not fundamentally alter any of the positions set out as early as 1900. He returns to the subject of breathing in (1905) and (1910b), and offers some observations on the 'vibrator' in (1902), while his thoughts on teaching speech are explained further in (1923). The 'resonator' and his vowel-scale are the most visited topics, being the subjects of (1900b), (1903), (1904), and—much later—(1927). Two longer papers,

(1906) and (1909) are effectively compact overall syntheses for more academic audiences of the materials which form the contents of the second edition of his book (1910a).

We now consider his contributions under these headings. Aikin's account of breathing for speech or song, and the measurements he applied to it, are essentially unoriginal. In medicine, the study of respiration already had a long history. Spirometry—the measurement of lung capacity—had been developed by an English physician, John Hutchinson (1811–1861) (Panconcelli-Calzia 1940: 11; Spriggs 1977; Bishop 1977). By 1895, respiration was a topic sufficiently developed to be chosen by Marcet as the topic for the Croonian lectures to the Royal College of Physicians, and when Scripture published his *Elements* in 1902, he was able to devote a whole chapter to breathing. One of Aikin's themes is the balance of breathing from the ribs or from the diaphragm, but this was certainly not new: by the late nineteenth century quasi-scientific controversy about the relative merits of different breathing techniques for singing was commonplace (Curtis 1896).

As for the 'vibrator', Aikin (1902) is probably the closest approach in Aikin's output to a conventional scientific paper (it is noted in Gutzmann 1909). Very clear drawings made from laryngoscopic examination of a single male singer illustrate various voice types and the possibility of combining them with breathiness achieved by an arytenoidal chink. The artist is not identified, and the implication may be that Aikin himself made the drawings. Given that laryngology was ostensibly Aikin's medical speciality, it is perhaps surprising that he published no more on the larynx itself. This is probably because he seems to have regarded the larynx as much less amenable to conscious control and training than the breath or resonator components of the voice: '… all that concerns the vocal cords belongs more to an automatic process …' (1910: 12).

Figure 3.5 Aikin's Resonator Scale in its fully-developed form (1910a: 53). The resonances of vowels from GOOSE to STRUT form a diatonic scale, which is continued in the upper resonance of vowels NURSE to FLEECE.

Aikin's Resonator Scale (Figure 3.5) is essentially a scheme of vowel resonances, derived from whispered vowels, and thus comparable with many previous attempts and with the later analysis made by Paget (1922). Though it underwent some elaboration over time, the system was basically founded on the idea that 'the resonance' of the vowel 'A' for an adult male speaker is the note c″ in the third space of the treble clef[17]—i.e. 528 Hz (1900: 43). The lowest resonance, that of 'U', Aikin places a fifth below—that is, the note f′ in the first space of the treble clef. The highest resonance, for 'I', he places on the note c‴ one whole octave above that of vowel 'A'. Between the two extremes there are thus twelve steps of the diatonic scale in C—approximately the right number to assign one step to each of the distinct vowel qualities of English, which Aikin proceeds to do. He is also aware that for some of the vowel qualities whose resonances are higher than that of 'A', there is a second lower resonance which progressively descends in pitch as the main resonance rises.

Aikin was clear that a given person's 'A' resonance need not literally be the note c″ at 528 Hz. For a female speaker, it would be higher, and there would be individual variation too. His idea, though, is that the musical relationships among the vowel pitches are maintained, so that after the necessary transposition to align the pitch of 'A', the vowel scale still applies. For this reason, the twelve steps of the scale on the large fold-out table of resonances inside the back cover of Aikin (1910) appear with a blank staff: the user is to determine and fill in the pitches of his or her own voice.

Aikin believed that he produced his vowel 'A' with a vocal tract tube unconstructed at any point, and he thus reasoned that the pitch of 'A' should be the quarter-wavelength resonance of a tube corresponding to the whole vocal tract length. The exact details of his own calculation (1900: 43) are not given, but it is true that if the vocal tract is reckoned at 6.5 inches (16.5 cm) in length, and the velocity of sound at 350 m/s, the resonance will be approximately 530 Hz—an almost exact match for the c″ he thought he heard.

Unfortunately, as both Perrett (1923: 60) and Paget were to point out, Aikin had made an octave error at the outset in identifying the resonance of 'A' as c″. All his resonance estimates must be moved up by an octave, so that the real resonance of his 'A' is rather more than 1 kHz. The calculation which Aikin had made in fact corresponds with the F_1 of a vowel of type [ə]. For this vowel, Aikin's symbol is 'ER', and he places it at VII on his scale, on the note e″, two steps above the resonance of 'A'. Transposed upwards by an octave to e‴, this yields about 1300 Hz. So it appears that despite Aikin's confident impressionistic reports of a maximally open articulation, his 'A' did of course have an unsuspected (pharyngeal) constriction, and that when the relatively open length of the whole vocal tract *is* implicated in the resonance—a situation that is approached with 'ER'—Aikin's estimate of the vowel resonance appears to follow F_2 anyway (modern estimates of F_2 in RP NURSE are about 1400 Hz).

From around 1902, Aikin began to use the word 'Phonology' in a novel sense, unrelated to those already in use in his time or to the sense which has prevailed since. The second edition of *The voice* (1910) opens with the sentence 'Phonology is the science of vocal sound'. It is hard to pin down exactly what he meant by this. It was apparently to be the comprehensive science of every aspect of vocal sound production, covering breathing, phonation and articulation, in both speech and song—and in principle including the vocalisations of other animals as well. He sent an autographed copy of *The voice* (1910) to Lord Rayleigh, together with a letter hoping to enlist Rayleigh's approval and support.[18] In the letter he defines 'phonology' as aiming to provide the 'scientific principles' upon which 'the voice must be trained from the first' and says that it must include 'the practical application of Physical and Physiological laws to speech, language and singing'.

The existing fields of phonetics, elocution, and music were to be mere components or ancillaries: 'The study of phonetics cannot be regarded as sufficient for the needs of the voice. The investigation of the sounds of speech and their reference to a phonetic alphabet no doubt greatly facilitate the correct pronunciation of languages, but they cannot reach any of the problems which underlie the actual development of the voice as a natural sounding instrument' (1910: 3).

But his aims did not stop at description—he has no interest in documenting ordinary colloquial speech, for example. On the contrary, phonology was to be concerned with the training of speech and song, and indeed with their optimal development and improvement. Aikin's 'phonology' is thus a very strange concept from a modern viewpoint, being a sort of union between a fairly recognisable speech science on the one hand, and prescriptivism on the other. Though he does not make the comparison explicit himself, a parallel with medicine may be appropriate. Medicine does not stop at anatomy and physiology, nor with the description and classification of diseases, but seeks to promote health and indeed to improve living conditions and extend life. Revealingly, Aikin uses phrases such as ‘public health’ and ‘public education’ (1910: 2–3) in his discussion of the function of ‘phonology’.

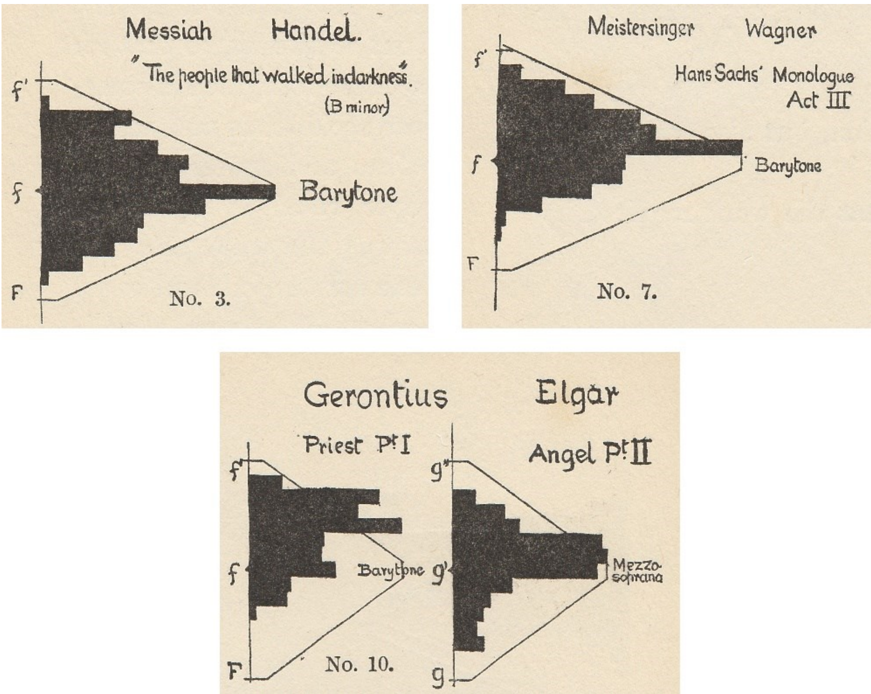


Figure 3.6 Examples of Aikin’s ‘Song diagrams’, actual size. (Re-arranged from Aikin (1910: 138–139).

Aside from the Resonator Scale, one of Aikin’s most original and interesting ideas was the ‘song diagram’, which seems to have had its first airing in 1909. It takes the form of a histogram indicating the time which requires to be spent at different pitches when performing a given song—essentially a vocal profile. Aikin compiled them manually by working through the score and entering tally marks in appropriate boxes. In the examples shown in Figure 3.6, the two baritone ('barytone') parts by Handel and Wagner (No. 3 and No. 7, upper row) show an excellent fit with the nominal baritone pitch range (the pitch scale covers two octaves), whereas the baritone part by Elgar (No. 10, lower row, left) requires the performer to spend a lot of time in the upper part of his pitch range. By contrast, Elgar produces an almost perfect match with voice range for the mezzosoprano part at right in No. 10.

Aikin claimed, very reasonably, that such a diagram gives a much better indication of the suitability of a piece for a particular voice than does the simple range alone, and suggested that such diagrams might be included on the cover of a printed song. (Aikin was himself a composer of songs; the earliest published seems to date from 1884). His histograms bear an obvious similarity to those which can now be extracted automatically to study the distribution of vocal fold periodicity.

In a way, Aikin might be described as the reasonable face of prescriptivism. He is not a bad scientist—it is true that he made an error concerning the octave in which his vowel pitches were located, but then many others made similar errors, including Helmholtz himself. Some of his midsagittal diagrams show somewhat implausible tongue positions (see Chapter 6), but in 1900 good evidence about vowel articulations was not plentiful. Particularly when addressing a scientific audience (as in 1900b, for example) his language is measured and precise. For other audiences, his writing is concise and stylish, avoiding technical terms, and resembling that of the belle-lettrist rather than the scientist. He was a member of the Society for Pure English from its foundation,[19] and no doubt found his aesthetic preferences and prescriptivism were widely shared among his fellow members. He seems to have been adept at engaging with the culture of his day, and his ideas were given a warm reception in the diverse fields of medicine, music, and speech training. There is no indication that he tried to interact in any way with the field of phonetics. Nevertheless, at least some phoneticians took notice of his work; his book is extensively cited—for example, in the successive editions of Ward's *Phonetics of English* (1929, etc.), in Gray (1936), and was still among the sources consulted by Pike (1943). Perret (1923: 60) states that Aikin's vowel-scale was unknown to him until August 1918, and this must presumably indicate that Daniel Jones was not aware of it until then either.

3.7 Ernest Richard Edwards (1871–1947)[20]

In the years 1903–1905, Daniel Jones was preceded as phonetics lecturer at UCL by E. R. Edwards. His most significant publication in phonetics was his doctoral dissertation on the phonetics of Japanese, completed in 1903 and published in the same year (Edwards 1903). It was novel in giving illustrations of experimental data from palatography and kymography alongside conventional phonetic description and transcription with IPA symbols.

Very few details of Edwards's life have previously been published. He was born in Yokohama on June 16, 1871, the son of James Edwards, described as a tea merchant of Cricklewood, who was then living in Japan. In the 1911 UK census Edwards is described as 'British by parentage', but this confirms only that his father was a British subject. No information has so far been found about his mother.

According to a brief obituary written by Daniel Jones (1948), Edwards remained in Japan up to the age of 11. On coming to England, Edwards is said to have completed his schooling at Dulwich College, though it has not been possible to find any record of him in the Dulwich College Register.

In October 1890 he was admitted to Selwyn College Cambridge, but he did not gain a degree, and seems in fact to have completed less than one year at Cambridge, since by April 1891 he was an Assistant Master at Skinner's School in Tunbridge Wells. There are indications (see below) that his student career had been cut short by 'family losses'—presumably meaning that he could no longer be supported at Cambridge and had to earn a living. It's unclear whether 'losses' refers to bereavement(s) or to financial losses. By 1892 he is associated with University College School. It appears that he may have been at the school for most of the period 1892-1899, and then again in 1902.

Edwards is listed as a new member of the IPA in the February 1899 issue of *Le Maître Phonétique* (p. 34). He was proposed for membership by W. G. Lipscomb, a senior languages master at University College School. In the same issue, a note from Passy (p. 37) announces that Edwards is going to publish a phonetic edition of an English reader by Viëtor and Dörr (it appeared as Viëtor Dörr & Edwards 1901). Edwards is known to have spent six months at the University of Marburg, and to have been taught there by Viëtor, though the year is unknown. Edwards must have completed work on the transcribed edition of the Viëtor *Lesebuch* while in Paris, since he signs the Preface of the book 'Paris, September 1900' and thanks Passy as well as Viëtor.

In the summer of 1899 Edwards married Alice Maria Herbage (b. 1872), the daughter of a London bank manager. Soon after the marriage they travelled to Paris, where both Edwards and Alice matriculated at the Sorbonne as students taking the literature course for the *Licence*. But his growing interest in phonetics led Edwards to give this up, and instead transfer to the École des Hautes Études, where Paul Passy was lecturing. Passy encouraged Edwards to expand a student essay on the phonetics of Japanese into a doctoral research project (Edwards 1903: 7). Accordingly, in the years 1900–1901 Edwards and Alice undertook an extensive field trip to gather data throughout Japan. Edwards took with him a kymograph and a phonograph, and also worked with Japanese consultants for whom artificial palates were made.

The purpose and extent of the trip are explained in the Introduction to Edwards's thesis (1903: 7–9), and the events of the trip are documented both by bulletins in *Le Maître Phonétique*, and by the contemporary reports and later recollections of the Japanese colleagues he worked with.

In the December 1900 issue of *Le Maître Phonétique* (131–132), Passy reports that Edwards has 'recently left for Japan, in order to study in depth the language of the country, which he already knows, and to work on a study which will have the title *Étude de la langue japonaise parlée*. This will, we think, be a work of great value' [My translation]. This suggests that Edwards was in some degree bilingual as a result of his childhood in Japan.

Writing in *Gengogaku Zasshi* [The Linguistic Journal], Izuru Shinmura (1876-1967)—later a celebrated lexicographer and linguistics scholar—reported:

> E. R. Edwards is from Britain and has arrived at the end of January [1901], to carry out research on the Japanese language, esecially on pronunciation. He has studied phonetics under great scholars such as Sweet, Passy, and Vietor. He studied experimental phonetics under Passy[21] for two years, and on this visit, in order to measure the sounds of Japanese, Edwards has brought along with him a 'kymograph' (a kind of voice recorder) and he plans to stay in this country for about half a year. [Transl. Hiroko Saito]

A little later Edwards sent a ‘Letter from Japan’ to *Le Maître Phonétique* (pp. 72–73). Referring to a talk on phonetics he had recently given to ‘the philological society’ in Tokyo, he says ‘I showed them also something of the experimental side’. Edwards makes clear that in the analysis of Japanese sounds he is working in collaboration with ‘Dr Shimmura’ [*sic*] and ‘Dr Yasugi’ (Sadatoshi Yasugi (1876-1966), subsequently famous as an authority on Russian). Yasugi was the consultant for two short preliminary specimens of Japanese included in the 1901 note.

When Shinmura later wrote the preface to the 1935 Japanese translation of Edwards’s thesis, he added more detail—recalling that Edwards came to Japan ‘in January 1901’ to observe the sounds of Japanese. Edwards stayed at the Metropole Hotel in Tsukiji [a district in central Tokyo] for 2 to 3 months for the purposes of this study. Shinmura was asked to have an artificial palate made, and in addition he made kymograms and recordings on the phonograph. Shinmura pronounced Japanese for Edwards every second day; on other days, a friend of Shinmura made visits.

A further report from Edwards (1902), published after his return to London, mentions by name a further Japanese contact, Professor Okakura (Yoshisaburo Okakura 1868–1936), and gives samples of ‘Lewchewan’ (=Ryukyuan)[22] and Korean. Probably the intention was provide some background on the affiliations of Japanese, whether close (Ryukyuan) or remote (Korean). There is a brief mention of this in the introduction to the thesis (p. 8). Of the trip as a whole, Edwards says (1903: 7) that he visited 23 of the 43 ‘prefectures’ (districts) of Japan, and additionally travelled to Korea.

In May 1903 Edwards defended the doctorate[23] and the published version bears the same year.[24] The work created quite a stir, and was highly thought of by Passy, who not only published in *Le Maître Phonétique* a long and positive review (Vianna 1903)[25], but also took the unusual step of adding his own praise of the work, with an account of the thesis defence, in which he complained that certain (unnamed) examiners had monopolised too much of the discussion, and should have given the candidate more opportunity to speak.

Vianna calls the work 'superb', and 'a triumph' and says that it will be a model for future studies. It is plain that he and Passy were impressed and gratified by the fact that Edwards had produced an extended study of a living language in its colloquial form, and that the alphabet of the IPA had proved fully equal to the task. But in the present context it is of great interest that Edwards made use of data from palatography and kymography alongside conventional phonetic description and transcription, and that he employed these instrumental techniques (along with the phonograph) in the field to gather data from native consultants.

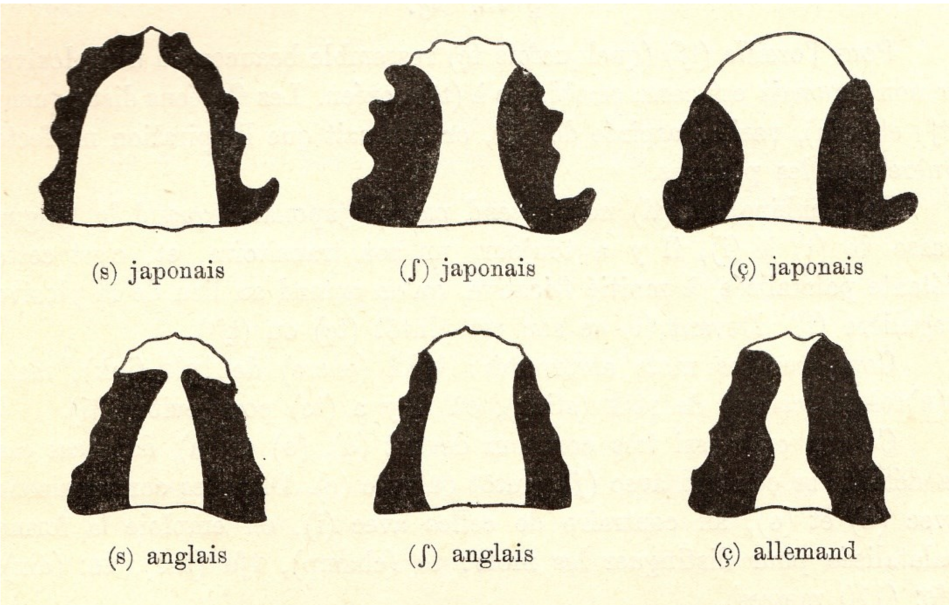


Figure 3.7 Palatograms from Edwards (1903: 37).

As an illustration of the type of data Edwards provides, Figure 3.7 shows his comparison of the articulation of sibilant fricatives from Japanese and English and of palatal fricatives in Japanese and German (p. 37), while Figure 3.8 shows a kymographic record of air pressure in an oral mouthpiece for a pair of words illustrating differences between single and geminate voiceless stops in Japanese (p. 29). On page 7 he states that a phonograph was used to settle difficult cases involving weak and voiceless vowels. No further details are given, but it seems possible that the device was used to facilitate auditory analysis by repeated or slowed-down replaying of certain portions of recordings. (No surviving recordings are known).

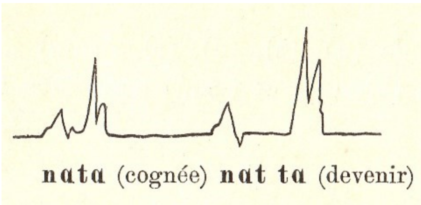


Figure 3.8 A kymogram from Edwards (1903: 29 illustrating a minimal contrast depending on consonant duration. The geminate [tt] of [nɑttɑ] (at right) exhibits both a considerably longer hold phase than the single [t] of [nɑtɑ] and greater airflow at release.

It has to be admitted that Edwards's use of experimental data is limited. The thesis contains in total only 10 kymograms, and 24 palatograms, and their function is always to *illustrate* aspects of the phonetic description, rather than to measure them or make new discoveries. Furthermore, the palatograms of German sounds are simply reproduced from Viëtor's *Elemente der Phonetik* (see Edwards 1903: 8). Edwards tells us that the kymograph he used was made in Paris, and perhaps it was the small portable model made by Charles Verdin (discussed in Chapter 5; see Figures 5.16 and 5.17). The kymograms published by Edwards have only a single channel (mouth pressure) and evidently—at least in the set-up used by Edwards—the mouth-pressure tambour did not respond to vocal fold vibration, showing instead a general indication of airflow even in voiced stretches of speech. It is noteworthy that his kymograms do not incorporate a time scale. One can imagine that a thoroughgoing experimentalist of the day (such as Rousselot) might have been dissatisfied with the limited use of experimental data, and the shortcomings in the data itself.

Later in the same year, more palatograms and kymograms of Japanese appeared in a paper by Okakura with the title ‘Quelques observations sur les sons japonais’ (Okakura 1903). It appeared in Rousselot’s journal *La Parole* for 1903, and judging from what is known of Okakura’s movements during a visit to Europe in the years 1902–1905, the work itself, carried out in Rousselot’s laboratory, must have begun in September or October of that year—that is, only about three or four months after Edwards’s defence. Okakura’s work seems to have been completed within a very short time and hurried into print without delay.

Okakura explains that he had been inspired with an interest in experimental phonetics after meeting Edwards in Tokyo ‘at the end of 1900’. But the most significant part of the encounter had been that Edwards had shown Okakura the first volume of Rousselot’s *Principes de phonétique expérimentale* (Rousselot 1897), where Okakura had read for the first time of ‘la seule méthode vraiment scientifique pour étudier la phonétique d'une langue’ (the only truly scientific method for studying the phonetics of a language).[26]

Where Edwards has a selection of two dozen palatograms, Okakura has nine full-page figures, each with multiple panels—and each of those with multiple palatographic tracings overlaid for comparison—which altogether present data from a series of at least 129 distinct palatograms. In fact Okakura goes systematically through the Japanese sound inventory, including co-articulatory variations. And while only one of his figures shows kymograms, it is a again a sophisticated composite, showing 6 syllables arranged in pairs for comparison of the fundamental frequency, and furthermore it has a timing trace from a tuning fork. It is hard not to see Okakura's paper as Rousselot's proxy response to Edwards's thesis—perhaps with the implication that this is what Edwards's thesis *should* have been like.

A paper for a non-specialist audience (1907, but delivered in 1904), produced during Edwards's brief period as phonetics lecturer at UCL, gives an insight into how he set about introducing the subject of phonetics to beginners, and suggests that his teaching probably used the same blend of descriptive and experimental elements found in his thesis. He evidently took a kymograph to the venue and demonstrated it at this presentation. Okakura, then in London, was in the audience, and his comments are minuted in the printed version. His remarks are self-deprecating, and elaborately friendly and flattering towards Edwards; they rather suggest that outside the reach of Rousselot's on-the-spot guidance his independent understanding of the capabilities of the instrumentation was limited.[27]

In 1905, Edwards left UCL to become an Inspector of Schools, giving up active work in phonetics, though he was to remain a respected figure within the IPA (he was re-elected as a Council member as late as 1939). In 1907, the vacancy at UCL created by his departure was filled by Daniel Jones.

Writing as 'A. Herbage Edwards', Edwards's wife Alice later published two largely autobiographical books which throw some light on the early years of their life together, and on Edwards's phonetic training. The first to be published, *Kakemono: Japanese Sketches* (1906), is presumably based on her experiences and observations during their lengthy field trip. Unfortunately it makes no mention of the linguistic motivation for the trip, but deals instead with lofty themes such as spirituality, beauty, and self-sacrifice, and is rather poetic and sentimental in style—very much in the manner of Lafcadio Hearn (1850–1904), the populariser and interpreter of Japanese culture, whose *Kokoro : Hints and Echoes of Japanese Inner Life* had appeared in 1896.

The later book to be published, *Paris Through an Attic* (1917) is a slightly fictionalised account of the period which the couple spent in a garret in Paris, living on a meagre £70 per year and attending lectures at the Sorbonne. She refers throughout to her husband as 'Richard'. It is here that the unspecified 'family losses' are mentioned that had curtailed his Cambridge career.

Her account of the chronology appears to be somewhat distorted for literary effect. She describes two continuous years in Paris leading to the doctoral defence (which is the story's climax), without the interruption of the field trip to Japan. But in other respects her account of the couple's teachers and of university life appears to remain close to the facts.

Of particular interest are her accounts of Passy (p. 146) and other lecturers, of how Edwards discovered the field of phonetics and his own aptitude for it, of the tension associated with his completion of the doctorate, and the drama of the doctoral defence, which she attended (pp. 345–351).

In a passage where she is explaining her admiration for the ethos and liberal outlook of the French university system, Alice also has a reference to Henry Sweet:

> [In Paris] … the door is wide open to the new man or the new science. And the University itself, while in no danger of running foolishly after new things, can adopt the new man or the new science when either has won his spurs. This is how new chairs get endowed in the Paris University with a frequency unmet elsewhere. While with us Oxford, that anointed teacher of dead learning, was only shamed by the ever-growing European reputation of Henry Sweet, and that after

> many years, into creating an ungenerous Readership in Phonetics which it comfortably allowed to lapse at his death. Thus we treat "new" men who are great scholars (of a world-wide fame) and a "new" science which in other lands has long since come into its own.
>
> (1918: 119)

Her remarks are a shrewd analysis of the situation which for so long delayed the establishment of phonetics as an autonomous science in Britain.

Apart from Passy, the only one of Edwards's teachers mentioned by Alice is Victor Henry (1850–1907), Professor of Sanskrit and Comparative Philology—and a significant linguistic thinker who has been seen by some as a precursor of Saussure (Joseph 1996). Henry was also to chair the examination panel (of three members) when Edwards defended his thesis, and he is presented by Alice in a very favourable light both as a lecturer (p. 150) and as a skilful, diplomatic and generous examiner (p. 348).

Henry is similarly praised by Passy (1903). Edwards's own acknowledgements (1903: 9) include Henry too, as well as Passy himself, and also Michel Revon (1867–1947), a lawyer turned Japanologist who was appointed to a professorship at the Sorbonne in 1899. Passy also mentions Revon's name, but it is unclear whether Revon was one of Edwards's three examiners. Like Passy, Alice says that one of the examiners made difficulties:

> … each professor … had his say, summing up the thesis in his own manner—one of them particularly running his own hobby so hard that he really wanted it done all over again and done completely differently that it might be his hobby and not Richard's thesis …
>
> (1918: 348)

She also tells us something about the examiners' deliberations concerning the grade to be awarded—whether 'reçu', 'reçu avec mention bien', or 'reçu avec mention très bien':

> We were told afterwards that [Henry] had wished to give the highest 'note' of all, the 'avec mention très bien', but the professor with the hobby objecting it had been compromised to the 'mention bien'.
>
> Then they all three came to the edge of the platform and shook hands with Richard, even the professor with the hobby.
>
> (1918: 349–350)

One cannot avoid the speculation that the difficult examiner might have been Rousselot himself—and the 'hobby' experimental phonetics. If he rather than Passy had supervised the thesis, it would indeed have been 'done completely differently', and the result would have been something like Okakura's paper produced a few months later.

We know that Edwards took a copy of Rousselot's *Principes* to Japan in 1900, but we have no certain information about whether Rousselot and Edwards actually met. In a similar way, Collins and Mees (1999: 209) point out that DJ could in principle have attended courses or lectures given by Rousselot while studying with Passy, though nothing is recorded. By 1900, Passy and Rousselot were the two most notable phoneticians in France; their two doctoral dissertations had won the Prix Volney in successive years, they were working under the umbrella of the same university, and they were connected by a network of influential figures in French linguistics (such as Henry) who knew and valued both of them. Although they differed fundamentally in their approaches to phonetics, there were numerous points of similarity between the two men, as Galazzi (1995) points out. Relations between them were (on the surface at least) scrupulously courteous. While always noting the differences in approach between them, Passy's announcement of Rousselot's own doctoral defence (1892: 96), his review of the thesis (1893: 104), and eventually his obituary of Rousselot are all generous with their praise. Jespersen describes a visit to Paris in 1894 when Passy took him to see Rousselot, who

> … very amiably showed us his instruments and rebutted the objections of Sievers and others … He reproached (in a very charming way) Passy and the other non-experimental phoneticians for wanting to solve far too many questions in all kinds of languages, whereas he himself asked only very limited questions of his instruments and considered then that he could give a final answer.
>
> (1995: 96–97)

Against this background, it is hard to see how a student of Passy, such as Edwards, would not have had at least some limited contact with Rousselot.

On the other hand, the experimental content of Edwards's thesis—such as it is—need not have resulted from contact with Rousselot. Palatography—and to some extent kymography—were already passing into the phonetic mainstream. Viëtor's *Kleine Phonetik* of 1897 already includes a sample kymogram (p. 104 – it is one of Viëtor's own, from his 1894 paper), and palatograms from English, French and German (pp. 116–117).[28]

When the work of Edwards has been remembered at all in the historiography of Japanese phonetics and phonology, its 'experimental' content has probably been over-emphasised. The survey by Suzuki (1961) depicts a turn-of-the-century scene with numerous Japanese contributors (including Shinmura and Okakura) but says 'It was E. R. Edwards who in 1903 finally wrote a work on Japanese phonetics based on experimental work' (in Yamagiwa 1961: 60).[29] And when Edwards was translated into

Japanese by Takamatsu in 1935, a frontispiece was added which shows a well-equipped phonetics laboratory (see Figure 3.9). The whereabouts of this laboratory, and it precise relevance to the text, are not explained, but it is presumably intended to be vaguely suggestive of ‘experimental’ content in the book. (The frontispiece was not included in the modern reprint of the Takamatsu translation).

But, as has been argued above, what is novel in Edwards is not the experimental material in itself, but rather the *combination* of descriptive and experimental methods, coupled with the gathering of data in the field. Seen in this light, Edwards’s work is a precursor of something like Beach (1938)—which in fact utilises exactly the same range of techniques as Edwards had—and leads directly to the type of approach described in Ladefoged (2003).

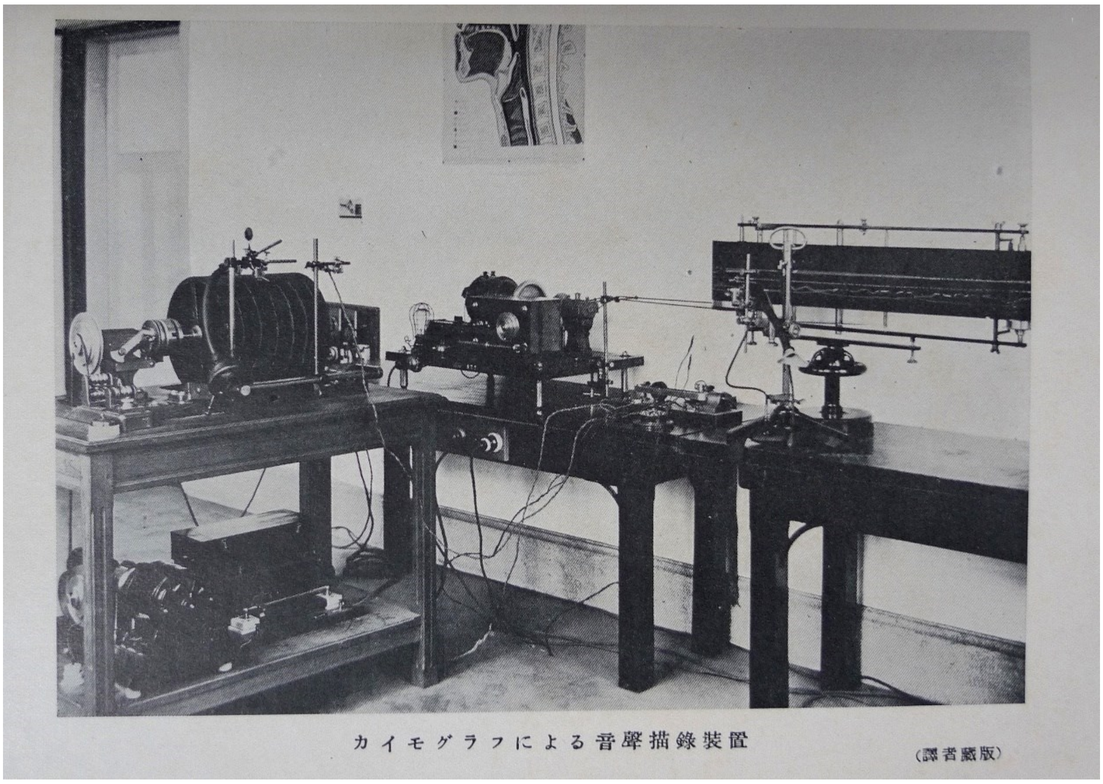


Figure 3.9 The frontispiece to the 1935 Japanese translation of Edwards (1903). At left is a large horizontal drum kymograph, with a belt drive from the enormous electric motor on the lower shelf. At right, extending out of the frame, is a vertical kymograph recording on a long paper belt. It is powered by another large motor (directly below the wall chart), which is mounted along with its control gear on a board raised on pillars. The smaller unit on the right-hand side of the central table is the electrically-maintained tuning fork to provide the timing trace. Each kymograph has a mask-type mouthpiece, linked to its tambour by a tube of unusually large bore. It does not seem possible to identify the type of tambours in use.

Notes to Chapter 3

[1] The spelling **McKendrick** is used here, as it was frequently in his lifetime, but his own preference seems to have been for **M'Kendrick**, with a reversed apostrophe (or 'turned comma'), as seen on the title page of his life of Helmholtz (1899). This 18^{th} and 19^{th} century usage may have originated as a printers' makeshift in the absence of a superscript **c** (Collins 2009). O'Connor (1988: 202) has only a partly correct account of this issue—writing **M'Kendrick** with a normal apostrophe.

[2] The chapter is a considerable expansion of one that McKendrick included in his own two-volume textbook of physiology (McKendrick 1888); the *Nature* paper is based in an address to the Physiology section at a meeting of the British Association in Glasgow.

[3] In this they were soon to be eclipsed, though, by Scripture (1902). McKendrick published a (characteristically amiable and generous) review of Scripture's *Speech Curves* in 1905.

[4] But the Cavendish Professor during DJ's student days was J. J. Thomson. Rayleigh filled the position for only five years.

[5] Ludimar Hermann (1838–1914) published a number of very high quality acoustic studies. He is credited with the introduction of the term 'formant'.

[6] Knut Hugo Pipping (1864–1944). See Aulanko (2015).

[7] Edward Bradford Titchener (1867–1927) was British, though he spent his working life at Cornell University. Like E. W. Scripture (see Chapter 8) he completed his doctorate in Leipzig under Wilhelm Wundt. He plays a significant role in the life story of Scripture, who, at an early stage of his career, was charged with plagiarising Titchener's translation of Wundt.

[8] The reference to 'English society' may be thought surprising, given that Wyllie was a Scot, living and working in Scotland.

[9] This was not a new idea by any means; see Rockey (1980: 151).

[10] Douglas Guthrie (1885–1975) graduated in medicine from Edinburgh in 1907. See https://library.rcsed.ac.uk/docs/GD15_Papers_of_Douglas_Guthrie.pdf

Oddly, Seth & Guthrie say (1935: 2) that Wyllie's work was published 'almost exactly half a century ago', though the interval is in fact 41 years.

[11] Sweet's translation (1884 [1882]: 102).

[12] The report is one of several components within Ellis's 'Presidential Address' for 1882. Volume 19 of the *Transactions*, published in 1884, contains material relating to 1882, 1883 and 1884. Sweet's report is reprinted in Wyld (1913: 148–153).

13 The eight parts of Lloyd's *Speech sounds*, listed for convenience as S1–S8. There is an error in Lloyd's numbering of sections, which jumps from §30 to §41 between S6 and S7.

	Sections (§§)	*Phonetische Studien* volume & part	pages	MacMahon's ref (2007: 324–326)
S1	*§1–§6*	3.3	251–276	1890e
S2	*§7–§11*	4.1	37–67	1890f
S3	*§12–§17*	4.2	183–214	1890g
S4	*§18–§23*	4.3	275–306	1891b
S5	*§24–§28*	5.1	1–32	1891c
S6	*§29–§30*	5.2	129–141	1891d
S7	*§41*	5.3	263–271	1892a
S8	*§42–§44*	11	1–24	1897/98a

14 Lloyd does not give a precise reference. Hermann published at least 6 papers in *Archiv für die gesammte Physiologie* in 1890–1891. See Breymann (1887:46; 52)

15 Dates given for Lloyd's publications retain the index letters of MacMahon's (2007) bibliography to facilitate identification.

16 O'Connor has Aikin's second given name wrongly as 'Alexander'.

17 The notation with primes was a German practice, and has been called 'Helmholtz notation' (Helmholtz 1885: 16; Scholes 1960, s.v. *pitch,* 6). Willis (1829: 239) uses the same notation.

18 The book and the letter are currently in the author's private collection.

19 SPE Tract I, 1919, Section 8, has a list of the original members from 1914.

20 Collins & Mees (1999: 28) give the year of his death wrongly as 1948.

21 This seems to reflect a misunderstanding. If Edwards studied experimental phonetics at all (and we don't know for certain that he did) it would not have been under Passy.

22 The sample was evidently collected in Tokyo from a Mr Onaga, a Lewchewan student of Okakura's. It is not clear which one of the several Ryukyuan languages is represented (cf. Shimoji & Pellard 2010).

23 Edwards gained the *Doctorat d'Université*, a recently introduced qualification, awarded by the university, which was suitable for overseas students who would not have been able to meet the requirements of the older *Doctorat* awarded by the State (for example, the French *Licence* was not a prerequisite). In the entry for Edwards in the Cambridge Alumni database, doctorates of both kinds are listed, 'D. ès L., Paris, 1903' as well as 'Docteur d'Université'. This is unexplained, and may be an error.

24 As explained by Alice Herbage Edwards (1918: 329–331), according to the practice of the time, the thesis was informally accepted some considerable time *before* the defence after being considered by three examiners. It was then 'recommended for

printing'. Edwards (1902: 115) describes the work as 'in the press now' around October 1902.

25 Aniceto dos Reis Gonçalves Vian(n)a (1840–1914), a Portuguese linguist, was a senior member of the IPA. A spelling reform of 1911 changed his name from 'Vianna' to 'Viana'. He published mainly on Portuguese (see Stammerjohann 1996: 354), but explains in his review that he had some limited knowledge of Japanese gathered from books, and had made some phonetic observations with a Japanese speaker (a Dr Murakami) in Lisbon.

26 The phrase recalls some of the praise accorded to Rousselot in contemporary reviews, such as 'der eingeschlagene Weg ist der einzig richtige' (the way [that Rousselot has] selected is the only right one). See Breymann (1897: 54).

27 Okakura's two early books on phonetics (*Hatsuongaku Kohwa* [Lectures on phonetics] Kaneikan Shoten (1901), and *Eigo Hatsuongaku Taikoh* [Principles of English phonetics] Sanseido, (1906)) are conventional (non-experimental) phonetics handbooks, making use of established phonetic terminology, and with transcribed texts—though both have detailed diagrams of a high standard, including palatograms. Rousselot is not among the sources cited. Okakura's numerous other publications are on subjects which include Japanese culture and aspects of English literature. He was also a lexicographer.

28 Edwards (1901) tells us that Viëtor's *Kleine Phonetik* was already known in Japan before Edwards's trip. Viëtor's longer work the *Elemente* (of which the *Kleine Phonetik* is an abridgement) is among the sources listed by Okakura (1901: 3). It contains the same palatograms (1898: 307–308).

29 Suzuki does not mention Okakura's 1903 paper.

4

Laboratories and their programmes

4.1 The first Continental laboratories

The techniques and instruments of experimental phonetics began to be settled from about 1890 onwards, particularly under the leadership of the abbé Rousselot (1846–1924) in Paris. Rousselot's ground-breaking dissertation appeared in 1891, the first volume of his *Principes de phonétique expérimentale* was published in 1897, and he set up a dedicated laboratory of experimental phonetics at the Collège de France in 1898. Similar laboratories soon followed elsewhere in Europe and around the world.

By 1916 one estimate (Barrows 1916: 62) put the number of phonetics laboratories worldwide at 'something more than twenty-five...most of which are in Europe'. Unfortunately, she does not give a list, mentioning by name only Rousselot's laboratory in Paris and the much more modern laboratory in Hamburg to which her visit was the immediate occasion for the report. Some idea of the proliferation of laboratories can be reconstructed from the retrospective notes in a much later survey (Pop 1956). In France alone, further early laboratories were those established at Grenoble in 1904, under the direction of Théodore Rosset (Boë & Vilain 2010), and in the same year at Montpellier under Maurice Grammont (Perrot 1956).

Rousselot's original motivation had been the study of dialectology (Rousselot 1891), but generally two purposes came to dominate the justification of experimental work in phonetics. They were (1) to help in the treatment of those with speech disorders, including the deaf, and (2) the teaching of foreign languages. At Grenoble the juxtaposition of a well-equipped laboratory with practical work on the teaching of pronunciation was particularly explicit, since the laboratory was partly funded by sales of Rosset's book of exercises for foreign learners of French (Rosset 1905). The Grenoble institute attracted the favourable attention of Daniel Jones (1909b), then at the very beginning of his career, and probably exerted an influence on the form of the phonetics department Jones was soon to develop in London.

Within a few years of the establishment of Rousselot's laboratory in Paris, the equipment he had assembled was being described to the lay reader as 'machinery for teaching pronunciation'. In a detailed article in *The Strand Magazine*, the journalist and traveller Grace Ellison (1905) describes a visit to Rousselot's laboratory.[1] The various instruments and techniques of the phonetics laboratory are discussed in some detail, and illustrated with photographs which show them being applied in teaching pronunciation. This article will have reached a circulation of around half a million readers, with the consequence that the idea of a phonetics laboratory possibly gained some visibility in British popular culture quite early, before any British laboratories were actually established.

Of course, there had been previous 'experimental' work on aspects of speech, and some of this had been conducted in 'laboratories' (though they were primarily physiology or psychology laboratories). What distinguishes a phonetics laboratory of the type modelled after Rousselot is that it is a dedicated facility, in some measure autonomous, pursuing its own agenda, and employing specialist staff. It was to provide a panoply of instruments and methods—especially the two cornerstone techniques of Rousselot's approach: palatography and kymography.

The early laboratories may be studied from a range of viewpoints. One may consider their physical facilities and location, the range of equipment they contained, the personnel, their funding, the research questions they were fitted to address, and the broad purposes they were intended to serve (teaching, speech remediation, language documentation, etc.). Contemporary accounts of the laboratories, particularly those in the popular press, focus sharply on the impressive inventory of apparatus, and to some extent—where there is anything to be said—on lavish premises. The laboratory is presented as an investment in equipment (and buildings) much more than as an investment in people or a commitment to goals.

The beginnings of the phonetics laboratory at University College London were in 1912, but, with an eye to the development of his subject at UCL, DJ must have been taking an informed interest in the worldwide proliferation of laboratories and institutes long before then. His account of the phonetics institute at Grenoble (1909b) appears to be based on the published prospectus[2] rather than a personal visit. Neither Rosset's prospectus nor DJ's summary of it gives much detail about apparatus. In addition to phonographs (in a special listening room), Rosset mentions kymographs, and cupboards containing various items of apparatus.[3] There were also photographic facilities—a darkroom, a studio, and an enlarging room. In fact, like some of the larger teaching spaces available for the phonetics institute, these were shared with l'Institut de Géologie et Géographie alpine, which was the other beneficiary of the university's expansion into a newly refurbished building (University of Grenoble 1909: 442–443), though Jones may not have been aware of this.

By the time the UCL laboratory was started, the laboratory at the Kolonialinstitut in Hamburg, under the direction of Giulio Panconcelli-Calzia (1878–1966) was establishing a reputation as the foremost in Europe. Collins & Mees (1999: 133) imply that DJ visited there in April 1914. This is highly probable, though direct evidence of a laboratory visit has not come to light. DJ (together with SJ and Perrett) was certainly in Hamburg for the Congress of Experimental Phonetics, which was held in Hamburg 19–22 April 1914 (Jones 1914) and organised by Panconcelli-Calzia. Along with accounts of the papers delivered, DJ's report mentions 'a wonderful exhibition of apparatus', but this may refer to apparatus displayed by the manufacturers rather than the apparatus of the laboratory itself. Admittedly, it seems very likely that a tour of the Institute and its facilities would have featured on the programme. While DJ and his colleagues must have been envious of equipment and facilities in Hamburg, and have been obliged to acknowledge the growing pre-eminence of the prolific Panconcelli-Calzia, they might also have had some reservations about the focus of the work being done there. *De la nasalité en italien* (1904), Panconcelli-Calzia's doctoral dissertation, completed in Paris under the supervision of Rousselot, had been unfavourably reviewed in *Le Maître Phonétique* (Tuttle 1904). Panconcelli-Calzia came to have a cavalier attitude to auditory phonetics and matters of symbolization, believing they were more-or-less irrelevant. Also on the staff of the Institute was Carl Meinhoff (1857-1944), Germany's leading specialist in African languages, with whom—despite a superficial appearance of great politeness—DJ was to be at loggerheads on many matters relating to symbolization and practical orthography. Meinhoff was among the delegates ranged against DJ at the Copenhagen Conference of 1926; see Collins and Mees (1999: 312).

Overall, it may be said that the UCL laboratory followed the Continental pattern which had been in vogue since the late 1890s, and was no doubt founded in the hope of emulating the latest prestigious examples such as those at Grenoble and Hamburg.

4.2 The UCL laboratory (1912–)

4.2.1 Location

The events leading to the establishment of the UCL Phonetics Laboratory have been well documented by Collins & Mees (1999: 131 ff). As for the laboratory itself, its physical location for the first 9 years of its existence is unknown. The basement laboratory at 21 Gordon Square—the 'downstairs' of Abercrombie's vivid account (1991: 1)—probably did not come into use until the academic year 1921–22. Collins & Mees (1999) give no dates for the occupancy and use of particular UCL premises, but the present author has identified two sources of evidence[4] pointing to 1921–22 as the year in which offices and classrooms in 21 Gordon Square came into use. This coincides with DJ's professorship, SJ's permanent appointment, and considerable general expansion in the Department's affairs and finances. The Department was then to remain in the same premises until 2008. No direct records have been found, but from the known floorplan of the building, the basement laboratory space probably did not exceed one fairly large room, perhaps with one or two smaller rooms to the back.

It is likely that most of the two dozen or so phonetics 'laboratories' mentioned by Barrows (1916) never occupied more than a single room, and even those which grew larger had started from modest beginnings. By the 1920s the Institute in Hamburg occupied the whole of a four storey building, with 23 rooms in all, 16 available for experimental work. But the starting point had been the allocation of a single room

(Gradenwitz 1922). Similarly, Grenoble by 1909 could boast relatively lavish refurbished accommodation, but in 1904–05 when the laboratory began, the university could not provide even a single room, and the civic authorities granted the use of a disused school classroom (Rosset 1909: 444). Here Rosset installed ‘two tables, a phonograph, and a portable kymograph’ and proudly declared open what he claimed was the first phonetics laboratory attached to an Arts Faculty.

Probably no theme occurs more frequently in the annals of university history worldwide than perpetual shortage of space and unsuitability of available accommodation. Phonetics laboratories may have been in vogue in the first decade of the century, and universities ready to establish them in their eagerness to modernise and expand, but—as always—what universities did not have was the money to pay for them. DJ must have been aware that the success of the Grenoble and Hamburg institutes was due in no small part to the extraordinary energy and resourcefulness of their directors, and their success in gaining supporters both within the university and outside. In attempting to start and sustain a laboratory at all in 1912, DJ was effectively committing himself to many years of hard work.

There is, of course, a historical association—indeed, one might say almost a symbolic association—between basements and laboratories. The basement of the Old Ashmolean Museum in Oxford (now the Museum of the History of Science) served as a chemical laboratory in the seventeenth century, and Michael Faraday’s laboratory at the Royal Institution was similarly in the basement of that building. In both places, beautiful architecture and airy public spaces above stairs contrast with cramped, badly-lit, and very possibly unhealthy conditions for research in the basement. In both cases, the safe and practical siting of a furnace may have been a factor, though a desire to keep mess, smoke and smells well out of the way was almost certainly involved too.

The phonetics laboratory of 1900 did not need a furnace but at least some of its activities were wet, smoky, messy and smelly. Suddard's unrealised design for an ideal laboratory (1917b) relegates them to a (distant) room set aside for the purpose. In Hamburg (Gradenwitz 1922) the basement was used for the wet and smelly process of X-ray and other photography.

Wherever the UCL laboratory may have been located prior to 1921, conditions cannot have been ideal. An entry in the laboratory petty cash book for November 1920 records nine shillings 'paid to Mills for repairing horn and small kymograph motor. Damage due to flood'. And soon afterwards, an item from early in the Gordon Square era (10 February 1922) indicates problems with the basement, too—an expenditure of one shilling on 'Keatings for killing cockroaches in Lab' (Keatings was a well-known brand of insecticide powder).

4.2.2 Equipment

A list of the main items of equipment available in the UCL lab during the 1920s and 1930s can be assembled from two sources which have apparently not been previously noted, Ward (1928) and Quick (1935). Neither is primarily an account of the laboratory in itself; rather they are brief outlines of the whole work of the department. Ward (1928) belongs to a recognisable genre of descriptions of this type, in which a concise account of an institution and its activities is published (within a research journal) for professional colleagues worldwide.[5] Quick (1935) is similar in coverage and content but may not have been published in the same way. Though typeset, it is known in a unique copy preserved in the Daniel Jones Papers. The pagination begins at 1, suggesting a separately issued booklet rather than journal publication. The date is actually uncertain; 'about 1935' has been added in DJ's hand on the first page. The date of 1935, if correct, would coincide with the second International Congress of Phonetic Sciences, organised at UCL, and the account may have been prepared as a publicity handout in connection with the Congress.

Ward's account of the laboratory is as follows

> The auditory method described above is supplemented by experimental methods, i.e. by analyses carried out with mechanical instruments of various kinds. The laboratory of the Department is well-equipped with all the necessary instruments : phonetic Kymographs, Lioretgraph (for magnifying the groove of gramophone records), a Laryngo-Stroboscope, Gramophones with repeating device, Rotating Mirrors; Phonographs, Tuning Forks, Electro-magnets for time recording, Sensitive Flames, etc. These have been used for the elucidation of many phonetic problems in many languages. Useful results have also been obtained by palatography and X-ray photography. Special attention has been given to the making of tambours.
>
> Students qualifying as teachers of phonetics are required to do a certain amount of work in the phonetics laboratory: they receive instruction in the use of the instruments and are generally expected, as part of their training, to solve some phonetic problems by experimental methods.
>
> A gramophone library has been started, and it is hoped to make a special feature of this in the future.
>
> (1928: 52):

The corresponding section in Quick's account runs:

> The experimental part of the research work is done in a laboratory which, though small, is well equipped. The laboratory contains, among other instruments, three kymographs of different size and different degrees of delicacy: a lioretgraph by which the grooves of gramophone records can be enlarged about three hundred times: a strobilion for showing intonation: a laryngostroboscope and an apparatus for manometric flames. X-ray photographs of the tongue and larynx are taken in

> the Anatomy Department of the College in collaboration with the Phonetics Department. The resources of the laboratory have been of considerable practical utility in the investigation of stress and intonation as well as of sounds, in dealing with speech defects, in testing degrees of deafness and in teaching the deaf.
>
> (1935: 4)

These accounts agree on all the main points: there were several kymographs, a Lioretgraph, a strobo-laryngoscope, and a manometric flame apparatus (this is undoubtedly what Ward means by 'Rotating Mirrors'). Use was made of X-rays, though there was no X-ray apparatus in the phonetics laboratory itself.

The few significant differences between the two accounts are (1) Ward specifically mentions gramophones and phonographs; perhaps by the time of Quick's account in 1935, gramophones could be taken for granted (or maybe those available at UCL were now somewhat behind the times). It is not clear what Ward meant by 'repeating device'. There were certainly available rather crude mechanical devices which lifted the needle from the end of a record as it finished and returned it to the beginning (thus repeating the whole record). But the context rather suggests a means of repeating short sections. (2) The later account (Quick) makes no mention of palatography. (3) Sensitive flames are included in Ward's account, but not in Quick's, while (4) Quick adds the 'strobilion'.

The techniques of palatography, kymography and X-ray photography are discussed in detail in the relevant sections of Chapter 2. The Lioretgraph and manometric flame apparatus (with rotating mirror) are also referred to in Chapter 2, under 'Waveforms and signal analysis', while an account of SJ using a sensitive flame appears in Chapter 6 (see also Ashby 2011).

As for the strobo-laryngoscope, there are clear indications that laryngoscopy was routinely demonstrated in the UCL laboratory. Catford[6] recalls that Stephen Jones readily tolerated the laryngoscope mirror and was thus able to demonstrate laryngeal action to students. A ‘Laryngo-Stroboscope’ is a laryngeal viewing system of this type used in conjunction with a device which periodically interrupts either the view or the light source at an appropriate rate, so as to slow down or arrest the vibrating vocal folds; see Scripture (1902:249). Any research applications of such a device would entail photography (or cinephotography) but there are no indications that this was attempted at UCL. In fact, there is nothing to suggest that the UCL laboratory was equipped for any kind of photographic work. Records indicate that at least some (fairly routine) photographic work was outsourced to the firm of Adam Hilger.[7]

The ‘strobilion’ (Scripture 1913a) aimed to teach pitch and intonation. A speaking tube conveys air pressure variations from a mouthpiece to a gas flame, following the same principle as the manometric flame. The pulsating flame illuminates the markings on a stroboscope disc which is being rotated at a known rate. The particular band on the disc which appears stationary gives an indication of the voice pitch. The device may have had some teaching or demonstration value but there seem to be no accounts of its application in research. Reasonably accurate real-time extraction of voice pitch, resulting in an immediately displayed intonation curve, did not become possible—even in the research laboratory—until the late 1930s (Obata and Kobayashi 1937), and relied on special-purpose electronics linked to a cathode-ray oscillograph display.

Despite the claim made by both Ward and Quick that the UCL laboratory was ‘well-equipped’, and even though it may appear that quite a long list of apparatus is given, it is not an impressive or up-to-date inventory. There is for example no microphone, no amplifier, and no way of producing permanent records of the speech waveform apart from the antiquated Lioretgraph—and indeed, apparently no means of recording speech itself. By the standards of the 1930s in particular, the research capabilities of the laboratory were very limited. At the same time, the laboratory certainly contained many smaller items which neither Ward nor Quick includes—for example, from purchases recorded in petty cash book entries in 1921 and 1923 alone we learn that there was a Savart wheel, and the 'Radio Rex' toy, perhaps the first ever speech recognition device (Paget 1930: 79–80; Jurafsky & Martin 2009: 365). Again, neither account mentions Atkinson's mouth measurer, even though it was to be one of the UCL techniques transplanted to Edinburgh by Elizabeth Uldall (Keller 1971).[8] The enumeration of items of this type would not fit the agenda being pursued by Ward and Quick since they would have been used only for demonstration or teaching purposes. Both Ward and Quick are attempting to portray the laboratory as a facility for research, when in reality it had by this date become a teaching laboratory.

4.3 The Newcastle laboratory (1934–)

It has generally been assumed that after the foundation of the UCL laboratory in 1912, no further phonetics laboratories came into existence in Britain until 1948, when a department of phonetics was founded at the University of Edinburgh.[9] However, a short article appeared in the journal *Nature* in 1934 announcing a newly established phonetics laboratory in Newcastle (Anon 1934).

4.3.1 Origin and location

The article begins by explaining the laboratory had begun following exploratory work done by Robert Curry at Armstrong College in Newcastle on the effectiveness of then available electrical recording methods for the recording and study of English dialects. The success of this evaluation had led to the granting of space for a phonetics laboratory:

> The work of making and testing different types of apparatus has been so successful that the Council of the College, mindful of the importance of the investigation, has granted space in a newly acquired building for a phonetics laboratory to house the apparatus, and in which records may be taken under conditions free from noise and vibration.
>
> The laboratory looks out upon an empty court, and the windows on this side are sealed and provided with dark blinds. The floor is of concrete and the partition walls are 1 ft. thick, so that it was thought unnecessary to introduce sound-insulating material except on the door, which is faced on the inside with a layer of Newall's Asbestos Blanket. A ventilating shaft in the thickness of the wall leads out to the roof at a point sheltered from Street noise. To make doubly sure that no 'ground noise' shall reach the recording apparatus, the microphones are placed in a double-walled insulated kiosk of the telephone cabinet type.
>
> (1934: 655)

We are also told there was provision for developing photographic plates (i.e., some sort of darkroom facility). Glass plates would certainly have been used for recording results in many types of apparatus in a physics laboratory of the day, though as is explained elsewhere, Curry's oscilloscope camera in fact used lengths of film, and this too was evidently developed on the spot. It was orthochromatic film, permitting development by inspection under a safelight. This implies a suitably equipped darkroom space, with running water.

4.3.2 Personnel

The 1934 announcement mentions five names: ‘Prof. W. E. Curtis and Prof. W. L. Renwick’ (they were the respective Heads of Department of Physics and of English, the two sponsoring departments), ‘Mr. R. O. L. Curry, Noble Memorial Scholar’ (his work had prompted the establishment of the laboratory), while the laboratory is said to be ‘under the joint supervision of Mr. H. Orton, of the English Department, and Dr. E. G. Richardson, of the Physics Department’. Curtis and Renwick probably had no direct role in the laboratory. Curry has all but disappeared from history, and his life and publications are treated in Chapter 8 of this work. The careers of Orton and Richardson are relatively well documented in existing sources.

4.3.3. Harold Orton (1898–1975)

Harold Orton was a notable English dialectologist. An account of his career appears in the *Oxford Dictionary of National Biography* (Ellis 2004); further details have been added here from what is probably a self-authored short biography (Pop and Pop 1960: 153) and various other sources such as the records of the IPA.

At the conclusion of the First World War (in which Orton served as a lieutenant in the Durham light infantry and was seriously wounded), he went to Merton College Oxford where he studied under H. C. Wyld (1870–1945). Joseph Wright (1855–1930), editor of the *English Dialect Dictionary*, was another of his tutors. He gained his BA in 1921 and the degree of BLitt in 1923. After a spell (1924–1928) as lector at Uppsala University, he became a lecturer at King’s College, Newcastle (which at the time was a component of the University of Durham). In 1933 he published *The Phonology of a*

South Durham Dialect, which dealt with the speech of his native village. According to Ellis (2004):

> At Newcastle he continued to develop his interest in collecting local speech and inaugurated a Northumbrian dialect survey. He used a portable disc-cutting machine to record local voices in homes and hostelries, and material from as early as 1935 survives, copied digitally to tape.

Orton remained at Newcastle until 1939, when he became Lecturer in Charge of Department of English Language at the University of Sheffield. He held this post for the period 1939–1946, though for much of that time he was seconded to the British Council. From 1946 he was Professor of English Language and Medieval English Literature at the University of Leeds, and Dean of the Arts Faculty. It was only in this post-war period that he began systematic work on the English Dialect Survey for which he is now chiefly remembered (Orton et al. 1973).

Orton undoubtedly had credentials as a phonetician, as well as a dialectologist. He maintained a membership of the International Phonetic Association throughout the period 1931–1965, publishing a paper in *Le Maître Phonétique* (Orton 1939), and served on the BBC Advisory Committee on Spoken English (1934–1940). He attended the second International Congress of Phonetic Sciences in 1935, as the 'Delegate of Armstrong College, University of Durham' (Jones and Fry 1936: 323).

It seems possible that the initial exploratory work on sound recording in the field, undertaken by Curry, may have been started at Orton's request or suggestion, so that the events leading to the creation of the Newcastle phonetics laboratory were evidently occasioned at least in part by Orton. It is not clear whether Orton (or indeed Curry) had at first any purpose in mind for the laboratory other than to support the project of field dialect recording. But very soon the attempt to quantify certain aspects of recorded dialects (such as their vowel systems) was stated as an explicit goal. Interestingly, after Orton's arrival in Leeds in 1946 a phonetics laboratory began there, too, within two years (MacCarthy 1956).

4.3.4 Edward Gick Richardson (1896–1960)

Richardson achieved considerable international prominence in the field of acoustics and ultrasonics, and obituaries appeared in both *The Journal of the Acoustical Society of America* (Anon 1960) and *Nature* (Curtis 1960).[10] The present account is based on those, supplemented by what can be learned from Richardson's own publications. He was born in Watford in 1896, and gained a University of London physics degree after studying at what was then East London College. He served in the Royal Air Force in World War I and after the war had a short spell as a school teacher. He then began some research at East London College, publishing his 1922 paper 'The theory of the singing flame' with East London College as his affiliation. In 1924 he became Lecturer in Physics at University College London, where he remained until 1931. While at UCL he gained his DSc for research in acoustics. He maintained his interest in the interaction of sound with flames and fluid jets; a letter to *Nature* on 'Sensitive jets and flames' appeared in 1925. In the same period he first published his textbook *Sound* (1927), which was to remain in print throughout his life, the fifth edition appearing posthumously in 1961.[11] Richardson remained at Newcastle for the rest of his (very active and prolific) career. In 1956, a chair in acoustics was created for him, and he was probably the first person in Britain to have the title 'Professor of Acoustics'.

The early editions of Richardson's *Sound* contain much that is of interest in the present context. Chapter 12, on 'Subjective sound', deals with voice, and the sounds of speech, and gives a brief characterisation of phonetics:

> The human being finds the correct position of the mouth for different sounds by instinct and imitation. It is the business of the science of phonetics to aid him in this process, especially when he is mastering a foreign language with foreign sounds, by discovering for him the positions and movements of the mouth in speech. These positions are found by probing instruments, when the system is set to produce the given sounds, and records or models are made. The pupil then endeavours to mould the mouth into the same shape.
>
> (1935: 258)

This appears to have a clear resemblance to the opening paragraph of Daniel Jones's Royal Institution paper of 1917:

> The art of speaking a foreign language demands (among other things) an ability to perform all kinds of difficult movements with the tongue and other parts of the speech-mechansim. Such ability may be acquired by the learner, if he is provided with precise instructions as to what he must do. It is the function of the phonetician to supply these instructions.
>
> (1917c: 1)

The work also contains an analysis of the operation of the sensitive flame (1935: 157–160), and a section on the acoustics of buildings (1935: 279–293), which includes a detailed discussion of the acoustics of the Great Hall, University College, concerning which Richardson had evidently been consulted in a bid to reduce reverberation for the situation where the hall was only partly filled, and sound absorption by the audience thus inadequate (the total audience capacity was 1000). His recommended solution was to cover the upper part of the walls with a sound-absorbing 'acoustic plaster', though it is not clear whether this plan was put into action. Interestingly, the

Great Hall was a main venue for the second International Congress of Phonetic Sciences in 1935. The total number attending was 262 (Jones and Fry 1936: ix), suggesting that reverberation may well have been a problem.[12] Richardson published a book on the acoustics of buildings in 1933, revised as *Acoustics for architects* in 1945.

It is remarkable that Richardson was working at UCL during the 1920s heyday of the Phonetics Department, writing and conducting research on matters that could potentially have been of great concern to the UCL laboratory. Whether he and the members of the UCL Department and laboratory were aware of each other we do not know.

4.3.5 Apparatus

The 1934 announcement in *Nature* begins by contrasting old and new: ‘In experimental phonetics the older mechanical methods still predominate’. The ‘new’ experimental phonetics laboratory in Newcastle is going to make use of ‘the new electrical methods of speech recording and reproduction’.

The apparatus is listed as (1) a kymograph, (2) an Einthoven string galvanometer, (3) cathode ray tube, and a moving-film camera (speed 6 ft per second) enabling cathode ray tube traces to be photographed, (4) an electric gramophone recorder.

The account of the apparatus has a physicist’s focus on essentials, appropriate for the journal *Nature* but disappointingly generic to a historian. At least some of the apparatus appears to have been constructed locally:

> For the benefit of others who may be intending to take up similar work, it may be mentioned that the cost of the equipment of a laboratory such as this is quite moderate. Excluding the string galvanometer (which is not essential), the whole of the equipment has cost less than fifty pounds, although it is true that this does not include the cost of the labour of assembling the apparatus.
>
> (1934: 656)

The string galvanometer (a sensitive electromechanical oscillograph originally developed for work on electrocardiography) can be identified from photographs as a model manufactured by the Cambridge Instrument Company (Phillips 1987: 169–171). It is unclear whether the cathode ray 'tube' mentioned in this 1934 account is the same as a self-contained cathode ray oscillograph manufactured by A. C. Cossor Ltd which is mentioned by Curry (1936) The moving-film camera was certainly constructed in Newcastle (Curry 1934); a full description is given in Chapter 8 below. As for the 'electric gramophone recorder', there were by this date several direct-to-disc recording systems on the market. A contemporary survey of recording technology is Frederick (1934), while Hanley (1936) addresses specifically the requirements of phoneticians, and gives names of manufacturers of record blanks and equipment. For the laboratory in Newcastle, elements of a system (for example, record blanks and cutter) might well have been purchased, while other components, such as the recording turntable, amplifier, etc., could have been constructed in the laboratory at lower cost. The construction of apparatus also depends, of course, upon the availability of workshop facilities and suitable tools.

The work of the Newcastle laboratory is also the subject of a second *Nature* article, (Richardson 1940). This adds no further information about the laboratory or its equipment. The 'Recent work in experimental phonetics' of the title is chiefly work in

Newcastle, but partial notice is also taken of work elsewhere—in fact the closing sentence of the paper refers to high-speed cinematography of the larynx then going on at Bell Laboratories. The bulk of the paper is concerned with the speech waveform, which the Newcastle laboratory was well-equipped to display and photograph. There is no doubt that Richardson shared the hope of Curry (and presumably Orton) that direct classification and comparison of sounds might be achieved from the superficial appearance of the waveform (i.e., without spectral analysis). After discussing some of the factors which tend to produce variation in the appearance of a vowel waveform within and across speakers, he goes on: 'Nevertheless, there is hope for the view that to each vowel a definite wave-*pattern* may be attached.' The only example he is able to give is a general characterisation of a vowel of type [ɑ]:

> The characteristic pattern of this vowel—a series of peaks diminishing in height within the period—can be recognized in each trace, although analysis of each trace into its acoustic spectrum scarcely helps the identification.
>
> (1940: 841)

Interpreted in the light of its context, this rather cryptic statement appears to mean that the acoustic spectrum (dominated by the first formant) will offer no invariant property that is characteristic of the type [ɑ] (since the frequency of the formant itself will also vary across speakers), whereas the general pattern can be recognised of a wave in which each voice period shows many cycles of a decaying formant resonance. He does not say how the analysis of the acoustic spectrum might be accomplished (although estimation of F_1 from the waveform is relatively straightforward in the case of [ɑ]), nor does he seem to consider the possibility that 'characteristic patterns' might be found in acoustic spectra quite as much as in waveforms. Overall, the optimism about direct classification of waveforms may be seen as wishful thinking arising from the capabilities of the equipment he had at his disposal. Bearing in mind the importance attached to the appearance of waveforms, it is curious that on close examination it appears that of the four waveforms reproduced in the paper (1940: 841) all but the first are reversed left to right (without comment).[13]

The remainder of the paper deals briefly with vowel theories, the topic of intonation (with a short discussion of methods of measuring and displaying fundamental frequency and intensity), and the physiology of voice production, including X-ray studies. On the whole, Richardson (1940) gives the impression of being the work of an informed observer and summarizer, rather than one directly involved in phonetic research himself. The *Nature* obituary remarks on his ability to compile and edit reviews of material outside his immediate field:

> In addition to his research work, Richardson found time to write upwards of a dozen books, including compilations which he edited or to which he contributed sections ... He wrote fluently and well, with an admirable clarity of expression and an easy mastery of his subject.
>
> (Curtis 1960: 678)

He is insistent on the need for 'co-operation in the laboratory between scientist and linguist' (1940: 841) (each serves as a corrective to the other in the interpretation of records of speech), but when he writes on linguistic matters himself he appears somewhat amateurish, using inappropriate terminology, and invoking stereotypes which presumably would not have met with approval from Orton. In his treatment of intonation, for instance, he refers to the 'level monotone' of Italian, the 'excessive modulation' of Swedish, 'the stolid utterance of the Somerset man' and 'the lilt of the East Anglian or Northumbrian'. Similarly, a discussion of the word *farmers* shows that he is aware in principle of the difference between rhotic and non-rhotic accents, though he characterises the non-rhotic variety as ‘normal English speech’.

Finally, it seems very probable that Richardson was the author of the anonymous 1934 paper which first announced the Newcastle laboratory. Leaving aside the obvious consideration that as the physicist in charge of the laboratory it would be natural for him to publish a technical specification of the equipment in *Nature*, we can point to the paper’s detailed account of the acoustics of the premises (and the sound treatment applied), and the mention of ‘a jet tone apparatus for studying the action of the vocal organs in speech’. It is unclear what form this took, and no other mention has been located. It may have been intended to model the production of certain fricative consonants. But, like architectural acoustics, ‘jet tones’ (the term is evidently Richardson’s own coinage) were one of his particular interests, and they are accorded a treatment in *Sound* (1935: 155–157).

4.4 Purposes and programmes

Among the various purposes for phonetics laboratories which were cited at different times and in different places, the main categories are (1) the investigation and treatment of speech pathology (including the teaching of the deaf), (2) the teaching of pronunciation in modern foreign languages, and (3) various forms of language description and of what would now be called ‘language documentation’ (Austin 2006): the recording and study of regional accents and dialects for the sake of preservation, or of lesser known languages, such as the many languages of the British Empire (though this had a practical goal, to facilitate commerce and development). Additionally, there are two related enterprises which border on the territory of the phonetics laboratory, such that the same apparatus, premises, skills and personnel were commonly involved. Neither was developed early in Britain, but it is necessary to give some account of them nevertheless, since they are relevant to the evaluation and comparison of the two British laboratories. They are (4) the use of recorded listening materials in the practical teaching of foreign languages—developments which eventually led to the concept of the 'language laboratory'—and (5) the systematic creation of recorded sound archives (for example, as national resources). Finally, in a commercial or engineering context, there was (6) basic research undertaken with a view to improving voice communications (such as telephony) and sound recording (Fletcher 1929).

4.4.1 The purposes of the UCL laboratory

All but the last of the purposes just identified formed explicit parts of DJ's ambitious scheme for an Institute of Phonetics (Collins and Mees 1999: 259–266). In practice, after the Institute project foundered, the UCL laboratory never acquired many of the facilities and resources planned for the Institute: there was no Professor of Experimental Phonetics, no recording equipment, no sound-treated recording and listening rooms, no in-house X-ray apparatus, no vast library of recorded material documenting the languages of the Empire. The modest laboratory which remained was effectively a relic of the failed Institute enterprise. The laboratory was claimed to have some usefulness in the teaching of pronunciation, and some applications in speech remediation and the teaching of the deaf, though neither claim is easy to support.

The teaching of pronunciation in modern languages had been on the agenda of the phonetics laboratory since Rousselot. With hindsight, we can see that in principle there are three ways in which the instruments of the laboratory might be of assistance to the learner. Firstly, they could be used to prepare pictures (such as vocal tract diagrams, or palate contact patterns) to serve as targets for the learner. Rousselot had already published a book aimed at the foreign learner of French (Rousselot and Laclotte 1902). It begins conventionally enough with a consideration of the best pronunciation model to follow, and a whole final section is devoted to fairly conventional practical pronunciation exercises. The chapters between deal with vowels, consonants, accent, liaison, etc., but they have palatograms and kymograms as appropriate, as well as photographs of lip-positions, instead of mere verbal description. In all, the work has some 83 illustrations. But Rousselot and Laclotte do not suggest that their readers should have artificial palates made in order to monitor their pronunciation progress, and the kymograph would have been out of the reach of most of their readers.

Secondly, another type of use is possible if the learner *does*—at least briefly—have access to the instruments. The graphic record can then furnish a visible diagnosis of the learner's error, and if the process can be repeated, successive improvements might be made in the learner's approximation to the target. Ellison (1905: 457) reproduces three palatograms showing the successive attempts of 'an English girl' to pronounce 'the French CH'—i.e., the voiceless palato-alveolar fricative [ʃ]. First the tongue is too far forward, the initial correction moves the tongue too far back, and at the third attempt the pattern is 'correct'.

The feasibility of such a method of instruction depends on the time and expense required. Indirect palatography has the disadvantage that a palate must be made for each speaker—a lengthy and relatively expensive procedure. Again, the kymograph *could*—in principle—have been used in teaching, say, the control of VOT in voiceless plosives. But it was cumbersome in operation, and required the ministrations of a dextrous technician (Suddard 1917a). Its practical application would certainly seem to entail an extended course of pronunciation instruction, and the opportunity for one-to-one tuition

Thirdly, laboratory instruments might be used to provide what we would now term 'interactive feedback'. For this, the device must function in real time, providing the learner with an easily interpreted visual indication, which can be monitored and used to control speech output. Interestingly, several of the photographs in Ellison (1905) show devices from Rousselot's laboratory which provided exactly that. In one, both teacher and student are watching the indicator dial of a spirograph, which is being used to indicate to a student the supposedly correct breath flow to use—different amounts were specified for English and for French. In another picture, nasal airflow is collected from the student's nostrils and caused to move a large indicator (the so-called 'quadrant indicator') across a scale. There is a reason, of course, why these feedback devices do not figure in Rousselot and Laclotte (1902). Unlike palatography and kymography, they do not produce any permanent graphical output, and hence are of no value to the student working from a book, who has no access to the device itself.

Before the widespread use of electronic devices, feedback had to be accomplished mechanically or by utilising the sound sensitivity of gas flames. Even after the advent of the cathode ray oscillograph, some work exploring the potential of the sensitive flame continued up until the Second World War (Fry 1938). Another flame device—the flame manometer—is shown in use for teaching in one of the photographs in Ellison (1905). Though the UCL laboratory certainly possessed a device of exactly the type pictured there, we have no indications that it was applied in pronunciation teaching.

Ellison (1916) reports to a limited extent on Rousselot's ideas about foreign accent and its correction, though these take the form of anecdotes about individuals rather than a systematic theory. Rousselot's basic idea seems to have been that the visible records from instruments could somehow assist the development of auditory discrimination in the learner, though exactly how this was to happen was not spelled out—and like many other pedagogical proposals of the time, its effectiveness was not subjected to any empirical test.

Rousselot seems to have made at least two assumptions which continue to be debated by researchers into pronunciation teaching—that 'insight' into the production of speech sounds can influence perception, and that an improvement in perception will be linked with an improvement in production. On the whole, laboratory phonetics came up with very much the same ideas for teaching the foreign learner as for teaching the deaf. There was a great emphasis on visual displays, particularly on static pictures of articulatory configurations. The learner must therefore be imagined as able to make use of pictures to control his articulation, though whether a picture could be expected ever to guide a learner to more than a rough approximation to the required output went largely unquestioned (and certainly untested).

Further unquestioned assumptions of the day were that speech can be adequately characterised as a sequence of static 'postures', and that individual speakers are sufficiently alike that precise positions determined for one speaker can be directly copied by another. Rousselot even arrived at specifications of tongue positions for French vowels expressed in millimetres of spacing between tongue and palate. The values are reproduced in the Appendix to Rousselot & Laclotte (1902; 209–210). It is not explained how learners were supposed to adjust their tongues to the nearest millimetre to match the specifications.

As is argued in Chapter 5, by the early 1920s Daniel Jones had reached the conclusion that visual representations of articulations had very limited value in teaching. Instead, auditory comparison with sounds already known to the learner was the method to follow. Nevertheless, in laboratories around the world, work continued on more and more accurate specification of 'the positions' of the speech sounds of various languages.

It is noticeable that many of the accounts of the supposed value of technology in language teaching appeared in popular press reports rather than academic forums. *The Straits Times* (an English-language Singapore newspaper) in 1923 carried an article ('London's Hall of Babel' 1923) on the University College London Summer Course in English Phonetics, designed for learners of English from around the world. (The course began in 1919, and continues to the present day). Along with an account of the students and the predictable stereotyped representation of phonetics as being concerned with vocal gymnastics and strange noises are awed mentions of the kymograph and the lioretgraph:

> Mechanical aids are invoked by the instructors. There is a kymograph, or instrument for registering waves of sound. Or, by the aid of the lioretgraph—named after the inventor—which enlarges 300 times the groove of a gramophone record, an expert can learn the most remarkable things.

In quite what way the mechanical aids were 'invoked' is not made clear. The phrasing rather suggests that the journalist—and presumably also the clientele for the course—were ready to be impressed by the mere availability in principle of these devices. They establish the prestige of the institution and of the instructors, even if they are put to no practical use on the course.

It looks as if Daniel Jones most uncharacteristically sought out publicity and did not discourage what seem like exaggerated claims for the technology. In a 1919 article in the *The Daily Chronicle* under the heading 'Empire language "factory". Institute as guide to 1000 tongues. Wonderful devices', we find:

> Explaining some of the remarkable features of the institute [i.e., the planned Institute of Phonetics] and the work now being done in the Gower Street laboratory, the principal [i.e., Jones] produced an instrument known as a kymograph.
>
> When a language quite untranslated is spoken into the kymograph by a native the voice vibrations move a needle, which writes the language down on a revolving drum upon smoke paper. The continuous curves thus drawn can then be translated into phonetic English, and step by step can ultimately be produced in book form.
>
> (*Daily Chronicle* 1919)

One wonders whether Jones was given any opportunity to revise this copy before it went to press, and whether he sanctioned the naïve (or deliberately deceptive?) simplification it incorporates. The suggested parallel between tracing a curve on the kymograph and 'writing down' a language is also hinted at in Ellison (1916: 457), where the illustration of a kymograph has the caption 'The machine for writing speech'.

A report in *The Sunday Times* (Sunday Times 1925) again carries rather extravagant claims about the value of the kymograph in teaching attributed to Jones. The grand Institute has been forgotten, and the emphasis is now on teaching English pronunciation to foreign learners:

> An instrument by means of which you can see yourself speaking, and so, with a little practice, can correct errors in pronunciation, is to be seen in a small laboratory at the back of London University [...] Prof. Daniel Jones, head of the Phonetics Department of University College, who showed the machine to the SUNDAY TIMES yesterday, said that, exceedingly useful as it was at present, there were even greater possibilities before it.
>
> By its means, at present, the pronunciation of even the most difficult foreign languages was made possible [...] and correct pronunciation would be much more readily obtained, he said, than was ever thought possible before the invention of the Kymograph.
>
> Similarly, for foreigners learning English, the machine was of inestimable value.
>
> "Among our students here learning English," he continued, "are men and women from Japan, China, Georgia, India, Switzerland, Africa, Poland, Hungary, Esthonia, France, Germany, Italy, Holland, Denmark, Sweden, Rumania, Russia, Spain, and Czecho-Slovakia, and to these the Kymograph has been of greatest use. Each can see his own errors of pronunciation and, in time and by practice, correct them."

If Jones's claim were literally true, it would imply something of the order of 20 individual programmes of study completed in the small basement laboratory (where tuition was generally on a one-to-one basis), none of which were reported in the academic literature or left any other historical footprint. And the implication that the kymograph was a recent invention is bizarre. By this time it had been a commonplace of the phonetics laboratory for nearly 35 years.

Turning to the other facet of pronunciation teaching—listening as opposed to production—the UCL laboratory seems to have contributed little. Live ear-training was of course a fundamental component of phonetic training at UCL, but the use of recorded material seems not to have been explored in any systematic way. In 1919, the plans for the Institute had been way ahead of their time in this regard. There were to have been five 'nearly sound-proof Phonographic Research Rooms', a 'large room containing 20 fairly sound-proof compartments (for language learners using phonographic records)' and a 'Phonographic Class Room' with accommodation for 40 students and '40 phonograph receivers connected to the phonograph at the head of the table' (Collins and Mees 1999: 264). Collins and Mees observe that the 'phonographic class room' is 'possibly the very first surviving plan for what today would be termed a language laboratory'. In the event, what happened at UCL was extremely modest; Ward (1928) mentions gramophones among her list of laboratory instruments, and concludes her account rather lamely with 'A gramophone library has been started and it is hoped to make a special feature of this in the future', while Quick (1935) makes no mention of recorded materials at all. A collection of several hundred gramophone records was indeed eventually amassed in the department, the bulk of which has since been accessioned into the British Library as 'The UCL Phonetics Collection', but how it was intended to be used is unknown. The developments in the management and systematic exploitation of recorded language materials which led to the 'language laboratory' took place elsewhere, especially in the United States—though it appears that the inspiration may have come from Rosset's laboratory in Grenoble. We do not know what form Rosset's 'special listening room' may have taken, but according to Roby:

> An American, Frank C. Chalfant, who studied there [i.e., Grenoble] in the summer of 1909, appears to have been the one who brought the idea back to this country [i.e., the USA]. He installed a "phonetics laboratory" at Washington State College in Pullman during the 1911–1912 academic year. Pictures of this installation in use show students listening via networked earphones. This lab also had a phonograph-recording machine so that students could compare their pronunciation with the native-speaker models.
>
> (2004: 524)

If this is so, it suggests that Daniel Jones's planned 'phonographic class room' may not have been as original as Collins and Mees wish to claim. DJ, like Chalfant, may have got the idea from Rosset. An enthusiastic advocate was Ralph H. Waltz (1930, 1931), who established similar facilities, first at the University of Utah and a few years later at Ohio State. He too gives a description which seems to have specific points of similarity with DJ's 1919 plan:

> As many as forty students could use a single unit or machine at one time. However, for our purposes we have fixed on a table seating 16 students. The table is 13 feet 4 inches long and 2 feet 9 inches in height. The width is 3 feet. The table is divided into compartments by a thin 18 inch board running down the center and boards of the same material running at right angles.
>
> The transcribing machine is placed at the end of the table and connected by its flexible tube to a nickeled brass pipe running the length of the table. This pipe has soldered into it a sufficient number of small nipples to feed the listening tubes. The listening head sets can be bought or made up in the laboratory. They are connected to the main tube by means of the small nipples. It is apparent that the entire set-up is mechanical. The units in our laboratories were entirely constructed with student labor excepting, naturally, the transcribing machines themselves.
>
> (1930: 28)

The mention of 40 possible simultaneous users, and the seating of users around a single table, correspond with Jones's proposal—suggesting, perhaps, that both are derived from a preceding source that has not been identified, possibly Rosset.

Waltz's statement 'It is apparent that the entire set-up is mechanical' reflects the fact that by the time of writing (1930), reproduction could in principle have been achieved electrically. Instead, however, he was adhering to a sort of acoustic plumbing (the networking of speaking tubes, leading to earpieces which are essentially like those of a medical stethoscope) which would have been available to Rosset.

One interesting feature of these early papers—and of others which contributed to a lively debate about the pros and cons of teaching this way (Schneck 1930, Levin 1931)—is that the installations are generally called 'phonetics laboratories'. According to Roby (2004: 524), although 'language laboratory' and various other competing names are also to be found from the beginning, 'the preferred term until after WWII was "phonetics laboratory"'. In part, this may reflect the fact that at first the main value of recorded materials was in teaching pronunciation rather than other aspects of language such as grammar or vocabulary (Clarke 1918). At any rate, the usage seems to indicate that the other meaning of 'phonetics laboratory'—a research facility with special purpose instruments such as the kymograph—was not so clearly established and familiar as to force the use of a different term for the language-teaching facility.

As for speech remediation and the teaching of the deaf, we can point to various assertions and good intentions, but documenting any substantial transfer from the UCL laboratory to the clinic is another matter. Ward, herself the author of a short work on speech defects and their remediation (1936), says:

> Some research has also been done on the application of phonetics to the cure of certain kinds of speech defects, and as a result, it has been possible to cure a number of people who had defective speech: indistinct and unintelligible speech, lisping of various types, voice weakness and cleft palate cases have been dealt with. One student has done pioneer work in phonetics applied to the teaching of speech to the deaf. She has invented numerous methods (some with and some without, apparatus) for helping deaf children to master the difficulties of speech.
>
> (1928: 54)

The ‘student’, and the ‘apparatus’ have not yet been identified. As mentioned above, Quick’s account of the laboratory concludes:

> The resources of the laboratory have been of considerable practical utility in the investigation of stress and intonation as well as of sounds, in dealing with speech defects, in testing degrees of deafness and in teaching the deaf.
> (1935: 4)

It is hard to give much substance to this. The sensitive flame and the ‘strobilion’ had some limited application in teaching the deaf. But what apparatus might have been used in ‘testing degree of deafness’ is unknown. The pure-tone audiometer certainly existed by this date, though it was not in general use in Britain. It is not impossible that an audiometer found its way—temporarily, perhaps—into the UCL laboratory, but this is no more than speculation.

4.4.2 Purposes of the Newcastle laboratory

The first announcement of the laboratory states its intended function very clearly. After initial work to collect ‘definitive pictures of the Standard English speech sounds’,

> Records of dialect speakers will then be taken for the purpose of the main object of the laboratory, which is the comparative philology of the region in which the University of Durham lies. In this connexion, room is provided elsewhere in the College for card indexes of local variants in pronunciation.
> (1934: 656)

The purpose of the laboratory was thus to support the making of field recordings (which it was intended to collect in an archive), and to analyse comparative acoustic data in order to facilitate classification of the sounds in the dialect recordings. Neither the 1934 nor the 1940 account mentions any other purpose (such as teaching or speech remediation).

Links between the resources of a laboratory and efforts at language documentation and the related enterprise of creating a recorded-sound archive were already long established elsewhere, and Richardson's comments (1940: 841) appear insular and ill-informed when viewed in the light of what others had said up to half a century earlier, or compared with the archiving and documentation efforts then going on—or indeed, already completed. The Vienna *Phonogrammarchiv* had begun in 1899 (Wild 1956). Specific proposals for a 'phonographic museum' had been advanced as early as 1900 in France, and echoed in Britain by McKendrick in 1901 (whether derivatively or independently is not certain; see Chapter 3). Even looking no further than the British Empire, the gathering of data for the vast Linguistic Survey of India (with recordings) was long completed, and Sir George Grierson, who had masterminded it, had passed his 90th birthday. Richardson (and presumably also the editors and readers of *Nature*) seems not to know any of that background, and his linguistics once again appears naïve. He speaks of:

> …local variants, which may be preserved in the form of gramophone or film records to remain, perhaps as museum pieces, when all speech has been reduced by education and broadcasting to a dull uniform 'Standard English' . To this end a number of linguists are exploring the country districts of Great Britain, where special forms die hard, with portable recording equipment, to persuade the older people to 'say something into the microphone'.
>
> (1940: 841)

The Swiss dialectologist and phonetician Eugen Dieth (1898–1956), who was Orton's long-term collaborator and co-author, was the director of the *Phonogrammarchiv* at the University of Zürich (founded 1909) from 1934, and added a phonetics laboratory the following year. The Zürich archive had moved from acoustic to electrical disc recording in 1932 (Dieth 1956: 330). Overall, the correspondence of dates, similarity of activities, and the known links between Orton and Dieth, make it very likely that Dieth was in large measure the inspiration for the Newcastle laboratory.

A number of the other institutions which are included in Pop (1956) similarly combine (in varying proportions) the three roles of phonetics research laboratory, dialectological research centre, and recorded sound archive. Despite Ward's claim (1928: 53) that at UCL 'special attention has been given to the various types of English pronunciation and of English dialects', almost nothing substantial came from the London department on British vernacular varieties in the pre-war period.

4.5 The conception of a laboratory

It is striking that in the period under investigation, and for some time afterwards, a phonetics laboratory was conceptualized as a *facility*—a physical location equipped with services such as water and gas supplies, and having a cabinet of tools or techniques ready to be deployed. Contemporary photographs may illustrate a particular experiment in progress, but commonly include impressive shelves or cabinets of apparatus visible in the background. This was to be an enduring concept throughout the twentieth century; the question repeatedly asked was how to set up and equip a laboratory—not how to solve a particular problem or obtain particular scientific data. For example Windsor Lewis (2003) writes, in an obituary of James Anthony (1921–2003):

> … in 1948 [he] was appointed by David Abercrombie as technician in the newly opened Department of Phonetics at the University of Edinburgh. There he set up their laboratory – so successfully that in the 1950s he was much in demand to advise others on how to set up their new phonetics laboratories.

The focus, therefore, at least in the academic field, was on experimental phonetics as an institutionalized activity rather than on any clearly defined scientific or technological goals.

An important exception was at Bell Telephone Laboratories, in the enormously fruitful programme of work that was to be summed up in Fletcher (1929). As the first line of the Preface to the work tells us:

> Some fifteen years ago [i.e. around 1914, at about the same time as the UCL lab was established] the Research laboratories of the Bell telephone System undertook a comprehensive survey of speech and hearing to obtain the fundamental facts on which to base the design of apparatus and systems for telephone use.
>
> (1929: v)

There were thus clear research goals, and the single-minded effort to reach them was conceptualised as an ‘attack’. Particular techniques and equipment were subsidiary to the overall strategy, and were changed, improved or invented as the need arose.

A commercial—or military—laboratory is created to tackle a specific problem. Once that problem has been solved, the *raison d'être* for the laboratory is removed, and the laboratory itself may well cease to exist. The phonetics laboratory within a university context is different in at least two ways. Firstly, it always had a generic rather than a specific purpose, so it can hardly render itself superfluous to requirements by ‘solving’ the problems it set out to address. Secondly, it is founded as an institution, and becomes a component of the prestige of the university within which it is located. Once it has premises, a salaried staff, and reputation (Clark 2006), one of its primary goals (though it will certainly be an unstated one) in fact becomes self-preservation, whatever its claimed mission may be.

4.6 Epilogue: decline of the phonetics laboratory

There is apparently no mention of the Newcastle laboratory after 1940, and presumably it did not long survive the departures of Orton and of Curry (see Chapter 8). Orton's early materials did not remain in Newcastle. Materials—including recordings—from his 1930s survey of Northumbrian dialects are now in the Leeds Archive of Vernacular Culture, along with the post-war material gathered for the Survey of English Dialects.

The UCL laboratory survived in name, but there was a complete break with the pre-war work. In the immediate post-war period, Fry formulated a list of research goals (a typed list is preserved in the Daniel Jones Papers). All were concerned with hearing and speech perception; no work was foreseen on speech production or the sound systems of particular languages—and none was done. For a time in the post-war boom in British linguistics, general-purpose phonetics laboratories on the old pattern were fitted up elsewhere (such as in Edinburgh). But they were much more suitable for teaching than for research, and as the research expectations placed on universities became more explicit and demanding, the general-purpose laboratory went into decline. At the same time, more and more of the functions which had once required special-purpose laboratory apparatus were taken over by computers—at first, large and expensive 'laboratory' computers, but before long by standard desktop machines and laptops. Speech research became in large part a knowledge economy, with software and algorithms replacing the instruments which had dominated the laboratory of 1900.

Notes to Chapter 4

1 I am very grateful to Joanna Przedlacka for first drawing my attention to Grace Ellison's article.

2 The Tracts (Experimental Phonetics I) contains a 15-page item *L'institut de phonétique de l'université de Grenoble* which is probably the very 'prospectus' referred to in Jones (1909). The first six pages are indicated to be an extract from the *Annales de l'université de Grenoble XXI*, and they agree verbatim with Rosset (1909), though they are paginated differently and were evidently typeset independently. The other pages are 'Documents' (which are not included in the *Annales*) giving an outline syllabus, lecture course outlines, and examination requirements.

3 '…les inscripteurs, et les armoires contenant les divers appareils' (1909: 448).

4 (1) A map showing the college and nearby properties, with dates indicating when particular buildings first came into use, shows that the use of 21 Gordon Square cannot have *preceded* 1921. (2) In the same year the listing of phonetics lectures in the college Calendars begins to specify lecture venues—for example 'III', which is probably the main classroom created in 21 Gordon Square, which was still known as 'Room 3' when it was used for the last time in 2008.

5 The genre continues today—for example as a regular feature in *The Phonetician* (journal of the International Society of Phonetic Sciences). For example, Ashby, Faulkner & Fourcin (2012) belongs to this genre.

6 J. C. Catford (1917–2009), in the first of 8 video lectures recorded at the time of his retirement in 1985, available via Bakovic (n.d.).

7 Adam Hilger Ltd, manufacturing opticians and scientific instrument manufacturers, ledgers, accounts, order books. NRA catalogue reference: NRA 35204 Hilger. Examined 31 January 2013.

8 Keller (1971) is a survey of laboratory techniques, based on a student dissertation she completed in Edinburgh some ten years earlier. Since her teacher, Elizabeth Uldall, née Anderson (1913–2004) was at UCL in the 1930s, the work perhaps offers an indirect and retrospective glimpse of teaching and practice in the pre-war UCL laboratory.

9 Edinburgh has long registered a claim to have the second oldest *department of phonetics* in Britain, not specifically the second oldest phonetics laboratory as such. But since the laboratory formed a component of the department from the outset, the consequence that the Edinburgh laboratory must have been the second might be assumed to follow. It appears from MacCarthy (1956) that 1948 was also the year in which a department and a laboratory began in Leeds. Edinburgh's precedence over Leeds appears to have been about 10 months (January to October).

10 I.e., the same W. E. Curtis who supported Curry's initial work, and thus Richardson's colleague of many years.

[11] The second edition of 1935 has mainly been consulted, as being contemporary with the Newcastle phonetics laboratory.

[12] The Great Hall was destroyed in an air-raid in WWII, and was never rebuilt.

[13] This could be the result of printing the negative film records from the wrong side in the reproduction process. The photographs are credited to a W. Riddell (about whom nothing is known), and may perhaps have been made in a somewhat different way from Curry's of about five years earlier. In terms of sharpness, they appear inferior to Curry's.

5

Daniel Jones as scientist

5.1 Introduction

The life and career of Daniel Jones (DJ) are comprehensively treated by Collins & Mees (1999), and DJ's engagement with, and attitudes towards, experimental phonetics are traced in numerous sections of that work. Collins & Mees depict DJ as initially open to experimental phonetics, though never entirely at home with it. On their account he became over the years increasingly indifferent towards experimental phonetics, and eventually opposed to it. In that context, the title of this chapter may seem surprising. But the present thesis argues that at the beginning of his career DJ was not merely tolerant of experimental phonetics, but notably science-oriented. Section 5.1.1 reviews the account given by Collins & Mees, and section 5.1.2 adds detail about DJ's mathematical training. Section 5.2 argues that even DJ's early descriptive work can be seen as scientific in its approach; indeed, he can be viewed as having assembled and annotated the first spoken corpus, while detailed analysis shows that *Intonation curves* (1909) deserves to be treated as a quantitative work. Section 5.3 attempts to rebut the suggestion that before the creation of a laboratory at UCL, DJ's own teaching of experimental phonetics must have been derivative. Section 5.4 presents new biographical information indicating that the young DJ was eager to establish scientific credentials. The Cardinal Vowel theory is examined in detail in Section 5.5. In conclusion (5.6), it is argued that DJ's disillusionment with experimental phonetics may have been justified, given the entirely practical aims he always had in mind.

5.1.1 Critique of Collins & Mees (1999)

Collins & Mees (1999: 131–135) deal with the establishment of the Experimental Phonetics Laboratory and appointment of Stephen Jones to superintend it. They say, 'Despite his Cambridge mathematical background, Daniel Jones was not happy with scientific machinery, and grew to be more unwilling to have anything to do with instrumentation as the years passed' (1999: 132). No evidence is provided for the first part of this claim, and in fact it appears partly to contradict what they say themselves when discussing the sections on experimental phonetics contained in the first edition of DJ's *Outline of English phonetics*: 'Jones appears in his treatment of the kymograph to be completely in command of his subject'… 'his university training would enable him to cope easily with the elementary physics involved with speech research at this period' (1999: 245–252). Perhaps Collins & Mees intend to make a distinction between scientific understanding on the one hand, and the practical handling and adjustment of apparatus on the other, though this is not made entirely clear. As far as scientific understanding is concerned, it may be claimed that everything DJ wrote for at least the first ten years of his career shows him to be entirely comfortable with the methods and findings of experimental work. And in newspaper articles, at least, he was to go on making large claims for the value of the kymograph in teaching pronunciation into the mid-1920s (see Chapter 4).

5.1.2 DJ and mathematics

No treatment of DJ as a scientist can ignore the fact that he had a Cambridge degree in mathematics. He was at King's College from 1900, gained his BA in 1903 and proceded to MA in the usual way in 1907. Collins & Mees (1999: 11) say that his student days are sparsely documented. They suggest that despite promise shown at school he had no great talent for mathematics, and gained little from lectures. By his own account, he 'hated' mathematics, and was placed 'bottom of the Second Class in the Mathematical Tripos'.[1]

It seems worth inquiring, however, into the syllabus and its assessment, and into what even an unenthusiastic Second Class student can be expected to have gained from the degree.

Until the mid-nineteenth century, mathematics was the only subject that could be read at Cambridge for an Honours degree, and it was treated as a preparation for all professions, including medicine and the law. A natural sciences tripos was added in 1851 but the mathematics tripos continued to be the most highly regarded qualification for those entering science, and many of the most eminent nineteenth century physicists had a Cambridge mathematics degree (e.g., Herschel, Kelvin, Maxwell, Rayleigh).

So the mathematical tripos was simultaneously a general-purpose degree and the most highly-regarded preparation for a scientific career. The differences lay in how much of the extensive examinations were attempted and completed, and in the marks obtained. According to Wilson (1982):

> Examiners designed the tripos primarily as a liberal examination to suit the majority of those taking it. According to Cambridge pedagogy, study of mathematics and of stable, mathematized subjects like mechanics and hydrostatics educated the mind in orderly thought. After such an education, a man could go on to grapple with law, medicine, or the natural sciences.

> Examiners always had to distinguish between the two kinds of students—"those who pursue the study as a means of general education" and "professed mathematicians"—as they were described in 1878.
>
> (1982:336)

About 100 students took the examinations each year in the late nineteenth century, and about one third of those became 'wranglers' (i.e., gained First Class marks). But less than one-half of the wranglers were likely to become 'professed mathematicians'. In other words, only about 10–15% of those taking the mathematical tripos were 'mathematicians' in this narrower sense—for example, likely to pursue a career in mathematics education or research.

Though there is no question of DJ being a mathematician in the narrower sense, the content of the degree was certainly not trivial, and DJ must have left Cambridge familiar with all manner of fundamental mathematical and physical concepts that would more than fit him for dealing with the experimental phonetics of his day. By the late nineteenth century various aspects of natural science had been incorporated into the mathematical tripos (though a mathematics student did not do any practical lab work). The Part I syllabus in force in DJ's day (Wilson 1982: 370–371) included not only algebra, geometry, trigonometery and calculus, but elements of dynamics, optics, astronomy and electricity. We do not know what Stage II subjects DJ may have covered, but those offered included, among many others, Fourier's theorem and some aspects of sound.

5.2 Intonation curves (1909)

An account of DJ's 1909 publication *Intonation curves* is given by Collins & Mees (1999: 60–65). They urge that the largely neglected work 'deserves recognition as being the first full-scale attempt at utilising an instrumental approach to intonation for the analysis of anything other than brief utterances.' The view presented here is that the work is in fact considerably more significant than Collins & Mees claim. For a number of reasons, to be explored below, it comes closest among DJ's publications to being a work of scientific phonetics.

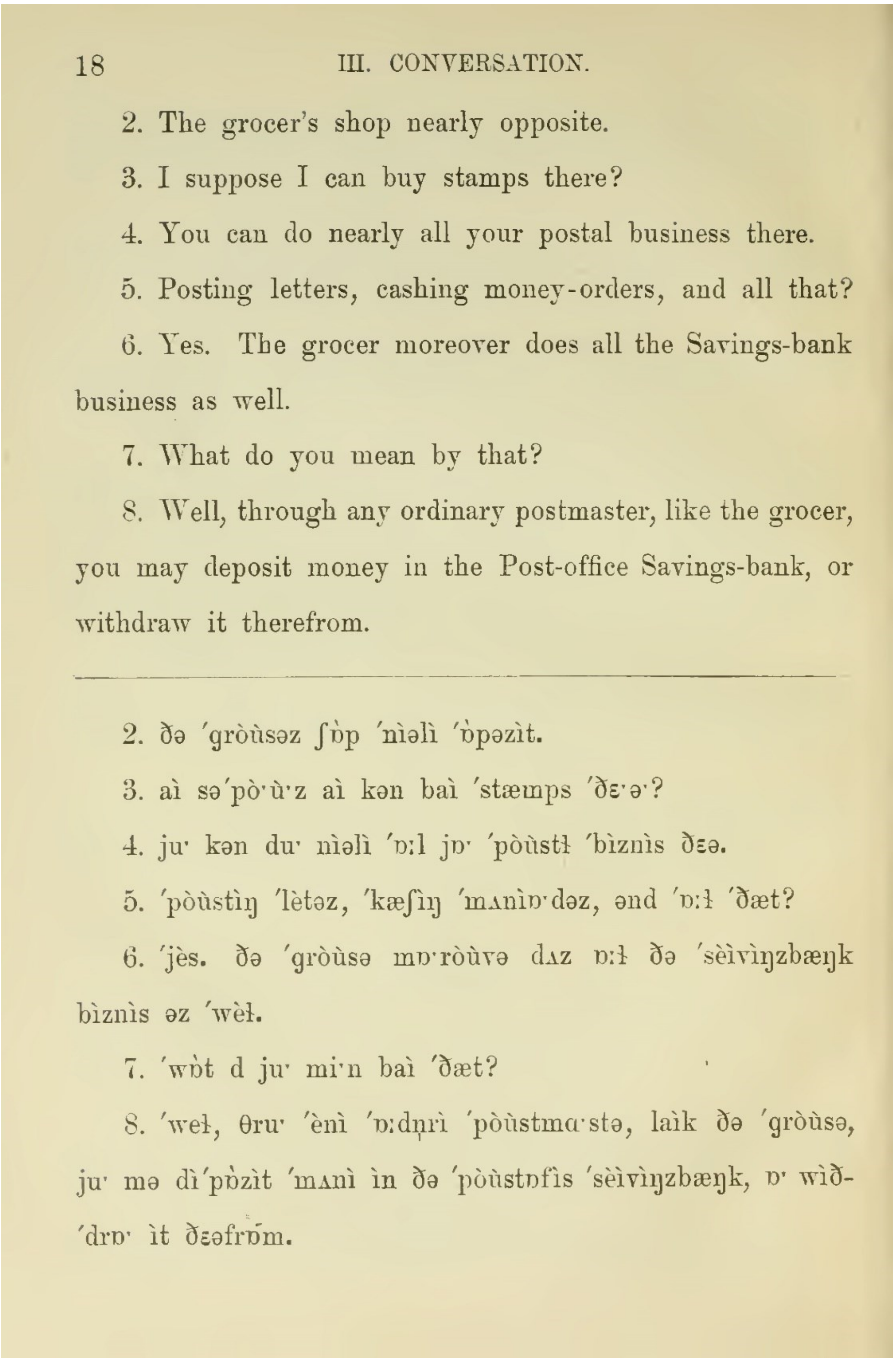
5.2.1 The book and its purpose

The full title is *Intonation curves: A collection of phonetic texts, in which intonation is marked throughout by means of curved lines on a musical stave.* Though it is has been regarded as of interest chiefly on account of the intonation curves it contains, it is important to realize that the book is—as the subtitle clearly states—a collection of phonetic texts, and the phonetic transcriptions themselves are also of great interest. Three languages—English, French and German—are included, and both literary and conversational styles. Unlike the situation in most phonetic readers, and unlike his own practice in later works, DJ is not representing imaginary or ideal pronunciations of the texts selected, but the specific (and potentially flawed) utterances of individuals in commercially available recordings. In a similar way, the first (1909) and second (1914) editions of *The Pronunciation of English* feature detailed analysis of the pronunciations of specified individuals and their sociolinguistic background. DJ's early work can thus be seen as more data-oriented and objective, potentially verifiable—and therefore in a sense more 'scientific'—than his well-known later works.

5.2.2 DJ's corpus

Intonation curves was based on the analysis of eight commercially available gramophone records (see Table 5.1). DJ gives full particulars of the records, which has made it possible to seek surviving copies of them and attempt a partial verification of his findings.

In addition to the intonation analysis, DJ gives (1) the orthographic versions of the texts, (2) a 'very detailed form of phonetic transcription' (i.e., to use later terminology, a 'narrow' transcription) of the particular pronunciations used on the records), and (3) a transcription of the 'standard' pronunciation of each language. This last is effectively a 'broad' transcription, approaching a phonemic transcription, but not necessarily of the same sounds in the same order as those shown in the detailed transcription, since in some places the 'standard' is shown as different from the idiosyncratic pronunciations of individual speakers (for example, in such things as sandhi-form choices). Sample pages are reproduced as Figures 5.1 and 5.2.



18 III. CONVERSATION.

2. The grocer's shop nearly opposite.

3. I suppose I can buy stamps there?

4. You can do nearly all your postal business there.

5. Posting letters, cashing money-orders, and all that?

6. Yes. The grocer moreover does all the Savings-bank business as well.

7. What do you mean by that?

8. Well, through any ordinary postmaster, like the grocer, you may deposit money in the Post-office Savings-bank, or withdraw it therefrom.

---

2. ðə ˊgròùsəz ʃɒ̀p ˊnìəlì ˊɒ̀pəzìt.

3. aì səˊpò·ù·z aì kən baì ˊstæmps ˊðɛ·ə·?

4. ju· kən du· nìəlì ˊɒːl jɒ· ˊpòùstɫ ˊbìznìs ðɛə.

5. ˊpòùstìŋ ˊlètəz, ˊkæʃìŋ ˊmʌnìɒ·dəz, ənd ˊɒːɫ ˊðæt?

6. ˊjès. ðə ˊgròùsə mɒ·ròùvə dʌz ɒːɫ ðə ˊsèìvìŋzbæŋk bìznìs əz ˊwèɫ.

7. ˊwɒ̀t d ju· mi·n baì ˊðæt?

8. ˊweɫ, θru· ˊènì ˊɒːdn̩rì ˊpòùstmɑ·stə, laìk ðə ˊgròùsə, ju· mə dìˊpɒ̀zìt ˊmʌnì ìn ðə ˊpòùstɒfìs ˊsèìvìŋzbæŋk, ɒ· wìð-ˊdrɒ· ìt ðɛəfrɒ́m.

Figure 5.1 A specimen page of the English conversation sample in *Intonation curves*. Above: orthographic version; below: 'standard' transcription.

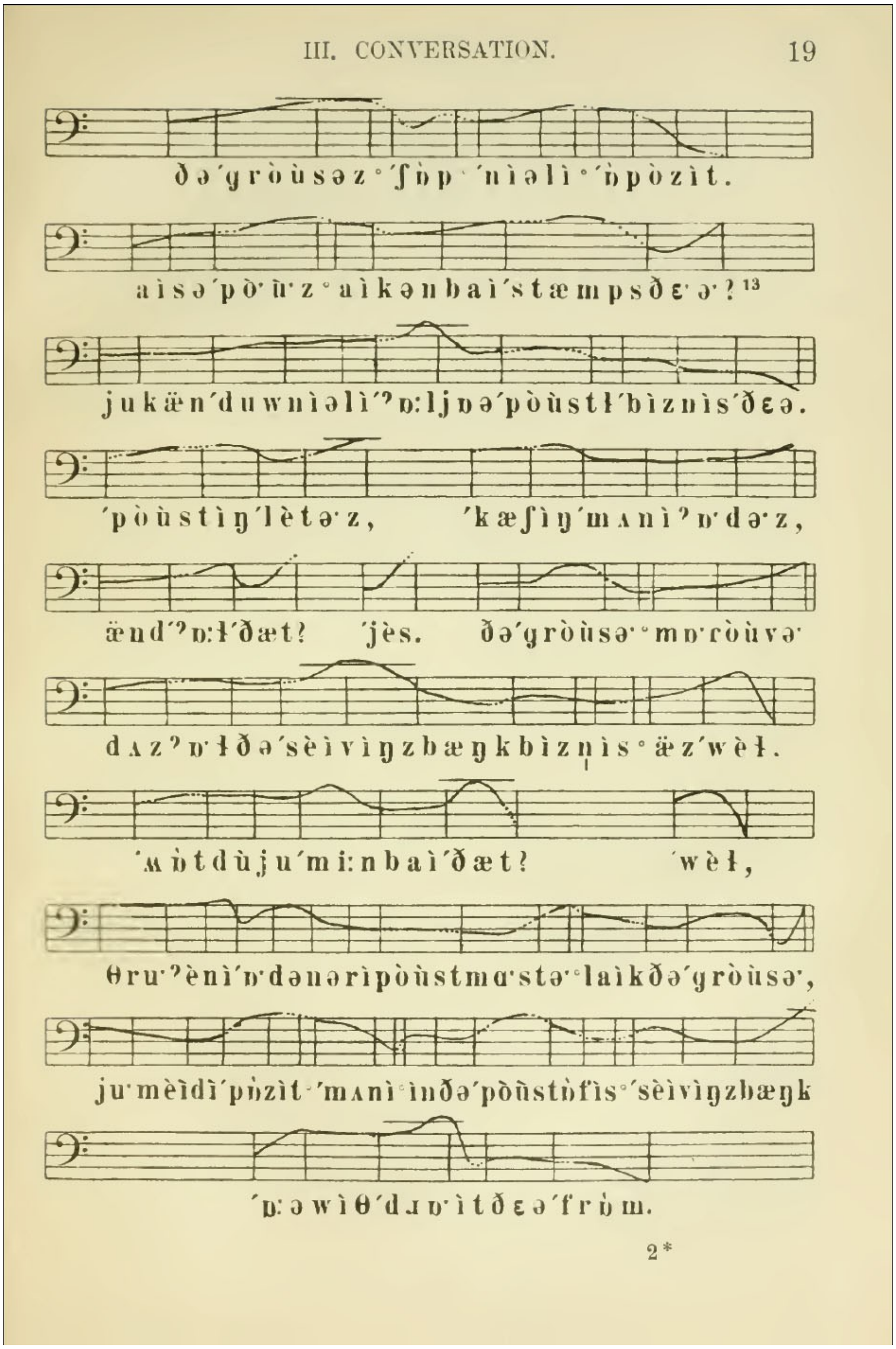


Figure 5.2 The page facing the sample shown in Figure 5.1, with intonation notation and ‘detailed’ phonetic transcription. Divergence of the latter from the ‘standard’ transcription is evident—for example, in the treatment of the second syllable of *opposite* in the first line.

Language	*GC cat no*	*Title*	*Speaker*
English	1315 III	Passage from Shakespeare's *Richard II*	Sir H. Beerbohm Tree
English	1356	Poe's *The Bells* (verses 1–3)	Canon Fleming
English	1286	Conversation from Langenscheidt's *Englisch*	Bernard MacDonald
French	31171 II	Passage from Rostand's *La Samaritaine*	Sarah Bernhardt
French	31253	Lafontaine's *Le Corbeau et le Renard*, and *Le Loup et l'Agneau*	Louis Delaunay
French	31284	Conversation from Barlet and Rippmann's *French Life and Ways*	unknown
German	11968 II	Passage from Schiller's *Wallenstein*	Max Montor
German	41319 III	Passage from Goethe's *Faust*	Otto Sommerstorff

Table 5.1 DJ's corpus of 1909 as used in *Intonation curves* ('CG cat no' = Gramophone Company catalogue number). Surviving copies of all the English and French recordings have been located and digitised for this study. The Geman recordings have not yet been found.

DJ was thus working with an ensemble of recordings and a set of time-aligned representations on several levels. It is not unreasonable to suggest, therefore, that DJ's 1909 material for *Intonation curves* constitutes an early—and possibly the first—spoken corpus.

5.2.3 DJ's method and sources

The method of working (1909: v) was to listen repeatedly to the records, lifting the needle from the record at successive points and noting the 'the impression of the sound heard at the instant when the needle is lifted'. It is plain that the method was used not only for matching the pitch, and thus plotting the intonation curves, but also in making the detailed segmental transcriptions: '…it is generally necessary to take several observations for the quality of a given sound and several more for its pitch'. The intonation curves are aligned with the detailed segmental transcriptions, and divided into 'bars', each corresponding to one syllable of the text. The length of each syllable on the page is simply the width occupied by the corresponding section of the phonetic transcription, which is printed with normal character spacing (but no word spacing), and as a result the indicated lengths of the bars have little correspondence with real durations. Exactly the same method had been used for analysis and display of intonation in one of the texts in Jones (1907) though that uses cursive handwritten transcription and no bar-lines are employed.

Whereas the symbolic transcription represents categorical judgments, and thus achieves a nominal level of measurement (Ashby 1990), the representation of pitch on a musical scale (calibrated with an absolute reference of frequency) constitutes measurement on a ratio scale, and is no different in principle from a physical determination made with an instrument. DJ's musical notation, and an objectively determined fundamental frequency curve obtained from the original recordings, are in principle interconvertible. Either direction of conversion is possible: either (1) DJ's representations can be turned into quantitative estimates of frequency, or (2) measured fundamental frequency can be plotted on a staff so as to resemble DJ's representations more or less closely. Both are attempted in the present analysis and for convenience they will be termed DJ > f_o and f_o > DJ respectively.[2]

DJ does not cite any references in *Intonation Curves*, and it is unclear whether he independently hit upon the idea of musical notation for intonation. The fundamental insight is that the musical staff, though developed for the notation of discrete pitches, can be turned into a scale representing continuously variable pitch. The indication on a musical staff of voice pitch in speaking can be traced to Steele (1775), who deserves credit for a number of profound insights into pitch variation in speech which we now take for granted. For example, he realised that pitch varies continuously (in 'slides') rather than in a step-wise fashion, and therefore requires representation by a continuous graph line, and that the extent of relevant variation is often relatively small in relation to the conventional intervals of diatonic music, necessitating judgements made with the precision of quarter-tones or less:

> I was of opinion that, in pronunciation, the voice moved up and down by such small gradations as that, whether the degrees were by quarters of a diatonic tone, or by smaller divisions, they seemed, in comparison with those of our chromatico-diatonic to be by imperceptible slides.
>
> (1775: 1–2).

Steele represented the pitch of male speakers by means of slanting and curved lines drawn on the bass (F) clef just as DJ was to do, and describes copying the intonation of speech with a suitable musical instrument such as the bass viol or a large organ pipe fitted with a sliding plunger (1775: xiv). By serving to externalize the pitch pattern and make it repeatable, the musical instrument performs for Steele something of the same function as the gramophone for DJ. Overall, there is every reason to believe that Steele's indications of pitch are not only qualitatively correct, but close in absolute pitch for the speakers he observed.

While there is no specific evidence that DJ was familiar with Steele's work, it seems very likely that he must have encountered at least mentions of it. It is not necessary to suppose that the young DJ would have needed antiquarian interests to uncover it; Barker (1924) probably exaggerates somewhat the obscurity into which Steele's work had fallen by the beginning of the twentieth century. In fact it had continued to be cited throughout the nineteenth century, and was treated by elocutionists such as Smart (1819) and Murdoch (1883). A source potentially even closer to DJ is Ellis (1874), in which a footnote (p. 157) makes it clear that Henry Sweet was present at the delivery of the paper. Hence—if by no other route—DJ could have learned of Steele by word of mouth from Sweet around 1907 (Collins & Mees 1999: 48).

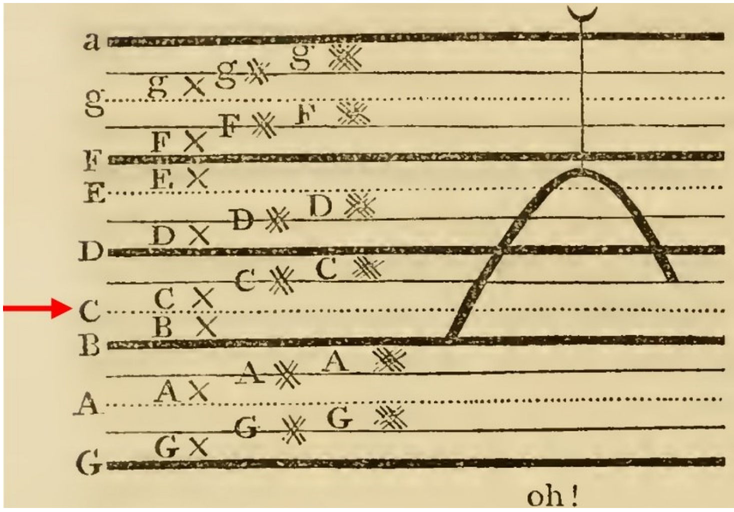


Figure 5.3 Steele's staff for the representation of intonation (1775: 6), showing proportional use of the intervals between staff lines. For example, the note C (arrowed) is located on a dotted line placed one-third of the distance between B and D.

Figure 5.4 Daniel Jones's explanation of the interpretation of his staff (1909: ix). Unlike Steele, he places the note C in the centre of the interval between B and D.

Both DJ and Steele give an account of how the staff will be used to indicate pitches that fall between the notes of the diatonic scale (Jones p. ix, Steele p. 6), though it must be said that Steele's account is more detailed and specific. Illustrations from the two accounts are reproduced as Figures 5.3 and 5.4. There are two important differences between Jones's and Steele's treatments of the staff. Firstly, the five (bold) lines of Steele's staff are unequally spaced, being the result of ruling appropriate fixed lines onto a logarithmic scale which is constructed from equal-sized semitone steps. Staff lines ruled at their true logarithmic positions will in some cases be separated by 4 semitones (as between G and B in the lowest two lines of the bass clef), and in others by 3 semitones (as in the next step, from B to D). DJ was of course aware of this, and illustrates a staff of this kind himself in the first edition of the *Outline* (1918: 181). The visual unevenness of spacing attracts no comment from DJ, and is probably of little importance to practised musicians, who are generally accustomed to reading irregularly sketched notation anyway. Nevertheless, in *Intonation curves* DJ employs a musical staff of regular spacing and allowance must be made for this in interpreting his analyses. Secondly, there is evidently a difference in the way that Steele and DJ interpret the Y coordinate within the 3-semitone spaces. Close examination of Figure 5.3 will show that Steele divides the space into 3 equal semitones, whereas DJ assigns a named note to the middle of the space, as it would be positioned in conventional musical notation. This has the somewhat illogical consequence that 50% of the space corresponds to one semitone, and the remaining 50% to two semitones.

As for previous work nearer his own time, DJ cites no references, but gives the following general characterisation:

> Accurate records of intonation have, it is true, been produced by means of tracings of voice vibrations, obtained by the use of a kymograph or otherwise. The vibrations may in this way be measured, or the number occurring in short units of time counted, and the results plotted on squared paper, the variations of pitch being thus expressed by curved lines. Such curves are, however, inconveniently large and elaborate, and the phonetic symbols to which the various parts of the curves correspond have to be placed far apart and at irregular intervals, thus rendering the text difficult to read. Besides this the work of preparing curves by this method is so laborious, that no one has ever yet analysed texts of sufficient length to be of any practical value to language students.
>
> (1909: iv)

Roudet (1899) is an example of the type of earlier work with which DJ explicitly contrasted his own. The stepwise appearance of Roudet's curve (Figure 5.5) results in part from the unsmoothed period-by-period measurement of the speech waveform, and perhaps in part from rounding errors and approximations in measurement. The lower half of the figure represents intensity. Frequency is plotted on a linear scale, but lines are drawn corresponding to the whole notes E, F, G, etc., and the treble clef sign added at the G position. Roudet remarks that the result is a sort of distorted musical staff (French *portée*). The shading between alternate pairs of notes reinforces the visual resemblance. The 24-page article makes large claims to have solved all the problems of analysis of 'l'accent' but results in the graphical representation of only this single short utterance, which is evidently of little interest in itself to the author. He tells us nothing about the speaker, though from the voice pitch reported we may suppose she was female.

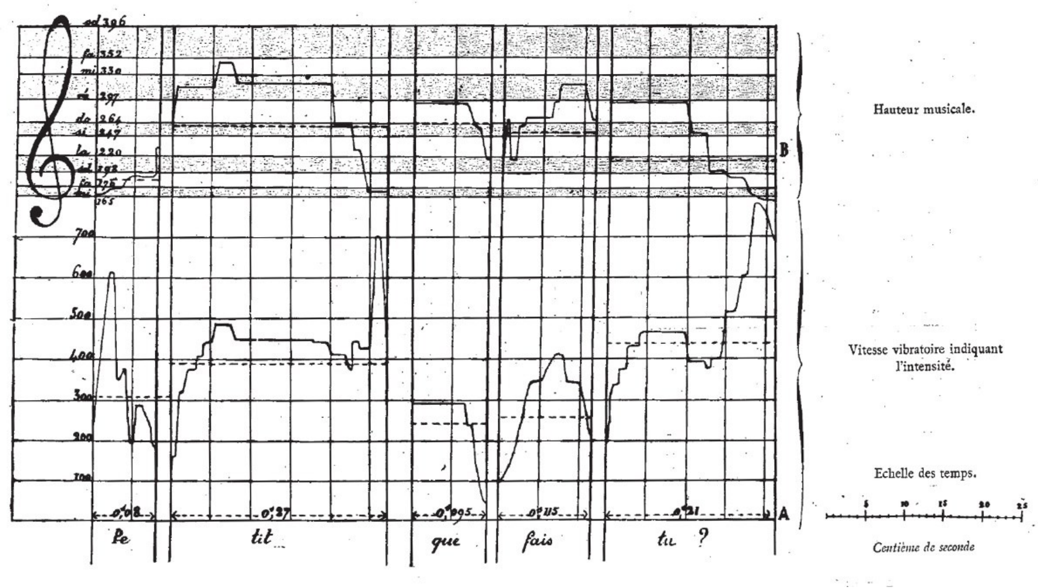


Figure 5.5 Intonation curves published by Roudet (1899).

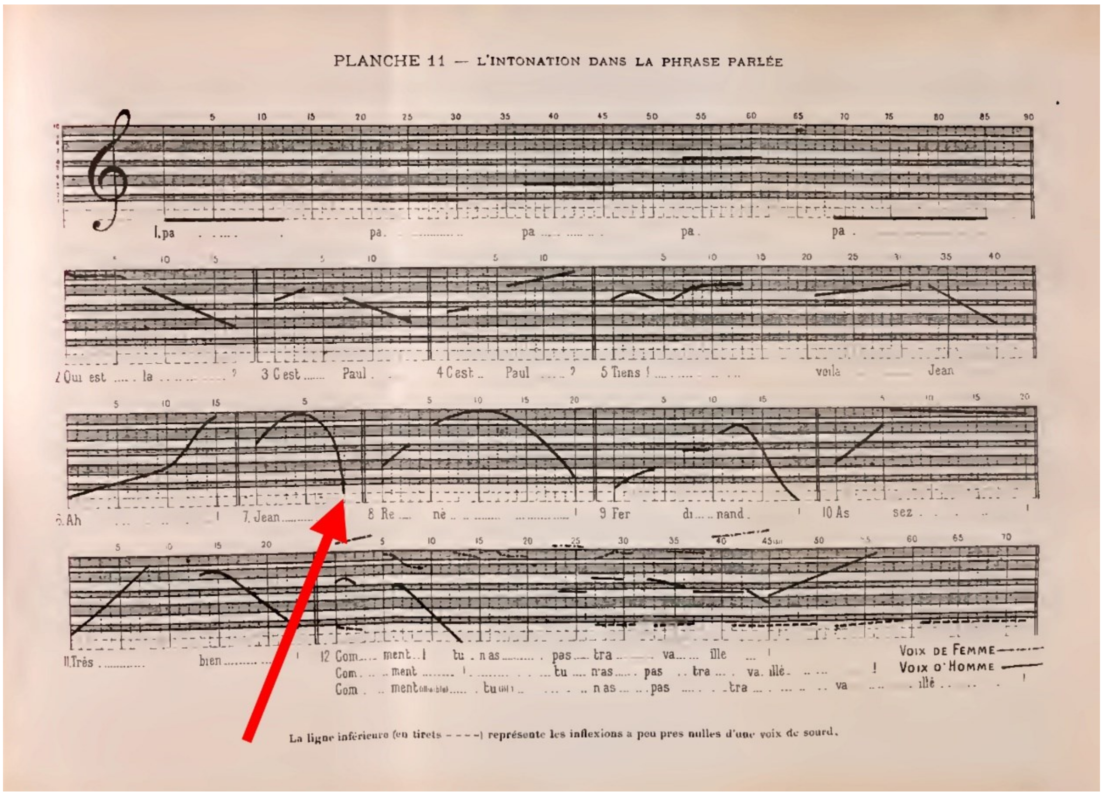


Figure 5.6 Intonation curves published by Marichelle (1897). The value arrowed is at the termination of a falling tone on *Jean* (line 3, section 7). The implied f_0 at that point is around 260 Hz—about three times the expected value for a male speaker (80–100 Hz).

Roudet (1899: 37) acknowledges Marichelle (1897) as the origin of his graphical display, and that work too may have been known to DJ. Marichelle's method was graphical (microscopic) analysis of the grooves on phonograph cylinders, and he determined pitch by measuring the periodicity of the wave patterns in the groove. He gives a single graph (plate 11, following p. 112) showing samples of pitch movements and it is reproduced here as Figure 5.6. The visual resemblance to Roudet will be evident. Examining the implied f_0 values, it seems likely that Marichelle's frequency estimates are higher than would be expected for a male speaker by a factor of 3 to 4 times; perhaps Marichelle was unwittingly tracing the periodicity of the third or fourth harmonic of the speaker's voice, as a result of the failure of the recording apparatus to respond to the fundamental or second harmonic. The ‘missing fundamental’ was a well-known deficit of early acoustic recordings (Lloyd 1898: 98-99).

5.2.4 Verifying DJ’s estimates

The original recording speeds of the records DJ used are not known precisely. By the early 1900s, records were usually made at speeds of 70–80 rpm but no standard had been agreed. Records produced in the USA were commonly at 78–80 rpm, while in Europe speeds were often around 75 rpm, but there was wide variation and inconsistency (Holmes 2006). In the case of a musical performance in a known key, the required rotation rate can be inferred, but speech records carry no inherent pitch reference to make this possible. When listening, DJ had no way to match the rotation rate of the original recordings exactly, so his absolute estimates of fundamental frequency were almost unavoidably in error. Modern determinations of fundamental frequency from the records are subject to the same difficulty. The most that can be said is that reproduction at the modern standard rate of 78 rpm is on balance likely to be somewhat fast (approximately 4%) bearing in mind the European provenance of the records under consideration.

DJ does not report any attempt to measure the empirically-adjusted rates he settled on, but they were almost certainly somewhat different from record to record. It was hypothesized, therefore, that the comparison of modern measurements made at a constant 78 rpm with his judgments from different records can be expected to show different percentage discrepancies, which should be roughly constant and characteristic for each record.

5.2.5 Method DJ > f_o

To convert DJ's musical representations into f_o estimates, positions of successive points on the curves drawn on the staff must be estimated graphically. This was accomplished on a large computer display. A scanned image of the printed page was viewed in high magnification and measured with an on-screen cursor. The cross-hair tool of a graphics application (picpick.exe)[3] was found suitable. As the cross-hair is moved with the mouse to new positions on the display, its coordinates can be read off as integral numbers of pixels.

Intonation curves is a rather small book of 80 + xvi pages 8vo (175 × 115 mm). The staves are approximately 5.5 mm high on the printed page. So each of the four 'spaces' on the staff is (5.5/4) mm. If it is desired to measure at least 10 steps within each such space, the minimum number of pixels required per mm is therefore 10 × (4/5.5), and the minimum scanning resolution which will achieve this is 25.4 × 10 × (4/5.5) = 184.7 dpi (where 25.4 is the number of mm in one inch). In practice, a scanning resolution of 600 dpi was employed, and the resulting images viewed at 100% magnification, with the result that there are about 35 measurable steps within each of the staff spaces. The staff must of course be displayed exactly parallel to the horizontal axis of the display; it was not found difficult to adjust the angular orientation of the image so that a line of the staff has a constant Y coordinate (within 2 pixels) across the width of the page.

The Y coordinate of each measured point must then be transformed into the logarithm of the corresponding frequency. This was accomplished by entering the coordinates into a spreadsheet and mapping them to corresponding log f_o values by means of a lookup table provided with 139 steps, one for each pixel of staff height on the bass clef. It is constructed by inserting values for the known frequencies of the notes G B C D E F A, determining their logarithms, and interpolating appropriately. The mapping which was devised compensates for differences between 4-semitone and 3-semitone steps, and for DJ's positioning of the named notes C and E in the middle of their 3-semitone spaces, instead of at the logical one-third positions.

A section of French conversation 25 syllables in length was taken from the beginning of *French Life and Ways*. Conversational style was preferred over verse, so as to sample realistic intonation patterns, and this particular record has a relatively good signal-to-noise ratio, producing clean f_0 tracks, but in other respects the selection was random. The audio file was opened in SFS (Huckvale 2013), downsampled to 8000 Hz, and a wideband spectrogram added to assist annotation. A fundamental frequency track was added, using 'fxrapt', an implementation of the RAPT algorithm (Talkin 1995). All of the various fundamental frequency estimation algorithms available in SFS were evaluated, and RAPT was found to give greatly superior results to the others. Annotations were added manually in a further track, to mark the time points at the beginning and the end of the voiced portion of each syllable, following the syllable division shown in the original text. Annotations were placed at the earliest and latest points within each syllable where the f_0 track showed a measured value and simultaneously the spectrogram indicated the presence of vocal fold vibration. If voicing and the successful determination of f_0 continued across a syllable boundary, a single sample point was taken at the boundary. If the rate of change within the voiced portion of a syllable appeared not to be constant, f_0 was additionally sampled at the temporal midpoint, or if the voiced portion contained a peak or trough, the additional sample point was located there. A script written in Speech Measurement Language

(SML) (Huckvale 2013) measured and tabulated f_o at each annotation point, and matching graphical determinations were then made of DJ's estimated f_o at all the corresponding points in the musical representation. The result was 50 corresponding pairs of f_o determinations at sampling points in 25 syllables, each syllable being represented by 1, 2 or 3 points according to the criteria just enumerated.

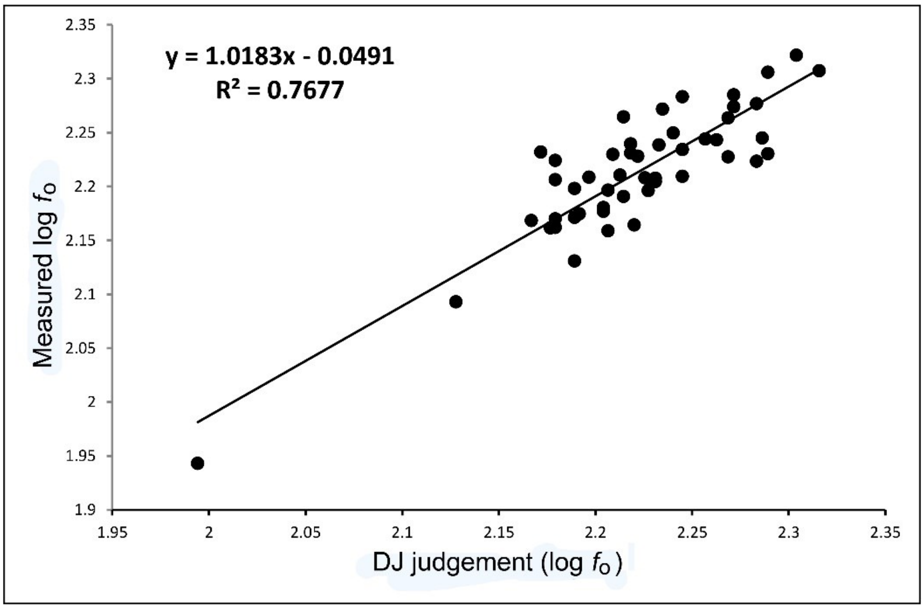


Figure 5.7 Correlation of automatically-made estimates of f_o with DJ's estimates (1909) using 50 data points from a sample of 25 syllables.

5.2.6 Results DJ > f_o

Figure 5.7 is a scatterplot showing DJ's estimates (x-axis) versus measured f_o (y-axis). The correlation is high ($r^2 = 0.77$), indicating an excellent degree of agreement. Figure 5.8 shows the same data in another way, plotting the two functions against sample number and thus aligning them in normalized time. DJ's estimate of f_o can be seen to track measured f_o with impressive accuracy.

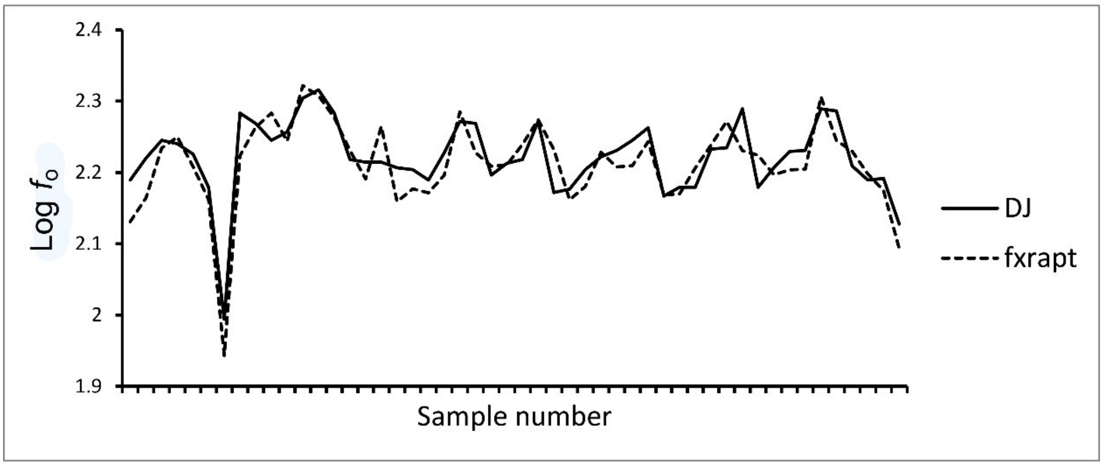


Figure 5.8 The f_o estimates of Figure 5.7 plotted as a function of sample number.

The test sample had been chosen essentially at random and there is every reason to expect that the high correlation seen in Figure 5.7 will be found generally. As for the absolute frequency calibration, it will be seen that the intercept of the fitted regression line in Figure 5.7 is negative, tending to indicate that DJ's judgements are on the high side, and thus that he probably chose to play this record somewhat faster than the modern standard of 78 rpm. But as Figure 5.8 suggests, much of the scatter in Figure 5.7 is probably owing to (unavoidable) misalignment in time of the selected sample points. The choice of the best-fit line is sensitive to all sources of dispersion—those which are effectively artefacts of the present analysis as well as real discrepancies in the original frequency estimates. As a consequence, the value of the intercept cannot be trusted as a numerical estimate of DJ's playback rate. As will be shown below, the f_o > DJ direction of analysis is affected by fewer sources of error and gives a clearer indication.

5.2.7 Method f_o > DJ

A spreadsheet was devised which accepts f_o values in Hz from a text file, and displays them on a logarithmic scale, along with constants (for the lines) and suitable graphic elements to plot the grand staff, consisting of the treble and bass staves and the optional leger line between them, as shown in Figure 5.9, which additionally shows a test signal ascending chromatically through the whole range. Bar lines, when required, are added as graphics. The numerical scale at the bottom is provided for convenience in working, and is cropped off after use. The size difference between the 3 and 4 semitone spaces is retained, and positions within the 3-semitone space are allocated linearly—both provisions following Steele (1775) rather than DJ.

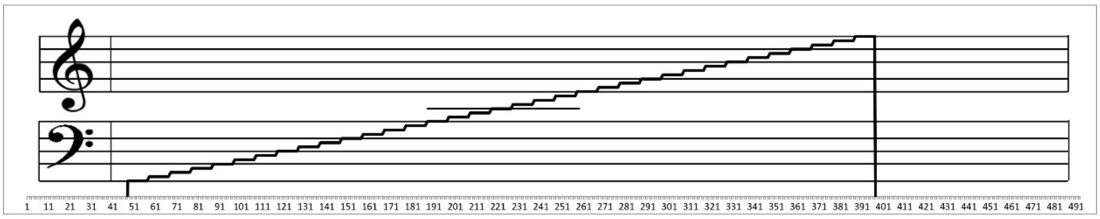


Figure 5.9 Output format of the system devised to plot fo automatically on a musical staff. The line shown is the result of analysing a test signal which ascends in semitone steps.

If f_o is sampled at regular intervals and plotted directly into such a system, the x-axis will of course represent real time. Visual comparison with DJ's versions could be made easier if the plotted f_o were selectively stretched and compressed at appropriate points to match the pseudo-time of DJ's bar lengths. To accomplish this, the length of each bar and of each voiceless region is measured in pixels on an image of the printed page. The audio file is opened in SFS, and annotations added manually at each corresponding position in the signal. A further script was written which uses these measured lengths to set the rate at which f_o is to be sampled in each annotated region. The result is an f_o track in which the number of samples in each region is linearly related to its desired length on the page. When this is used as an input to the conversion system already described, the resulting plot will align with DJ's version almost perfectly.

Figure 5.10 Top: automatically plotted intonation curve for one phrase of the English Conversation passage (male speaker); below: DJ's version.

5.2.8 Results f_o > DJ

Figure 5.10 shows the comparison for the phrase *The grocer's shop nearly opposite*, which is the second line of the English Conversation section (p. 19). This was selected as a representative English pattern, using most of the speaker's range. In terms of the intonation system of Wells (2006) the pattern heard on the record is easily recognised as **the ˈgrocer's \/shop | ˈnearly \opposite |**. There are two intonational phrases (IPs), a fall-rise nuclear tone on *shop*, a high fall nuclear tone on the first syllable of *opposite*, and pre-nuclear accents on each of the first syllables of *grocer's* and *nearly*. There is a remarkable degree of agreement between the automatically-extracted version and DJ's. The shape and range of the two nuclear tones correspond closely, and DJ has also accurately tracked the rising f_o pattern on each of the two pre-nuclear accents. Even the pitch of the weak anacrusis *the* has been heard correctly.

Also from the English Conversation passage (line 5) is *Posting letters, cashing money orders* (Figure 5.11). This line was selected because much of *money-orders* is on an approximately constant pitch, notated by DJ as exactly on the F line, and thus offers a means of estimating the pitch offset between DJ and the measured f_o.

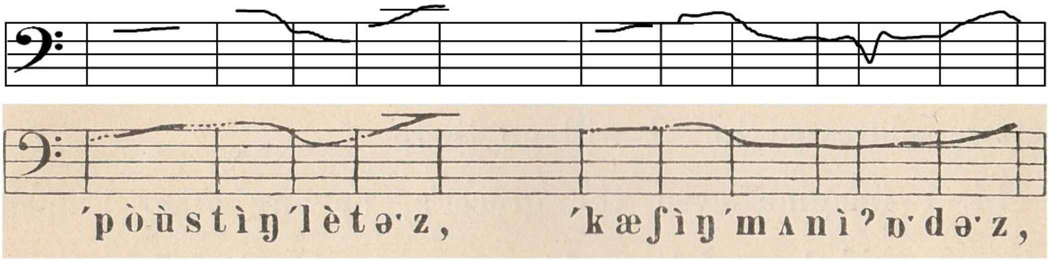


Figure 5.11 Top: automatically plotted intonation curve for a second representative phrase of the English Conversation passage (male speaker); below: DJ's version.

Again, the two versions show a close agreement throughout. Notice for example the meticulously tracked pitch trough in the first syllable of *letters*, the small peak located in the second syllable of *cashing*, and how the greater part of the final rise is correctly placed in the second (unstressed) syllable of *orders*. Comparing the measured (level) fundamental frequency in the second syllable of *money* with DJ's notated level, it appears that DJ's judged pitch at that point is about 4% flat—suggesting that he may have been listening to this record at approximately 75 rpm. DJ was acquainted with the speaker, Bernard P. MacDonald, so it is likely that the selected rate gives values typical for the speaker.

Figure 5.12 illustrates the comparison for *Keeping time, time, time, in a sort of runic rhyme* from *The Bells*. This line of verse was chosen because it is intoned almost on a monotone and offers a good opportunity to estimate the pitch offset between DJ and the measured f_0. As pitch varies so little in the line, the comparison also enables one to see that DJ's version is remarkably accurate in registering small changes in pitch.

Figure 5.12 Automatically extracted f_0 (top), and DJ's analysis (below) of a line from Fleming's recitation of *The Bells*.

Notice the downward drift of pitch in each of the two syllables of *keeping*, the two minute undulations in the first occurrence of *time*, the minor peaks in each of *sort* and *runic*, and the narrow rise on the final syllable, *rhyme*. As for the absolute frequency comparison, it will be seen that DJ's estimate appears to be a little more than one semitone flat compared with the modern determination. This is larger than the offset noted for the English Conversation record, and implies that in this case DJ selected a rotation rate of about 73 rpm. The hypothesis that different records would exhibit different offsets is therefore supported.

5.2.9 Discussion

Intonation curves has been treated at some length because its nature and significance appear never to have been fully appreciated. The favourable assessment given by Collins & Mees (1999: 63–65) is certainly along the right lines. They are correct in saying that '[i]t was the only analysis of a relatively large corpus of speech available at the time, and indeed for many years thereafter' (1999: 63). Bearing in mind the accuracy and detail of DJ's analyses as demonstrated above, the sheer scale of his corpus is remarkable. Instrumentally-assisted analyses of reasonably long texts began to appear in the 1930s, especially from Germany, but even then the quantity of material is not large in comparison with DJ's achievement. For example Hensel (1940) deals with the two sides of a single gramophone record—a quarter of the size of DJ's corpus thirty years earlier.

As for the conclusion of Collins & Mees that '…it deserves recognition as being the first full-scale attempt at utilising an instrumental approach to intonation for the analysis of anything other than brief utterances' (1999: 65), some reservations must be raised over the word 'instrumental'—especially since their statement seems partly to contradict what they say a little earlier: '…analysis by ear…provides a convenient short cut over the more rigorously scientific methods of acoustic analysis' (1999: 63). Of course *Intonation curves* makes 'instrumental' use of a gramophone (and indeed, of a tuning fork), but does it rely fundamentally on 'an instrumental approach' or instead on 'analysis by ear'? Understanding and attempting to answer that question may throw light on the distinction between 'taxonomic' and 'scientific' phonetics which is one of the themes of the present work. Hart *et al.* (1990) put the work in a category of 'dubious empirical status' and say 'impressionistic auditory descriptions remain difficult to interpret and may not be representative of other listeners' perceptions'. But as has been demonstrated, DJ's judgements—though not presented in an obviously numerical form—are quantitative, precise, objective and verifiable. It is hard to see in what respect they fail to be 'empirical'.

The interrupted listening technique which DJ applied—essentially an early form of 'gating' (Grosjean 1980)—probably has the effect of favouring a certain mode of listening. He was selectively attending to very short portions of the signal, and furthermore listening analytically to just one characteristic—pitch. As a result, there was a greatly reduced load on short term memory, and little or no need to impose a linguistic categorization on what he was hearing. Any listener under those conditions is likely to be pushed away from a 'speech mode' of perception towards a psychoacoustic mode of response. What this suggests is that characterisations such as 'analysis by ear' or 'impressionistic auditory description' are simplistic: there are in fact many modes of listening. Therefore any proposed dichotomy between 'instrumental' and 'auditory' will be meaningless and facile unless the precise meaning of 'auditory' is specified.

In *Intonation curves*, assisted by the gramophone, DJ hit upon a particular blend of modes of listening that was extraordinarily successful.

5.3 DJ's own 'Experimental phonetics' teaching, 1909–1911

Daniel Jones introduced his own course 'Experimental Phonetics' which ran in the years 1909–10 and 1910–11, before the appointment of Stephen Jones or the establishment of a laboratory. It consisted of 'A course of eight lectures illustrated with apparatus and lantern slides'. Collins & Mees (1999: 81–82) suggest that this course must have been rather derivative. They assume that the lantern slides must have illustrated work done by others, and don't comment at all on the 'apparatus' which the course description appears to imply.

In fact, we can find definite clues to what some of the slides may have contained, and very specific indications that DJ had some at least some smaller and less expensive instruments at his disposal several years before the establishment of the laboratory. The first (and second) editions of DJ's *Outline* contain fairly extensive material on 'experimental' phonetics (the extent of this material dwindles markedly in successive editions). The early descriptions are commonly accompanied by the names and addresses of makers and suppliers of equipment, and even by prices and the cost of postage.

For example, the successive editions of the *Outline* retain his description of the fabrication and use of artificial palates, though with modifications to suit the passage of time. In the first and second editions there are instructions for making one's own palates, and the address in Paris of a Monsieur Montalbetti,[4] who will make them to order (those made of gum-stiffened paper cost only 5 francs, metal or vulcanite are more expensive). By the third edition (1932), M. Montalbetti has disappeared, but a footnote still informs us that 'They can be made for about 10 shillings'. It seems very likely, then, that demonstrations with an artificial palate must have formed part of his 'Experimental' lectures.

Late editions of the *Outline* also retained details of an instrument which had been mentioned form the beginning, a tube with an adjustable piston to demonstrate 'the effect of a resonance chamber in modifying quality of tone'. Even in the final (ninth) edition (1960) we are told that the makers are Messers Spindler and Hoyer of Göttingen.[5] Earlier editions even give the price (10 marks in 1922, 25 marks in 1932). DJ would hardly name the makers or specify the price unless he had handled a specific instrument, and probably purchased it. Besides, Paget tells us (1930: 17) that DJ owned just such a device, and describes his own experiments with it; they must have been carried out in the 1920s.

Atkinson's Mouth Measurer appears in the first and second editions of the *Outline*, again complete with the maker's postal address and the price. It was announced for sale in *Le Maitre Phonétique* in 1910, though it had been developed almost 15 years earlier. It even seems possible that DJ provided the stimulus for

Atkinson to put the device into production. Interestingly, DJ was in contact with both Henry Sweet and Bernard MacDonald around 1909–1910, and both of them, like DJ, provide testimonials to the usefulness of the device which appear on the printed instruction sheet.

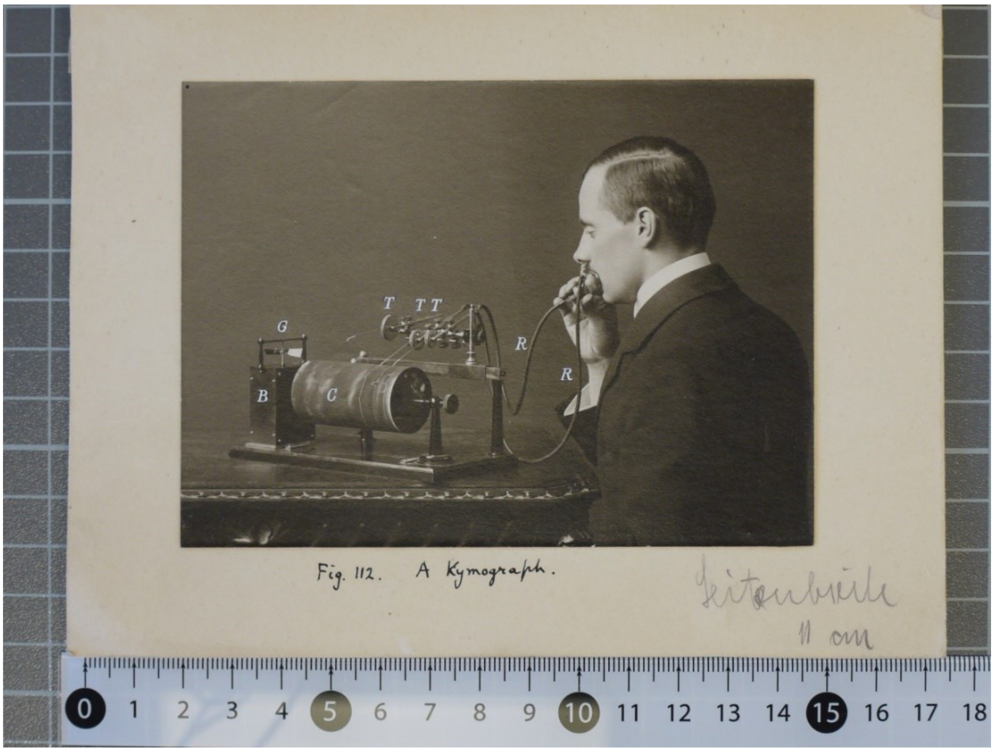


Figure 5.13 Photograph (used to prepare one of the figures for the first edition of the *Outline*), showing DJ with a clockwork kymograph. He is using a mouthpiece and a nasal olive, connected by rubber tubes to tambours of Marey pattern. The kymograph has a third tambour but no connection has been made to it. The smoking of the kymograph drum is very uneven. Notice the pencilled instruction to the printer 'Seitenbreite (page width) 11 cm'.

Early photographs[6] show Jones with a small clockwork kymograph (Figure 5.13). As is seen in Figures 5.14 and 5.15, in numerous details it closely matches the 'Petit enregistreur' sold by the French maker Charles Verdin (Verdin 1890)[7] This surely implies that DJ had access to, and possibly owned, such an instrument before the large UCL model was constructed.

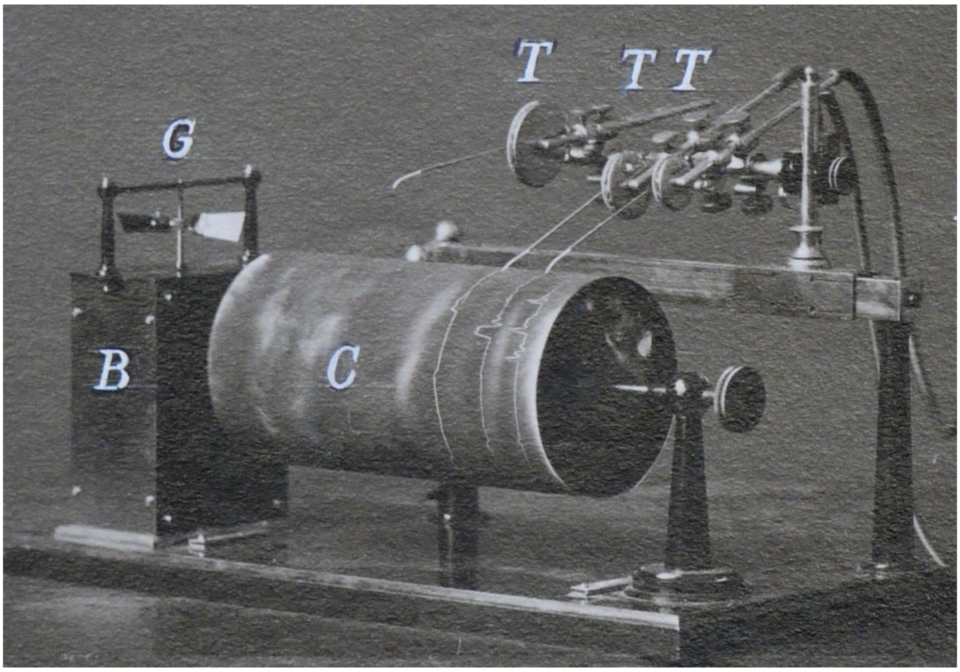


Figure 5.14 Enlarged view of the kymograph seen in Figure 5.13

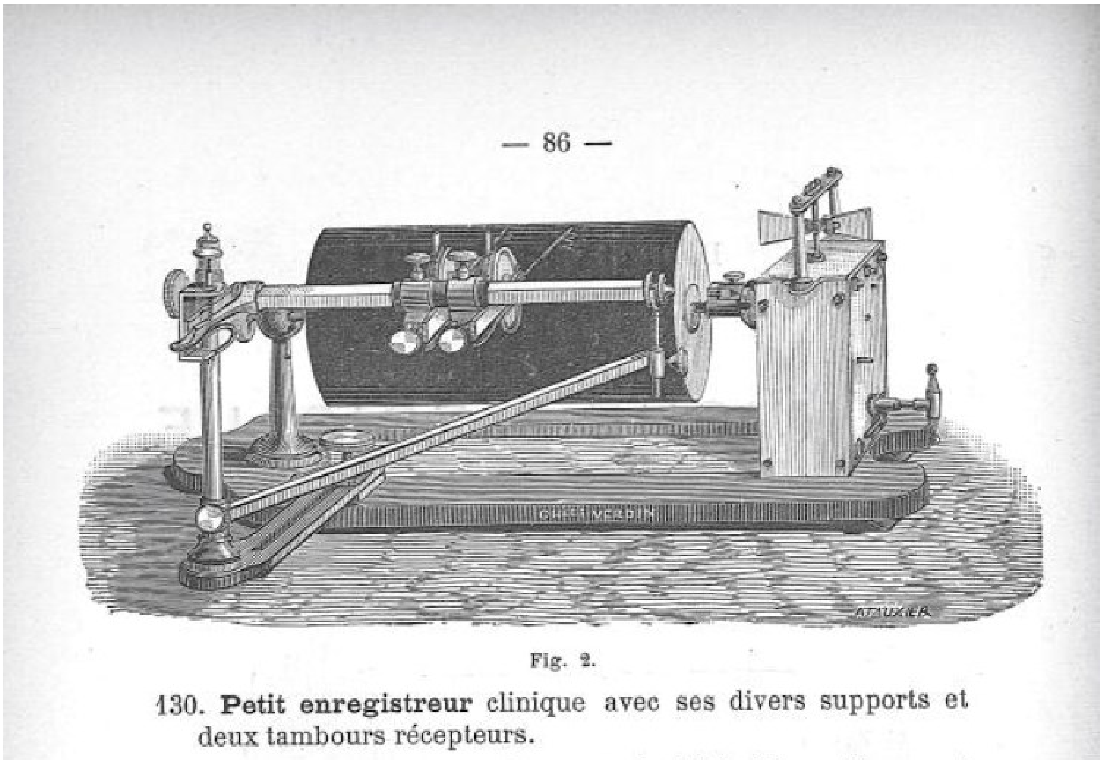


Figure 5.15 The ‘petit enregistreur’ made and sold by Charles Verdin. There are numerous points of similarity with the instrument used by DJ. The clockwork motor and wind-vane governor are identical. Notice also the rectangular (not round) section of the bar on which the tambours are mounted.

With a kymograph at his disposal, lantern slides prepared from original kymograms, or—since it was small and portable—demonstrations of the device itself, could therefore easily have formed part of DJ’s lectures.

In addition, his first work on the analysis of intonation (Jones 1907) indicates that already by that date DJ must have had access to a good-quality gramophone. His account of Rosset’s Grenoble laboratory (Jones 1909b) includes remarks on the superiority of disc recording over the cylinders used by Rosset, implying that DJ had evaluated the two sound recording methods critically.

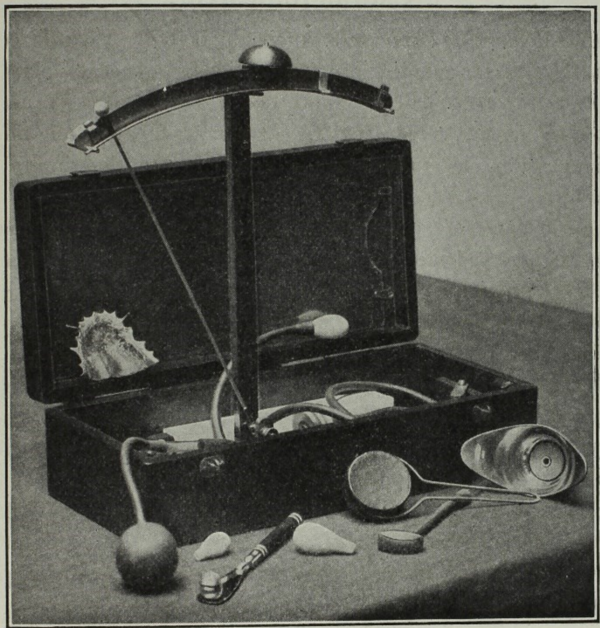
DJ wrote in paragraph §87 of the *Outline*$_1$, ‘The apparatus used in elementary instrumental phonetics includes the artificial palate, the kymograph, the laryngoscope, the mouth measurer, the gramophone and other talking machines, and a number of less important instruments.’ As has been shown it seems likely that he would have been in a position to demonstrate most, if not all, of these in his classes between 1909 and 1911. Palatography and kymography in particular were the cornerstones of contemporary experimental method; in the preface he contributed to Marichelle (1897), Marey accurately characterised the essence of Rousselot’s contribution as the bringing together of kymography applied to speech (pioneered by Rosapelly) and palatography (pioneered by Coles).

The ‘less important’ instruments mentioned by DJ may have included his resonance tube, and demonstration items from the Zünd-Burguet kit. (See Figure 5.16).

Nécessaire de Phonétique Expérimentale.

Par

Ad. Zünd-Burguet.



Diplôme d'Honneur, Exposition Universelle de Liège, 1905. Tous droits réservés.

Le «Nécessaire de Phonétique expérimentale et pratique» contient un Cadran indicateur pliant à soufflet, à timbre mobile et à curseur (Modèle de Zünd-Burguet), un Signal du larynx à suspension élastique (Modèle de Z.-B), un Signal du larynx simplifié (Modèle de Z.-B.), une Embouchure en aluminium avec bouchon en caoutchouc et tube de verre (Modèle de Z.-B.), deux Ampoules en caoutchouc, une ronde et une plate, deux Olives nasales et environ un mètre de tube de caoutchouc.

Figure 5.16 The cover page of the instruction leaflet which accompanied Zünd-Burguet's experimental phonetics 'outfit' or 'kit' (Fr. *nécessaire*). An artificial palate is visible in the open lid of the box, though no palatography materials were supplied with the kit and palatography is not mentioned in the leaflet. No other copy of this item has been located, and this example in the *Bound offprints* may be unique.

As for the lantern slides, there are relatively few indications of what they might have included, but we need not accept the assumption made by Collins & Mees that they were 'obviously an attempt to make up for the lack of a proper phonetics laboratory' (1999: 82). Undoubtedly some would have illustrated the work of others, but this is natural if the intention is to provide coverage of recently published research. Jones's reading was extensive, and his work for *Le Maitre Phonétique* kept him in contact with newly published books and journals. In the Bound Offprints *Experimental Phonetics Various Articles A* are two copies of Meyer (1907), and in one of these DJ has modified a figure, also adding pencilled notes to the photographer on how to prepare two slides from the page (see Figure 5.17). The resulting slides, showing sectional diagrams of vowels and certain consonants from X-rays, can hardly have been for any other purpose than the 'Experimental' course. Among a small number of lantern slides actually surviving at UCL, some from *Intonation Curves* could have been made as early as 1909 and might well have found a place in these lectures.

All in all, there is no reason to suppose that DJ's 'Experimental phonetics' lectures were derivative and inferior. On the contrary, the lectures may well have combined practical demonstrations of basic concepts with accounts of the latest research findings, including his own—surely an excellent mixture.

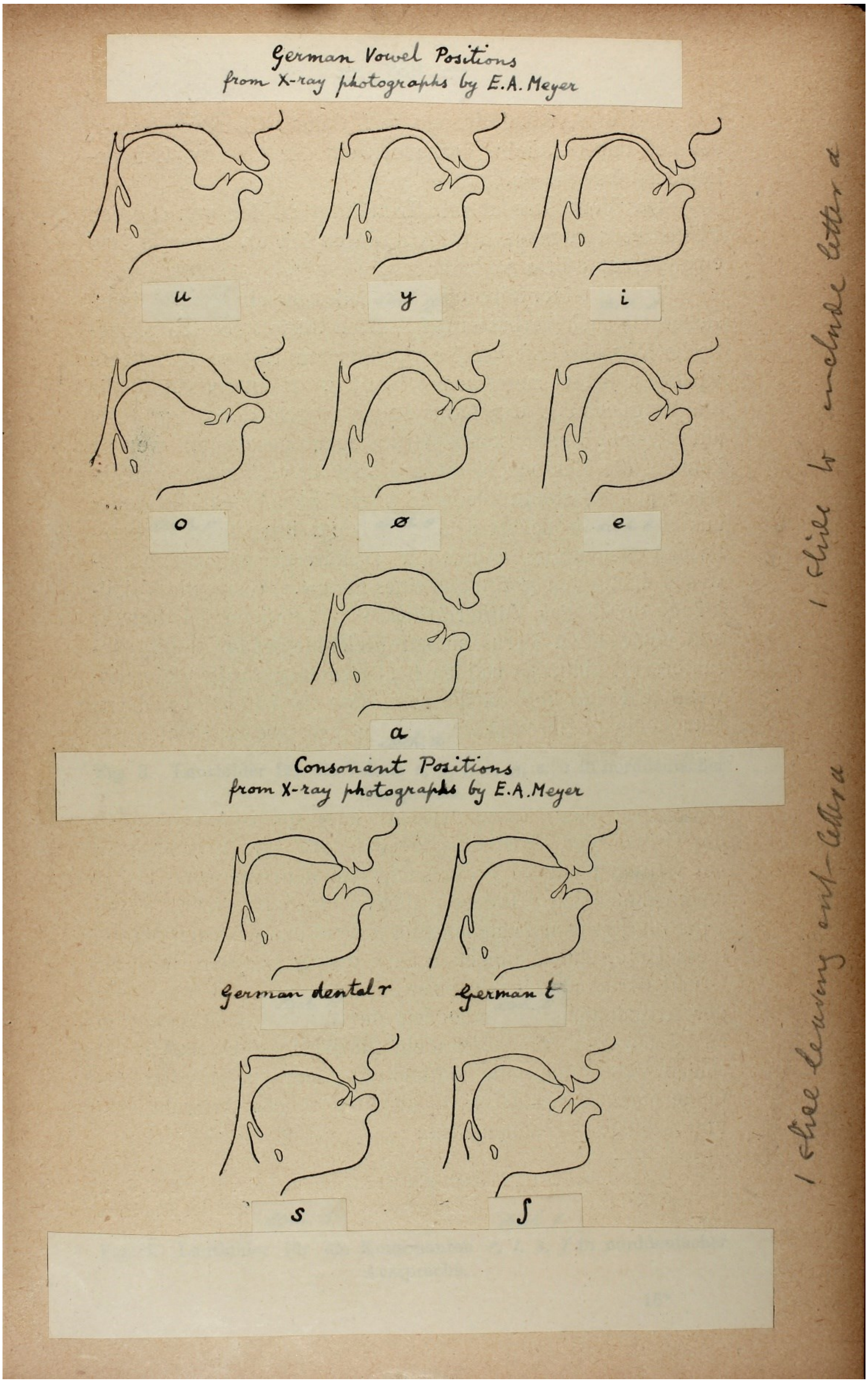


Figure 5.17 A page from one of DJ's two offprint copies of Meyer (1907). DJ has re-labelled the diagrams, and the pencilled instructions at right specify how the photographer is to split the page, producing two lantern slides.

5.4 Jones and the Royal Institution

In 1917, Daniel Jones delivered some talks at the Royal Institution (RI), and he mentions them in his Departmental Report for 1916–17. The major one was a Friday Discourse (Weekly Evening Meeting) delivered by DJ in the celebrated lecture theatre at the Royal Institution in Albemarle Street, on Friday February 9th, 1917, with Lord Rayleigh in the Chair. It resulted in the 1917c[8] publication 'Experimental Phonetics and Its Utility to the Linguist' in *Proceedings of the Royal Institution of Great Britain* XXII: 8–21.

Collins and Mees make a valid point when they say that these talks at the Royal Institution were likely to increase DJ's prestige among scientific colleagues. The Weekly Meeting is described in this way in the RI's List of Members, 1912: '…a lecture of one hour is delivered by some recognised authority, either upon some novel discovery, some recent development of scientific speculation, or some fresh aspect of a social, literary, artistic or scientific problem.' Addressing the Weekly Meeting was therefore prestigious in itself, and implied that the speaker was a 'recognised authority'.

At that period, the papers printed in the *Proceedings* (they are called 'Abstracts') vary greatly in length. Some are extended scientific reports with many plates, and must contain far more material than could have been covered in an hour-long lecture to a non-specialist audience; the authors of these evidently treated the *Proceedings* effectively as a scientific journal. For other papers, there is no abstract at all, merely a title, and yet others are in note-form—presumably, some speakers extemporised their talks and did not get around to submitting full written versions for publication. When compared with others in the 1917 *Proceedings*, DJ's paper is relatively long and has a large number of figures, so he appears to have treated the publication as a serious opportunity.

At the beginning of this publication, DJ styles himself 'M.A. M.R.I.' (Member of the Royal Institution). Collins and Mees represent DJ at this point in his career as eager to further his promotion. It might therefore be expected that Jones's membership of the RI would turn out to have been for a short period, to facilitate presentation of this one paper.

In the early twentieth century, the conditions of membership of the RI[9] were that each candidate had to secure the support of four existing members; a vote by ballot took place, and the new members were confirmed at the next General Monthly Meeting. The costs were 10 guineas on election, and thereafter 5 guineas a year or 60 guineas 'in lieu of all payments'—i.e. for life membership. The RI published annual Lists of Members, giving the name of each member, their degrees and honours, one or more contact addresses, and the year of joining. From these we can see that DJ's membership was not a brief one around the time of his paper. On the contrary, he joined in early 1909, a little more than a year after he had started teaching at UCL. Furthermore, he is shown as having 'compounded' for annual payments; this presumably means he had paid the 60 guineas required to acquire life membership. Indeed, DJ continued to be listed as a member to the end of his life; by the 1960s his details include his last home address and the honorary degrees he received from Zurich and Edinburgh. This shows that he evidently went to the trouble of keeping his entry in the membership list up-to-date over more than half a century.

DJ was elected a Member of the Royal Institution at the General Monthly Meeting held on Monday March 1st, 1909.[10] In the present context, the most notable of his four proposers[11] was Sir William Henry Preece KCB FRS MInstCE, Officer of the Legion of Honour. Preece (1834–1913) was a Welsh engineer and inventor, a student of Faraday, with interests in telephony and radio communication (see Baker 1976). He was a consulting engineer to the Post Office and became its Chief Engineer in 1892. In collaboration with Augustus Stroh (1828–1914), Preece had worked on vowel synthesis, producing a phonograph-like device which turned artificially-created waveforms into sound, which he described in a paper to the Royal Society (Preece and Stroh 1878). It cannot be an accident that DJ's Bound Offprints *Experimental Phonetics. A* contains an autographed copy of this paper.[12]

The foregoing establishes that DJ was a life member of the RI from 1909, and that his membership was thus not an opportunistic one acquired briefly with the aim of self-promotion. Overall, it seems that DJ took very seriously the opportunity to join the Royal Institution, and to present himself to the scientific world as a 'recognised authority' on speech. Preece was uniquely appropriate as one of DJ's proposers, as he has a claim to be considered the first British speech engineer. The Discourse which DJ delivered and the related published papers from 1917 and 1918, are of very high quality: clearly explained and organised, and superbly illustrated with original figures (though the papers have been for a long time neglected parts of DJ's output). The presentation, with live demonstrations (presumably assisted by Stephen Jones), had the potential to be both informative and accessible. DJ was especially fortunate in that the Chair was taken by Lord Rayleigh. It is impossible to think of any senior scientific figure of the day who would have formed a more fitting patron for experimental phonetics in Britain.

Finally, it has not previously been noted that the 1917 presentation may have served as a stimulus to DJ in the development of his theories. According to DJ, the X-ray photographs of Cardinal Vowel tongue positions were taken in January, 1917. Within weeks, they were presented (along with X-rays of some other sounds) in his RI Discourse on February 9th. It is hard to escape the inference that the X-rays were made with some urgency as the date of the prestigious RI presentation approached. In fact DJ remarks that the photographs were 'taken specially for this occasion' (1917c: 4).

5.5 The Cardinal Vowel system[13]

5.5.1 The X-rays

The development of the Cardinal Vowels is extensively documented by Collins & Mees.(especially Section 7.3, pages 173ff). The system was put into its complete form in 1917: the year saw not only Jones's RI Discourse and publication of the X-rays, but the first appearance in print of the Cardinal Vowel diagram (in DJ's *English Pronouncing Dictionary*) and the first recordings of the Cardinal Vowels. There are reasons to believe that DJ's recordings of the vowels were made on 16th and 17th January 1917. Their proximity in time to the printed publication of the system and to the recordings suggests that the 1917 X-rays, far from being in any sense the basis or starting-point of the system, must have been relatively late additions to a system already well-developed.

It is, of course, possible that the 1917 X-rays, while they are the only ones known to us, were not the only ones DJ made. They could in principle have been preceded by any number of earlier attempts which served in the development of the system but of which no trace remains.

By the standards of the day, DJ's X-rays are good (they show more gradation and detail than the few images published by Meyer (1907) or Barth (1907), for example), and must have impressed any of those in a position to judge who saw them projected at the RI. The high quality also perhaps hints at earlier attempts in which Trevelyan George, DJ and SJ might have refined their technique. DJ says that Trevelyan George 'has displayed much ingenuity and patience in getting over the numerous difficulties which present themselves in the course of this work' (1917 4/11); this sounds as if it implies rather more than one X-ray session.

Though DJ published X-ray images of three Cardinal Vowels, (numbers 1, 5, and 8) in 1917, the story of their subsequent republication (and the eventual addition of Cardinal Vowel number 4) is complex and puzzling. It is probably unsuspected, too, by any modern reader who has worked only with post-World War 2 editions of DJ's works, such as the ninth edition of the *Outline*. That final edition has the well-known frontispiece showing Cardinal Vowels 1, 4, 5, and 8, and a caption which reads 'The photographs were taken by Dr. H Trevelyan George in St Bartholemew's Hospital, London, in January, 1917'. Anyone turning to the publication history sees 'First published, 1918'[14], and it is a natural supposition that the 1917 X-rays must have been a feature of the book since its beginning. In fact, however, they are absent from the first eight editions[15] of the work, over the whole period 1918–1956.

After including them in his papers of 1917 and 1918, Jones did not republish the Cardinal Vowel X-rays until they appeared in the form of the familiar frontispiece, initially in his smaller work *The Pronunciation of English*, 3rd edition (1950) and 4th edition (1956), and then finally in the 'reprint' of the eighth edition of the *Outline* in 1957, some 40 years after they had been made. In the intervening years, the only other appearance seems to be in Noël-Armfield (1931), which reprints the photographs of Cardinal Vowel 1, 5, and 8; but the X-ray of Cardinal vowel number 4, [a], did not appear anywhere until 1950. There is evidence among DJ's notes, however, that the frontispiece was actually planned much earlier, sometime between 1922 and 1932.

Did DJ effectively conceal the Cardinal Vowel X-rays throughout the heyday of his career, the period when the Cardinal Vowel system was most widely used and taught? Why wait so long that by the time they reappeared they had the character of museum exhibits, long overtaken by modern X-ray techniques—and those colleagues who had witnessed or assisted the development of the system (SJ, Trevelyan George, Noël-Armfield) were all dead? And was there something in particular about the X-ray of Cardinal [a] that had made him unwilling to reveal it for more than 30 years?

One possible explanation for the reluctance to republish would be that DJ felt that the X-rays did not really support his interpretation of them. Jones's own claims were modest; he writes in the *Outline*$_3$, §134: 'The drawings of the tongue-positions of Cardinal Vowel 1, 4, 5 and 8 are *adapted from* X-ray photographs', and the accompanying figures (13 & 14) are labelled '*Approximate* Tongue-positions of the …cardinal vowels'. [My italics]. This is a dilution of the claim in Trofimov & Jones (1925: §120): 'The tongue-positions of cardinal vowels 1, 4, 5 and 8…are *drawn from* X-ray photographs'. Stronger claims came from others; e.g., 'The tongue positions of the cardinal vowels are *established* by examining X ray photographs' (Lloyd James 1925). [My italics].

DJ's interpretations are all in terms of 'the highest point of the tongue' and it is first necessary to say something about that rather strange quantity. As far as can be determined, by 'highest point of the tongue', DJ did not mean 'the place of maximum constriction in the vocal tract', nor even 'the point of closest approach of the tongue to the palate', but quite literally the highest point on the tongue, reckoned without any reference to the palate—though presumably with an implicit assumption that the speaker is in an upright and level position. Clearly, this quantity can have at best an epiphenomenal relationship to the vocal tract adjustments which determine vowel quality. DJ is very far from developing an acoustic theory of vowel production, and certainly does not claim to be doing so. But his characterisation of tongue positions in vowels was hobbled from the outset by an exclusive focus upon the readily observable body of the tongue, to the neglect of the tongue root and the pharynx. No doubt this is to be attributed in part to limitations in what he could see and feel himself, in part to the assumptions he had inherited from his predecessors.

It is plain from offprints in DJ's possession that he had done a great deal of relevant background reading at the time he was elaborating the Cardinal Vowel system. The technique of rendering the tongue outline more visible in X-rays with a ladder of small lead plates threaded together comes from Meyer (1907). But in the paragraph of the *Outline* dealing with sources of information on the articulation of vowels (it remains as § 129 in the ninth edition) DJ does not give the principal place to Meyer's (rather sparse) X-ray results, but instead to extensive tongue-profile data which Meyer obtained in an entirely different fashion, using a cumbersome artificial palate carrying a row of deformable thread-like projections—a technique which Meyer termed 'plastographic'. That data is reported in a substantial study of more than 80 pages (Meyer 1911) which seems to have created a considerable stir among phoneticians—Passy (1911) reviews it at some length and is driven to various *ad hoc* measures to rescue his own vowel framework in the face of what he sees as unassailably strong counterevidence from Meyer. DJ had at least two copies, one of which bears Meyer's autographed greeting. DJ's unquestioning high regard for the scientific accuracy of Meyer (1911) may follow Passy. In fact Meyer's cumbersome 'plastographic' technique is open to numerous objections, and it now appears strange that these were not raised at the time. The crucial consideration in the present context, however, is that it utilised an artificial palate and thus even at best could provide data only from the tongue front. Hence the most important data on which DJ based his ideas about vowel articulation was intrinsically flawed.

Revealingly, DJ writes in the first edition of the *Outline*, §71,

> The shape of the air passage above the larynx is governed, and hence vowel quality is governed, chiefly by the position of the main part of the tongue (though also to a large extent by the position of the lips, § 88). It is therefore convenient to classify vowels *according to the position of the main part of the tongue*.'
>
> [Original italics].

In the third and subsequent editions, 'the main part of' is dropped, and the phrase becomes simply 'the position of the tongue'; but the successive versions of the figures showing 'approximate tongue-positions' of the Cardinal Vowels continue to indicate variations only in the position of the body of the tongue, with the root (and hence the pharynx opening) represented by a constant standardised position.[16]

With those reservations in mind, it is possible to enquire whether the X-ray images support the limited interpretation DJ wishes to make. Using his own reference mark (the X at the hard palate) as (0,0), the x and y coordinates of the dot marking the highest point of the tongue in Cardinal Vowel 1, 4, 5 and 8 were determined from a high-definition image of the 'original' Cardinal Vowel X-rays, using the on-screen cross-hair measurement tool already described. The results are plotted in Figure 5.18, overlaid with DJ's 'more accurate' form of vowel diagram (*Outline*$_3$, §148), which is reproduced at a scale chosen to maximise the correspondence.

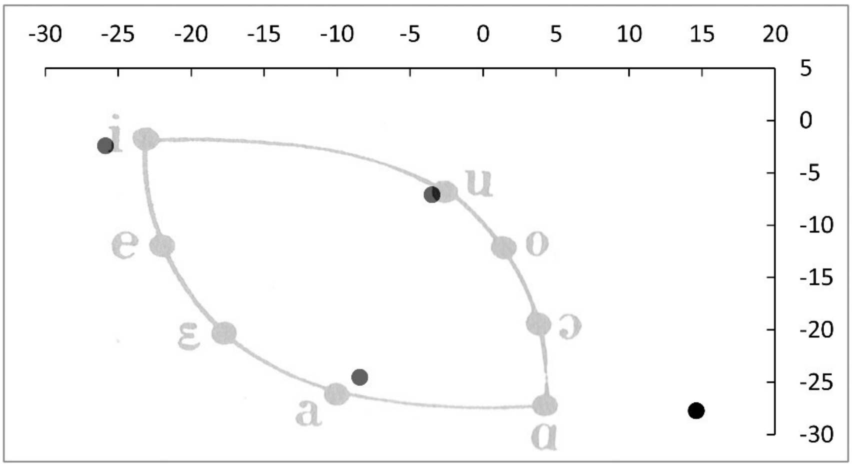


Figure 5.18 Measurements of the 1917 Cardinal Vowel X-rays. The black points show measured coordinates of DJ's mark showing the 'highest point of the tongue' reckoned in millimetres relative to his reference mark (which is placed at the junction of the hard and soft palates). The exact reproduction scale is unknown, but is probably in the region of 1.1 times life size. In grey is DJ's 'accurate' form of vowel diagram, before simplification into a quadrilateral.

From this it is apparent that DJ places Cardinal Vowel no.5, [ɑ], much too far forward, and must have deliberately chosen to do so. As early as the Preface to the second edition of the *Outline*, he was forced to admit 'The direction of the line of back vowels in the charts…and in Fig. 11 is incorrect. The **u**-sounds are more *forward* than the **ɔ**-sounds; consequently the right hand line should slant from left top to right bottom' (1922: ii). DJ evidently struggled to reconcile himself with the evidence which showed that the tongue was much further back for [ɑ] than he had initially believed (he had started with a vowel triangle, and progressed to a 'symmetrical' trapezium, both of which placed [ɑ] further forward than [u]).

The very approximate correspondence of the X-ray evidence with DJ's plotted positions of the Cardinal Vowels may perhaps explain his reluctance to republish the images over many years, but it does not explain why the photograph of Cardinal Vowel number 4 should have been withheld from the beginning. The match between DJ's diagram and the X-ray location for [a] is actually rather good, as Figure 5.18 illustrates; the major difficulty concerns the position of [ɑ], and the availability of the [a] image would appear to have no bearing on that. There must, therefore, have been another reason for the long delay in its publication. Perhaps it was not the tongue position but some other aspect of the [a] image which DJ considered unsatisfactory.

Examination of the four X-rays suggests that the problem lies in the velopharyngeal area. Close scrutiny shows that around 1 cm more has been masked off the right-hand edge of [a] compared with the other images. It is unclear whether any part of the remaining image for [a] corresponds to the velum—but if the shadow at top right does indicate the velum, it appears to be in a pendant (lowered) position. The purpose of cropping the image may be to conceal this—or perhaps to conceal just how closely the tongue root is approaching the rear pharynx wall. The visual indication of apparent nasality, would conflict with DJ's characterisation of the vowel as 'oral' (not nasalised), and a narrow constriction in the pharynx would conflict with DJ's characterisation of the vowel as 'open'.

To make comparison of the four images more precise, superimposed tracings were made of the velum as visible in the [i], [u] and [ɑ] images, and the resulting composite tracing placed over the [a] image.

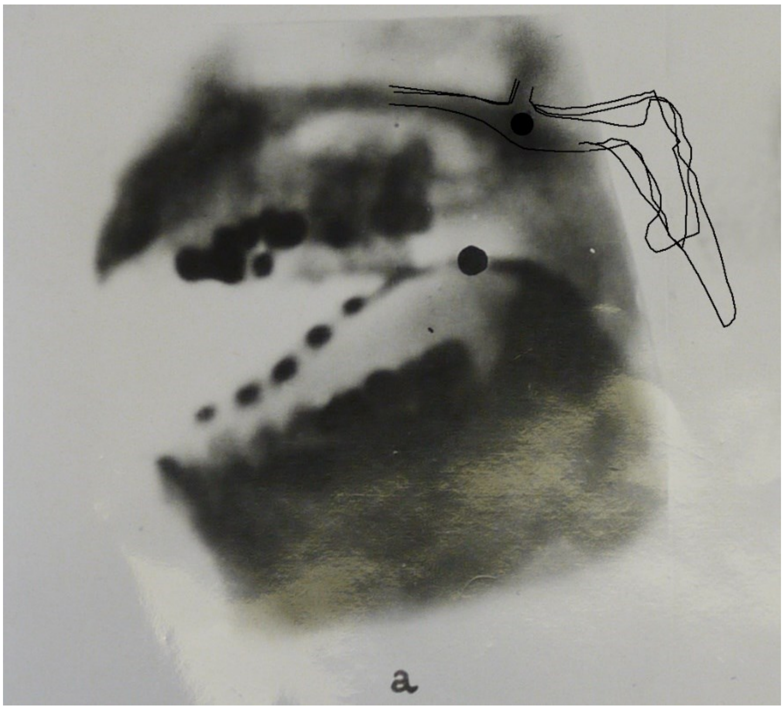


Figure 5.19 DJ's X-ray image of Cardinal Vowel 4, [a], overlaid with outline tracings of the velum position from the vowels [i ɑ u]

Tracing and superimposition of images was accomplished by using a stack of transparent 'layers' in a PhotoShop document. The result is shown in Figure 5.19; it is evident that an extensive portion of the pharynx must have been removed from [a]—enough to conceal a considerable degree of velum lowering, if that were the motivation.

5.5.2 The Cardinal Vowel recordings

DJ's recordings of the Cardinal Vowels must also be considered among his contributions to phonetic science. They certainly constitute the most notable attempt ever made to render the classification and symbolisation of vowels repeatable across observers. DJ explicitly thought of them as 'scientific':

> It has therefore been found necessary to choose a standard set of vowels for reference—a scale of vowels which can be used in the same sort of way as a

> scale of degrees which enables us to specify temperatures. The vowels sounds of such a scale are called CARDINAL VOWELS. They are chosen on a scientific basis and are independent of the vowels of any language.
>
> (Jones 1943: 4)

That account is in the text published to accompany his second recording of the vowels, in 1943, but no doubt represents his thinking from the beginning.

DJ recorded the vowels on three occasions—in 1917, 1943 and 1956. The 1917 and 1943 recordings are alike to the extent that each occupies just two sides of a single record, and includes only the eight primary vowels, but they naturally differ in the recording technology used (the 1917 recording for The Gramophone Company (HMV) was an acoustic recording, the 1943 Linguaphone recording is well into the electrical era). The 1917 recordings were published without any accompanying text. The 1956 version, also for Linguaphone, added a second (optional) disc containing the secondary Cardinal Vowels and additional contrastive listening material.

The wide separation in time of the 1917 and 1943 recordings requires explanation. The 1917 version is an excellent example of good acoustic recording, and DJ's performance is assured, fluent and precise—but, as indicated below, the acoustic technology of the day severely impairs the fidelity of the recording. The introduction of electrical recording in the mid-1920s completely transformed the reproduction of speech. A high quality electrical recording of the Cardinal Vowels would have been possible as early as 1926, yet 17 years were to elapse before DJ produced one. Perhaps he felt that since the Cardinal Vowels were a standard scale they should not be replaced. Whatever the reason, he certainly retained an attachment to the 1917 recordings, even after the 1956 version was available: a footnote to §141 of the ninth edition of the *Outline* mentions the 1956 Linguaphone record, but adds:

> There exists also a record of these vowels made in 1917 by the H.M.V. Gramophone Co., 363 Oxford Street, London W.1, and numbered B 804. Although this record was made before the invention of electric recording, the reproduction is very good. It is no longer on sale through the ordinary channels, but the H.M.V. Gramophone Co. have preserved the matrix, and are willing to print off copies specially for anyone who orders a sufficient number.

It seems very unlikely that anyone can ever have taken up that far-fetched offer. But what is remarkable in the present context is DJ's claim that 'the reproduction is very good'. In fact, as the succeeding sections will show, quite the opposite is true: the reproduction is very bad—and the specific defects in reproduction appear to obliterate some of the most characteristic acoustic aspects of vowel quality as currently understood.

A digitised copy of the 1917 recordings was obtained from the British Library, and analysed with the *formanal* program within SFS (Huckvale 2013). *Formanal* is a formant tracker based on LPC polynomial roots and dynamic programming. For comparison, the same procedure was followed with corresponding sections of the 1956 recordings (which are readily available).

The absence of high frequency energy is the best known characteristic of acoustic recordings, but far more serious problems in the reproduction of vowel spectra are likely to arise from the curtailment of the low frequency response and the presence of unquantified resonances in the low and mid-frequency regions. Examination of narrowband spectrograms from the 1917 recordings indicates that while they have a respectable frequency range for their period (and periodic energy extends in places as high as 4 kHz), there is essentially no energy below 250 Hz, with the consequence that the second harmonic of DJ's voice is the lowest component present. The shape of the low-frequency roll-off is unknown, but it is evidently quite gradual, since generally the second harmonic itself is weaker than the third.

Plainly, radical attenuation of the lowest frequency components of the voice will have a major impact on the frequencies of the poles required to model the spectrum—that is, the ‘formants’ will no longer be at the expected frequencies. The effect will be worst for F_1, and will particularly affect high vowels where F_1 is low in frequency and thus nearest to the harmonics with disturbed amplitudes. Figure 5.20 illustrates this for Cardinal Vowel number 1, the close front vowel [i].

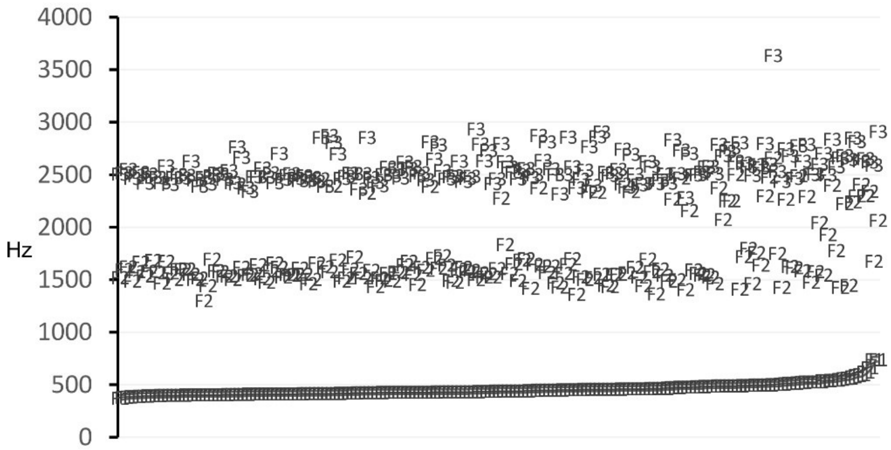


Figure 5.20 The frequencies of F_1, F_2 and F_3 of Cardinal [i] in 181 estimates obtained from the 1917 recording (sorted from left to right in ascending magnitude of F_1 estimate).

The figure shows a large number of estimates for F_1, F_2 and F_3 of [i]. As predicted, the F_1 estimates are much higher than would be expected for a close vowel: values of 300 Hz or less would be typical, while these estimates are nearer 500 Hz. The ‘F_2’ around 1500 Hz is completely unexpected—the ‘real’ F_2 of a vowel of this type is likely to be in the 2200–2500 Hz range (and thus probably corresponds to the ‘F_3’ of Figure 5.20). Perhaps the spurious formant in the 1500 Hz range reflects a resonance in the recording equipment. Overall, the three resonances in Figure 5.20 are actually near to those expected for a central vowel of type [ə]. Yet it was a high front vowel of type [i] which DJ recorded (and which is plainly heard when the recording is played). It is important to realize that the formant estimates of Figure 5.20 are not ‘errors’ in the ordinary sense of the word. They really are the frequencies of the poles which characterize the signal. They are only ‘errors’ in the sense that they do not match the expected formant frequencies for a vowel of this type.

Mean estimates of F_1 and F_2 for all eight of the 1917 Cardinal Vowels are plotted in Figure 5.21. For comparison, values from the (relatively high fidelity) 1956 recording are shown in Figure 5.21,[17] and the two sets are plotted together in Figure 5.23. It is evident that the two sets of estimates only agree well for Cardinal Vowel 4. In every other case, F_1 of the 1917 vowels is considerably too high. The agreement in F_2 is better, but for the high vowels [i] and [u] an unexpected formant around 1500 Hz has been identified, causing those vowels to be allocated bizarre locations in the F_1/F_2 space.

It is a sobering thought that for a generation the Cardinal Vowels were successfully taught and learned from recordings in which the pattern of formant frequencies is so badly distorted. We must infer that human listeners are able to apply a complex and sophisticated normalization process to recover the vowels from what is in objective terms a highly unsatisfactory signal. It also seems to follow that the shorthand specification of vowels in terms of two (or three) formant frequencies, which has become standard practice in acoustic phonetics and its applications (such as sociolinguistics), must fall far short of a true perceptual characterisation of vowel quality.

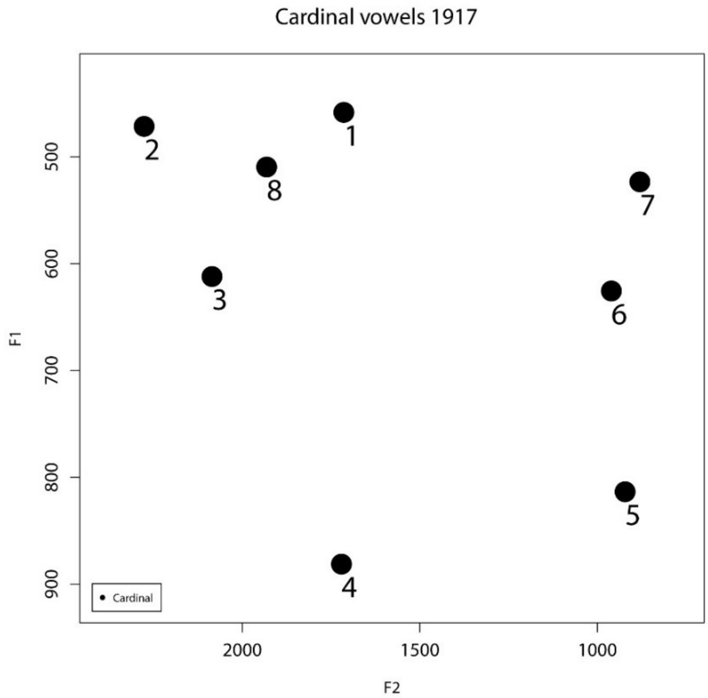


Figure 5.21 The Cardinal Vowels of 1917 plotted in F_1/F_2 space

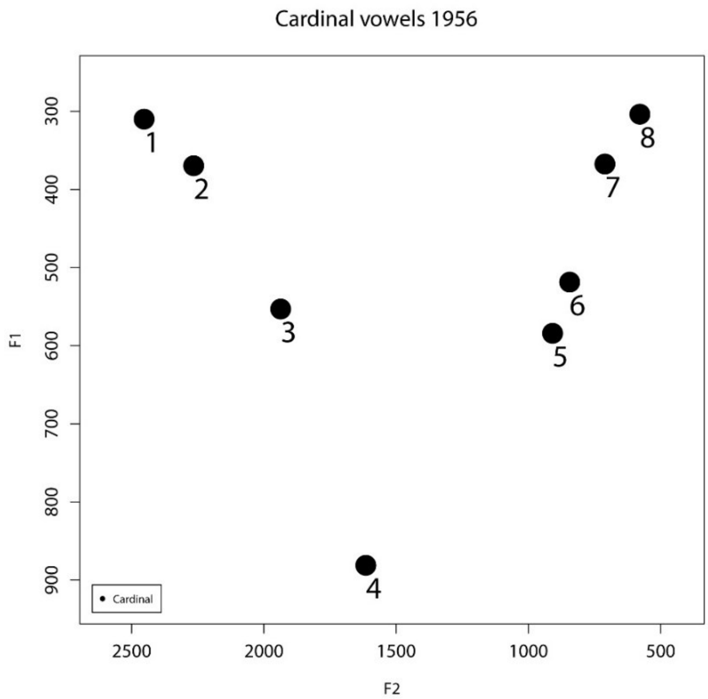


Figure 5.22 The Cardinal Vowels of 1956 plotted in F_1/F_2 space

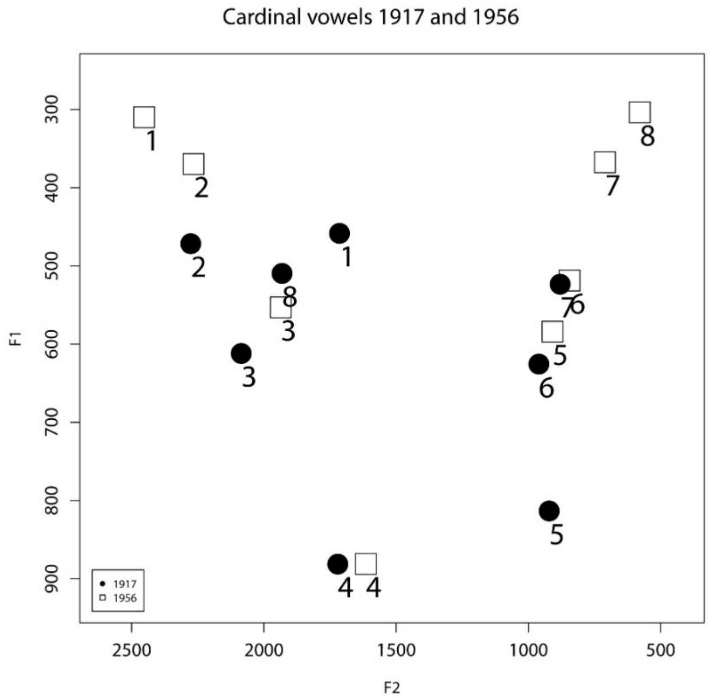
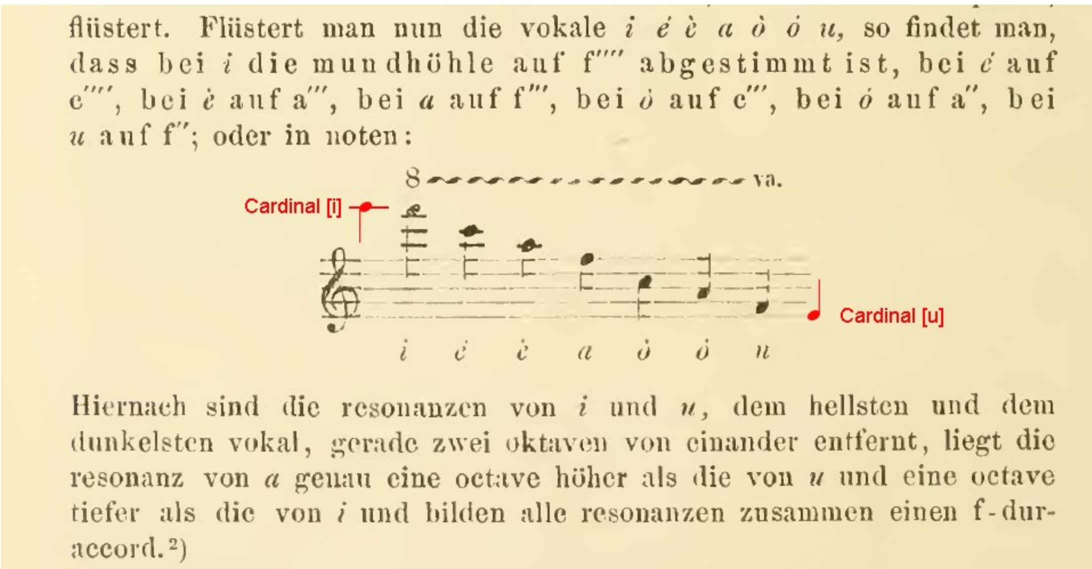


Figure 5.23 The Cardinal Vowels of 1917 and 1956 plotted together in F_1/F_2 space

5.5.3 Auditory estimates of vowel resonances

If we take into account the combination of DJ's auditory gifts, his musical skill, his familiarity with the earlier literature on vowels (particularly the writings of Lloyd), and his associations with both Perrett (see Chapter 7) and Paget (see Chapter 8), it would be extraordinary if DJ had never attempted the auditory estimation of vowel resonances—and the vowels of his own cardinal scale would be obvious targets for such analysis. He plainly *did* conduct such analyses, but the only results which have so far come to light are the few he published in connection with the description of Russian. They appear in §§ 268–285 of Trofimov & Jones (1923: 72–77), and in modified form in the successor work, Jones & Ward (1969: 34-35, 67–68). DJ speaks of a single 'vowel pitch', by which he explicitly means the 'whisper-pitch' (1923: 72–73). The specific use made of this in relation to Russian is to specify qualities among a range of high (close) vowels which differ by degree of advancement (frontness) and lip-rounding—a particularly apt use for what is effectively an estimate of F_2. In the course of the discussion, DJ gives his estimates of the whisper pitches of Cardinal [i] and Cardinal [u], and it is of interest to compare these with estimates of the whisper pitches of additional vowels made by others. Figure 5.24 reproduces (in black) the series given by Trautmann (1878: 590),[18] which is one of the more accurate sets of early estimations, and (in red) DJ's 1923 values for Cardinal [i] and [u]. It is striking that DJ's pitches are located just a little above and a little below the ends of Trautmann's scale, exactly as might be expected for extreme peripheral qualities.


flüstert. Flüstert man nun die vokale *i é è a ò ó u*, so findet man, dass bei *i* die mundhöhle auf f'''' abgestimmt ist, bei *é* auf c'''', bei *è* auf a''', bei *a* auf f''', bei *ò* auf c''', bei *ó* auf a'', bei *u* auf f''; oder in noten:

Hiernach sind die resonanzen von *i* und *u*, dem hellsten und dem dunkelsten vokal, gerade zwei oktaven von einander entfernt, liegt die resonanz von *a* genau eine octave höher als die von *u* und eine octave tiefer als die von *i* und bilden alle resonanzen zusammen einen f-dur-accord.[2])

Figure 5.24 The whisper pitch vowel scale of Trautmann (1878). Added in red are DJ's estimates of the whisper pitches of Cardinal [i] and Cardinal [u], first published in 1923 though probably determined around 1917.

It is also plain from DJ's accounts that when he spoke of a vowel being 'mid way' between two other vowels he did not mean this in some vague unspecified sense as has often been assumed. He specifically meant that the whisper-pitch of the intermediate vowel was to be musically half-way (as reckoned in semitones, for example) between the whisper pitches of the two reference vowels (Jones &Ward 1969: 35).

Why DJ did not do more with vowel pitches is puzzling. Perhaps he found that students were unable consciously to make the pitch judgments required, but were able to benefit from the Cardinal Vowel system without enquiring closely into the auditory scales on which it rests. From 1922, following the appearance of Paget's *Vowel resonances*, he may additionally have felt that his own observations on vowel pitches, like those of others, were comprehensively eclipsed by Paget's *tour de force*.

5.6 Conclusion

Throughout his career, DJ took the view that the fundamental purpose of phonetics was to assist in the teaching of languages:

> The art of speaking a foreign language demands (among other things) an ability to perform all kinds of difficult movements with the tongue and other parts of the speech-mechanism. Such ability may be acquired by the learner, if he is provided with precise instructions as to what he must do. It is the function of the phonetician to supply these instructions.
>
> Instructions as to how to pronounce must, in order to be efficacious, be based on accurate analysis of the pronunciation. Many of the facts of pronunciation can be ascertained by direct observation (by auditive, visual, tactile and muscular sensation) on the part of those who have a specially trained ear and a highly-developed control over their vocal organs. These methods are extremely important, and no satisfactory analysis of a language can be made without them. Other methods, however, may be used to supplement these, namely mechanical analysis by means of specially-designed apparatus. Analysis of this kind constitutes the branch of phonetics known as experimental phonetics.
>
> (Jones 1917c: 1)

DJ never deviated from this focus. The whole *raison d'être* of experimental phonetics was to support the practical needs of pronunciation teaching. The 'experimental phonetics' of his early days was restricted in the main to providing two kinds of possible assistance: either demonstrating aspects of speech with simple devices such as Zünd-Burguet's voice indicator (which presumably was used once, in an introductory lecture) or producing visual representations of articulatory activity (mouth mapping, palatography, kymography, and X-rays). There was essentially no acoustic analysis—even visualizing or plotting a reliable approximation to the speech waveform was out of reach. But by the mid-1920s, DJ had reached the conclusion that visual representations of articulations had very limited value in teaching: 'A most accurate diagram of the tongue-position obtained by X-ray photography will not of itself enable the student to pronounce the sound correctly' (Trofimov & Jones 1923: 25). Instead, auditory comparison with sounds already known to the learner was the method to follow. This may explain his lack of interest in re-publishing his Cardinal Vowel X-rays True, to the last edition the *Outline* retains more than 25 palatograms, with the visible imperfections and asymmetries of the speakers who made them. But all had been made many years previously, and had featured in the book since the beginning. In fact, DJ seems to have published no new palatograms after 1918; there are none in either of his treatments of Russian (1923; 1969), for example, even though the palatalized consonants of the language would appear to be ideal subjects for the technique. He went on using diagrams—his last publication (Jones and Ward 1969) has more than thirty—but he came to prefer perfect stylised figures which are hardly more than diagrammatic interpretations of articulatory classification labels. Probably he felt in relation to the representation of vowel and consonant articulation something analogous to what he expressly states about the representation of intonation patterns in the final paragraph of the 'Intonation' chapter of the *Outline* (§1088 from the third edition onwards):

> Accurate curves…have scientific value, but their use in practical language teaching is limited, since they only record what is objectively present. To get good results in practical teaching it is necessary to have regard continually to the intonations *aimed at*, i.e. the intonations which are *subjectively* present to the speaker. These often differ considerably from the objective intonations actually employed.

DJ had reached the conclusion that the way to help learners was to give them auditory targets, and these furthermore must be normalized across speakers and abstracted away from local contextual disturbances and performance limitations. Once that is formulated as the method to be followed, he was surely right that the experimental phonetics of his day had little to offer by way of assistance.

The arguments of this chapter run the risk of appearing contrarian. Against the prevailing view of DJ's attitude to experimental phonetics we have assembled a number of provisos and new pieces of evidence. But the conventional wisdom contains much truth. Plainly, DJ was not *primarily* a scientist; after a promising beginning, the UCL laboratory of the 1920s and 1930s was sidelined. Plainly too, DJ's attitude *did* change over time: he had taught a course on experimental phonetics in 1910, and delivered a Discourse at the RI in 1917, but already by the mid-1920s it appeared unlikely that he would do such things again. Once we assume that DJ has to be classified as *either* 'taxonomic' *or* 'scientific', then obviously he's 'taxonomic' and a very clear example of that category, too. If we have succeeded to any degree in justifying the superficially contradictory claim that in various respects DJ was also ‘scientific’, it is not because he or his work were greatly different from the accounts of them given by Collins & Mees (1999), but rather because the relation between ‘taxonomic’ and ‘scientific’ is more complex than has been supposed. Even if the two can be sharply distinguished (which is doubtful) they are not necessarily incompatible. They may be twain, but a meeting is not out of the question.

Notes to Chapter 5

1 'Tripos' /ˈtraɪpɒs/ is the Cambridge term for an examination in a certain subject, and hence by extension a syllabus or course of study in that subject.

2 There has been widespread inconsistency in the symbolic notation of harmonic and formant frequencies, so that fundamental frequency is to be found variously written f0, f_0, F0, F_0, etc. In the present work, an attempt has been made to follow the recommendations of Titze et al. (2015). Source frequencies are written with lower case italic *f* and formant frequencies with upper case *F*. The subscript for the fundamental is not zero, but the letter o (since, absurdly, so-called 'f-zero' is mathematically = f_1). So the frequency of the fundamental will be written f_o, the second harmonic f_2, the first and second formants F_1 and F_2 respectively, and so on.

3 Available at http://www.picpick.org/en/

4 Montalbetti was evidently an agent or supplier of items required for experimental phonetics.

5 Founded 1898. The successor company of precision instrument manufacturers is still in existence.

6 The superficially similar photograph reproduced by Collins and Mees (1999) as their Figure 36 is from a different negative. Notice that in the Collins/Mees image DJ is not using the nasal olive.

7 Manufacturers' catalogues, being 'grey literature' are often not dated. BIUM which makes available the digitised version of Verdin used here, estimates the date as 1890–1895.

8 Collins and Mees (1999: 520) give the title wrongly as 'The use of experimental phonetics and its utility to the linguist' and the year of publication wrongly as 1919. The date error may originate from the DJ bibliography in Abercrombie et al. (1964) which also gives 1919. But that was the year in which Volume XXII was *completed*; at this period, each *Proceedings* 'volume' covers three years, and 1917-1919 is one of these three-year groupings. Each year's *Proceedings* were separately issued, in paper covers, so the true date of publication is 1917. Binding into volumes was done variously and inconsistently by individual members and libraries. In their discussion of the paper, Collins and Mees also appear to mix up the Royal Institution and the Royal Society (1999: 185). There is no indication that DJ ever addressed the Royal Society.

In section 7.6 (pages 188–192), Collins and Mees have another mention of the RI paper, now dated to 1917, but said to have appeared that year in *Nature*. Here they are conflating the RI paper with a much shorter *Nature* paper of the same title. In fact DJ used almost exactly the same material as found in the RI paper to produce two papers in *Nature* (1917a; 1917d), and another in in *Modern Language Teaching* (1918). This last was again the result of a lecture (to the Modern Language Association on January 13, 1918b). Large sections of it match his RI Discourse verbatim, and the same figures are used. There is little doubt that by modern standards these four publications represent extensive self-plagiarism.

[9] The procedures were reconstructed by examination of documents held in the Royal Institution's Archive.

[10] *Proc. Royal Institution*, XIX: 496.

[11] The others were Francis Fox JP, MInstCE., Henry Daw Ellis, MA, and Charles Sweet, Esq. Fox was a notable civil engineer; like Preece, he lived in Wimbledon and thus was probably a friend of DJ's family. Henry Daw Ellis was the author of *Graphic Arithmetic* (1895) and *Poems Mathematical and Miscellaneous*, (1912). The poems are mainly slight and whimsical, but one of them is of very great interest, as it refers to DJ. It appears to indicate that DJ was able to play the serpent (an archaic bass wind instrument), and suggests that DJ may in his younger days have been part of a circle of musical companionship and humour. Charles Sweet (brother of phonetician and philologist Henry Sweet), was a lawyer, author of *A Dictionary of English Law* (1882).

[12] The paper had been written before DJ was born and the offprint must have been about 30 years old when the elderly Preece presented it to Jones. Adjacent to it in the same collection is an autographed copy of Preece (1880), dealing with Bell's 'Photophone', presumably presented at the same time. On a visit to the USA, Preece met Alexander Graham Bell in 1877 and encountered the telephone soon after its invention (Baker 1976: 162). Preece and Stroh (1878) shows that Preece had an excellent grasp of the general acoustic properties of various categories of speech sound, and an understanding of the limited speech intelligibility offered by both the early telephone and the phonograph.

[13] DJ is inconsistent in the use of capitals, writing variously 'cardinal vowel', 'Cardinal Vowel' and 'Cardinal vowel'. The capitalized version 'Cardinal Vowel' is used here as the established proper name of the system.

[14] The year 1918 is the 'official' date. Collins and Mees (1999: 203–204) make a convincing case that the true publication date was in fact 1919.

[15] The *Outline* exists in at least 14 distinct versions, spanning 1918–1976, and consisting of 9 'editions' and 5 so-called 'reprints' (which retain the edition number but nevertheless all incorporate at least some textual changes and additions). The major re-writings were for the 3rd and 8th editions, so that the succession of versions falls into three main groups: Editions 1 & 2 (1918–1932); Editions 3 to 7 (1932–1956); Editions 8 & 9 (1956–). A paperback edition did not appear until 1976, but a paperback version of *The Pronunciation of English* was common from as early as the mid-1960s. The paperback versions lack the photographic frontispiece. Hence many who acquired the *Outline* in hardback and *The Pronunciation of English* in the common paperback—such as the present author—must have formed the wrong impression that the X-ray photographs are a feature of the *Outline* but not of *The Pronunciation of English*. (Though the frontispiece is missing from the paper versions, the text references to it are uncorrected, and the list of illustrations includes it).

[16] The front and back series were represented on separate figures. Later versions of the figure for back vowels do indicate some slight variation in the position of the root, but the front vowels are always drawn with a standard pharynx configuration, of a sort appropriate for [i].

17 The 1956 formant values were determined in joint work with Dr Joanna Przedlacka. It may be noted in passing that that when F_1 and F_2 are estimated with the model which has been applied here, the projection of the Cardinal Vowel space on to the F_1/F_2 plane appears as a triangle—not a quadrilateral as has been widely stated and believed. Those determinations of formant frequency which lead to a quadrilateral arrangement were made without an explicit model of the spectrum and involve rough-and-ready approximations applied when F_1 and $F2$ are near together in back vowels (Ladefoged 1967: 81–86). It seems that these procedures tend to overestimate the first formant frequency. It is even possible that an expectation that the vowels *should* be disposed so as to form a quadrilateral may have biased the formant frequency estimates which were obtained.

18 Moritz Trautmann (1842–1920) published his estimates of vowel pitches first in the course of a review of four recently published modern-language textbooks in the first volume of the journal *Anglia* (Trautmann 1878). He concluded his review with strictures on the approach to pronunciation in all of the works, and used the opportunity to air his own proposals for 'eine bessere methode für den lautlichen teil des neusprachlichen Unterrichts' (a better method for the phonetic part of modern language teaching). No doubt his full treatment *Die Sprachlaute* (1884) was already in preparation. His estimates of vowel pitch were obtained by whispering. They were noticed by Ellis, who added them to the table of vowel resonances on page 109 of his translation of Helmholtz, alongside the estimates of Helmholtz, Donders, Merkel, Koenig and others, remarking that Trautmann 'very confidently gives results utterly different from all the above'. Though 'utterly different' (in many cases they are on the order of two octaves higher than previous estimates), they seem to be the first approximately correct estimates to be published. Even Perrett, who has a strong anti-German bias, conceded 'By some fluke or other, Trautmann certainly hit upon the right octave' (1916: 94). (Trautmann did not, however, manage to escape from a number of widely–held misconceptions of the day: he found only a single resonance, and he thought the resonance was constant across all speakers—male and female, for instance. He also believed that the vowel pitches were the notes of a musical scale or chord.)

6

The life and career of Stephen Jones

6.1 Introduction

Stephen Jones (1872–1942) was one of the earliest appointees to Daniel Jones's Phonetics Department at University College London (UCL), served for a quarter of a century as 'Superintendent' of the first phonetics laboratory attached to a teaching department in Britain—from its inception in 1912 until his retirement in 1937—and made a pioneering contribution to the phonetic study of his native Welsh. Nevertheless, even though those facts alone would seem to render him worthy of note, almost nothing has hitherto been known about him. He is known, if at all, only negatively as 'no relation' of the other Jones who overshadowed him. Collins & Mees (1999) note only two of Stephen Jones's publications and give no biographical information beyond a (wrong) date for his death.[1] Thanks to the testimony of Abercrombie (1991: 1–21), Stephen Jones may also be known to some as an associate of another figure who overshadows him, J. R. Firth (1890–1960). On Abercrombie's account, it was in the basement laboratory of Stephen Jones that the disaffected 'downstairs' phoneticians led by Firth gathered to discuss their reservations about the phoneme theory associated with the 'upstairs' group of DJ and his close associates.[2] As will appear below, there are indeed good reasons to believe that Firth and SJ were close colleagues, and in particular that Firth must have had a high opinion of SJ. But Abercrombie's evaluation of SJ—as 'a nice, kind generous man, and a fine teacher' is hardly a sufficient memorial. SJ received no obituary, and even *Le Maître Phonétique* carried only a simple announcement of his death. He does not appear in ODNB, and the only Stephen Jones known to linguistic historiography is the much earlier namesake (1763–1827) who published *Sheridan improved: A general pronouncing dictionary of the English language* in 1798.

6.2 Biography

In the absence of any more direct sources such as personal memoirs or correspondence, the following sketch of SJ's life has been assembled mainly from civil records using established methods of family history research (Hey 2010); some details have come from surviving archive material held by SJ's employers.

Stephen Jones was born on 22nd June 1872 in Cynwyl Elfed, a village about 5 miles north of Carmarthen. The place-name is shown on the birth certificate as Conwil; the full anglicized form is Conwil Elvet.

SJ was the fourth child of John Jones (*b. 1831*)[3] and his wife Ann[4] (*b. 1829*) whose maiden name was Davies. The others are Anne (*b. 1862*) Benjamin (*b. 1867*) and Hannah (*b. 1870*). The occupation of John Jones is given on SJ's birth certificate as 'General Labourer' (probably agricultural labourer is meant).

By the time of the census of 1881 the family was living in Ystradyfodwg, Glamorganshire, and both father and the eldest son Benjamin (still aged only 14) were working as colliers. Anne was now married to David Davis (*b. 1854*), another miner, and that couple were living at the same address with the rest of the family (14, Queen Street). The family seems therefore to have been caught up in the very widespread move to the valleys of South Wales occasioned by the burgeoning coal industry.

Quite how the young SJ avoided a life of labour in the mines we do not yet know. Perhaps he showed unusual ability and was fortunate in gaining a scholarship to continue his education. At any rate, over the period 1886–1890 (i.e., for four years from age 14) he was a pupil at Oswestry High School.[5]

In the census of 1891, at age 18, Stephen Jones, described as 'student in science' is recorded as one of two students boarding at an address in Aberystwyth, and was evidently attending what was then University College Wales (founded 1872), which at the time was awarding external degrees from the University of London. He was awarded an intermediate certificate from London in 1894.

However, SJ's BSc degree was gained not from Aberystwyth, but from Cardiff. In 1894 the colleges in both Aberystwyth and Cardiff became constituents of the new University of Wales—and SJ's BSc, gained in 1897, must place him among the very earliest students to gain a University of Wales degree.

SJ's higher education seems therefore to have been protracted over the period 1891–1897, and he was 25 years of age when he gained his degree. It is possible that at least some of this extended time was spent as a part-time student, since there are indications that he may have taught at Oswestry High School for a period from 1891.

At the time of the 1901 census, Stephen Jones had moved to London. Now aged 28, he is described as a school teacher, and was lodging in a boarding house in Islington. It must have been a rather cramped home, since in addition to the landlady (a widow) and her two grown-up children, there was a servant girl and 5 young gentlemen boarders. One of the other boarders, a Cecil E. Kerridge (*b. 1875*), was also a school teacher. We do not yet know in what London school or schools SJ started his teaching career, but on 15th January 1903 he began at Haberdashers' Aske's Boys'

School, with which he was to maintain a connection until 1921. The school traces its foundation to 1690, and following re-organisation in 1873, had been located in Hoxton. It moved to Hampstead (Westbere Road, Cricklewood, NW2), opening there in January 1903 on the same date that SJ's employment began. The school remained at the Cricklewood location throughout SJ's career (though it is now located in Elstree, where it moved in 1961). SJ started as a probationary Master, but gained a permanent appointment from 1st May 1903.

On 28th December 1907 Stephen Jones married a Frenchwoman, Gabrielle Adèle Halot, who was some 14 years his junior (he was 35, she 21). The following year the Joneses had a son, Gérard Trystan. There were apparently no other children.

It cannot be an accident that in the same year as his marriage, the further qualification 'Diplôme Français, Granville' appears against SJ's name in the school records. SJ's qualification in French must have been gained on a holiday course intended for school teachers, of which there were many flourishing examples, supported by an excellent network of organisation and publicity in Britain. A May 1907 article in *The Practical Teacher* (presumably the kind of periodical to be found in school staff common rooms) explains the content and purpose of such courses (Anon 1907) and also gives specific details, including syllabus and all costs, relating to the Granville course. The course at Granville is linked with a similar one at Bayeux, and for both the syllabus begins with 'Phonetics, Conversation and Pronunciation'. Evidence of SJ's interest in languages thus pre-dates his encounter with DJ by at least four years, and it goes without saying that SJ may have attended similar courses in other years, not just the 1907 course which led to his Diplôme. Numerous courses were offered in German and Spanish, as well as French. Interestingly, one of SJ's publications in *Le Maître Phonétique* was to be a review of a work on Spanish (SJ 1923)—an undertaking which presumably presupposes some knowledge of the language.

There is a further possible significance in the selection of the Granville course. Granville is on the coast at the south-western corner of Normandy, just a short distance from Brittany. It was apparently common for holiday course participants to prolong their stay, and use their holiday-course accommodation as a base for further travel. If, as a native speaker of Welsh, SJ had already developed an interest in other Celtic languages,[6] Granville would have formed an ideal base for excursions into the Breton-speaking area. The Diplôme evidently added to SJ's teaching expertise at the school, as his subjects were listed as Maths, Science and French.

By 1911, SJ and his wife and son were living in the relative affluence of newly-built Hampstead Garden Suburb, still today a comfortable and desirable locality, and conveniently situated for Haberdashers' School. The Joneses were almost certainly the first occupants of a new house for which the address is at first given as 'Plot 559 Erskine Hill N.W'; it later becomes number 64 Erskine Hill. This was to be SJ's address for the remainder of his life. He was still living at this Erskine Hill address at the time of his retirement in 1937, and, though he died away from home, it was given as his permanent address at the time of his death in 1942. Intriguingly, his fellow UCL phonetician Wilfrid Perrett (see Chapter 7) was a near neighbour who similarly moved in before the house numbers were allocated—he had plot 544 Erskine Hill. It is possible, therefore, that they were neighbours before they became UCL colleagues.

According to Collins & Mees (1999: 132), SJ read a paper on vowel resonances in a research class organised by DJ in the academic year 1912-1913. The offer to SJ of a part-time position as assistant in charge of the phonetics laboratory must however have come earlier than that, since SJ is recorded as leaving the school on 25th July 1912, with his destination given as ‘University College London—Demonstrator’, and SJ is first noted as a member of the International Phonetic Association in 1911. The appointment at UCL was at first, and for many years, only a part-time one, and SJ also continued part-time at Haberdashers’, the school’s annual report listing him as a form master in each year. Annual reports ceased in 1919, but there is a final mention of SJ in the School Magazine in Summer 1921, with ‘best wishes on leaving at the end of the Summer Term’ and this evidently corresponds with the beginning of his full-time post at UCL—by which time, of course, he was almost 50 years old. Most of his published output on phonetics was to come in his sixth decade.

The precise manner of SJ’s introduction to phonetics is unknown. A possible link via Wilfrid Perrett has already been mentioned. It may also be significant that another participant in the 1912–1913 research class was H. O. Coleman, who is remembered for a pioneering contribution to the study of English intonation, published in *Miscellanea Phonetica I*, dated 1914.[7] In that piece, Coleman gives his affiliation as Senior German Master at Haberdashers' School, making it very probable that Coleman and SJ were acquainted before they met in DJ's research class, and indeed that one might have introduced the other to the field of phonetics.

From 1921 to 1937 SJ continued in his role as Superintendent of the UCL Phonetics Laboratory which he had helped to establish, being absent only for relatively short visits overseas, the most notable of which was to the University of Iowa in the Summer of 1929. During the 1930s, Daniel Jones's earlier enthusiasm for experimental phonetics declined markedly, and the UCL phonetics laboratory was somewhat sidelined; it is plausible that SJ may indeed have formed part of a group, centred around J. R. Firth, which became increasingly critical of Daniel Jones. Nevertheless, the list of co-workers mentioned in one of Stephen Jones's last published pieces of research, (1935), indicates that he worked happily with D. B. Fry (1907–1983), chosen by Daniel Jones to succeed SJ as superintendent of the laboratory, and whose appointment overlapped with the last two years or so of his own. No portrait-style likeness of SJ has been identified, but he appears with reasonable clarity in a surviving photograph, which shows him operating a kymograph along with Arthur Lloyd James and Eileen Macleod. (Colllins & Mees 1999: 184). A film from the 1920s, discovered and restored by the present author (2011), similarly shows him at work with the kymograph, though his face remains partly obscured by the kymograph mouthpiece.

No recordings of SJ speaking English have been located, but the situation is different for his native Welsh. In May 1925, SJ made a series of eight recordings for the *Lautabteilung an der Preussischen Stattsbibliothek*, Berlin (later to become the *Berliner Lautarchiv*), which had an ambitious program to make recordings of all the languages of the world. In 1925, recordings were made of all languages around Britain, perhaps under the direct supervision of the Director, Wilhelm Doegen (1877–1967). Doegen had previously made extensive recordings of English accents and dialects from prisoners of war held in Germany during WW1. Digital copies of some parts of the *Berliner Lautarchiv* (including SJ's recordings of Welsh) are now held in the British Library and may be consulted there. The recordings partly correspond to texts in SJ's Welsh phonetic reader (1926b), which must have been in an advanced stage of preparation at the time of the recordings, since some of the deposited documentation is evidently in the form of proofs of the book.[8]

To accompany the recordings, SJ completed a pre-printed questionnaire termed *Personal-Bogen* which aimed to collect relevant information about informants' linguistic experience and background. The brief details recorded on that form correspond with the reconstruction above: birthplace 'Conwil Elvet' (he writes the anglicized form), residence during the first 6 years of life in Carmarthenshire, and from ages 7 to 20 successively Rhondda Valley, Oswestry and Cardiff. Interestingly, he gives his mother tongue (*Muttersprache*) as Welsh and states that he additionally speaks English and French.

A strange feature of the responses on the *Personal-Bogen*, however, is that SJ clearly writes his birth date as '22 June 1874'—that is, two years later than the correct date. This remains unexplained.

SJ retired in 1937, having reached the age of 65 in June of that year. On June 29th, the UCL Provost, Alan Mawer,[9] wrote to SJ:

> May I, on the occasion of your retirement, write on behalf of the College Committee and myself personally to offer you our most sincere thanks for all that you have done for us during the twenty-four years you have been on the staff of the Department of Phonetics? Your work has added greatly to the lustre of that Department and we are sincerely grateful to you.
>
> With every good wish for your retirement, I am, Yours sincerely, Alan Mawer
>
> (Source: UCL Records Office)[10]

Though eloquent enough, the brief letter is probably little more than a formality, and there is no indication that the end of SJ's career was marked or celebrated in any way.

There is some evidence that SJ maintained contact with colleagues and the world of phonetics for at least two years following his retirement. He spoke (in English) on a linguistic topic at an evening meeting of the Honorable Society of Cymmrodorion in London on 9th December 1937.[11] . It was published in the society's *Transactions* the following year with the title 'Speech sounds and their bearing on the linguistic problem in Wales'.

SJ is not listed among participants at ICPhS III which took place in Ghent in 1938, though his autograph appears on an offprint of DJ's paper delivered there. SJ's UCL successor D. B. Fry did attend the Congress, and Fry's paper acknowledges assistance from SJ. The last documentary trace of SJ is an autograph dedication on an offprint of his Cymmrodorion paper (1938), dated 1st January 1939.

SJ died on 22nd August 1942 at an address identified as 'Ryecroft' in Tower Road North, Heswall, Wirral, two months after his 70th birthday. His wife was present at the death, and was the informant when the death was registered four days later.

Two years after SJ's death, some of his books and papers were given to the College by his son. In the UCL Library Accessions book covering 1944 is recorded the gift of '219 vols and 301 pamphlets from the library of the late Stephen Jones' which were presented by 'Lt. Comm. G.T. Jones R.N.V.R.' At least some of the relevant books (such as Kanehiro 1933) contain bookplates commemorating the gift. It would be of great interest to know exactly what all the items were, but unfortunately there appears to be no complete list. An attempt has been begun to reconstruct the list, with assistance from the UCL Library, though with limited success so far. Unfortunately the relevant items are dispersed in storage and cannot be browsed in a search for the bookplates—nor can the items be identified from their online Library catalogue entries.

6.3 Chronology of career and publications

SJ's career in teaching and research has hitherto been as obscure as his biography, and no list of his publications has previously been assembled. This section chronicles what we can glimpse of his activity and output.

Between 1912 and 1914, SJ must have done the design and development work on the large electrically-driven horizontal kymograph which came to be the centrepiece of the UCL laboratory. It was to be put into production by the London firm of C. F. Palmer (Sykes 1995) and sold around the world. A somewhat smaller version was also produced, and a range of accessories. The distinguishing characteristics of the kymograph as developed by SJ were large drum size and thus high paper speeds (up to 0.7 m per second), facilitating accurate time measurements, and uniform rates of rotation, achieved by electric drive, and from 1930 with a synchronous motor achieving speed stability of better than 0.2% (SJ 1930a: 50-51). Compared with those from elsewhere, kymograms from UCL also exhibit comparatively high sensitivity at audio frequency, though this may be partly a result of skilful setup and handling. C. F. Palmer supplied special sensitive tambours for speech, but it is unclear whether SJ employed them or had any role in their development. Surviving photographs and film from UCL appear to show only tambours of conventional Marey pattern in use.

By 1914, if not somewhat earlier, SJ was able to produce kymograms which were the equal of those being made anywhere in the world—and for the most part they were far better. The earliest to appear were probably in Noël-Armfield (1915); others of the same vintage were used in the first edition of DJ's *Outline* (though it was not published until 1918) and his RI discourse of 1917.

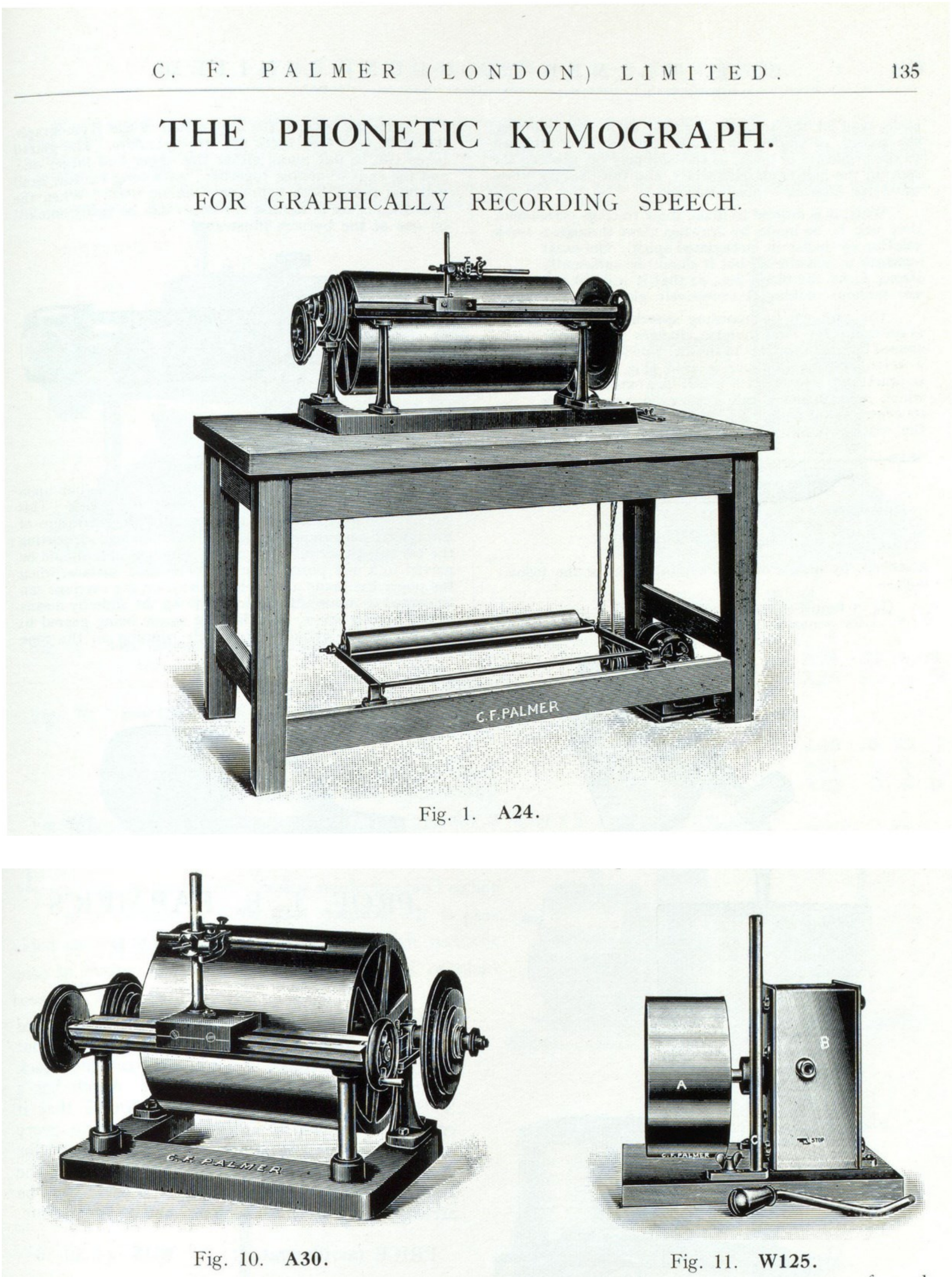


Figure 6.1 Palmer kymographs associated with SJ. Top, the large model based on the prototype constructed during 1912–1914 for the UCL laboratory. Lower left, a horizontal model of moderate size. Lower right, a portable clockwork type which SJ is known to have taken to Iowa when lecturing there in 1929. Photographs by the author, from the Palmer catalogue (undated), pp. 135–137.

136 C. F. PALMER (LONDON) LIMITED.

easily read off the tracing. In this, as in the following, the record, or tracing, is formed by the point attached to the armature or lever, as the case may be, rubbing the soot off the highly glazed surface, and thus leaving white wave-like lines on a black surface.

When it is desired to make these tracings permanent, they can be so made by drawing them through a weak solution of shellac in methylated spirit. The exact strength is immaterial, but it should be sufficiently strong to fix the black (i.e., so that it will not rub off) without making it excessively glossy.

The problem in recording speech is to show graphically the very minute changes in pressure caused by the vibrations in the air. For this purpose a very sensitive tambour is used (Fig. 6), which is practically a tube with a bell-like extremity, on which is stretched a thin rubber diaphragm, with its centre connected to a light lever, terminating in the writing point. To one of these tambours is

Fig. 6. **W100.**

attached, by means of a rubber tube, one of the following :—

(1) A funnel-shaped mouthpiece (Fig. 7, two shapes) for a mouth record.

Referring back to the illustration of the Kymograph, it will be seen that there are two cylinders. The glazed paper can be put round either the upper and larger one, and the ends cemented together ; or a long endless band reaching round both cylinders, can be used. When the cylinder/s is/are in motion the paper can be easily smoked by one of the burners illustrated.

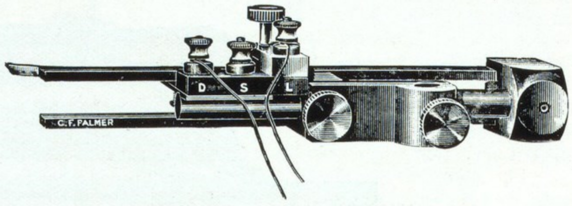


Fig. 5. **B12.**

Near the top of the large cylinder is the bar upon which the time-marker and tambours are fixed. This bar has a fine adjustment for regulating the pressure of the writing-points upon the paper. The carriage supporting the bar can either remain still (in which case it should be moved to a new position on the blackened surface, when the paper has made a complete turn), or the carriage can be caused to move automatically along the slide by means of the leading screw, such leading screw being geared to the axis of the cylinder by a belt running on the cone

W111. Fig. 7. **W112.**

(2) A nasal olive (Fig. 8, three sizes) is connected up and lightly inserted into the nostril, when it is desired to map out a nasal.

(3) A larynx capsule (Fig. 9), for determining the boundaries of a voice plosive.

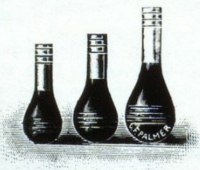


Fig. 8. **W115/7.**

This latter record has to be taken from the outside of the larynx by placing the capsule on the neck, in the space (left or right) between the thyroid cartilege and the hyoid bone. The capsule is now made slightly bent so that this can be done more readily. It is obvious that the three records can be taken simultaneously.

pulleys seen on the left of the illustration. By this means a long spiral tracing is obtained.
The usual method of driving the Kymograph is by a small electric motor, fitted with worm gear to reduce the speed to that required.

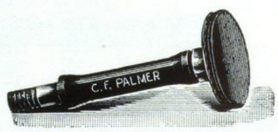


Fig. 9. **W120.**

We are indebted to Mr. Stephen Jones, of the Phonetics Department, University College, London, for his assistance, and would refer all readers interested in Experimental Phonetics to him for further details.

* * * *

Since the foregoing was written new forms of smaller kymographs have been constructed. That shown at Fig. 10 is a useful instrument for general work, it having a cylinder 10″ long by 9″ diameter. In other respects it follows closely after the larger model.

Figure 6.2 Kymograph fittings for phonetics by C.F. Palmer. Left column, from top: high sensitivity tambour, mouthpiece, nasal olives in three sizes. Right column: time marker, another pattern of mouthpiece, larynx capsule,

Figure 6.3 Palmer kymograph fittings preserved at Oxford University Phonetics Laboratory (cf. Figure 6.2). From top: speech tambour, mouthpiece (note the relief valve), nasal olives in three sizes, angled and straight larynx capsules. Photographs: the author.

In the spring of 1914, SJ (together with DJ and Perrett) visited Hamburg for the Congress of Experimental Phonetics, which was held in Hamburg 19–22 April 1914 (Jones 1914).

Also in 1914 came SJ's first known publication, a *spécimen* of Welsh following the more-or-less standard pattern of *Le Maître Phonétique* (SJ 1914). Corrections to the transcriptions of three words were published later the same year (p. 70) though SJ is not explicitly credited.

Between 1915 and 1922 SJ has no known publications, and there are few glimpses of his activities. At some point before October 1916, Wilfrid Perrett visited the laboratory to use what at first he called the 'cymograph' (Perrett 1916: 69). He mentions trying 'several Marey tambours', in a search for one with a suitably high frequency response), so evidently the laboratory already had a stock of different examples. SJ is not explicitly mentioned, but it may safely be assumed that as superintendent of the laboratory SJ assisted him in this trial.

SJ's next publication is perhaps the oddest. It is a review of Walker (1922), *Introduction to Spanish* (SJ 1923). Even more surprising than the subject matter is the fact that it is written in Welsh—and, since the journal was *Le Maître Phonétique*, phonetically-transcribed Welsh into the bargain.

In 1924 *Le Maître Phonétique* published SJ's review of Perrett's *Some Questions of Phonetic Theory*. This review is concerned with the later parts of the work, published from 1919–1924. DJ's review for *Nature* (DJ 1917) concerned only Part 1.

In 1925 there appeared a book *What is rhythm*? with E. A. Sonnenschein as the main author. SJ and Eileen Macleod (1895–1987), who was then a research student in the department, were the two authors of Appendix II (pp. 217–228), which reports duration measurements made from numerous kymograms. The experiments are stated to have been carried out between 1921 and 1923. The names of SJ and Eileen Macleod also appear on the title page of the book, with the result that in certain library catalogues they are listed as co-authors of the whole work.

There is circumstantial evidence that from around 1923 SJ may have had some contact with E. W. Scripture (who was in London by this date). The evidence is considered in Chapter 8.

In 1926 SJ published his *Welsh Phonetic Reader*. This is his most substantial work, though it has no connection at all with his experimental work. The descriptive account of Welsh phonetics (pp. 9–31) gives no hint of his scientific interests. The same year saw three pieces in *Le Maître Phonétique*, one being an investigation of the relative intensity of sounds, conducted with a sensitive flame as the measuring instrument, another dealing with the aspiration of plosives in final position, and the third his review of Paget's 'Nature and artificial production of consonant sounds' of 1924. Also in 1926, an article 'Seeing our speech as others hear it' appeared—somewhat surprisingly—in *Scientific American.* This seems to be his one attempt at a popularizing presentation, and almost certainly the only foray by a British phonetician into an American popular science journal. One wonders if it had been inspired by Lloyd James's almost exactly contemporary piece in the popular British illustrated paper, *The Graphic* (Lloyd James 1925). Or perhaps the choice of journal was encouraged by an American colleague—perhaps Scripture.

His one short publication in 1927 is a note in *Le Maître Phonétique* dealing with 'Apparatus for experimental phonetics', which calls attention to the catalogue of C. F. Palmer. The note is more than a little disingenuous, since he was largely responsible himself for the design of the kymograph and was almost certainly the author of the very parts of the catalogue he calls attention to in the note.

As explained in Ashby (2011), film showing SJ and the UCL kymograph in use, and the operation of a sensitive flame—another device with which SJ was particularly associated—also dates from the late 1920s, perhaps about 1928.

Around this time SJ was no doubt kept busy on a collaboration with laryngologist Victor Negus whose major study *The mechanism of the larynx* (1929) contains extensive contributions from SJ. In the same year there appeared an article 'Voice' in the 14th edition of Encyclopedia Britannica credited jointly to Negus and SJ, and SJ's own best-known paper 'Radiography and pronunciation' (offprints of which were distributed with *Le Maître Phonétique*), which, as will be shown below, is itself a spin-off from the collaboration with Negus. In fact 1929 was a very eventful year, since over the summer he spent a period of time teaching at the University of Iowa.

In June 1930 SJ attended the first congress of the International Society of Experimental Phonetics in Bonn. He delivered a paper 'Two methods of measuring intonation', which is published in the congress report (Menzerath 1930). The membership list included at the end of the volume (pp. 120–124) identifies SJ as an officer of the Society: he is 'Sekretär der Abteilung für Experimentlphonetik und philologische Praxis' ('Secretary of the section for experimental phonetics and philological practice'). Very probably he was elected to this role at the 1930 congress. Inside the back cover of SJ's own copy of the report is a scribbled note headed 'Possible Members', listing eight names including his colleagues Firth and Ward, and the American phonetician J. S. Kenyon (1874–1959), whom SJ had probably known since about 1927 as a visitor to UCL . The next list of members, printed in the Society's first Bulletin (1931), does not indicate that SJ had been successful in recruiting any of the eight, though SJ's colleague Arthur Lloyd James has been added. SJ also contributed a brief account of the 1930 congress to *Le Maître Phonétique*, though it does not mention his own role in the Society (SJ 1930a).

The year 1930 also saw two reviews from SJ. One is a short but glowing notice for *Le Maître Phonétique* of Negus's *The mechanism of the larynx*, in which he had himself played a large part (1930b), and the other a much more substantial review, written jointly with Firth, of Paget's *Human speech.* The note on Negus, and the 1927 note on C. F. Palmer's experimental apparatus, seem to show SJ using short pieces in *Le Maître Phonétique* to drip-feed snippets of information on experimental phonetics to the IPA membership which was otherwise insulated from such developments. SJ's review of Negus (1929) specifically says that the book's chapter on the mechanism of phonation (i.e., the chapter SJ effectively co-authored) will introduce 'philologists' to technical matters (SJ 1930: 34). For the most part, *Le Maître Phonétique* was overwhelmingly devoted to taxonomic and descriptive concerns.

The same motivation—and perhaps a deliberate pointing of the contrast—can be seen in SJ's (1931) 'review' in *Le Maître Phonétique* of a very different journal, the *Archives Néerlandaises de Phonétique Expérimentale*. This is not a review of any particular published papers, nor indeed of a specific issue, but rather a calling of attention to the existence of the journal, and to the depth and variety of the papers it was publishing. SJ no doubt perceived very accurately that the *Archives* was establishing itself as the leading forum for a new, professional kind of experimental phonetics which was emerging—particularly in the Netherlands and the USA—during the 1930s, and which was to set the tone for the first three meetings of ICPhS in that decade (Amsterdam, London and Ghent). Indeed, the very existence of the journal was a factor facilitating this development. Embedded in this short (14 lines) review, and enclosed in quotation marks, is “eppur si mwɔːve”—that is, a phonetic transcription of the Italian phrase *eppur si muove* (‘and yet it moves’) supposedly spoken defiantly by Galileo after being forced by the Inquisition to recant his claim that the Earth moves around the Sun. Perhaps SJ simply meant to call attention to potential conflict between new kinds of (experimental) evidence and the established ‘corpus of doctrine’ (his own phrase in the review); or perhaps it referred specifically to his own situation, and was a coded way of saying that he felt compelled to silence within DJ’s department.

SJ’s 1932 article for *Le Maître Phonétique* with the title ‘The accent in French—what is accent?’ starts as if it is going to be a conventional ‘philological’ discussion of the matter, complete with references to authorities as far back as Ellis, but quickly shifts into a different mode, provokingly throwing in terms such as *decibel*, *visual-kinaesthetic stimulus*, *exteroceptor* and *proprioceptor*, which were almost certainly making their first appearance in the journal. The background to the paper is undoubtedly Stetson’s *Motor phonetics* of 1928 (which, significantly, had been published as a special number of the *Archives Néerlandaises*), and indeed Abercrombie (1991: 2) tells us of SJ’s enthusiasm for Stetson’s work. This 1932 paper by SJ probably gives us the clearest glimpse we have got of the sort of topics addressed in the fireside debates of the ‘downstairs’ group.

SJ’s 1932 paper contains the striking sentence ‘It is possible to exaggerate the importance of the ear in speech’ (1932: 74). Essentially, SJ is arguing in his paper that ‘accent’ is something felt rather than heard, and he is specifically talking about speech perception among ordinary users of a language, rather than the specialist metalinguistic judgements of the phonetician. So, in its context, the dictum does not mean anything like ‘ear phonetics is overrated, and must be supplemented by instruments and experiment’. But SJ must surely have seen that taken out of context the sentence does have that other possible interpretation, and by framing it as a short sentence easily cut from its setting, he perhaps even invited it.

In his 1933 review of work by the Americans Parmenter & Treviño, who were to make prolific contributions to the literature of experimental phonetics during the 1930s, we see SJ contemplating the contrast between the new electronic apparatus that was conspicuously missing from his own laboratory (amplifiers, electric filters and high-performance oscillographs) on the one hand, and the simple kymograph on the other. He makes the very fair point that for the ‘determination of frequency’ (i.e., f_o), the kymograph is in principle just as satisfactory as the new apparatus, since if the tambour responds at all it can hardly distort the fundamental frequency. But he’s overlooking all the inconveniences of the kymograph—the difficulty of adjusting the tambours in the first place, the smoking of the paper, and the rigmarole of varnishing the completed traces and finding somewhere to dry them.[12] The kymograph might still have had a value in teaching, but for research purposes nobody who had the means of producing high quality traces quickly, cleanly and automatically on film would have favoured the old method.

SJ’s last two *articles de fond* (‘main articles’ or ‘leading articles’, as opposed to reviews, etc.) in *Le Maître Phonétique* (1934, 1935) deal with pharyngeal fricatives [ħ] and [ʕ] in Somali, and voiceless lateral consonants from Welsh and Icelandic. His final review for the journal (1936) is a brief and glowing notice of another ambitious research project done with modern electronic apparatus—and this time, there is no mention of the kymograph. SJ’s paper for ICPhS in London—delivered in 1935, published in 1936—reports work apparently done several years earlier on a case of what he calls ‘double voice’ (probably simultaneous vibration of both true and false vocal folds; see Negus (1928: 430)).

In the months January–March 1937, a few months before his retirement, SJ delivered a series of broadcast talks on speech for Welsh schools. A booklet was issued to accompany the series, with the title *Seiniau Llafar Cymraeg a Saesneg* (‘Speech sounds of Welsh and English’). It is typeset and illustrated with photographs and drawings to a professional standard. The only known copy is in the Tracts.[13]

6.4 SJ’s writings on experimental phonetics

The chronology above may give the impression of a considerable published output, though in reality SJ’s writings are disconnected and mainly slight. He did not initiate—or certainly did not *report*—any extensive programme of experimental research. The periods of work he did in conjunction with Victor Negus, and separately with Edward Sonnenschein, come closest to being proper research projects, though neither enterprise has been noted before this present account. Whatever may have been the original intention, under DJ’s direction the Phonetics Laboratory became, as it were, largely a service department. Nearly all of the work that SJ carried out in what remained a modest basement laboratory was done in support of linguistic phonetic observations initiated by others, whether staff colleagues or research students, and the short notes he published often arose as by-products of that work.

For example (1926a) arose from uncertainties in syllable segmentation encountered in measuring durations for the work with Sonnenschein; (1929) is a spin-off from the collaboration with Negus; (SJ 1934) was occasioned by Armstrong's investigation of Somali (1934); and (1935), which incorporates material on Icelandic, probably arose in connection with the student research work of David Abercrombie, whose *spécimen* of Icelandic appeared in 1936.

By far the best known of SJ's publications is 'Radiography and pronunciation' (1929), which includes plates showing X-rays of the Cardinal Vowels (as pronounced by SJ himself). The paper has, however, been almost entirely misunderstood.

Probably most of those who have seen the work are familiar with the offprint, which is relatively common, having been distributed with *Le Maître Phonétique* (and sometimes now encountered bound into MF). But it is instructive to examine the whole journal issue in which it appears, to get an idea of the context. It turns out that 'Proceedings of the Society of Radiographers' (the heading on the front of the offprint) is not at all the same thing as *The British Journal of Radiology*. Rather, certain reports of Proceedings of the Society of Radiographers appeared from time to time in the *Journal*, which mainly carries longer research papers. SJ's publication is evidently a brief report of a lecture presentation. He was no doubt invited to deliver it by the F. Melville he mentions, who had in fact taken the X-rays. Frederick Melville (1891–1954) was Honorary Secretary, then General Secretary, of the Society of Radiographers.

SJ's paper is puzzling. Although the live presentation must presumably have been considerably fuller, and space is evidently very short in what amounts to a written abstract, he retains an elaborate joke based on the Society's Latin motto *ex umbris eruditio*, thereby using up space which could have been devoted to the scientific content. He gives a few radiograms, reproduced on a very small scale—and tantalizingly mentions that there were many more (‘about 40’). Why do all that expensive and difficult work, and publish only a selection of images and a short summary, part of which is taken up with jokes? The answer is that this paper is not at all the primary report on the work to which it refers. The presentation was really just a popular spin-off from the work he had done collaboratively with Negus on the latter's *Mechanism of the Larynx* (1929), a whole chapter of which (pp. 344–441) depends on joint work with SJ, and includes more from the same set of radiograms, and in better reproductions (See Figure 6.4). The comprehensive programme of X-rays had been planned in conjunction with Negus, with a view to answering numerous questions concerned with phonation. It is plain that alongside the Cardinal Vowels, the study encompassed vowels of particular languages, voice qualities which involve vocal tract settings (such as ‘throatiness’), nasality, and various consonant articulations.

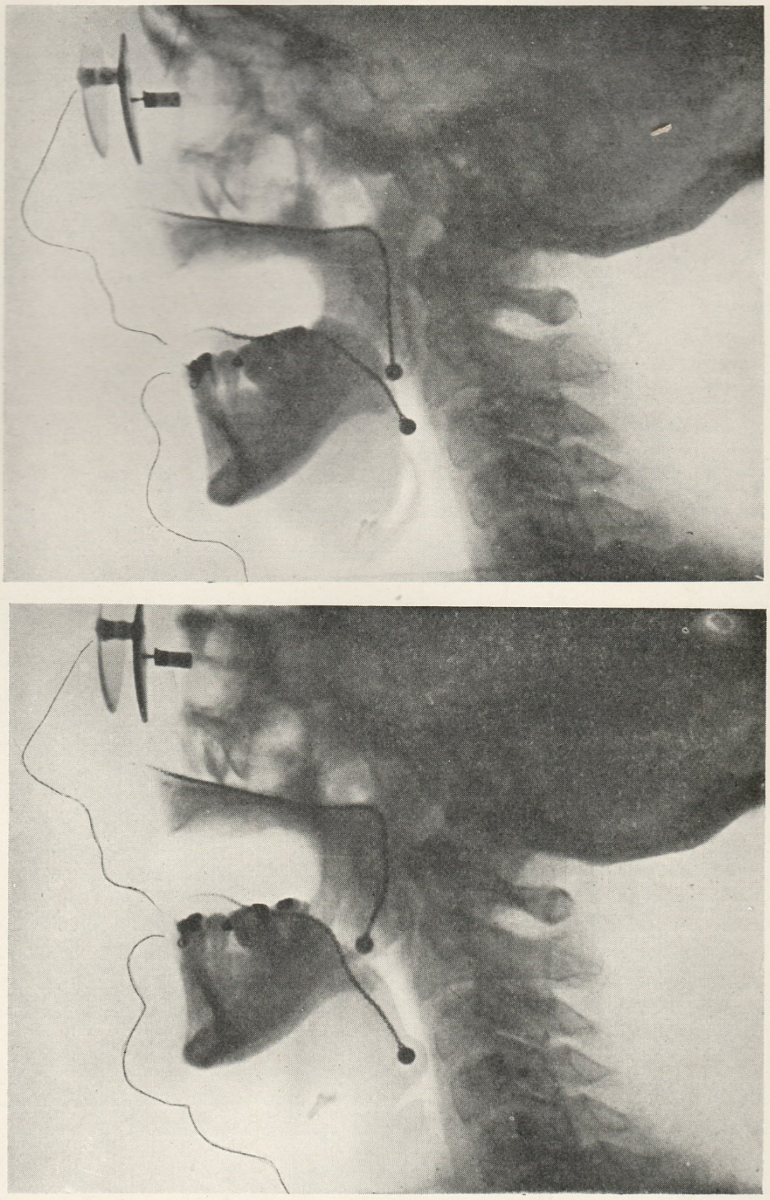


FIG. 145.—POSITION OF THE PHARYNX DURING PHONATION.

The subject—Mr. Stephen Jones—is uttering the vowel sound [a] between cardinals 4 and 5, as "a" in "father" on a pitch of 128 ~. In the upper X-ray photo there is shown the position of good intonation, the larynx being low in the neck and the pharyngeal cavity freely patent. When the lower photo was taken the intonation was throaty; the larynx is pulled upwards and backwards by contraction of the inferior constrictor, and the pharyngeal cavity is much diminished in size. The action of muscles is well shown by the position of the epiglottis and the presence of a double chin. Chains are present in nasal and oral cavity to show position of soft palate and tongue. (X-rays taken by Mr. Melville, in Dept. of Anatomy, University College, London.)

Figure 6.4 A figure from Negus (1929: 408), reproduced actual size. Note (i) the larger scale and better definition than the figures in SJ (1929), (ii) use of an English vowel (not Cardinal), and (iii) in the caption, the attention paid to the pharynx cavity rather than the tongue position.

SJ (1929) is probably largely known today as a result of the attempt made by Peter Ladefoged (1925–2006) to explore the full implications of SJ's radiograms for the Cardinal Vowel System. A composite diagram showing 'the highest point of the tongue' for the full set of vowels, as extracted from SJ's photographs, was a feature of Ladefoged's influential *Course in Phonetics* from its first publication in 1975, and has been retained through the work's many editions and revisions. Ladefoged concludes, on the basis of SJ's evidence, that 'the position of the highest point of the tongue is not a valid indicator of vowel quality', preferring instead 'the term *vowel height*—meaning an auditory quality that can be specified in acoustic rather than in articulatory terms' (6th ed., 2011: 220).

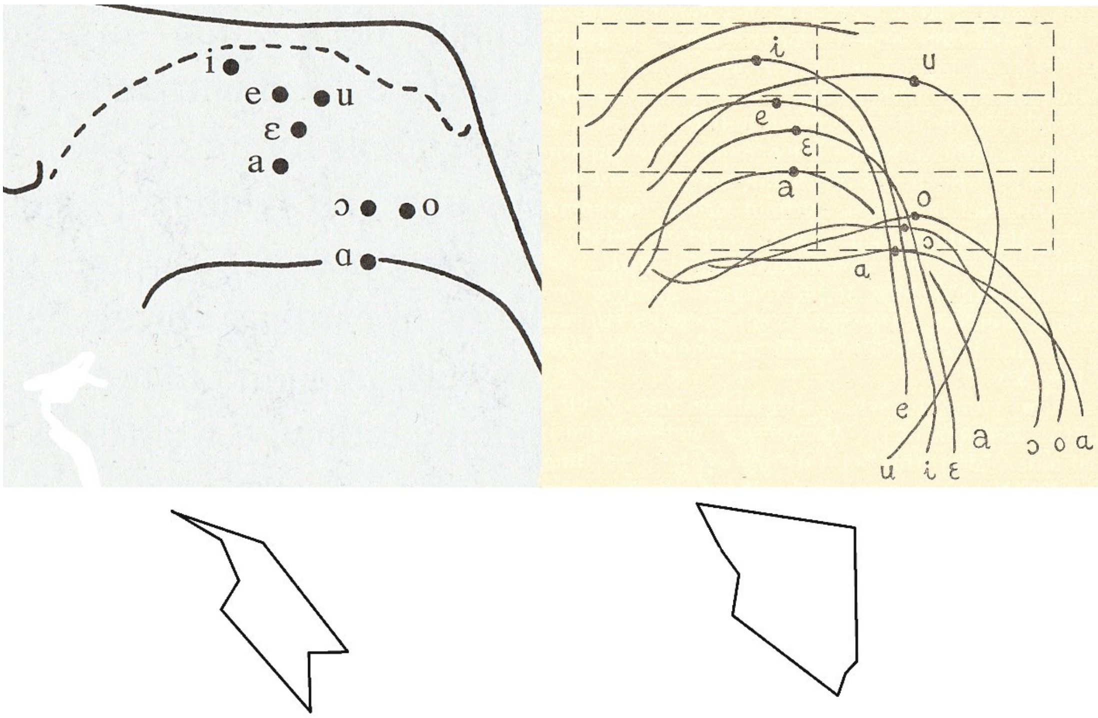


Figure 6.5 Tongue height in SJ's Cardinal Vowel X-rays (1929) as measured (at left) by Ladefoged (2011: 221), and (at right) by Chiba (1931: 32). Below each is the 'vowel space' it implies. Chiba's measurements—probably the more accurate—support a shape more like the conventional quadrilateral.

We may wonder just how accurately Ladefoged's measurements can have been made from the small images (which are further degraded by the dot-screen process used to print them). He was apparently not aware that his efforts had been anticipated more than 40 years previously (Chiba 1931)—and probably with somewhat greater precision, since Chiba began by photographically enlarging the X-rays to 112% of life size before attempting to measure them. Chiba's results do not agree in all respects with Ladefoged's; while it is true that both sets of measurements produce overall outlines very different from the Cardinal Vowel quadrilateral, Chiba's do at least preserve the expected rank ordering of back vowel tongue heights (Figure 6.5).

SJ would probably have seen the narrow concern with 'tongue height' as an irrelevance anyway. It is plain from the illustrations in Negus (1929) that the two authors had a much more subtle and nuanced approach to the characterisation of whole vocal tract configurations: notice in particular the comments on pharynx size in their caption reproduced in Figure 6.4. There is nothing to suggest that SJ regarded the Cardinal Vowel system as a testable empirical hypothesis of any kind, least of all a hypothesis about vowel *articulation*. As Abercrombie observes, 'The system of Cardinal Vowels constitutes a technique, not a theory' (1991: 39).

SJ's point is not that his radiograms refute the Cardinal Vowel system. The Cardinal Vowels are used only as samples of various vowel types. His point rather is to call attention to the contrast between X-ray pictures of real configurations, and the 'fanciful pictures' of supposed vowel positions in wide use. Two such diagrams were shown by way of illustration on a slide in the talk, but they are not reproduced in his paper. SJ does not specify the references in full, but there is enough information to suggest that the 'fanciful' representation of [i] probably came from Aikin (1910: 50)—a configuration altogether more bizarre than anything drawn by DJ.

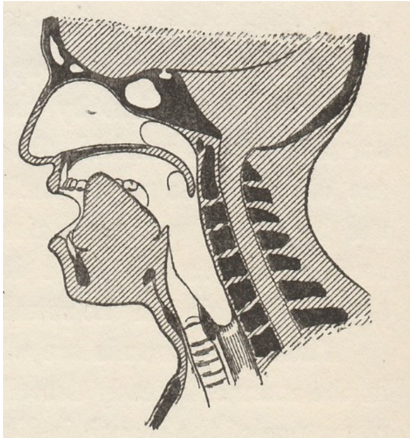


Figure 6.6 A 'fanciful' representation of the position of the vocal tract for '*ee*' (that is, [i]). Aikin (1910b: 50)

SJ certainly does not reject the Cardinal Vowel system, but actually points out its advantages: 'It must be clearly borne in mind that these vowels called "cardinal" are acoustic units of definite quality or timbre. A certain principle was first laid whereby they were physiologically determined. They are independent of key words, which are, of course, pronounced differently by different persons'.

6.5 Influence

6.5.1 Writings

The direct influence from SJ's writings was limited at the time of their appearance and has remained small. His name is *mentioned* in many an acknowledgement and footnote, but he is not a widely cited author; DJ, for example, seems to have cited published work by SJ on only one occasion (Jones and Ward 1969: 11 cites SJ 1926a), and that one citation takes the form of a bizarre footnote. By the 1960s it was an extraordinary choice to offer evidence about the relative sonority of vowels which had been gathered more than 40 years before using the primitive technology of a gas flame—and of course by the time the citation appeared in print both SJ and DJ were dead.

6.5.2 SJ as teacher

Abercrombie tells us that SJ was 'a nice, kind generous man, and a fine teacher'. His teaching had an impact on numerous students who were to become notable, including Masao Kanehiro (1883–1978) (founder of the first phonetics laboratory in Japan), Yuen Ren Chao (1892–1982) (one of the leading Chinese linguists of his time and author of the classic 1934 paper 'On the non-uniqueness of phonemic solutions of phonetic systems'), Gim Sun-Gee (1907–1992) (who wrote a pioneering account of the phonetics of Korean as an MA dissertation at UCL, and—with SJ—produced what are probably the first measurements of VOT for the celebrated 3-way stop system of Korean), Jean-Paul Vinay (1910–1999), and J. C. Catford (1917–2009).

A unique insight into SJ's teaching is provided in a memoir written by Loren D. Reid (1905–2014)[14] who attended the Iowa summer course in 1929 (he was to gain a PhD in speech from Iowa in 1932):

> Fully a hundred students gathered at the physics building for the first phonetics class to meet with the professor, Stephen Jones of the University of London. He began by describing the International Phonetic Alphabet as a way of recording pronunciation by using one symbol for each sound, comparing and contrasting this System with the more awkward use of diacritical marks found in most dictionaries. Jones covered the blackboard with illustrations from many languages. […]
>
> For the opening assignment he asked us to prepare a list of ten words illustrating a variety of spellings for the sound of *ee* as in *feet,* such as *heat* and *police.* "Can you do it?" he asked, solicitously. "Is it too much?" "No," we

> replied, in concert, "it is not too much." Five minutes, we said to ourselves, will be ample. The previous summer I had spent hours on Anglo-Saxon and the accompanying linguistics. Jones's daily assignments were ingenuous. "Can you do it? Is it too much?"
>
> One day he discussed the *s* sound, contrasting it with *z*. Some foreign-born speakers of English have difficulty distinguishing between *seal-zeal, rice-rise.*
>
> "To show the difference," he explained, "I have prepared a sensitive flame." He exhibited what looked like an ordinary Bunsen burner, connected its long hose to a gas outlet, turned the valve, lit the burner, and adjusted it so we saw a tall, blue flame. "When I say *s-s-s-s*;" he demonstrated, "the sensitive flame burns steadily. But when I say *z-z-z-z,* you see what happens." The flame flickered noticeably.
>
> "Suppose the foreign-born speaker tries to say *raze* but it comes out *race."* We saw the flame bobble on *raze* but not on *race.* "You can use this device, asking your student to try to say *raze* until he gets it right and the flame wavers." We were astonished. We had worked with Bunsen burners and were unaware that they had this characteristic. […] When we questioned him, he said blandly that he had specially adjusted the flame.
>
> "Here, it will work for you." He pointed to a woman in the front row. "Say *maze,"* he asked. She did, and the flame ducked. "Say *mace,"* he said to another student. He did, and the flame burned steadily.
>
> Eventually we realized that our professor had, in fact, unknown to us, pinched the hose when he wanted the flame to flicker. We had been taken in. That moment of enlightenment led to another: he knew that he was giving us minimum assignments. Nevertheless the flow of information and the good humor were so stimulating that we enjoyed our daily meetings.
>
> (Reid 1981:214–215)

There are two corrections we can suggest to this account (which was after all written 55 years after the events it describes). First, it would almost certainly have been [s] which caused the flame to dip, and [z] which left it unaffected. In all probability, the flame was not responding to the voicing difference as such, but to the greater intensity of high-frequency noise found in [s]. Reid's account is consistent, but switched around.

Second, SJ may indeed have been observed pinching the hose, but this was almost certainly not as a means of faking the effect. More probably he was adjusting the flow so as to keep the flame at its sensitive point. If this had been done at the gas cock where the tube was attached, turbulence at the partly closed valve could have destroyed the laminar flow required for the undisturbed, resting position of the flame, and made it impossible to achieve sensitivity at all. It was better to restrict the flow by a smooth narrowing of the pipe, achieved by squeezing. This had been observed by Rayleigh, who gives a detailed analysis of the effect in one of his short papers (1900: 100–102).

6.5.3 Regard of colleagues

All the signs are that SJ was a capable and willing co-worker, more than ready and able to hold his own, and to rise to the occasion when a colleague came along who was not under the stultifying domination of DJ—either because the field of enquiry was not one in which DJ could claim pre-eminence (for example Laryngology, as was the case with Negus), or because the colleague was more ready than SJ to be outspoken and risk disagreement with DJ (as was the case with Firth).

SJ's most significant collaborators were V. E. Negus (1887–1974) and Edward Adolf Sonnenschein (1851–1929); both easily achieved a degree of notability that means they have *ODNB* entries. Briefly, Negus was one of the most outstanding laryngologists of his time, while Sonnenschein was a classical scholar of international repute, educational innovator, and grammarian. Hudson and Walmsley (2005) identify him as one of the few British thinkers to make significant contributions to grammar (that is, in modern terms, to *linguistics*) in the early decades of the twentieth century.

There is every indication that SJ worked happily and on equal terms with these outstanding and energetic people, and that they valued his contributions.

It is, of course, SJ's link with Firth that has received the most attention hitherto. While SJ remained in the shadows, Firth attained or laid claim to a degree of notability, not only within the UCL Department but in the national and international academic community—and, from Firth's relatively numerous mentions of SJ, it seems that Firth was urging SJ to do likewise, and was attempting to push SJ into some degree of prominence. For example, in Firth (1930), we are given the following account of the development of 'experimental phonetics during the last fifty years':

> Rousselot, Scripture, Meyer, Poirot, Panconcelli-Calzia, Jones and others laid the foundations of this method of investigation which is still being pursued and developed in London, Paris, Hamburg, and Bonn, as well as in Iowa, New York and other American universities.
>
> (1930: 15-16)

(It is not strictly clear whether 'Jones' refers to DJ or SJ, though probably SJ is meant). The ordering of laboratories is also noteworthy. In reality, SJ's London basement could not compare with the others either in facilities or achievements, but is put first by Firth. And in the bibliography to this little book *Speech*, Firth includes both Negus (1929) and Negus and Jones (1929), though neither was likely to be of much use to the curious general reader that Benn's Sixpenny Library was supposedly addressed to.

The emphasis is even clearer in Firth (1935), which is Firth's report in the journal *English Studies* on ICPhS II, which took place that year in London. SJ played a small part in the congress, delivering a modest paper, and apparently not even being charged with chairing a session on his home ground. Yet in his short report on the whole congress, Firth contrives to mention SJ by name three times, and to cite one of his publications (SJ 1934, on pharyngeals)—this being furthermore the only citation in the whole report.

Firth adds SJ's name somewhat incongruously to lists of the famous, and—though he delivered a single paper—he is included in two such lists:

> …experimental phonetics…was well represented…by the presence of Professors Flatau, Menzerath, Oscar Russell, Scripture, Stetson, Dr. Kaiser and Mr. Stephen Jones…
>
> (1935: 179)

> Among other distinguished phoneticians and linguists who read important papers to the well attended sessions of the Congress were Professors Van Ginneken, Bröndal, Grammont, Duraffour, Sommerfelt, and Mr. Stephen Jones.
>
> (1935: 182)

Without a doubt, Firth was SJ's greatest admirer, but by no means the only one. For example G. Oscar Russell presented SJ with a copy of his major work *The vowel* (1928) which is inscribed 'With personal expression of highest esteem'. Testimonials such as that must surely mean something. Through some cause or other, SJ did not receive within UCL anything approaching the recognition and regard given him by those—like Russell—best qualified to judge.

6.6 SJ's view on the role of experimental phonetics

The clearest statement of Stephen Jones's thinking is in a handwritten letter to Masao Kanehiro dated 16 September 1932, which Kanehiro then reproduced in facsimile as the frontispiece of a book (Kanehiro 1933). This letter has previously not been known in Britain; it certainly contains opinions rather different from those of Daniel Jones. Whereas, even as early as 1918, DJ took the view that experimental phonetics must be subservient to ear-phonetics (it was to be ‘kept in its proper relation to ordinary practical phonetics’ (DJ 1918b: 132), Stephen Jones has a very different opinion:

> It cannot be too strongly emphasised that Phonetics without Experiment is unreliable to a degree. Most students and teachers of Phonetics do not realize how untrustworthy the unaided ear is. Experiment with them, as with the ancients, is superfluous. But subjective observation is not enough. An organ phonetically defective as the human ear is, needs to be supplemented and aided, if phonetic data are to be ascertained with precision. The beautiful traces in this volume will bring home to the student details which the ear alone would not have noticed. Experimental work thus actually trains the ear and to deny its pedagogical value is to be ignorant of the bearing of Psychology and Phonetics on language teaching.

It may be no more than an accident, but it does seem remarkable that the page containing these outspoken views is somehow missing from the UCL Library copy of the book. Perhaps SJ regretted having expressed himself so freely, and removed the page to avert disagreement. At the time of the book’s appearance, he had another five years left to serve as a member of DJ’s staff.

On the other hand, SJ was himself an accomplished all-round phonetician, and we do not find him disparaging practical phonetic skills in the way that Panconcelli-Calzia and Scripture did. He sought a balance between the two kinds of phonetics, and thus is the heir to Ellis, Lloyd or Rayleigh. In his copy of Scripture (1923), SJ has pencilled in the margin his reservations about Scripture’s interpretations of some kymograms. The note ends ‘…after all, the trained ear cannot be left out of account’.

Notes to Chapter 6

1 Collins and Mees give the date of SJ's death as 1941, an error repeated in the present author's earlier publications (e.g., Ashby 2011).

2 The story of the 'upstairs'/'downstairs' division has passed into general circulation among those who take an interest in the history of phonetics and linguistics in Britain at this period, whether their interest begins with Firth (Plug 2008) or with DJ. It is worth pointing out, however, that Abercrombie is the sole source on which the story is based, and that several other contemporaries interviewed by Collins in the 1980s failed to corroborate the account in any detail. It seems fair to say that Collins and Mees treat Abercrombie's version of the story somewhat uncritically.

3 Birth years shown in italics are the estimates given on census transcripts, since (bearing in mind the commonness of the family name Jones) it has not yet been practicable to locate the birth certificates for all family members; however, the census questionnaire asked for age at time of census (not birth year). Since censuses were generally taken in March or April, a birth-year estimated in this fashion has in general a better than even chance of being wrong by one year. Birth years not in italics have been verified directly against birth certificates.

4 On the birth certificate she appears as 'Anne' rather than 'Ann'. She was probably illiterate, as she marked the register with a cross rather than signing her name.

5 Information on SJ's secondary education comes from the archives of Haberdashers' Aske's Boys' School, where SJ worked as a schoolmaster from January 1903. I am very grateful to Mr. K. G. Cheyney, Honorary Archivist at the school.

6 Among the Tracts (many of which are certainly from his personal collection of materials) are items concerned with both Irish and Breton.

7 Though dated 1914, *Miscellanea Phonetica I* was not widely distributed until 1925. Henry Oswald Coleman (1884–1942) is chiefly remembered as the author of 'Intonation and emphasis' which appeared in that volume (see Cruttenden 1990) but continued to raise a variety of phonetics questions in *Le Maître Phonétique* into the mid-1920s. A volume of his collected poems was published after his death.

8 At the time of writing, the accompanying documentation had not yet been accessioned into the British Library, and I am very grateful to Jonathan Robinson, Lead Curator, Sociolinguistics & Education, for granting me access.

9 Alan Mawer (1879–1942) was a philologist who became a world expert on English onomastics. He served as UCL Provost from 1930. He is described by Hudson and Walmsley (2005) as one of Britain's 'most capable linguists' of the inter-war period. As Collins and Mees point out (1999: 323) '[h]e might have been expected to favour a linguistically-oriented discipline such as phonetics' though in fact relations with DJ and his Department were cool from the outset and increasingly distant. It is not known whether Mawer and SJ were acquainted, though clearly they would potentially have had philological interests in common. Mawer received a knighthood in 1937—the year of this letter and SJ's retirement.

[10] What is preserved is the institution's carbon copy of the letter, bearing a rubber stamp for Mawer's signature. Hence we do not know whether the original may have been signed in a more personal manner. A similar short letter was sent to SJ from the Secretary of the College Committee. Apart from these letters, the only other materials surviving in the UCL Records Office relate to SJ's annual re-appointment and salary throughout the 1930s. His salary was stable at £600 per annum from 1930 to the time of his retirement. No records prior to 1930 have been found. Assistance from the UCL Records Office in locating these materials is gratefully acknowledged.

[11] Founded in 1751, the Society is still in existence. It is interesting to speculate that SJ might well have been a long-term member of the Society. The current membership secretary of the Society kindly answered enquiries from the present author, but membership records from the early twentieth century have not been found, and may indeed not have been preserved.

[12] D. B. Fry recalled that varnished kymograms used to be hung up to dry in the 3-storey stairwell of 21 Gordon Square (personal communication from Adrian Fourcin).

[13] I am grateful to Dr Gwen Awbery for detailed advice on the nature and purpose of this publication, and valuable discussion of SJ's writings on Welsh.

[14] This is not an error: Reid lived to the age of 109.

7

Other UCL phoneticians

7.1 Introduction

Though DJ himself appears to have made no use of the laboratory after 1919, even his closest ‘upstairs’ colleagues nevertheless continued to call upon its services, and to seek assistance from SJ up until his retirement. For example Ida Ward (1880–1949)[1] may qualify as the first UCL phonetician to include instrumental data in a phonetic description of a relatively undescribed language. Her *Phonetic and tonal structure of Efik* (1933) contains 15 superb kymograms, carefully selected to illustrate specific points in the phonetic description. As she tells us (1933: vii), they were made in conjunction with Stephen Jones. Her *Phonetics of English* (1929 etc.) contains some beautifully clear ‘drawings of the vocal cords’ (i.e., representations of various states of the glottis). They were made by the notable medical illustrator A. K. Maxwell (Elliott 1999). Ward says that they represent ‘the vocal cords of Mr. Stephen Jones’. There are several indications in other sources confirming that SJ was indeed very adept at using the laryngoscope mirror.

Produced at about the same time as Ward’s work on Efik, Lilias Armstrong’s[2] (1934) study of Somali has several mentions of kymograph tracings being used to clarify the voicing status and initiation mechanism of various stop consonants. Her later work on Kikuyu (1940), includes a number of kymograms, illustrating, for example, the voicing of intervocalic [h], and the degree of aspiration of plosives.

Ward and Armstrong are thus examples of UCL phoneticians who were happy to mix auditory and experimental evidence in the making of language descriptions. The remainder of this chapter is concerned with three figures who, like Ward and Armstrong, were close associates of DJ, but have publications specifically in the general field of phonetic science. The chapter concludes with a short note on the early career of D. B. Fry.

7.2 George Arthur Noël Armfield (1867-1937)

'G. Noël-Armfield' is remembered as the author of *General phonetics for missionaries and students of languages*, which went through four editions during the heyday of the UCL department (1915, 1919, 1924, 1931). 'Noel' was a given name, but he seems early to have adopted the style with diaeresis and hyphen, giving the impression of a compound family name, and thus is commonly found indexed under N (for example, in Pike 1943).

He was born to middle-class parents in Whitby, Yorkshire, where his father, an architect and artist, worked as the York diocesan surveyor. Collins and Mees (1999: 89) repeat a story that his mother was French, and that he spoke perfect French 'which his French mother had passed to him'. In fact this contradicts both what DJ himself (1909:109) tells us ('his mother came from Yorkshire'), and the evidence of the civil record, which indicates that she was Marion Lee, born in Leeds.[3] DJ further tells us that Noël-Armfield 'studied at London University and at the University of Lille', though it is unclear whether he completed a degree.

He joined the IPA in 1908 and was the author of numerous short notes and reviews in *Le Maître Phonétique* up until 1912, though he does not appear to have renewed his IPA membership after 1913. He is described as a ‘schoolmaster (secondary)’ in the Census of 1911, and was appointed as DJ’s first (part-time) assistant in phonetics at UCL in the same year (Collins & Mees 1989: 89). There are scattered records indicating that he taught phonetics on various summer courses in Britain and on the Continent up to about 1919, but the Preface to the third edition of his book speaks of ‘a breakdown in health’ which has slowed its preparation (1924: vii). His last years seem to have been spent living Cambridge, where his wife pre-deceased him in 1934, but very little information has come to light.

From the earliest edition of the book, Noël-Armfield included five beautifully reproduced kymograms ‘made in the Phonetic Laboratory at University College, London’. They appear to be reproduced approximately full size. They are of considerable interest since they are probably the first output of the UCL laboratory to appear in print (they precede DJ's Royal Institution paper by two years). And whereas DJ's *Outline* was by now ready for publication—Noël-Armfield refers to it as ‘forthcoming’ (1915: vi)—and while it is also true that Noël-Armfield pre-published a number of figures from that work, the kymograms appear to be distinct from any which were used there or elsewhere by DJ (see Figure 7.1).

The kymogram of the Welsh pronunciation of Llanelly (Figure 7.2) is similarly not found elsewhere, and is of additional interest since presumably the pronunciation it represents must at least have been vetted and approved by SJ, even if he was not the speaker.

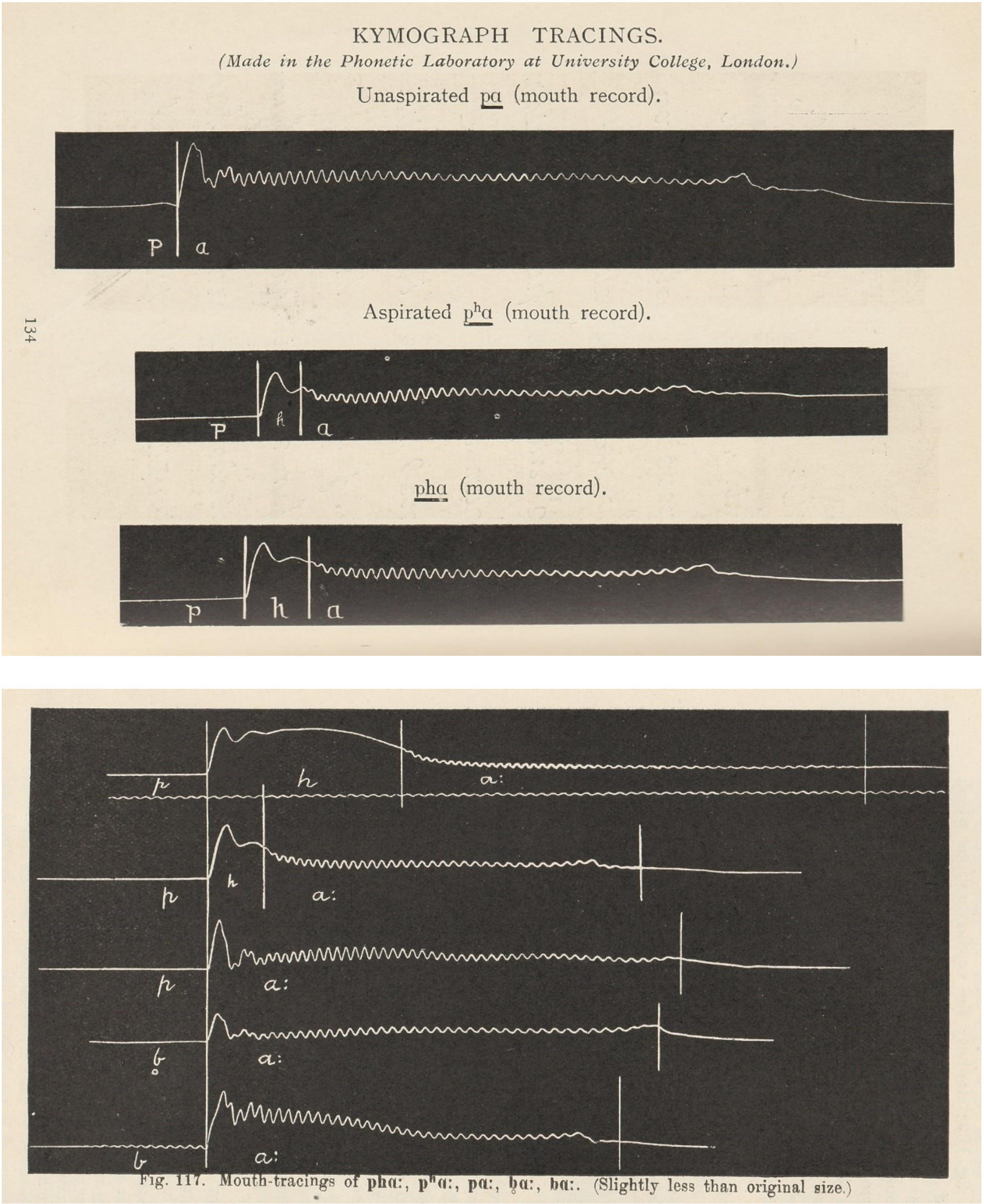


Figure 7.1 Top: Kymograms from Noël-Armfield (1915: 134), below: a tableau from DJ (1918: 172) including comparable items. Careful comparison reveals that despite superficial similarity, the corresponding tracings are in every case different in detail. (Note that in the first three editions Noël-Armfield uses heavy underlining to demarcate phonetic transcription. The 1931 edition adopts the convention of using square brackets).

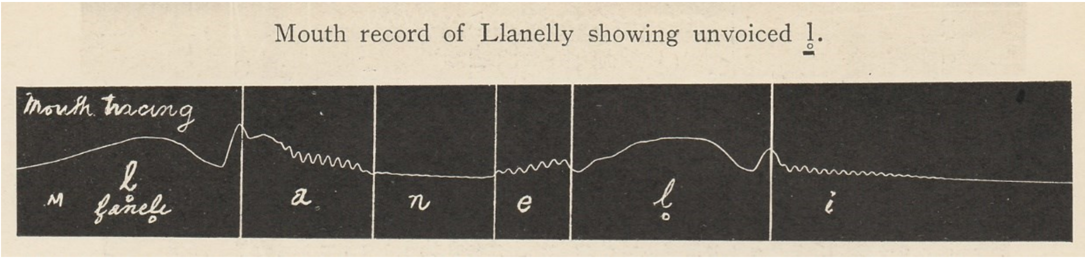
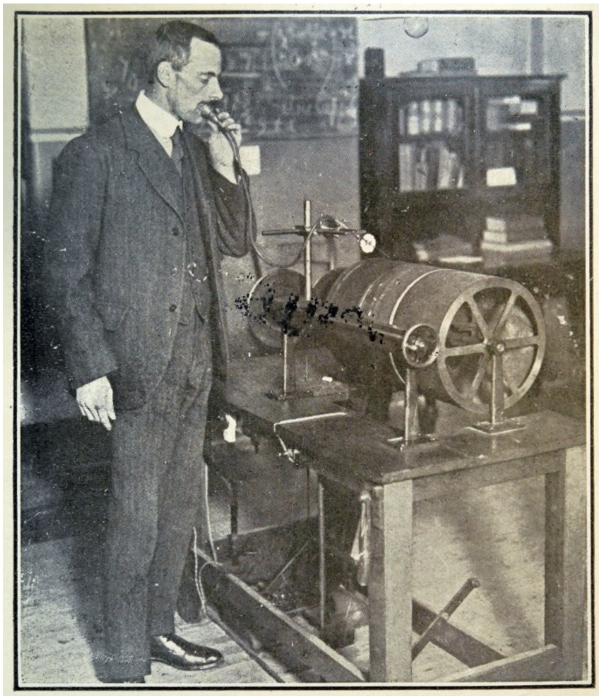


Figure 7.2 Kymogram of *Llanelly* (Noël-Armfield 1915: 135), showing the mouth tracing produced by examples of the voiceless lateral fricative [ɬ]. The [n] shows little or no vibration because the mouth tambour did not respond to nasal energy.

Noël-Armfield thus has a claim to have published the earliest known output from the UCL laboratory. The third edition acquired a frontispiece showing DJ with the kymograph (Figure 7.3), and Noël-Armfield added considerably more material on experimental and instrumental topics, though it remained in an appendix. For the new material, he drew heavily on DJ's *Outline*, but augmented the treatment of palatography with an account of the work of Carruthers (see Chapter 2), whom he had evidently consulted, and reproduced many of Carruthers' palatograms. In the fourth edition the appendix is turned into a chapter (1931: 138–167) with the title ‘A sketch of experimental and instrumental phonetic methods’, though the contents are virtually identical with the third edition’s appendix, and still describe the experimental techniques of 20 or 30 years earlier.



Professor Jones at the Kymograph.

Figure 7.3 The frontispiece used in the third (1924) and fourth (1931) editions of Noël-Armfield.

As discussed in Chapter 5, Noël-Armfield seems to have been the only phonetician linked to UCL who republished any of DJ's 1917 X-rays between 1918 and 1950. In Noël-Armfield (1931), some of the X-rays appear in the experimental-and-instrumental chapter, and others in a new chapter devoted to Cardinal Vowels, a short account of which had first appeared in the second edition (1919: 117–120). That early treatment already contains an interesting statement (retained in the later editions): 'Investigations are being made to determine the absolute musical pitch of the cardinal vowels, and when this is done the result will be invaluable in fixing for all time their acoustic value' (1919: 120). It will be noted that this precedes by at least two years DJ's first encounter with Paget, and confirms that DJ was attempting auditory analyses of vowel resonances, though few of his estimates were ever reported (see Chapter 5). In the 4th edition, Noël-Armfield even ventures a composite diagram traced from the

X-rays showing Cardinal Vowel tongue positions (Figure 7.4). This appears to be original; it is apparently not based on anything actually published by DJ (though of course DJ might have used something like it in teaching or informal presentations). Noël-Armfield's diagram is probably of no great merit, and is based to some extent on guesswork, since he notes that '[u]nfortunately there is no satisfactory photograph available of cardinal [a]'. (The X-ray photograph of [a] which DJ was eventually to publish in 1950 did of course already exist, so we must assume that Noël-Armfield was either not aware of it, or thought it unsatisfactory for some reason). The interest of the diagram resides rather in the fact that Noël-Armfield was attempting to give a bolder and more explicit account of the implied basis of the Cardinal Vowel system than DJ himself was ready to publish.

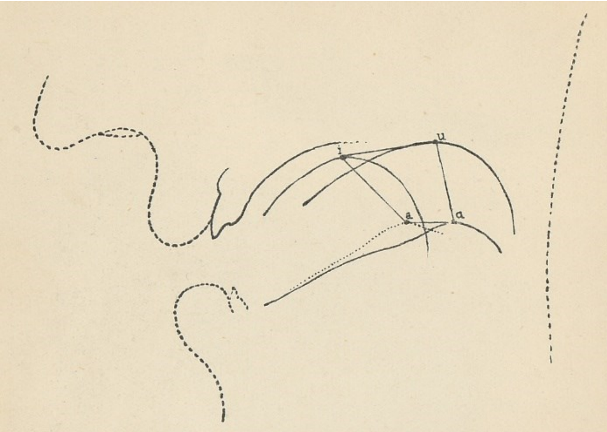


Fig. 14. Tracings of superimposed X-ray photographs of the cardinal vowels [i, u]. The dotted line represents the conjectured position of the tongue for [a]. The lips are in neutral position.

Figure 7.4 Noël-Armfield's composite diagram of Cardinal Vowel tongue positions, with the original caption (1931: 18). He conveys a general impression of the relationship between the X-ray images and the familiar vowel quadrilateral, though probably even at the time the diagram would not have stood up to critical peer review. There are similarities with a questionable diagram and commentary given by DJ's pupil O'Connor (1973: 51).

Though Noël-Armfield is certainly ready to give a nominal place to experimental phonetics alongside the cultivation of practical phonetic skill, he gives the impression of making a compilation of material he does not fully understand, and uses some very odd terminology. For instance in (1924: 147) we find the strange misspelling 'Lériotgraph'. This is corrected in the next edition (1931: 167), which however tells us that 'A comparatively new instrument, called, after its inventor, the Lioretgraph ... gives tracings, 300 times magnified, of gramophonic lines'. In fact, the disc version of the lioregraphe, of which UCL had an example, was produced in 1921 (Anton 2006: 77) and was arguably old-fashioned technology even at the time of its introduction, while the strange expression 'gramophonic lines' presumably refers to the grooves on a gramophone record. A Google search produces no results at all for the collocation "gramophonic line(s)", but about 185,000 for "gramophone groove".

As another example, Noël-Armfield evidently did not understand the details of the X-ray technique at all clearly. He writes 'My former colleague, Mr. Stephen Jones, of University College ... allowed the leaded-laden ribbon (technically called chain) to pass through the nose' (1931: 164). A Google search for "leaded-laden" produces only 4 results, which are apparently mistakes for 'lead-laden'. He is evidently mixing up the ladder of lead plates and the simple (silver) chain—and it is hard to understand how he can have thought that *chain* was a technical term.

Further puzzling neologisms appear in his attempt to classify types of research: 'The manners of research sketched above are what might be termed physico-instrumental. Those of which I shall attempt to deal in the rest of this Chapter are, to use a word of my own, instrumento-physical' (1931: 148). The nature of the distinction he intended to draw is entirely obscure.

In both 1924 and 1931, he ends the section on experimental phonetics with the following avuncular advice:

> May I add a word of warning? Some years ago a popular magazine published an article called, as far as I remember, "Teaching Pronunciation by means of

> Instruments."[4] "With the aid of Instruments" would have been a better expression, but we should remember that instrumental phonetics should be used not so much to teach pronunciation, but rather to teach the teacher how to teach pronunciation. Unless you are doing purely scientific work, don't obtrude the instrument or drag it in at any possible moment. Keep it in the background, and show it just often enough to whet the appetite for more instrumental work. Thousands of teachers of language use phonetics, few have the opportunity of instrumental study. Therefore I venture to suggest that the practical phonetician should, although taking every advantage of instrumental research, not thrust too much instrumentalism down his pupils' throats. A little won't hurt. On the other hand, let the instrumental phonetician, as far as possible, keep to his, the purely scientific side. A degree of overlapping of interests is unavoidable, but let both bear in mind Pliny's proverb:
>
> "Sutor ne supra crepidam."[5]
>
> (1931:167)

DJ was very cautious in his use of the Cardinal Vowel X-rays, and careful in his claims about the Cardinal Vowel system itself. Similarly, though he had doubts about the utility of experimental phonetics, he expressed his reservations only in a very measured way. No doubt he felt that what he wrote must be acceptable to his peers in the academic and scientific community worldwide. Perhaps in the more relaxed and undisciplined writing of Noël-Armfield we get a glimpse of what DJ and his associates said and thought in less guarded moments.

7.3 Wilfrid Perrett (1873-1946)

7.3.1 Biography

Wilfrid Perrett was a close contemporary of Stephen Jones. He was born into a large family in Bridgwater, Somerset. In the 1881 Census, he is the youngest of 5 boys and 3 girls in the household. His father is described as a 'tea dealer, grocer, etc.', and the

1891 Census adds ‘Local Preacher, Town Councillor, Member Of School Board’. In the 1891 Census, Wilfrid, aged 17, is still at home with his retired parents, and is described as ‘scholar and student London University’. According to a family genealogy blog,[6] Wilfrid won a £15 scholarship that year, studied at the University College of Wales in Aberystwyth and gained a University of London degree in 1893. His time at Aberystwyth thus overlaps that of Stephen Jones, and it seems not impossible that they might have become acquainted at this early stage.

Perrett appears to be absent from the 1901 UK Census, and was no doubt in Germany, since he completed a PhD in Jena in 1902, which was subsequently published (1904, etc.). It is an investigation of the origin of the King Lear story, which is still cited in Shakespearean scholarship today. He was back in the UK by 1904, and married a Swedish woman, Hedwig Eleonora Matthiesen, in Bridgwater in 1908. She was 10 years his junior. By 1911 he was Reader in German at UCL and living in Erskine Hill in Hampstead Garden Suburb, a very near neighbour of Stephen Jones (see Chapter 6). He was a member of the IPA between 1912 and 1914, though he seems not to have renewed his membership after the war. Perrett was a member of the German department at UCL, but his lectures on German phonetics were listed on the College Calendar along with those of DJ and his colleagues under the heading of phonetics.

Perrett must have reached retirement age in 1938, and in the 1939 Record he is described as ‘retired university professor’.[7] He died in Cheltenham in October 1946. It appears that Wilfrid and Hedwig had no children.

7.3.2 Publications

Perrett's main work in phonetics is *Some questions of phonetic theory*. This is a somewhat rambling and protracted production that appeared in parts over several years. Part one, with four chapters, was published in 1916, then further one-chapter parts in 1919 and 1923, plus a further addition to the last of those in 1924. He also published a volume *Peetickay* (1920), which can be read alone but is described as a 'sequel' to Part 1 of *Some questions of phonetic theory*. It proposes an ingenious reformed alphabet, and is of great interest, but since it has no experimental content is not considered here. From 1926 Perrett began to issue the similarly-titled *Some questions of musical theory*, which was also published in many slim parts. They continued to appear until at least 1934. The reason for piecemeal publication seems to have been that at least the later parts of *Some questions of phonetic theory*, and possibly all those of *Some questions of musical theory*, were printed on a limited budget at Perrett's own expense. The consequence is that the later parts of *Some questions of phonetic theory* are not easily located. A complete set of all four parts, with marginalia evidently added by SJ (who reviewed Parts 2 and 3 for *Le Maître Phonétique*), is included in the Tracts. In addition, both DJ's and SJ's personal copies of Part 1 are currently in the author's care, having been thrown away from the UCL department at various times.

In the early 1920s, in collaboration with UCL mathematician G. B. Jeffery,[8] Perrett produced the standard English translations of various seminal papers on relativity by Einstein and others (Lorentz *et al*. 1923). Around the same time, the names of some of those he was translating are mentioned in Perrett's own work: 'Need one be a Minkowski or an Einstein to apprehend that if the spatial dimensions of the cochlea are increased, the time dimension must be correspondingly lengthened?' (1924: 92).

It will be convenient to deal separately with the content of Perrett's phonetic work, then with its somewhat extraordinary style, and finally with the reception it was given, particularly DJ's high opinion of it.

7.3.3 Content of Perrett (1916)

For a summary of Perrett's main work, Part 1 of *Some questions of phonetic theory*, one can do no better than to quote in full DJ's brilliantly concise review (less than 250 words) for *Nature*:

> This book forms a notable contribution to the literature on the science of speech. The first chapter exposes some current misconceptions as to the position of rest of the organs of speech. In the remaining three chapters (entitled "Willis on Vowel Sounds," "The Wheatstone Test," and "The Compass of the Mouth") Dr. Perrett deals with the intricate subject of vowel-pitches. He gives examples of the hopelessly divergent results which have been arrived at by different authorities on acoustics, and endeavours, in our opinion with success, to bring some order into the chaos. Naturally the work of those who have contributed to bring about the chaos comes in for strong criticism. Upon Helmholtz Dr. Perrett is particularly severe; he shows that "wherever it bears upon phonetics Helmholtz's book has no right to be considered authoritative," and states that even in other branches of the theory of sound Helmholtz attained a reputation to which the quality of his work did not entitle him. The Helmholtzian harmonic overtone theory of vowel-quality is shown to be untenable by simple experiments described on pp. 79, 81, and 107—experiments which may be performed without difficulty by any phonetically trained person.
>
> The methods by which Dr. Perrett arrives at his interesting table of vowel-pitches (p. 98) appear to us to be sound.
>
> We commend the work to the notice not only of those interested in the science of speech, but also of students of Sound generally.
>
> (1917b: 184)[9]

As DJ implies, the bulk of the work is a preliminary to Perrett's own determinations of vowel resonances, the listing and discussion of which occupy the last dozen pages. Of course, there had been many such estimates made previously. What is different in Perrett's case, however, is that they were made on the basis of voiced vowels (not whispered), and without the assistance of selective acoustic resonators such as had been used by Helmholtz or Felix Auerbach (1856–1933). Perrett trained himself to attend to, and identify, specific harmonics in a sustained vocal note. Then, producing a range of vowels on a known fundamental of 128 Hz, he found the harmonic number, and thus the frequency, of the component he heard most strongly. That in principle yields an estimate of the resonance frequency (in modern terms, F_2), since in general it ought to differ by at most 64 Hz from the component he had identified. He obtained results for 21 vowels, identified with IPA symbols or English keywords (1916: 98–99).

Perrett makes some remarks on the vowel resonances determined by Aikin and Paget in (1923: 60), though he does not seem to have published any further observations of his own on vowel resonances after 1916. The later parts (Chapters 5 and 6) of *Some questions of phonetic theory* (1919, 1923, 1924) are largely devoted to another issue, the theory of hearing. Perrett was particularly concerned to demolish the Helmholtz theory of pitch perception by resonance, versions of which were still widely held and taught in the early twentieth century. Perrett sums up the idea: 'The perception of musical sounds he [Helmholtz] located in the fibres of Corti, regarded as a kind of miniature piano' (1916:47). Perrett was not an anatomist or physiologist, nor was he able to make use of a mathematical or mechanical model of the ear. Instead, his objections were based on common sense and on his own sensations and perceptions when listening to speech or music. To re-phrase his chief objection to the resonance theory concisely in modern language, if pitch perception were based on resonance, each resonator would require such high *Q* (selectivity) that its time response would be too sluggish to match common experience. The abrupt cessation of a sound, for example, would give rise not to a sensation of sudden silence, but to a jangling and ringing in the ears as the resonators continued their slowly-decaying response. Both speech and music would be effectively impossible, because their temporal structure would be hopelessly smeared. It should be impossible, for example, to hear a voiceless stop interval in an intervocalic position.

7.3.4 Verification of Perrett's results

Perrett did not find a very effective way of presenting his results on harmonics in vowels. Figure 7.5 displays them graphically, with a frequency scale in Hz replacing his musical notation. He reported harmonics ranging from the 4^{th} to the 18^{th}.

No audio recording of Perrett is known, so it is impossible to verify his results directly. In an attempt at verification, the author himself recorded a series of vowels corresponding to the IPA phonetic symbols and English keywords given by Perrett. Just prior to the recording, a reference tone of 128 Hz was played from an oscillator. Each vowel was sustained for approximately 2 seconds, and separated from the next by a gap of approximately 2 seconds. The recording was opened in the program ESection[10] and for each vowel in turn the spectrum from 0–5 kHz was averaged over the duration of the vowel. The number was then noted of the harmonic forming the third peak in the spectrum (the peak of lowest frequency is the fundamental, and the next is within the first formant resonance). See Figure 7.6. In all, peaks were identified for 16 vowels,[11] and their frequencies are plotted on Figure 7.5 for comparison with Perrett's.

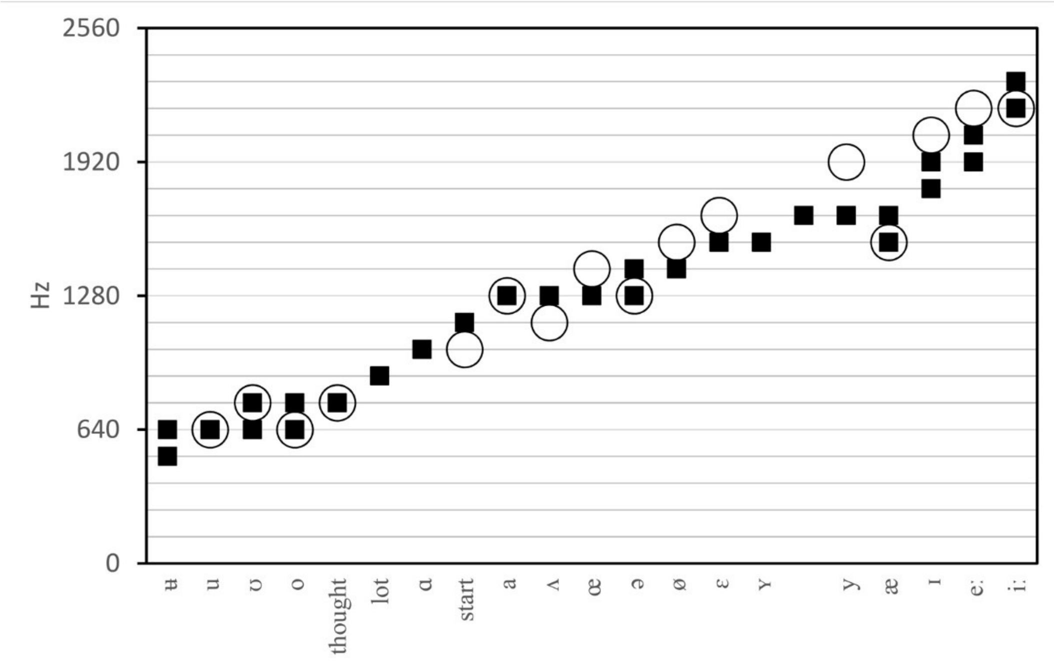


Figure 7.5 The results of Perrett (1916: 98–99), plotted on a linear scale of frequency. The horizontal gridlines are located at intervals of 128 Hz. The filled black markers show the frequency of the predominant harmonic(s) he identified in vowels of the types indicated, when f_0 = 128 Hz. The circles show estimates obtained from a modern analysis of a similar set of vowels spoken by the author.

The level of agreement is remarkable. The same harmonic is selected in 50% of cases, and the adjacent harmonic in all but one of the others. The single case of [y], where the difference is two harmonics, probably arises from differences in the amount of lip-rounding applied. Although Perrett's results represent a remarkable auditory feat, they are in reality just the first step towards the determination of 'the' vowel resonance (i.e., F_2). The one dominant harmonic frequency which is picked out requires to be interpreted in the light of an overall spectrum model.

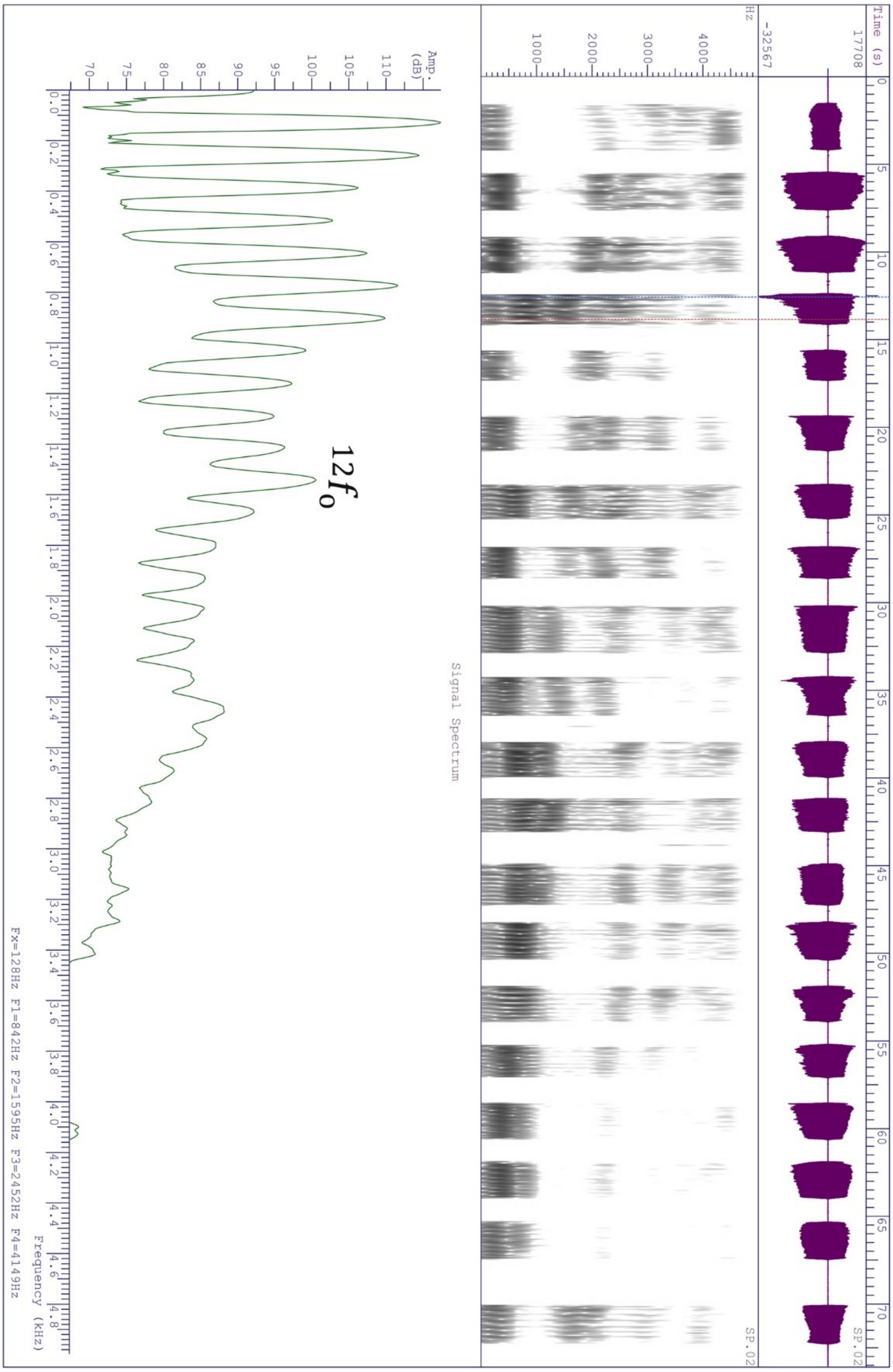


Figure 7.6 Display from ESection, illustrating the analysis of [æ]. At top, waveform and spectrogram of the complete recording: the selected vowel [æ] is the fourth item. Below, the spectrum averaged over the whole vowel duration of approximately 2 seconds, with f_o constant at 128 Hz. The peak harmonic is the twelfth (labelled 12f_o), exactly as Perrett heard.

Perrett was able to make some rough intensity estimates, too (they are not incorporated in Figure 7.5), which would have assisted. But in general the data he was able to gather is probably insufficient to lead to a complete solution. Perrett was well aware of these limitations:

> Remembering the difference between a free and a forced vibration, we also see that a single table of this kind cannot be expected to give the inherent pitch of each vowel straight away. But it affords a valuable corroboration of what has been observed in whispering…
>
> (1916: 98)

Perrett made no attempt to identify the lower resonance, and his method of working could have led only to very coarse estimates of F_1 anyway, since the spacing between harmonics becomes proportionately greater in relation to the resonance frequency as the resonance frequency falls.

7.3.5 Perrett's style

Perrett's written style has none of the virtues of clear scientific prose. He is rambling, satirical, immoderate, exclamatory, confrontational, fragmentary, highly allusive—and often very funny. Though he was Reader in German, he displays a powerful and pervasive anti-German bias, seen most clearly in the contempt and ridicule he heaps on Helmholtz and his followers (including British followers such as McKendrick). He represents Helmholtz as having appropriated his ideas on vowels from Willis and Wheatstone, without proper acknowledgement, and failing to understand them correctly.

A few examples will give a flavour of his extraordinary style. For instance, in this passage, he is making the point that perceived vowel quality is not the same thing as perceived resonance pitch:

> The change of quality from **i** over **ɑ** to **u** is a continuous function of something; the fall in the whisper-pitch along the same track is also a continuous function of something. The two are not the same, since any ear can hear a difference in the quality of whispered vowels, and some ears can also hear at the same time a difference in the pitch of the whisper. Are the two related, and if so, how? That is the question. Which Helmholtz burked. It certainly is a poser. So was Helmholtz.
>
> (1916: 82–83)

We notice the change from long sentences to very short (from 47 words to 3), the jocular allusiveness ('That is the question'), the switch from literary and scientific diction to discordant slang terms (from 'continuous function'[12] to 'burke' and 'poser'), the cheap pun on two meanings of 'poser', and the outspoken rudeness (Helmholtz, usually regarded as one of the greatest German scientists of the nineteenth century, was a fraud).

In other places, the language is fanciful and the style borders on fairy-tale. The exploration of the range of vowels is imagined as a sea-voyage: 'The cry of "Land ho!" came from the look-out some time ago … We have had a very quiet voyage of something under two octaves' (1916: 68). Imaginary interlocutors interpose comments: 'At this point Professor Dr. Dormouse, whose education was finished at German universities before it began, will ejaculate "Subjective", and go to sleep again' (1916: 77).

The point of dwelling on Perrett's style is to throw light on the very diverse evaluations his work provoked.

7.3.6 Reception of Perrett's work

It seems likely that Perret's work was never widely read, and still less understood. A remark in the Preface to Part 2 of *Some questions of phonetic theory* (1919: 2) suggests that Part 1 had sold just 55 copies since 1916. It did attract some reviews, and Perrett seems also to have sent copies to selected people, soliciting comments. Perrett printed selections from these reviews and comments on the back page of (1919)[13], characteristically (and perhaps even proudly) including unfavourable assessments along with the endorsements.

Jespersen politely avoided the issue by admitting he had read only the beginning; a Swiss phonetician, Edgar Jacot, was struck mainly by Perrett's humour ('for the first time in my life I caught myself laughing heartily again and again while reading about phonetics'); respected Dutch phoneticians L. P. H. Eijkman and H. Zwaardemaker[14] said that the work was suggestive and stimulating, though open to counterargument. But writing in *Science Progress*, October, 1918, p. 342, an anonymous reviewer 'E. H. B.'[15] found it 'utterly unconvincing … can scarcely be called *science*, or be mistaken for an aid to its *progress*'. Perrett was no doubt greatly entertained by these divergent assessments.

7.3.7 Mutual regard of DJ and Perrett

Among the comments which Perret assembles, only DJ's review in *Nature* is unhesitatingly complimentary. There is evidence that DJ went on recommending his students to read Perrett up to the time of his retirement thirty years later.[16]

Equally, Perrett had a high regard for DJ, because it was DJ who had first alerted him to the audibility of separate harmonics in sustained voiced sounds, on 4th October 1915.

> On that date—a red-letter day in the history of phonetics as I read it—Mr. Daniel Jones asked me to listen for harmonics while he sang a note with a series of changes in the mouth adjustment. To my astonishment and delight I immediately heard a delicate arpeggio ascending and descending the harmonic scale through some six or seven steps, the tones of the familiar sequence following one on the other like a peal of fairy bells.
>
> (1916: 79)

What Perrett describes here matches DJ's notes from 1915. DJ also demonstrated the technique of making harmonics audible at the conclusion of his 1917 RI Discourse. When DJ made a fair copy of his notes on harmonics more than 30 years later, he proudly mentioned Perrett's phrase 'red-letter day', showing that Perrett's recognition of his role meant a great deal to him.

While DJ was able to render voice harmonics audible with a sort of vocal party-trick, and to attach estimates of pitch to those selectively picked out by special mouth adjustment, it is plain that in the year between October 1915 and October 1916 (when *Some questions of phonetic theory* was completed), Perrett had gone much further. He had trained himself to identify those harmonics which were reinforced only by virtue of lying near the second-formant resonance of a normally-produced vowel.

DJ must have recognised that Perrett's achievements in hearing vowel harmonics constituted a virtually unparalleled auditory *tour de force*, and his estimate of Perrett was perhaps disproportionately influenced by that. But his apparently uncritical high regard for Perrett is remarkable, especially since Perrett's immoderate style contrasts so completely with his own, which was concise, quiet, polite and reserved.

7.4 Arthur Lloyd James (1884-1943)

7.4.1 Life and career

An outline of Lloyd James's life and work is given by Carley (2013). Lloyd James was born barely a stone's throw from the childhood home of SJ, though the 12-year age difference, and the class mismatch between their families, make it unlikely that they were acquainted before 1920 when Lloyd James joined DJ's department. He stayed at UCL until 1927, when he moved to the School of Oriental Studies (later the School of Oriental and African Studies, SOAS), becoming Professor of Phonetics there in 1933. From 1924 he made numerous early radio broadcasts,[17] including talks on phonetics, and in 1926 he was a founder member of the BBC's Advisory Committee on Spoken English, later becoming its Secretary. He came to be regarded as an authority on the application of phonetics to broadcasting and delivered papers on the topic at both the second ICPhS (1935) and the third (1938).

7.4.2 Broadcast perception tests

Much of Lloyd James's output concerned the selection of suitable accents for broadcasting, and adjudicating on forms to be adopted by the BBC for words of disputed pronunciation. But his interests also extended to questions concerning the intelligibility of speech when reproduced via the radio and other channels. His early broadcasts included some attempts to conduct perception tests, inviting written responses from listeners. Disappointingly, his report (1925) of an experiment on the identification of voiceless fricatives [f θ s ʃ x ɬ] does not contain sufficient detail to permit a complete confusion matrix to be constructed (Table 7.1), though the analysis can be taken at least a little further than Lloyd James's own conclusions. He reported that only [ʃ] could be recognised with a fair degree of success (50 out of 70 correct responses). But since the probability of a correct response at random is only one in six (0.167), a binomial test reveals that the success rate of 18 out of 35 obtained for [x] is also very highly significant ($p<0.00001$). At the same time, the extremely low success rate of 3 out of 70 obtained for [s] is significantly *worse* than chance by a very wide margin. This, and the fact that [s] is not among the erroneous responses given for any target, suggest that for some reason participants had a bias against using [s] as a response category at all. Oddly, the stimuli in the experiment seem to have been friction noises performed in isolation rather than fricative consonants embedded in nonsense syllables or words, and no results are given from any live-speech controls.

		TARGET					
		f	**θ**	**s**	**ʃ**	**x**	**ɬ**
RESPONSE	**f**	12	e_1				
	θ	e_2	7	e_3			
	s			3			
	ʃ	e_4			**50**		
	x		e_5	e_6		**18**	
	ɬ	e_7		e_8			3
	h	e_9	e_{10}	e_{11}			
	wh	e_{12}					

Table 7.1 An attempt to analyse the incomplete results reported by Lloyd James (1925) using a confusion matrix. e_1 to e_{12} represent numbers of error responses falling in various categories and are all unknowns (though we can infer that $0<e_i<64$). The error response categories for the targets [ʃ x ɬ] were not reported. Note that h and 'wh' seem to have been accepted as responses, though they were not targets. There were 70 separate judgements for each of [f θ s ʃ], and 35 for each of [x ɬ].

7.4.3 'Speech signals in telephony'

Lloyd James's involvement with questions of intelligibility continued with work on the screening of prospective telephone operators, reported in another paper for *Le Maître Phonétique* (1929). But the discussion is disappointing, failing to distinguish acoustic and linguistic sources of difficulty. Lloyd James uses the term 'acuity of hearing' without clarifying whether he means speech discrimination ability or simple sensitivity to sound as such (1929: 14). Indeed, it is not clear that he grasped the distinction.

Despite its promising title, Lloyd James's book *Speech signals in telephony* (1940) is also a profoundly disappointing work. According to the Preface, '[the] book has been written to help, in the first place, those who have the important duty in war-time of establishing communication by telephony'. The book is described as arising from work done by and for 'a unit' established in the Royal Air Force for training and research 'as to the best methods of training operational officers and pilots in radio-telephonic speech'.

The book is non-technical, and much of it consists of a somewhat superficial treatment of communications systems in general. On page 41 there is mention of 'a recent investigation by the R.A.F. R/T Speech Unit', tending to indicate that some practical intelligibility testing had been done, but if so, the results are certainly not reported.

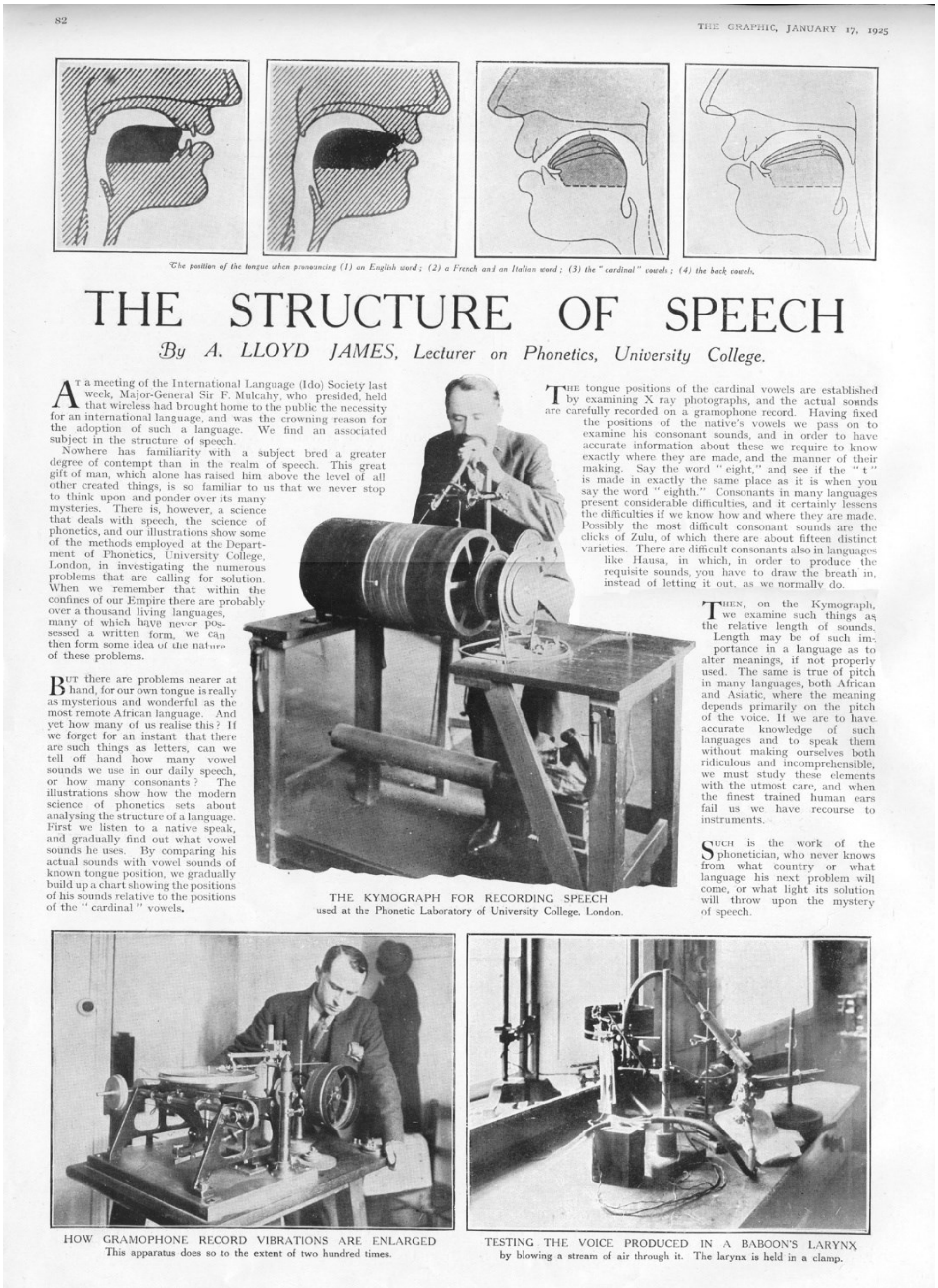
There is passing mention (p. 32) of words used in spelling aloud, given that many letter-names are easily confused, but surprisingly no complete communications alphabet is put forward.

Lloyd James's ideas on English rhythm are summarised on pp. 24–27. Speech rhythms (i.e., across languages) are said to be of two kinds: 'Morse code rhythm' (English, Arabic and Persian are said to be good examples), and 'machine gun rhythm' (French and Telugu are named as good examples). Lloyd James asserts (p. 25): '[i]n Speech signals as in Morse signals, the better the rhythm the better the signal'. This is an unsupported empirical claim: he does not actually present any evidence that rhythmic speech has any advantage in intelligibility.

The book concludes with a number of recommendations: to plan before speaking, to speak without great variations in tempo or pitch, not to tail off at the ends of words and phrases. This advice is unexceptionable, but seems rather tame as a conclusion to 49 pages of text, and paltry if representative of the RAF Speech Unit's efforts, since any simple elocution manual would have given the same advice.

7.4.4 Lloyd James and the laboratory

More than one published photograph shows Lloyd James with the kymograph, and in a 1925 piece in *The Graphic*,[18] a popular illustrated weekly paper, he is shown with the UCL kymograph and also with the lioretgraphe (See Figure 7.7). The content and style of the article no doubt resemble those of the radio talks he was giving around the same time. No radio talk with the exact title 'The structure of speech' has come to light, but Lloyd James did broadcast one called 'The structure of English speech' via the London station 2LO on Tues 23 June 1925, following the 22:00 news and weather forecast (which he also read).



82 THE GRAPHIC, JANUARY 17, 1925

The position of the tongue when pronouncing (1) an English word; (2) a French and an Italian word; (3) the "cardinal" vowels; (4) the back vowels.

THE STRUCTURE OF SPEECH

By A. LLOYD JAMES, Lecturer on Phonetics, University College.

At a meeting of the International Language (Ido) Society last week, Major-General Sir F. Mulcahy, who presided, held that wireless had brought home to the public the necessity for an international language, and was the crowning reason for the adoption of such a language. We find an associated subject in the structure of speech.

Nowhere has familiarity with a subject bred a greater degree of contempt than in the realm of speech. This great gift of man, which alone has raised him above the level of all other created things, is so familiar to us that we never stop to think upon and ponder over its many mysteries. There is, however, a science that deals with speech, the science of phonetics, and our illustrations show some of the methods employed at the Department of Phonetics, University College, London, in investigating the numerous problems that are calling for solution. When we remember that within the confines of our Empire there are probably over a thousand living languages, many of which have never possessed a written form, we can then form some idea of the nature of these problems.

But there are problems nearer at hand, for our own tongue is really as mysterious and wonderful as the most remote African language. And yet how many of us realise this? If we forget for an instant that there are such things as letters, can we tell off hand how many vowel sounds we use in our daily speech, or how many consonants? The illustrations show how the modern science of phonetics sets about analysing the structure of a language. First we listen to a native speak, and gradually find out what vowel sounds he uses. By comparing his actual sounds with vowel sounds of known tongue position, we gradually build up a chart showing the positions of his sounds relative to the positions of the "cardinal" vowels.

The tongue positions of the cardinal vowels are established by examining X ray photographs, and the actual sounds are carefully recorded on a gramophone record. Having fixed the positions of the native's vowels we pass on to examine his consonant sounds, and in order to have accurate information about these we require to know exactly where they are made, and the manner of their making. Say the word "eight," and see if the "t" is made in exactly the same place as it is when you say the word "eighth." Consonants in many languages present considerable difficulties, and it certainly lessens the difficulties if we know how and where they are made. Possibly the most difficult consonant sounds are the clicks of Zulu, of which there are about fifteen distinct varieties. There are difficult consonants also in languages like Hausa, in which, in order to produce the requisite sounds, you have to draw the breath in, instead of letting it out, as we normally do.

Then, on the Kymograph, we examine such things as the relative length of sounds. Length may be of such importance in a language as to alter meanings, if not properly used. The same is true of pitch in many languages, both African and Asiatic, where the meaning depends primarily on the pitch of the voice. If we are to have accurate knowledge of such languages and to speak them without making ourselves both ridiculous and incomprehensible, we must study these elements with the utmost care, and when the finest trained human ears fail us we have recourse to instruments.

Such is the work of the phonetician, who never knows from what country or what language his next problem will come, or what light its solution will throw upon the mystery of speech.

THE KYMOGRAPH FOR RECORDING SPEECH
used at the Phonetic Laboratory of University College, London.

HOW GRAMOPHONE RECORD VIBRATIONS ARE ENLARGED
This apparatus does so to the extent of two hundred times.

TESTING THE VOICE PRODUCED IN A BABOON'S LARYNX
by blowing a stream of air through it. The larynx is held in a clamp.

Figure 7.7 Lloyd James's one-page account of phonetics applied to language description (1925). He is seen with the large UCL kymograph (centre) and the lioretgraphe (lower left). The experimental setup at lower right undoubtedly illustrates work in fact done by Negus and SJ.

Overall, the *Graphic* piece implies a much more ‘scientific’ orientation in phonetics as applied to language description than was really the case, at least in London. He writes ‘… there is … a science that deals with speech, the science of phonetics’, and ‘The illustrations show how the modern science of phonetics sets about analysing the structure of a language’. This seems to be wishful thinking rather than a description of the real situation. His own contributions to the phonetic description of Yoruba (1923), Hausa (1925c), Marathi (1928) and Pashto (Morgenstierne & Lloyd James 1928) do not make use of a single instrumental measurement, and the only mention of the kymograph seems to be a remark in Morgenstierne & Lloyd James (1928: 55) pointing out that the instrument has *not* been used: ‘… the unvoiced stops are probably pronounced with unclosed glottis; but this question cannot be finally decided without kymograph tracings’. Later, Lloyd James reviewed Beach’s work on Hottentot (Lloyd James 1938) but does not even mention the instrumental data which form a prominent part of Beach’s work.

Lloyd James is listed as a member of the International Society of Experimental Phonetics in 1931, so was evidently ready to subscribe and support—though in reality he was something of a spectator. Perhaps he chose to be photographed with the kymograph not because he used it regularly, but simply because it provided a striking focal point for a good picture, and lent some scientific *cachet* to the phonetician’s activities.

7.5 Dennis Butler Fry (1907-1983)

A small number of early publications of Dennis Fry fall within the period covered in this study. He had been appointed as SJ’s successor in the UCL laboratory, and his early work shows connections and continuity with SJ's own. The Serbo-Croat phonetic reader (Fry & Kostić 1939) parallels SJ's similar work on Welsh (1926b), and no doubt served to establish Fry's credentials as a capable all-round phonetician outside the laboratory. A review of some American experimental work on speech respiration (1938) is in the same vein as SJ's later reviews (SJ 1931, 1932). A copy in the Tracts of the work reviewed, Gray (1936), actually has SJ's autograph, so perhaps SJ himself received the work for review, and passed the task to Fry. Fry's paper at the third ICPhS (Fry 1938) is concerned with the use of the sensitive flame (a speciality of SJ) in speech training, and acknowledges the loan of several jets from SJ.

But after 1940, Fry's work, and that of the UCL laboratory, took different directions altogether. Fry's PhD (1947) deals with the testing of speech reception. The focus shifted to perception, hearing and deafness.

Notes to Chapter 7

1 Ida Caroline Ward (1880-1949): see Tucker (1950). She joined the UCL Department in 1919 and followed her younger colleague Arthur Lloyd James to the Institute of Oriental Studies in 1932, becoming Head of the African department on its creation in 1937.

2 Lilias Eveline Armstrong (1882–1937). Her work on Kikuyu was completed by Beatrice Honikman (1905–1997) and published posthumously (Armstrong 1940).

3 Neither of *her* parents was French, either. But as a young child Marion had a governess (her father's cousin) who was British but born in France—so perhaps French was used in the nursery and there may have been childhood visits to France.

4 Perhaps the article referred to is Ellison (1905).

5 'A cobbler should stick to his last'. The Latin tag is a common, slightly modified version of *ne supra crepidam sutor iudicaret* (literally, 'a shoemaker should not judge beyond a sandal'). Cf. Simpson & Speake (2008) s.v. **cobbler**.

6 http://p-rr-tt.org.uk/wordpress/?p=20, consulted 5 August 2016. This information has not been independently confirmed.

7 1939 Record: Ref: RG101/7065J/005/40 Letter Code: WPIB

8 George Barker Jeffery (1891–1957), a mathematician and educationist; see Titchmarch (2004).

9 This review is a previously unnoted addition to the DJ canon. It is missing from the bibliographies given in Abercrombie *et al*. (1964) and Collins & Mees (1999).

10 A component of the Speech Filing System (SFS). (Huckvale 2013).

11 The 5 omitted are [ɑ] and LOT (for which no dominant harmonic could be identified), [ʉ] [ʏ] for which there are no established Cardinal values), and a vowel which Perrett represents [ɛ́], where it is unclear what he intended. Perhaps he meant a vowel intermediate between [ɛ] and [e]—i.e., effectively the DRESS vowel.

12 The mathematical terminology is probably from Whitehead (1911), a work which Perrett references (1916: 18).

13 The page is unnumbered but is the verso of p. 39.

14 Leonard Pieter Hendrik Eijkman (sometimes spelled Eykman). (1854–1937) and Hendrik Zwaardemaker (1857–1930).

15 Almost certainly Professor Edwin Henry Barton FRS FRSE (1858–1925), author of *A textbook on sound* (1908), as Perrett himself must have guessed.

16 A notebook which belonged to Olive Tooley (1911–1996) records DJ's advice that Perrett's work was very important, but would have to be consulted in the British Museum. (Air-raids in 1940–1941 destroyed many of the phonetics holdings in the UCL Library).

17 They can be found via their *Radio Times* listings (http://genome.ch.bbc.co.uk/).

18 I am grateful to Professor John Coleman for drawing my attention to this item.

8

Speech scientists outside UCL

8.1 Edward Wheeler Scripture (1864–1945)

8.1.1 Biography

The active career of Edward Wheeler Scripture extends over the whole 50 year period covered in this thesis (his earliest publication dates from 1891, and the last from 1938). He qualifies for at least some consideration as a speech scientist active in Britain because parts of his long and eventful life—though not by any means the most important and productive parts—were passed in Britain. His *Elements of experimental phonetics* (1902) has already been mentioned, but both that survey and all his most significant and original contributions to speech research, notably his study of ‘speech curves’ (1906), were completed long before he first came to Britain around 1912. By the time he did come, he had added a medical qualification and an interest in psychiatry to his training in psychology and experimental phonetics, and he was active chiefly in the field of speech and language pathology and therapy (which is not treated in any detail in this thesis).

There are numerous biographical sources on Scripture, though most attention has been directed to the very earliest part of his career, during the establishment of experimental psychology in the USA, and before Scripture began to focus exclusively on speech (Boring 1957: 524–528; Berry 1965; Black 1980; Sokal 1980). A general outline of his life and career is given by Sokal (2000). Scripture's own biographical memoir (1936) is also of great interest, though demonstrably unreliable and embroidered in some respects. The present account adds some new details from the civil record and other UK sources.

He was born in Mason, New Hampshire, and grew up in New York City. After graduating in 1884, he studied in Berlin, Zurich, and then at Leipzig, where in 1891 he gained a PhD in experimental psychology under the direction of Wilhelm Wundt. While in Germany he married May Kirk (1864–1943), a fellow American who was studying vocal music in Germany. They were to have three children. The couple returned to the USA and by 1892 Scripture was 'Instructor in Experimental Psychology' at Yale, and began to issue a series of *Studies from the Yale Psychological Laboratory*. Scripture became a strident advocate of experimental approaches and angered distinguished contemporaries by disparaging the 'armchair psychology' he accused them of indulging in. Two books *Thinking, feeling, doing* (1892), and *The new psychology* (1897) led to controversy, unfavourable reviews and accusations of plagiarism.

By 1899 the main work of his laboratory (by now he was the Director) was devoted to speech. *Elements of experimental phonetics* appeared in 1902, and in the same year he received the Carnegie Institution grant of $1600 which was to fund the worked reported in *The study of speech curves* (1906). But his relations with colleagues deteriorated to such an extent that in 1902 he was sacked from Yale with one year's pay.

The Scriptures returned to Germany; and Scripture gave lectures in Berlin and Marburg. He gained an MD from Munich in 1906. On a visit to Zurich he met Carl

Jung, who introduced him to the ideas of Freud by lending him a copy of Freud's *Die Traumdeutung* ('*The interpretation of dreams*'), (1899). After an initial resistance—Scripture apparently told Jung that 'any fool could write a better book on dreams' (1936: 248)—he began to read his way through the psychoanalytic literature. He gradually 'became converted' and at some point underwent an analysis himself in Vienna. He wrote 'The dream analysis revealed a world of indescribable fascination and led to a complete revision of my psychological views' (1936: 248). In fact, he remained a vociferous advocate of experimental methods and quantification, but added a (modified and idiosyncratic) psychoanalytic dimension when it suited him.

In the obituary of Rousselot which Scripture contributed to *Nature* (1925: 165) he says 'I had the privilege of spending a few weeks in Rousselot's laboratory in the early days; his spirit and his methods have been the inspiration in all I have attempted since'. It is unclear exactly when this occurred. Scripture's lengthy autobiographical essay (1936) has no mention of their meeting. The extensive acknowledgements at the beginning of Scripture's *Elements* (1902: vii–viii) make no mention of him. One short article by Scripture appeared in Rousselot's journal *La Parole* in 1903, suggesting that the contact was around that date.

The Scriptures returned to the USA in 1906, and May Kirk Scripture began to teach speech correction at Teachers College, the graduate school of education for Columbia University. She was to have a long career in speech pathology and remediation, with a series of publications extending into the 1930s (Berry 1965: 9). Scripture himself became associate psychiatrist at Columbia University Medical Centre; working on children with speech disorders. In 1912 he published a book *Stuttering and Lisping*, but in the same year his wife sued for legal separation, citing

Ethel King, Scripture's laboratory assistant. The affair was treated sensationally in the press, and Scripture was represented as having ill-treated and deserted his wife, sold the furniture, and fled to England. It is unclear how much of this was in reality a stratagem to engineer a separation within the legal framework of the day. They certainly remained married until her death in 1943, and it was only then, in the last months of his life, that Scripture remarried in Britain.

Scripture's whereabouts for the next 2–3 years are not altogether clear. Perhaps, as *The New York Times* reported, he had indeed 'absconded' and was 'concealing himself'. His membership record with the IPA shows a USA address up to 1913, but a UK address in 1914. He obtained the British medical qualifications MRCS (Eng) and the LRCP (Lond) in 1914 (by this date the two were gained in a single 'conjoint' examination)[1]. He appears in *The Medical Register* from 1915. From at least 1923 his registered address as a medical practitioner is 62 Leytonstone Road in London.

Scripture appeared in *Who's who* from 1917. According to Eldridge (1968: 88), he began treating stammering patients at the West End Hospital for Nervous Diseases in 1919. But he was to some extent dividing his time between London, Germany and Austria. In January 1921 *Nature* (Jan 13) reports that Scripture is now resident in London,

> …where he has for some years been engaged on studying records of speech in epilepsy, general paralysis and other nervous diseases. Prof. Scripture has recently returned from Germany, where he has been lecturing on experimental phonetics applied to the study of English. These were the first lectures delivered in Germany since the war by a professor from a former enemy country.

The following month *Nature* reports that Scripture has been appointed to the faculty at Hamburg 'for the summer semester'. He was to lecture on 'English philology and experimental phonetics'. Among a number of publications in *Volta Review* around this time was his account of the Hamburg phonetics laboratory (1921a).

Scripture had a knack of keeping himself visible, seizing opportunities to deliver talks, and getting announcements of his activities into *Nature* or *The Times*, and he did not hesitate to turn a temporary association into a prestigious-looking affiliation. In a short biography (probably written by Scripture himself, 1931), we are told that 'in 1922 he became Lecturer in Phonetics in King's College London', though elsewhere the appointment is described specifically as 'Honorary'. *Nature* for 7 October 1922 reports a lecture by Scripture in Vienna, and describes him as being 'of London and Hamburg'. By 1923 he is styled 'Professor of Experimental Phonetics, University of Vienna' and a letter to *Nature* that year has 'University of Vienna' in the byline. But *The Times* continues to list various lectures in London over the period 1923–1925.

In 1924, Scripture invited the pioneer speech therapist Winifred Kingdon Ward (1884–1979) to assist him at the West End Hospital. She was already developing her own views on the nature and treatment of stammering, and the two came into conflict since she regarded his 'octave twist' method of treatment as an ineffective 'gimmick'. As a consequence, she resigned from the West End Hospital in early 1926, but retained appointments at other London hospitals, where she continued to develop her own methods of treating stammering. She was later to publish a major work on the subject (Kingdon Ward 1941), much more balanced and better researched than anything Scripture wrote on the topic. A few months after her resignation, Scripture himself departed for Vienna, whereupon she returned to the West End Hospital as Director of

Speech Therapy. The notable neurologist Cecil Charles Worster-Drought (1888–1971) was overall Director of the Speech Department.

Over the years 1928–1932, Scripture was the main mover in an association which termed itself the 'Internationale Gesellschaft für Experimentelle Phonetik', 'International Society of Experimental Phonetics' or 'Societé Internationale de Phonétique Expermentale'. Scripture founded the Society in 1928 following a small meeting in The Hague, and became its first President. It held a congress in Bonn in 1930 and a report was published under the editorship of Menzerath (1930). Its contents vary from abstracts to relatively complete short papers. The volume includes the statutes of the association (pp. 115–119) and a list of 175 members (pp. 120–124). Accounts of the congress appeared in *Nature* and *Le Maître Phonétique*.

The Society produced a *Bulletin*, printed within the journal *Archives Néerlandaises de phonétique experimentale*, (which from 1930 onwards added to its title page a note that it also served as the organ of the Society). The *Bulletin* was meant to be annual, but after the first two issues in 1930 and 1931, there was a long gap before the third (and final) one in 1936. The contents of the *Bulletin*, though unsigned, give every indication of having been written by Scripture. The Society also started a separate journal, *Zeitschrift für Experimental-Phonetik*, with Scripture as Editor, to be published 'at intervals', though only the first two numbers of a single volume seem to have appeared. The front matter of the *Zeitschrift* (in German) claims it is the official newsletter (*das amtliche Mitteilungsblatt*) of about 15 laboratories, which are listed, along with the names of their directors. Included in this list is 'The Speech Laboratory of the West End Hospital for Nervous Diseases, London (Worster-Drought)'. Scripture himself appears as the director of the Vienna Laboratory. The address of the

'Secretariat' of the association is given as '73 Welbeck Street, London W.1, England'—which was in fact the street address of the West End Hospital.

Scripture's role within the Society looks suspiciously like an attempt to hijack and control the activities of experimental phoneticians worldwide. This impression is reinforced by the extraordinary sequence of events recounted in the front matter of the proceedings of the first ICPhS (International Congress of Phonetic Sciences 1932: 1–4). Following the first congress of the International Society of Experimental Phonetics in Bonn in 1930, Scripture proposed that a second congress should be held in the Netherlands in 1932. This suggestion was taken up by a committee in Amsterdam, which however greatly widened the remit to include a range of related sciences, and phonology (the 'Prague school' is specifically mentioned). A prospectus was sent out for a meeting to be held 3–7 July 1932.

> Immediately after the distribution of this … Professor Scripture decided not to hold a Congress of the International Society of Experimental Phonetics in 1932. At the time several speakers had already promised to read a paper. Besides, several members of the Council of the International Society and ordinary members thought it advisable to hold the Congress at the fixed date.
>
> So the preparation was continued and in April 1932 a provisional program could be distributed. About the same time Professor Scripture sent an announcement that no Congress would be held, which in a few cases was misunderstood as referring to the International Congress of Phonetic Sciences which was no longer identical with the Second Congress of the International Society of Experimental Phonetics.
>
> (1932: 4)

The Amsterdam committee effectively created the template of the International Congress of Phonetic Sciences which has remained to the present day. It is hard not to see Scripture's actions as an attempt to sabotage a development that was not to his liking. Though he sent a brief written greeting which was read out in one of the sessions, Scripture played no part in the 1932 Congress, which concluded (1932: 212) with the establishment of a Permanent Council to plan the second Congress on the same lines as the first in Amsterdam. Little is heard of the International Society of Experimental Phonetics between 1932 and 1935. It seems that Scripture may have continued as President, through under the Society's statutes an election for President should have been held in 1932.

In 1933 Scripture, now approaching 70, returned to London and spent the remaining 12 years of his life in Britain. Berry (1965: 9) says that Scripture's later publications show his address as 'University of London Phonetics Laboratory', though the present author can only find '(The) Phonetic Laboratory'; that is, 'Phonetic' without *–s*, and no mention of 'University of London'. Sokal (2000) appears to inherit this error from Berry. In fact, the 'Phonetic Laboratory' was simply 62 Leytonstone Road, Scripture's premises listed in T*he Medical Register* since 1923.

8.1.2 Frieda Janvrin

In this final period, Scripture worked with a protégée, Olga Frieda Janvrin (1901–1989). She was born Olga Frieda van Homan, but changed her name to Janvrin by Deed Poll in 1927. Little is known about her education and training, but she appears to have entered the then little-regulated profession of speech therapy. A number of her publications have been identified, and one of these (Janvrin & Worster-Drought 1932: 1384) describes her as 'Assistant in the Speech Department, West End Hospital for Nervous Diseases'. An Australian newspaper report of 1933 describes her as 'the director of the speech research laboratory of the West End Hospital for Nervous Diseases, London'.[2]

Early in 1934 she had a child out of wedlock, and the presumption must be that Scripture was the father (or if not, he decided from the outset to assume the role of father), since though the birth was registered in the name Janvrin, the given names were Nicholas Edward. The couple began to live together, and acquired a house in Bristol. But whether they represented themselves as man and wife, or as related in some other way, is uncertain. In the 1939 Register, the son's name is given as Nicholas Scripture; Frieda's name, entered first as Janvrin, is changed to Scripture (though her marital status is given correctly as 'Single'), and Scripture's own marital status is given (wrongly) as 'Widowed'. In fact, he was not widowed until 1943. He and Frieda then married in 1944, though he died the following year.

There is a further puzzling connection between Scripture and Frieda's family which has not yet been fully clarified. Scripture was associated with a firm of instrument makers trading as 'F. Homan', and used various opportunities to publicise items which the firm manufactured. For example, an advertisement for the firm appeared in the short-lived *Zeitschrift* in 1932. Listed among the firm's products is the 'strobilion', a manometric flame stroboscope intended for pitch and intonation training, described in Scripture (1913). Again, *Nature* for 2 February 1935 (191–192) has a short review of a 'traversing microscope' manufactured by F. Homan which has been 'submitted for examination' by Scripture. It is said to have been designed by Scripture, and is no doubt the 'Meßmikroskop' also mentioned in the 1932 advertisement. It will be recalled that (van) Homan was Frieda's family name before she changed it to Janvrin by Deed Poll. There must be a real possibility that Frieda is in some way linked to the firm, and even that ‘F. Homan’ and Frieda Janvrin are the same person.[3]

8.1.3 Publications

Scripture was prodigiously prolific. The bibliography compiled in Murchison (1934: 436–440), covering the date range 1891–1932, contains some 216 items, and even then is certainly not quite complete (it lacks his 1903 paper in *La Parole*, his 1925 *Nature* obituary of Rousselot, and his 1925 review in *Le Maître Phonétique*, for example). Fifty-six of the items (just over 25%) are in German. For a period following 1926, presumably corresponding to a period of residence and work in Vienna, his output was almost entirely in German. For the most part, Scripture’s publications after 1932 are very short (many were letters to *Nature* rather than journal papers in the ordinary sense). They show Scripture well past his best, and fall far short of the standards of his early work. In fact, from as early as 1915 onwards Scripture’s publications deal repetitively with a small number of themes (or perhaps one might term them ‘hobbyhorses’) and exhibit extensive self-plagiarism. Thus essentially the same material is re-used over a period of nearly 40 years in ‘On the nature of vowels’ (1901),’Nature of vowel sounds’ (1921), ‘The nature of the vowels’ (1931) and again ‘The nature of the vowels’ (1936), together with several more closely similar papers, including German versions. They all argue—supposedly ‘with’ Willis and ‘against’ Wheatstone and Helmholtz—that vowels are characterised by inharmonic vibrations set off by ‘puffs’ from the glottis. But the papers do not show development and increasing sophistication over time. On the contrary, by far the most balanced and best documented of them is the earliest (1901).

Other recurrent themes in Scripture are the idea that certain neurological disorders can be diagnosed from speech records (a sequence of very similar papers spans the years 1908–1930), and the investigation of metre in verse, which had started before 1902 in his Yale days as an attempt to settle differences among classical scholars over the nature of Ancient Greek verse, and continued to 'Experimental phonetics and Ancient Greek verse' (1935).

The publications attributed to Frieda Janvrin (1930, 1931, 1933; Janvrin & Worster-Drought 1932) reveal research interests identical with Scripture's own, and—on the evidence of the known publications at least—she appears to be little more than his mouthpiece.

8.1.4 Speech and neurological disorder

Scripture's basic idea on the relationship between neurological disorder and speech is that the disorder will give rise to ataxia (loss of muscular control) in the larynx, and this in turn will be reflected by irregularity of vocal fold vibration. He examined the speech either in kymograph tracings—which he came to call 'macrophonic' registrations—or in oscillograms ('microphonic' registrations) made using a variable-width film soundtrack process. Though not entirely without some foundation, this work reflects little scientific credit on Scripture. He did not hesitate to make audacious statements on the basis of very meagre evidence. A few suggestive positive cases of irregularity in short sample traces obtained from patients with 'disseminated sclerosis' (multiple sclerosis) prove little by themselves. Scripture had no controls, and no normative data. How is the patient with early signs of disseminated sclerosis to be distinguished from a neurologically healthy individual who has a harmless sore throat or a habitually creaky voice? Even more surprising, the degree of irregularity was unquantified, being judged by simple visual inspection. This is ironic, given Scripture's proclaimed attachment to 'numbers' and 'measurement'. He offered this quotation (1936: 219) from William Thomson, Lord Kelvin (1824–1907) as summing up his approach to speech:

> When you can express what you are speaking about in numbers, you know something about it; but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind; it may be the beginning of knowledge, but you have scarcely in your thoughts advanced to the stage of science.
>
> (Thomson 1889: 80)[4]

In fact, the attention to measurement and quantification in Scripture's work declines progressively over the years—almost, it seems, in proportion as his rants about their importance become more raucous.

8.1.5 Reception of Scripture

Characteristic of Scripture's later work are repeated claims of novelty and originality made for ideas, techniques and devices that were far from new or original. For example in the prestigious British Academy publication 'The study of English Speech by new methods of phonetic investigation' (1923), the vaunted 'new methods' are kymography—using a single-channel clockwork kymograph that is no improvement over the first efforts of Rosapelly half a century earlier, and his own plotting of speech curves from gramophone recordings, done almost twenty years before. He similarly announced 'A new method of studying the pathology of speech' in 1916, and 'New methods of studying verse and poetry' in 1924.

Scripture went on gaining access to prestigious audiences and publications, but we may suspect that while he continued to receive a polite hearing on account of his early work, few were actually taken in by his later bravado. This is suggested clearly in the documented discussion which followed a paper he delivered to a meeting of the Laryngology section of the Royal Society of Medicine in London (1920). The paper was a description of a laryngeal stroboscope, illustrated with photographs, together with some characteristically dogmatic assertions about the nature of vowels. In thanking Scripture and opening the discussion, Dr. Donelan[5] delivered a put-down that is a masterpiece of irony:

> We are much indebted to Dr. Scripture for giving us an opportunity of reviving our recollections of the elements of our knowledge of the functions of the larynx. These opportunities are rare in our Section because it is fundamentally clinical and we deal rather with pathological than with physiological conditions. Members specially interested in this side of our work will remember that Dr. Scripture's writings already occupy a well-marked place in bibliography. I think, however, that in his description of his exhibit he does himself less than justice by his omission of all reference to previous workers. He leaves us in the dilemma of having to regard him as a somewhat late claimant to be the inventor of the stroboscope or that he is assuming that our clinical pre-occupations have made us lose sight altogether of the physiological work done in Europe during the past half-century.
>
> (1920: 129)

After the elaborate politeness of the opening, Donelan’s criticisms become much more direct:

> … Dr. Scripture's observations with this instrument merely confirm those of previous investigators…
>
> I should like to ask Dr. Scripture if he really seriously invites us on his bare assertion to regard the larynx as the sole regulator of quality of tone?[6]

> Are we to cast aside, without the smallest proof to the contrary, the results of the classical investigations …?
>
> (1920: 130)

Donelan's remarks on this one paper exemplify most of the criticisms that can be raised in relation to Scripture's later work as a whole. Scripture is known and respected from his early writings (they have 'a well-marked place in bibliography'), but he is now telling his audience what they already know ('reviving our recollections of the elements of our knowledge'). He pays scant attention to the work of predecessors ('he does himself less than justice by his omission of all reference to previous workers'), insultingly underestimates his audience ('he is assuming that our clinical pre-occupations have made us lose sight altogether of the physiological work done in Europe during the past half-century'), and makes iconoclastic claims which are not supported by evidence ('he invites us on his bare assertion … to cast aside, without the smallest proof to the contrary, the results of the classical investigations').

8.1.6 Interaction with UCL and the IPA

There is circumstantial evidence that Scripture probably had some contact with the UCL department. The possible links with SJ around 1924 have been considered in Chapter 6. At almost exactly the same time, Scripture prepared a review of Panconcelli-Calzia (1924), which appeared in *Le Maître Phonétique* the following year (1930). Scripture had been a member of the IPA since 1901, but this review is his only publication in the journal. In accordance with the journal's policy, it is in phonetic transcription—but interestingly, the pronunciation indicated seems to be entirely British, and the transcription system identical with the one that Ida Ward was then using in the journal. It seems possible that Scripture submitted his review in conventional orthography, and the transcription was done on his behalf.

In the same issue of *Le Maître Phonétique* as his review is an announcement that Scripture has opened an institute for the study of phonetics in Vienna (1930: 21). The year 1925 is the last in which Scripture is listed as a member of the IPA, though whether any significance can be attached to that is a matter for speculation.

Though he played no role in the planning, Scripture was at UCL as a speaker and participant at the 1935 ICPhS—in fact the session in which his paper appeared is billed as a joint meeting with the International Society of Experimental Phonetics. There appears to be no mention of the Society's administrative affairs, though if Scripture was continuing *de facto* as President without elections this would have been in contravention of the Society's own statutes.

8.2 Sir Richard Arthur Surtees Paget (1869–1955)

8.2.1 Biography

Paget's life and scientific career are outlined in numerous obituaries and short biographies (e.g., Lowery 2004), and a version with an accurate (though very brief) assessment of his work in speech is given by Fry (1977). But speech research was only a small part of Paget's work, and something on which he did not embark until he was in his mid 50s. Bearing in mind the wide range of his activities, and the extraordinarily colourful and eccentric nature of his life, his family and the circles in which he moved, it is perhaps surprising that no extended biography of him has been produced. There are certainly ample materials.[7] He is, however, covered extensively in Blunt (1962), which is a biography of Paget's first wife, Lady Muriel Paget (1876–1938). Particularly relevant sections of Blunt (1962) are pages 12–41, 128–132, and 218–229.

Paget was the eldest son of Sir Richard Homer Paget, MP, first baronet (1832–1908), of Cranmore in Somerset. He was educated at Eton and at Magdalen College, Oxford, where he gained a third class degree in Chemistry in 1891. He was called to the bar by the Inner Temple in 1895, specializing in patent law. This period of his life included a world tour which took in India (where he stayed in Government House), China, Japan and the United States. In 1897 he married Muriel Finch-Hatton, daughter of the twelfth earl of Winchilsea. In 1900 he became secretary of the patent law committee, and his combination of scientific and legal credentials was to fit him for service in a number of other influential roles. Paget succeeded to the baronetcy on the death of his father in 1908. He was too old for war service at the outbreak of WW1, but set about trying to persuade first the Army and then the Navy to establish departments for examining new inventions relevant to the war effort (Blunt (1962:128–129). In 1915 the Admiralty did indeed set up a 'Board for Invention & Research', divided into several sections, and Paget was invited to become Secretary of any section his choice and to put forward suggestions for a suitable committee. He chose the 'Submarine and electrical' section, and nominated a team which included two Nobel prizewinners, William Henry Bragg (1862–1942), then professor physics at the university of Leeds, and Ernest Rutherford (1871–1937), who then occupied the chair of physics at Manchester, as well as the outstanding engineer William Duddell (see Chapter 2).

Paget had a remarkable auditory gift. 'I could to some extent analyse by ear a complicated "noise", so as to recognise the musical notes of which it chiefly consisted'

(1955: 241). He put this to direct use in the work on submarine detection, since by simply submerging his head and making an auditory estimate of the pitch of waterborne sounds from ships, he was able to suggest the appropriate frequency range in which underwater microphones had to be made sensitive.

8.2.2 Publications on speech

Paget's publications on the analysis and synthesis of speech sounds are concentrated into a period of less than a decade. He began with auditory analyses of his own vowels, published as *Vowel resonances* in 1922. By 1923 he added vowel synthesis, using various forms of two-resonator model, many made from Plasticine, which were blown with the breath or with bellows (1923a). He also began to explore the manipulations that could cause the models to produce consonant-like sounds (1923c). In further work he investigated the resonance characteristics that could differentiate 'voiced' and 'unvoiced' sounds in the absence of larynx vibration (1927). But soon his 'gestural' theory of the origin of speech began to claim his attention (1928a, 1928b), and it occupies a large part of his book *Human speech* (1930). That work does, however, contain a wide range of auditory analyses and synthesis experiments not reported elsewhere. Thereafter, though he continued to draw on his early findings, his focus increasingly shifted to the origin of language, and then to the promotion of sign language as the basis for a universal language (1943)[8] and eventually as a communication system for the deaf.[9]

By far the most important of Paget's works is *Vowel resonances* (1922b) itself, a pamphlet of scarcely more than 3000 words. In very concise form, and with no reference to any previous work, it details Paget's estimations of the resonance frequencies of his own 'breathed' (i.e., unvoiced or whispered) vowels

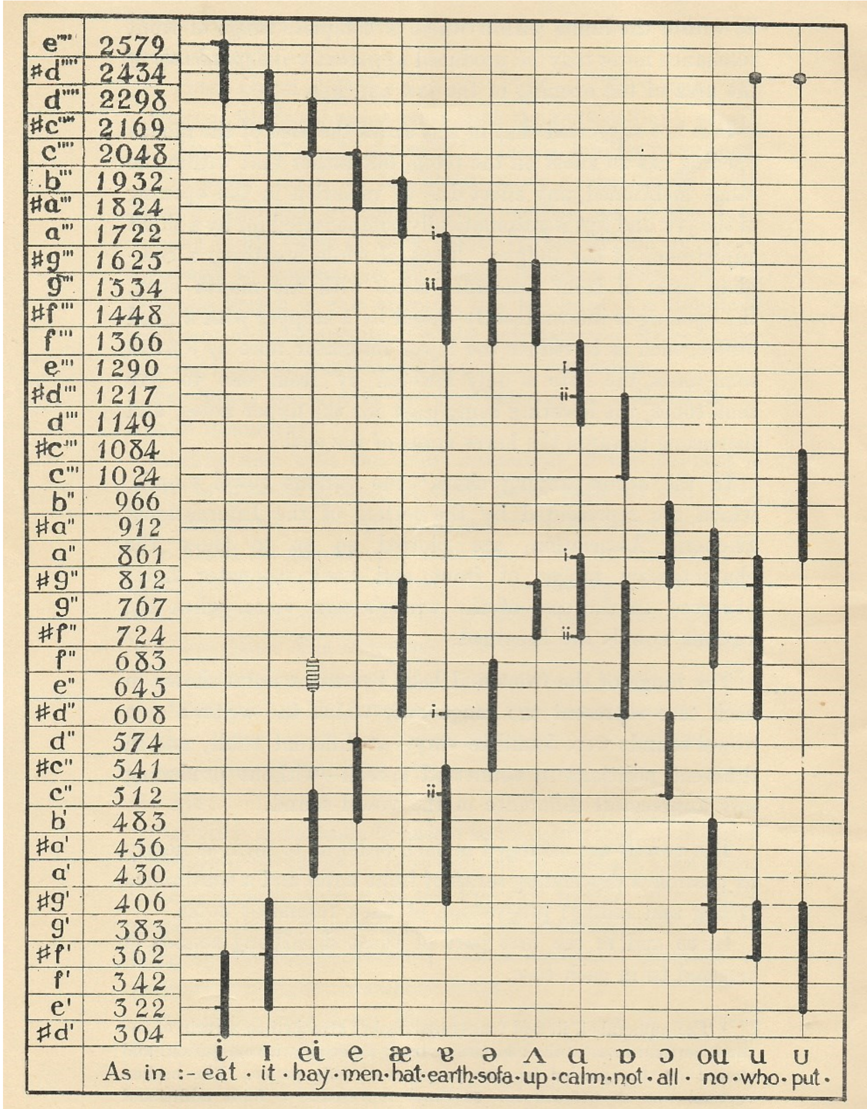


Figure 8.1 Vowel resonances as noted by Paget (1922b: 4). The vertical extent of the bars indicates the range of permissible variation. The horizontal tick-marks to the left of most of the bars indicate the frequencies achieved by particular synthesis models, and the roman numerals (i) and (ii) to resonances in successive models of the same vowel; see (1923c: 22).

Paget unhesitatingly identified two resonances in all vowels (including those back vowels where others had found a single resonance), made plausible estimates of the resonance frequencies, and displayed his results graphically in a way that enabled their orderliness to be taken in at a glance. The chart of resonances (1922b: 4), reproduced here as Figure 8.1,[10] was to reappear many times in his papers (1923a: 753; 1923b: 46; 1923c: 22; 1924b: 70).

The story behind the observations is recounted in (1930: 40–41):

> Eventually, in November of 1921, the occasion offered itself. Being alone in London, confined to bed with a slight chill, and disinclined to read, it occurred to me to try and listen to the whispered resonances of my own voice, as I had begun to do during the war.
>
> The work required no appliances but pencil and paper, for though I have not got the faculty of recognizing instinctively the "absolute pitch" of any musical sound, I can always identify any note—within less than a semitone—by mentally comparing it with a fixed note which I literally carry in my head.

Paget explains that he can elicit reference pitches of 812 Hz and 966 Hz by tapping on his skull in different places. Perhaps surprisingly, even when no longer confined to bed, Paget depended upon these head notes rather than a tuning fork.

The observations were not easy:

> At first the observations were very difficult—mainly owing to the fact that, to recognize the resonances of the whispered vowel sounds, it was necessary to hear these sounds not as vowels but as new musical effects heard for the first time. The very fact of their familiarity as speech sounds made them almost impossible at first to analyse as musical sounds. Various expedients were tried such as putting one's head under the bedclothes, and stopping the ears so as to hear only internal sounds; indeed, I listened so hard to the sounds in my mouth that the strain produced a feeling of sickness. Gradually the perception grew, and as each vowel sound was whispered in tum, the combination was recognized of two faint but definitely musical notes—a different pair of notes for each vowel sound.

Paget sent a note of his preliminary results to Sir William Bragg (Paget 1930: 41). Bragg had been awarded the Nobel prize for physics in 1915, and knighted in 1920, and was now Quain Professor of Physics at UCL. DJ's promotion to Professor in 1921 had just made him one of Bragg's newest professorial colleagues. There was a sense in which Bragg was—in succession to Rayleigh—the natural doyen for the emerging field of speech science. In 1919 (a few months after Rayleigh's death) he delivered the celebrated Royal Institution Christmas Lectures for young people on the subject 'The world of sound' (published as Bragg 1920). Bragg was a significant figure within the Royal Institution and was to move there as Resident Professor in 1923 (Caroe 1985: 104), but in 1935 he accepted the invitation from UCL to be one of the 'Patrons' of the Second International Congress of Phonetic Sciences, held at UCL under DJ's presidency (Jones and Fry 1936: vi). When Bragg passed Paget and his vowel observations over to DJ in 1921 it may have been nothing more than one department of the College re-directing an enquiry to another more appropriate department. But an alternative possibility is that Bragg was aware of DJ's 1917 Discourse at the RI, recognised its quality, and knew that DJ was ideally qualified to assist Paget in the development of his research.

It seems likely that DJ understood at once the significance and originality of Paget's observations. Paget says of DJ 'It was largely owing to his encouragement that I was led to make a more serious study of the subject' (1930: 41). DJ seems to have facilitated the rapid publication of Paget (1922b), and meanwhile directed Paget's attention for the first time to previous work, such as that of Lloyd, enabling Paget to set his own work in a broad historical context when he wrote the longer account in *Human Speech* (1930).

At first, Paget needed to use artificial manoeuvres to separate the two resonances, as had both Lloyd and Alexander Graham Bell (1879: 66–67)[11]; but Paget's later writing confirms that he had the ability to perform some degree of auditory spectrum analysis without such means, and even upon sounds voiced in the normal way (rather than whispered sounds, or vowels excited by a single shock, as in tapping on the throat).Though his first observations were made on himself, Paget was eventually able to extend his method to the determination of resonances in the speech of others, including female speakers (1930: 85–93). There can be little doubt that if DJ and Paget had worked together on the analysis, it would have been possible in this way to produce a quantitative characterisation of the Cardinal Vowels as early as 1930. Why this did not happen we can only speculate. It certainly seems to be one of the great missed opportunities in the history of phonetic science.

Vowel resonances was preceded by a letter to *Nature* which appeared on 16 March 1922. This does not tabulate the resonances of the separate vowels, but records the claim that two resonances are found for each vowel, and gives the overall ranges of the two resonance frequencies. That letter, and the precise dating of *Vowel resonances* itself to '24 March 1922', seem to show that Paget was eager to establish priority for his claims—no doubt a reflection of his experience as a patent law agent. In fact, most of Paget's research results on speech did find their way into two patent specifications.

8.2.3 Artificial vowels

Paget (1923a) was certainly the first to produce a wide range of vowels by means of models, while providing a reasoned justification for why particular models gave the vowels they did. In general, Paget's models (see Figure 8.2) involved resonators in series but this caused difficulty in tuning, since when connected together neither of the two resonators gave the same note as when used alone, and re-tuning by trial and error was required. But the same paper stakes a claim to the idea that resonators may alternatively be joined in parallel (1923a: 765). When this is done, he says, there is 'no pitch reaction between the resonators'. It is interesting to note that the relative merits of series and parallel connection for formant synthesizers were still being debated sixty years later (Holmes 1983).

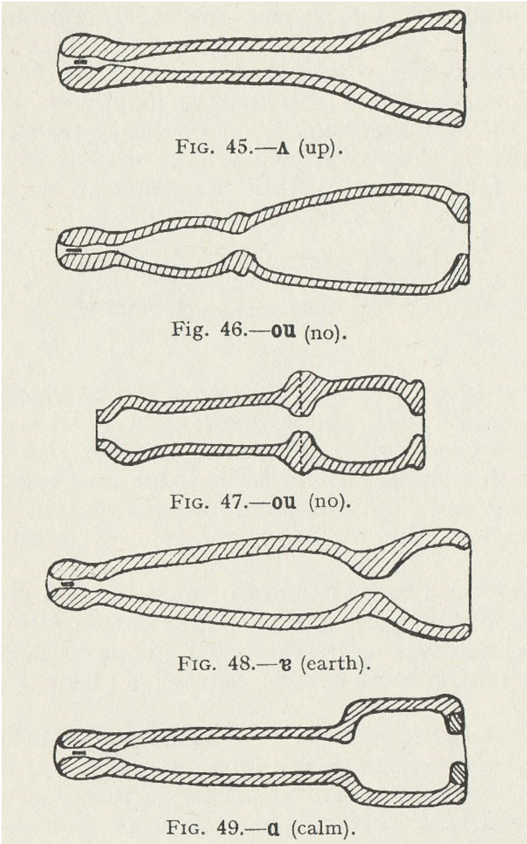


Figure 8.2 A selection of Paget's vowel resonators shown in section (1930: 62). The illustrations of resonators in Paget's academic publications have no indication of dimensions, but his patent 214281 does specify the dimensions of certain resonators precisely.

Paget also showed that the larynx analogue (a vibrating reed) could be located *between* two acoustic resonators through which air was flowing, demonstrating that in principle sub-glottal resonances could have an audible effect on vowel quality (1923b: 47).

Corroboration of the resonance frequencies which Paget had determined was indeed required. DJ himself might have been in a position to appreciate the originality of Paget's findings, but others were understandably sceptical. After all, there had been many previous attempts. What reason was there to think that Paget's estimates were any better than many earlier offerings? For example, reviewing *Vowel resonances*, Guittart (1924), probably spoke for many:

> The theories as to the question what a vowel is, of Willis, Wheatstone, Helmholtz, Trautmann, Lloyd, Scripture, Perrett do not seem to be conclusive. We still feel that this essential question has not yet been solved. Just like most of the other investigators, Sir Richard arrives at the conclusion that a vowel consists of two component sounds. He has been able to fix their pitches for his own voice and has put this down in a graphical representation ... Many similar investigations of other able phoneticians will be required to test the correctness of this new method of research.
>
> (1924: 73)

In the absence of any simple method of measuring the resonance frequencies, re-synthesis was the only means open to Paget of corroborating his findings.

8.2.4 Perception tests

Since the work of Kratzenstein in the eighteenth century there had been disputes over whether particular synthetic vowel sounds did or did not resemble the originals closely enough to be recognisable. This was treated as effectively a matter of opinion. According to Paget (1930: 81) it was Perrett who first proposed an 'experiment' in which examples were to be presented blind and in random order to a panel of suitable listeners who furnished categorical written responses which were collected for analysis. The first such experiment in Britain may have been conducted at UCL on

Friday 1 December 1922 in the course of a talk given by Paget to the Philological Society.[12]

Twelve Plasticine double-resonator models were sounded from behind a screen 'in indiscriminate order'. An audience 'interested in phonetics' recorded their impression of the resulting vowel sounds (mainly, it appears, by writing an IPA symbol). Both the procedure and Paget's reporting of the results (1930: 82) fall somewhat short of modern expectations. The number of listeners is given only as 'about 20'. It is unclear whether each model was sounded only once, giving a total of 12 trials, but this seems likely. It is not explained whether the listeners were given any practice with the synthetic sounds, or whether they knew how many vowels were to be used in the test. In certain cases, the total number of mishearings is given, but their distribution across different error categories is not precisely reported. Nevertheless, an attempt is made here to re-analyse the experiment by means of a confusion matrix.

Although Paget did not have a very effective way of visualising the results, the conclusions which he drew from the experiment are entirely reasonable. He says that 'in almost every case the vowel-sound was either recognised or was recorded as a vowel whose resonances were of the same order of frequency as those of the model (1930: 82–83). This is the same as saying, in relation to Table 8.1, that most responses lie on or close to the diagonal. The only clear failure among the synthetic vowels used on this occasion was **ɐ** (the NURSE vowel), which seems to have elicited no correct responses. Paget comments that the model was afterwards 'retuned … to distinguish it more from [the other] vowels'. With hindsight, it seems likely that the model—which, after all, was formed only from Plasticine—had somehow been damaged or distorted in the demonstration so that the F_2 resonance was too high. A further possibility is a simple error in which another resonator—probably that for **e**—was sounded by mistake instead of the **ɐ** model.

		TARGET											
		i	**ɪ**	**ei**	**e**	**æ**	**ɐ**	**ʌ**	**ɑ**	**ɒ**	**ɔ**	**ou**	**u**
RESPONSE	**i**	**17**		1									
	ɪ	1	**19**										
	ei	1		**17**	6	1	*10*						
	e			2	**9**	2	*10*						
	æ	1			3	**11**		6					
	ɐ				2	5			1		1	1	
	ʌ							**13**		3			
	ɑ					1		1	**13**				
	ɒ								1	**7**	1		
	ɔ		1						5	*5*	**15**		1
	ou									*5*	3	***9***	1
	u											*8*	**17**
	ʊ											2	1

Table 8.1 The results of Paget's 1922 experiment on the perception of artificial vowels produced by resonator models, plotted in the form of a confusion matrix. It has been assumed that there were exactly 20 listeners. The **mode** response is shown in bold. Numbers are shown in italics within a column when the exact apportionment to error categories is unclear from Paget's narrative (1930: 82). Notice that **ʊ** is included as a response category, but was not a target.

Paget also—though only jokingly—points out the very relevant lack of a real-speech control condition:

> The recording of vowel sounds, detached from their normal environment of words, is difficult; it is not improbable that if the same experiments had been performed with the same series of vowel-sounds, intoned by voice,

> with the “short” vowels prolonged to the same duration as the “long”, the percentage of recognition might not have been greatly raised!
>
> (1930: 83)

Paget may have been right, too, that duration was a factor influencing the results. Leaving aside **ɐ**, the two lowest mode scores are for the ‘short’ or ‘checked’ vowels **ɒ** and **e** (though on the other hand the highest score of all was obtained for another short vowel, **ɪ)**.

The plasticine resonators were easily damaged, and were soon replaced with more durable materials, and with designs capable of adjustment, so that a single model could yield a variety of vowels (1930: 69). Paget showed that the material from which the resonator was made was in principle irrelevant (although less dense materials, being to some degree acoustically transparent, tend to raise the resonant frequency). Vowel synthesis with metal cylinders having adjustable plungers was demonstrated at the Royal Institution by 1923, and Paget began to use excitation provided by a professionally-made organ reed with metal tongue in place of the crude rubber strip vibrators he had made at first. The results of experiments with several generations of tubular models are tabulated in Appendix IV and Appendix V of Paget (1930: 302–309). He had in mind (but does not seem to have put into final form) the idea of an adjustable synthesis model using sliding tubes marked with calibrated scales, producing standard vowels to be used for phonetics teaching (1930: 241). This would have had the advantage over a gramophone recording of not being subject to variation in reproduction speed.

8.2.5 Electrical resonators

As early as April 1922 Paget realised that ‘it might be possible to substitute electrical resonators for acoustic resonators, so as to produce vowel sounds in a telephone by passing an intermittent current through two resonating circuits and thence to the telephone’ (1923a: 764–765). He suggested work along these lines to the notable radio and electronics pioneer, W. H. Eccles (1875–1966)[13], but although Eccles produced a working apparatus by the following year, their work was anticipated by the American J. C. Stewart (Stewart 1922). As elsewhere, Paget is specific about dates; recording 26th April, 1922 as the date of his idea for electrical synthesis and noting that Stewart’s letter to *Nature* describing his apparatus is dated 8 July 1922.

8.2.6 Artificial consonants

From the beginning of his work on artificial vowels, Paget had found that manipulations of the models could produce certain consonant sounds in CV or VC syllables, particularly labials [p, b, m ,w] when the opening of the resonator was closed with the hand. He was surprised to find that in the case of a two-cavity resonator, a closure formed at the constriction inside the model did not give [t] or [k] as might be expected but again [p] and [b]. He summarized his initial observations as follows:

> This result indicated that, in the voice, the formation of the plosive consonants (p.b, t.d, k.g) is determined, not by the position in the mouth in which the closures and releases are made, but by the effects which the muscular movements of closure and release have on the resonances of the vocal cavity, according as these are made in different attitudes of the tongue, etc. In other words, these consonants are recognized by the nature and the changes of the resonances which are set up, and by their curves of

approach to or recession from the resonances of the vowel with which they are associated.

(1924a: 150—151)

Whether anyone at the time understood or believed him (in SJ's copy of the paper, for example, this paragraph is accompanied by puzzled and cavilling marginalia), Paget seems to have arrived in one leap at an understanding of what would later be called the locus theory of place perception (Delattre *et al.*, 1955).[14]

Paget encountered the difficulty that the manipulations which aimed at producing consonants often had the side-effect of detuning the vowel resonators. He was driven to contrive a collection of elaborate series-parallel models (see Figure 8.3), with the result that much of (1924b) makes very difficult reading.

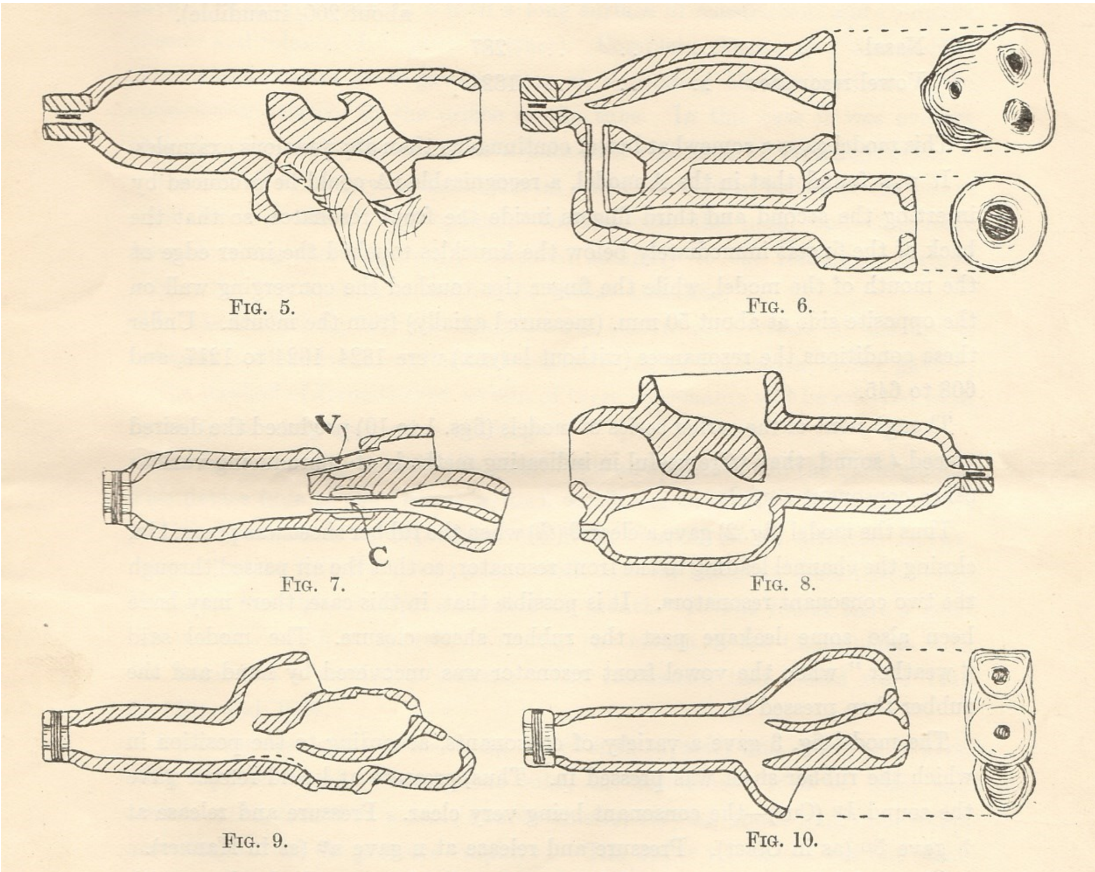


Figure 8.3 Various complex series-parallel models developed by Paget (1924b: 155). All were attempts to produce a syllable with a 'continuant' [ɹ] and an accompanying vowel. None was particularly successful.

The most systematic of Paget's experiments on consonant production utilised a uniform length of rubber tube which could be constricted at various positions. By 1924 he had settled on a tube about 1 inch in diameter and 16 cm long excited by an organ reed at the 'larynx' end. A fixed constriction was introduced by means of an adjustable clamp around the tube about 4.5 cm from the larynx end, so as to yield a vowel [ɑ]. Partial or total closure of the tube by external pressure at various positions, with appropriately synchronised excitation, then forms a range of consonants in CV syllables. A tube of exactly this kind is demonstrated in the 1936 film and is made to say [pɑ tɑ kɑ] and to produce a very fair approximation to the English word *father*.

Paget's resonators basically produced static vowel qualities. Suitably manipulated by hand, they might produce syllables or recognisable short words. His patent 214281 envisaged mechanising such manipulations so as to produce, for example, talking signal horns. These would produce voice-like words of warning rather than plain tones or noises. Beyond this, Paget did not attempt to design or produce a general-purpose synthesizer:

> No attempt has yet been made to devise an instrument capable of producing connected speech "synthetically". The great variety of changes of resonance which are required for this purpose would, almost inevitably, necessitate a complicated mechanism. If mechanical production of connected speech is required, it is obviously simpler to start with human speech and reproduce it by phonographic means.
>
> (1930: 230–231)

8.2.7 Connected speech

Nevertheless, he was to some extent able to synthesize connected speech, and the demonstration of this was to become a feature of his frequent talks and performances. To achieve the necessary dynamic control, he formed the resonance cavities entirely with his cupped hands, blowing into the cavities through a ‘pharynx’ tube equipped with a rubber-strip larynx vibrator. He called this arrangement the ‘Cheirophone’,[15] and rather oddly refers to it as if it were a device, rather than a skill (‘a simple form of hand-operated talking machine’ (1923b: 47). This again is an attempt to establish a proprietary right over the idea. The corresponding patent 237316 does indeed describe a complete device in which the hands do not directly form the resonators, but are placed around ‘an enclosing envelope, parts of which are flexible and parts of which are comparatively rigid’, though it is uncertain whether such a device was ever constructed. The 1936 film shows Paget using his hands to synthesize the sentences ‘Hello, London, are you there?’, and ‘Oh, Leila, I love you!’

Paget’s gestural theory of the origin of language may seem to have little connection with his phonetic investigations, but in fact it grew out of his observations on the production of consonants. From the failure to find any acoustic invariants for most consonants, he passes rapidly through what appears to be an early preview of the ‘motor theory’ of speech perception to his claims concerning gesture:

> This absence of any general characteristic resonant change seems to indicate that, in identifying these particular consonant sounds, and probably also in identifying all speech sounds, the ear is primarily concerned in recognizing where and how the closure (or release) is made in the vocal cavity, rather than in identifying the actual resonant changes themselves […]
>
> In other words, human speech appears to be essentially a branch of human gesture, which the ear has learnt to identify-without the aid of sight-by means of its secondary effects in modifying the resonances produced by the passage of air by or through the gesticulating members of the vocal cavity.
>
> (1924: 173–174)

8.2.8 Evaluating Paget's estimates of vowel resonances

Paget's results on the acoustic properties of vowels have been recognised as remarkable anticipations of what would later be largely confirmed by instrumental methods. Thus Catford writes 'The formant frequencies established by Paget are precisely (well within the limits of personal variation) those established twenty to thirty years later by instrumental means' (1977: 62). But this represents Paget's findings as merely estimates of the right order of magnitude for a male speaker. The question seems never to have been addressed of whether they might in fact be accurate estimates of Paget's own formant frequencies. It was decided to investigate the matter by searching for a recording of Paget from which an acoustic analysis could be attempted.

A single recording of Paget was identified in the British Library, catalogued as T6016R. The recording is undated, but since it consists of Paget reading a monologue in Basic English, it cannot be earlier than 1930, the year in which Basic English was launched (Ogden 1930).[16] It is a good quality recording, evidently from a 78 rpm original, and runs for approximately 3m 25s. Paget is the sole speaker.

A digitised copy was furnished by the British Library, and was opened in SFS (Huckvale 2013) for analysis using *formanal*, a formant tracker based on LPC polynomial roots and dynamic programming. The recording was manually annotated to select sampling points in as many tokens as possible of the vowel types considered in Paget (1922). In general the sampling point was placed at or near the temporal midpoint, and only vowels longer than 50 ms were used. Paget treats the narrow diphthongs FACE and GOAT as simple vowels, no doubt because he was able to isolate and prolong the starting qualities for analysis. In the recording, it proved impossible to identify a relatively well-defined starting position for either, and the FACE and GOAT tokens were dropped from the analysis. The numbers of examples annotated were DRESS 15, FLEECE 11, FOOT 4, GOOSE 4, KIT 6, LOT 12, NURSE 11, SCHWA 5, START 10, STRUT 9, THOUGHT 12, TRAP 15. A script was then used to collect and tabulate the F_1 and F_2 estimates from the 124 sampling points, and these in turn were plotted on a logarithmic scale of frequency in such a way that they could be superimposed accurately[17] on a scan of the final version of Paget's figure (1930: 86). The result appears in Figure 8.4.

In the discussion which follows, it should be remembered that Paget himself makes no use of the term 'formant'. He speaks instead of the 'upper' and 'lower' resonances, sometimes referring to the lower resonance as the 'throat' resonance.

Figure 8.4 reveals an impressive degree of agreement between Paget's auditory estimates and those made automatically from a recording of his own speech. The correspondence is especially remarkable for F_2, where, with the one exception of the GOOSE vowel (Paget's *who*), the great majority of all the observed values lie within the ranges specified by Paget. Possible reasons for the case of GOOSE are examined below.

It is also plain that Paget's estimates of the 'upper' resonance do indeed correspond with estimates of F_2 obtained instrumentally, and not, as might perhaps have been expected, with an aggregated higher formant which combines the perceptual effect of F_2, F_3 and possibly higher formants—the so-called 'F2 prime' of Fant and Risberg (1963). On the contrary, there are numerous indications in Paget's writings that he was sometimes able to resolve F_3 as a separate auditory feature of the sound complex—and indeed a number of F_3 estimates are included at the top of his 1930 version of the chart.

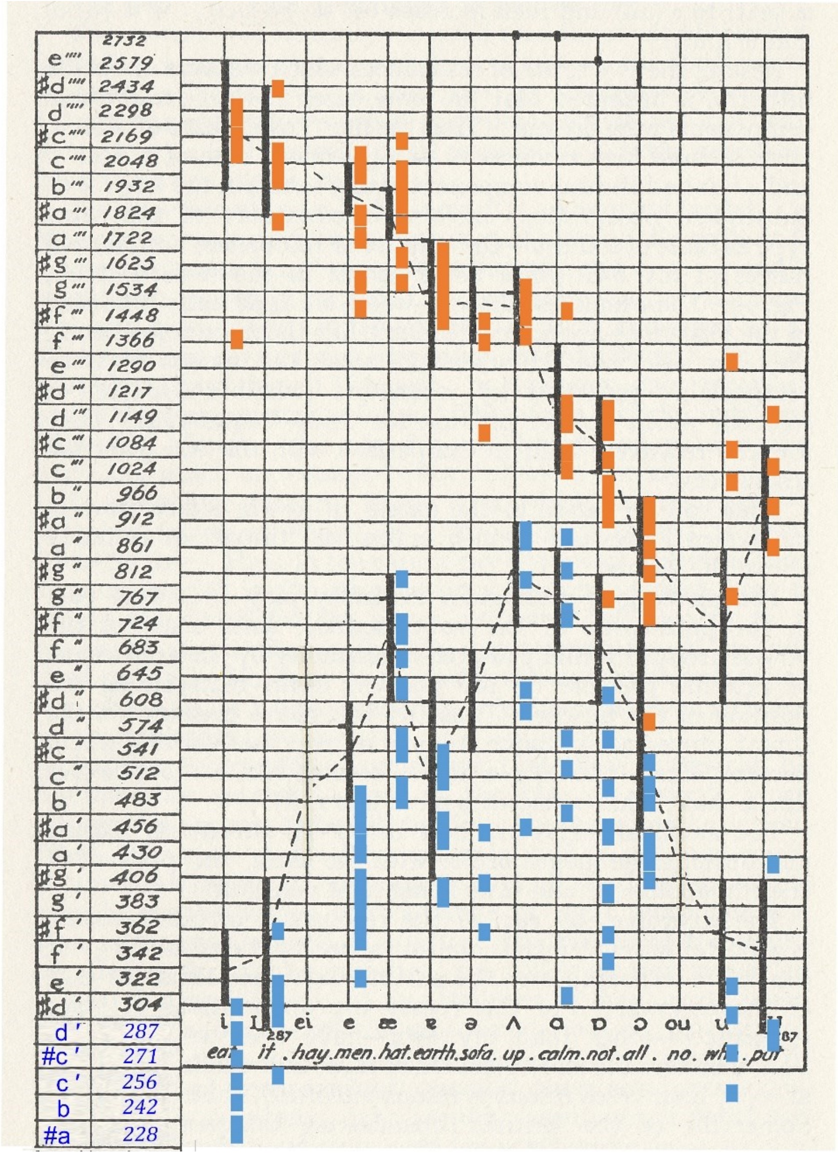


Figure 8.4 Automatically extracted estimates of F_1 (blue) and F_2 (orange) plotted on Paget's figure (1930: 86) for comparison with his auditory estimates. Paget's estimates for *hay* and *no* (i.e., FACE and GOAT) have been deleted (for clarity), and the frequency scale extended downwards by 5 semitones (shown in blue).

The situation for F_1 is somewhat different. Across the board, the estimates extracted automatically are considerably lower than those given by Paget, with the result that the frequency scale of his figure has had to be extended downwards by a further 5 semitones to accommodate them. There are two relevant considerations which may help to explain this discrepancy.

First, it should be remembered that the automatic analysis has been carried out on a recording of normal phonated speech, whereas Paget's estimates were made on whispered vowels. A range of studies have reported higher formant frequencies, particularly for F_1, in whispered speech than in phonated speech. For example, Kallail and Emanuel (1981: 248) report higher mean F_1 for whispered examples of all 5 vowels tested in their study. An examination shows their whispered F_1 values are 3 to 5 semitones higher than the phonated values—a difference of the same order as that seen in Paget's estimates.[18]

A second consideration is that Paget's estimates were made on isolated vowels, taken out of any context, and prolonged, whereas the instrumental estimates have been made on running speech, in which the ideal or target configurations for vowels are not necessarily achieved. At least some of the discrepancies in F_1 estimates may have arisen this way. The expected direction of difference is that Paget might have adopted and maintained a somewhat wider jaw aperture for stationary examples of his vowels than was typically reached in running speech—and this would have raised the F_1 that he heard and analysed. It seems particularly likely that the 'open' vowels of shorter duration in running speech would be affected this way, and this appears to be confirmed by the results for the LOT vowel (Paget's *not*) in Figure 8.4.

Articulatory undershoot in running speech is also the probable explanation of the discrepancy in F_2 estimates for GOOSE (Paget's *who*)[19] which has already been mentioned. In this case, Paget's estimates are much *lower* than those obtained instrumentally. When he sustained the GOOSE position, he probably adopted a close lip-rounded position, which had the effect of reducing F_2 to a very low value. In running speech the degree of rounding achieved was probably considerably less, and F_2 correspondingly higher.

Overall, therefore, the analysis suggests that the nature of Paget's achievement in *Vowel resonances* has been considerably underestimated by later commentators. The indications are that his estimates are not merely of the right order, and close to values 'established twenty to thirty years later by instrumental means', but instead extremely accurate characterisations of his own vowels whispered in isolation.

8.2.9 Evaluating Paget's results: consonants

This naturally raises the question of whether Paget's auditory analyses of consonants can be assessed in a similar way. Unfortunately, Paget failed to develop for consonants a concise summary or graphical representation of the kind that made his vowel resonance findings so easily understood and memorable. He is, however, entirely clear about two remarkable insights—first, that the consonant resonances vary over a wide range as a function of those in the neighbouring vowel, and second, that very close to the moment of consonant closure or release the vowel resonances undergo rapid change (with resonances sometimes appearing to split or merge as the spectrum changes).

Paget did publish various tables of quantitative results on consonant resonances (e.g., 1924a: 160, 172; 1930: 107, 310–314), though they are not at all easy to interpret. Somewhat clearer than the published examples is the table given in his patent 214281 (See Figure 8.5). It appears that Paget had not only grasped the fundamental idea which was to be reinvented with the 'locus' theory, but also arrived at realistic estimates of some of the locus frequencies themselves. Paget seems never to have hit upon the idea of plotting resonances as a function of time, though his astonishing narrative accounts of dynamic spectral change are crying out for graphical representation. For example, he describes the characteristics of the lateral [l] in these terms:

> This is almost wholly a transient. Thus, if the 1 sound, in speech, is prolonged, it is even more difficult to identify than a prolonged m, n or ŋ. The resonances of 1 include a comparatively invariable upper oral resonance of 1625 to 1932, a faint nasal resonance 683,[20] and one or more low throat resonances of 228 to 406. The characteristic resonance changes in transit to or from a vowel are: (1) The upper (oral resonance) moves rapidly and continuously upwards or downwards to the upper resonance of the associated vowel. When the associated vowel has its upper resonance within that of 1, a difference of at least a semitone is instinctively made between the two resonances. (2) The lower (throat) resonance rises about five semitones at the moment of release, and then rises or falls to the pitch of the lower resonance of the associated vowel.
>
> (1924a: 166)

This shows a remarkable degree of correspondence with examples of [l] from his own speech. Figure 8.6 illustrates this for an instance of [l] in the word *belief*—essentially chosen at random as the first example encountered in the recording.

214,281 6

TYPICAL CONSONANT RESONANCES.—*Continued.*

M			
Mi	1722—1625	1217	270
MΛ	1448	1084—1217	270
Mu	1448	861	270
	Note: The upper resonance in M*i* is formed similarly to the middle resonance in M Λ and M*u* and *vice versa*.		
N			
Ni	1824—2048	1217—1366	203
NΛ	1448—1932	1217—1290	228
Nu	1534—1824	1217—1290	203
	Note: The chief difference between M & N is in the nature of the release which for M is like P (initial resonance of release 1625) & for N is like T (initial resonance 2049—2169).		
L Ll (Welsh)	1448—2169	683	322—512
	Note: The lower resonance rises steeply to or falls steeply from that of the associated vowel. The upper resonance remaining substantially unchanged.		
K. G.			At instant of release
Ki	3067 rising to 3249		322
KΛ	1290 „ „ 1366		362
Ku	767 „ „ 812		322
iK	vowel res: 2434	Intermediate resonance sibilant 3249	Release 2434 rising to 2895
ΛK	1625	2169 1534	1824—2434
uK	966	2117 912	1217—1290
	Note: In the voice the release of K is doubled and always contains a high sibilant. This is not heard in T, which has only slight sibilance.		
T	Sibilant		
Ti	2298		362
TΛ	1722—1932		430—456
Tu	1024—1290		342
iT	2048		362
ΛT	1722—1932		362
uT	1024—1932		362

Figure 8.5 Part of the tabulation of consonant resonances from Paget's patent 214281. The values for F_2 of [t] with various vowels (circled) are plainly related to the locus frequency of approximately 1800 Hz which became generally recognised about 30 years later.

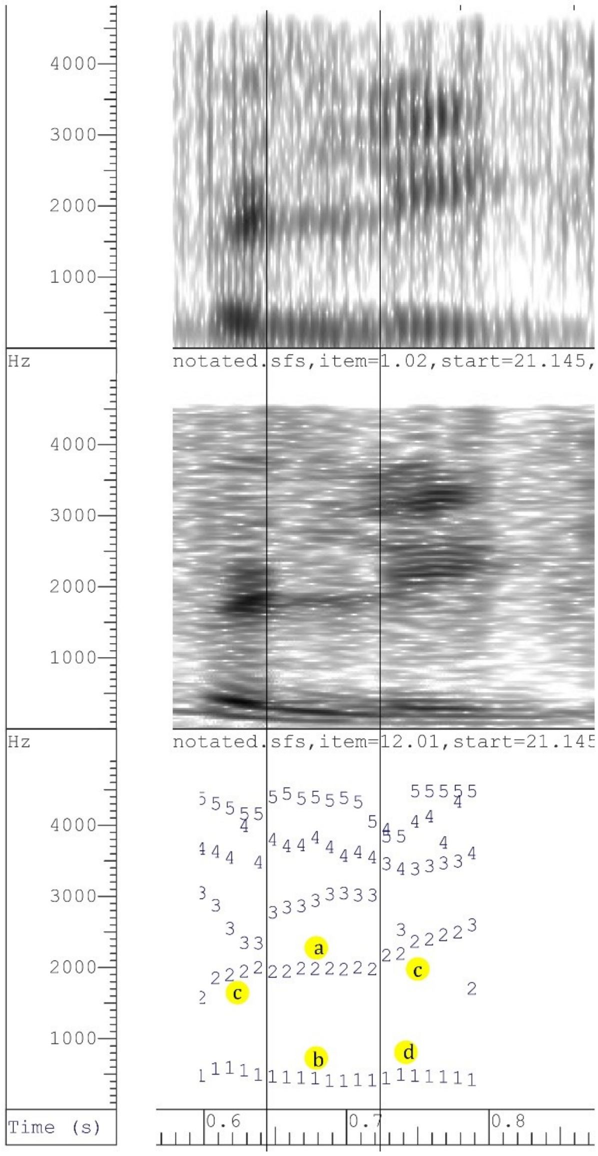


Figure 8.6 The word *belief* [bəˈliːf] spoken by Paget. From top: wideband spectrogram, narrowband spectrogram, formant tracks estimated with *formanal*. Segmentation lines have been drawn to demarcate the period of contact for [l]. The highlighted markers ⓐ–ⓓ call attention to features which closely match Paget's description (1924a:166):

ⓐ 'a comparatively invariable upper oral resonance of 1625 to 1932' [Mean here=1789 Hz]

ⓑ 'one or more low throat resonances of 228 to 406' [Mean here=251 Hz]

ⓒ 'The upper (oral resonance) moves rapidly and continuously upwards or downwards to the upper resonance of the associated vowel.'

ⓓ 'The lower (throat) resonance rises about five semitones at the moment of release, and then rises or falls to the pitch of the lower resonance of the associated vowel.' (The rise, from 230 Hz to 314 Hz, is indeed approximately 5 semitones as specified by Paget, but appears small in this figure on account of the linear frequency scale of the spectrogram).

8.2.10 Reception of Paget

Paget was an eccentric amateur with extraordinary auditory gifts. The proper evaluation of his work on speech acoustics has always been hampered, in his own day and in our own, by its entanglement with the other less respectable aspects of his linguistic ideas, and more widely by the sheer range of his activities—from silversmithing to the study of animal behaviour and from song-writing to motor vehicle design—which brings with it the suspicion of superficiality and dilettantism. The result is that his remarkably accurate auditory analyses have not been fully understood.

In the final chapter of his autobiography, Paget himself gives a fair estimation:

> My friends could fairly have described me as a free-lance, for there was no knowing in what direction I might thrust. My harsher and more academic critics might well have muttered 'blasted amateur'.
>
> I could agree with both sides, for I have been a free-lance, and what I prefer to call a minor rebel, with a strong inclination to butt in where I saw possibilities of effecting some clarification or real improvement.
>
> It was this instinct that led me into such unpromising ventures as listening to the whispered vowel sounds of human speech, and thence to the reproduction of vowels and consonants by artificial models. The lure in this case was that Helmholtz's classical conclusions lacked confirmation. I had an analytic gift for strange noises; why not try and clear up the Helmholtz puzzle?
>
> (1955: 285)

Paget was well aware that he attracted the charge of being an amateur:

> It must have been self-confidence that tempted me to drop vocal acoustics and follow the untrodden and even officially forbidden trail which led from acoustics to the study of hand- and mouth-gestures, and to the origin of speech itself. It was a narrow and unpopular field which seemed to lead nowhere. I knew nothing about the meaning of speech—nor, for the matter of that, did the Philologists—and my appearance in the lists must have seemed like flagrant amateurism to Philologists, Anthropologists add Phoneticians alike.
>
> (1955: 286)

The important point here is that Paget was not only an amateur from the point of view of an anthropologist or philologist, but also an amateur in phonetics, and was regarded with suspicion by academic phoneticians. It is plain that Paget's work was neither universally trusted, nor welcomed:

> These models when first exhibited were viewed with great suspicion. One eminent Doctor, for whose benefit I had blown each of my vowel-sounding models, said, "I hope you will not mind my saying that I think all the various vowel sounds are really made in your own mouth, and that the plasticine models only act as megaphones". So I had to get a pair of bellows, and show that the various vowel sounds were still produced when the air came from the bellows instead of from my mouth!
>
> (1955: 244–245)

> When the resonances of all the English vowel sounds had been recorded, I sent the results to Sir William Bragg, who passed them to Professor Daniel Jones, then Head of the Phonetics Department of University College, London. He reported that some of my resonances were new, and others similar to those of Helmholtz, but an octave higher. Sir William Bragg then asked me to lecture at University College, and offered to take the Chair. As the audience (mostly students of Phonetics) were leaving the Hall after the lecture, one of them was heard to say, "Well, if this is Phonetics, I've done with them"!
>
> (1955: 246)

It is also clear that Paget loved an audience. Amateur dramatics and improvised musical performances played an important part in his life from childhood onwards, and loom large in his autobiography (1955). He appeared in a radio broadcast as early as 1925 and was one of the first scientists to appear on the then new BBC Television Service in 1937. He readily accepted invitations to speak, and talks that had started as scientific presentations were transformed by degrees into after-dinner entertainments.

His presentation at the International Congress of Linguists in Geneva in 1931 is graphically described by Haruko Ichikawa:

> One day I looked in at a meeting of the Linguistic Congress. Various scholars were reporting the results of their studies in various rooms at the same time, so the audience chose whichever one of them they wished to hear.
>
> I chose Sir Richard Paget's 'The Gestural Origin of Language'. He elucidated his theory by giving examples, for instance, he showed that the Word 'blow' was originated by the shape of the mouth in blowing out breath, and the word 'sip' arose in a similar way. His lecture was accompanied by very amusing experiments. He used a rubber hose, which is usually fixed to a water-tap, the one end of which was fixed to a pair of old-fashioned bellows, and the other end he held in his hand covered with glycerine all over. Every time he held it tight, or twisted it, sending out air through it by stepping on the bellows, the rubber tube uttered words, 'Hallo, London! Where are you?' it spoke with a yawning voice. 'Mamma, I love you'. The faint dying away of the 'you-uuu' was drowned by the cheers of the audience. It was indeed a sight to see Sir Richard, an English peer, who probably had a country estate for game-hunting, making the tube speak with his sticky hand.
>
> (Ichikawa 1937: 300)

We notice that his experiments were 'very amusing' and the artificial speech was 'drowned by the cheers of the audience'. Her description closely matches aspects of the demonstration seen in the 1936 film. Paget was to go on repeating such popular demonstrations into the post-war period, long after he had ceased to add any new scientific content. But the emphasis on the entertainment value and humour of the presentations undoubtedly weakened the scientific credibility and impact of his findings, and made it possible to dismiss him as an eccentric irrelevance.

It is noteworthy that Paget did no collaborative work on speech, and one may suspect that he would have been very difficult to work with. Had he subjected his remarkable auditory gifts to objective analysis with the help of a physicist or experimental psychologist (it would not have been difficult, for example, to construct test signals for him to analyse), more might have been ready to believe him. But Paget was apparently used to getting his own way, and to having the last word. There is stubbornness, and perhaps even conceit in his attitude:

> The great Lord Rayleigh had also tried his ear at the problem. His widow, sister of A.J. Balfour[21], once asked me to show her my voice models. She then said: “John was always trying to reproduce the human vowe1 sounds; he never succeeded, but you have!” It was certainly encouraging to have made good where such giants as Helmholtz and Rayleigh had both drawn blanks.
>
> (1955: 285–286)

8.3 Robert Curry (b. 1910)

8.3.1 Biography

Robert Oswald Leonard Curry is the most recent figure in this study, but details of his life are sketchy. He was born in Durham in 1910, the son of a schoolmaster at St. Oswald's school. According to Guthrie, in the Foreword to Curry (1940: v), ‘After graduating with honours in English he devoted himself to Education and then, succesively, to Physics, to Phonetics and to Psychology, and finally he studied Anatomy, Physiology and even Laryngology’. By studying the affiliations given in Curry’s various publications, and considering these along with the few dates and events glimpsed from other sources, the following chronology of his career can be assembled.

From about 1932 to 1934 he was at Armstrong College in Newcastle, and held a Noble Memorial Scholarship to fund his PhD. During this period (or possibly just before it), were visits to the universities of Strasbourg and Paris. We know that the kymograph he used in the Newcastle laboratory had been purchased in France. He gained his doctorate with a thesis entitled ‘The theory, practice and application of experimental phonetic methods’ (1934). His first journal publication came the same year.

He attended the second ICPhS in London in 1935, but later that summer he departed for the USA on the Cunard liner *Scythia*, arriving in New York on 24th September. Curry was the recipient of a Commonwealth Fund fellowship, a prestigious US award better known under its present name of Harkness Fellowship. Curry spent his fellowship period at the Speech Laboratory of Ohio State University, working with G. Oscar Russell, author of a major X-ray study *The vowel* (1928).

Curry returned to Britain in June 1937 and spent some time as ‘Demonstrator in Psychology’ at Edinburgh University, probably at the invitation of Douglas Guthrie. The two did some joint research, published in 1938, and Curry gained funding from the Moray Research Fund, a competitively awarded internal grant to support research. An endorsement from Guthrie was probably influential in gaining Curry another prestigious award, a Beit Memorial Medical Research Fellowship, which he held in the Physiology department at UCL. During this period he seems to have written his book *The mechanism of the human voice*, published in 1940.

In August 1939, he returned to the USA, apparently as a ‘Student’ at Columbia University in New York. Mysteriously, he is then documented as arriving in Miami on 27 March 1940 on a flight from Havana—presumably after a temporary stay in Cuba. This may have been simply an Easter holiday break (Easter Day was on March 24 that year). But significantly, his ‘Purpose in coming’ is recorded as ‘to reside permanently’. A few days later on April 1, the 1940 US Census finds him at Columbia University.

No further trace of him has yet been found, and the date of his death has not been established.

8.3.2 Oscillograms

The most original aspect of Curry's PhD research was the development and construction of a camera to capture the display on a cathode ray tube on a rapidly moving photographic film. It is described in his earliest publication (1934b) and illustrated in photographs included in his thesis (1934a). Waveforms produced by the camera are reproduced in his ICPhS paper (1936). For visual display of the speech waveform, the cathode ray oscillograph had many advantages over electromechanical types such as the Duddell oscillograph. But when it came to making a permanent record for analysis, the cathode ray method posed far greater difficulties. Instead of a spot of light from a brilliant source, reflected from a tiny mirror and made to fall directly on the photographic film, it produced a relatively dim trace on a phosphor screen. That screen had to be imaged on to the film by means of an optical system. Furthermore, the light output of the screen was low, and even still photography of the screen was challenging (Hercock 1947). Curry may have been the first to accomplish this type of recording. This was a notable technical achievement, though there was nothing essentially new in his device.

D. B. Fry, in the interview he recorded (1981) for Beverley Collins, describes how he and DJ travelled to Newcastle in order for DJ to produce oscillograms of the Cardinal Vowels with Robert Curry. Fry says this occurred in 1935 or 1936, but since the recordings were mentioned in Curry's presentation at the second ICPhS in July 1935 it must have been in 1934 or 1935. The distance from London to Newcastle is over 240 miles, and the visit must have entailed at least a two-day round trip. Up to this time the only available recordings of the Cardinal Vowels were the acoustic recordings of 1917, which could not provide Curry with the high-fidelity signal he needed for accurate analysis. Curry's account (1936: 196) mentions first that DJ 'paid us a visit to see the apparatus' and implies that the recording of the Cardinal Vowels was a result of the visit rather than being the reason for it.

In the course of the interview Fry is heard to hand over to Collins for safe keeping a photograph of DJ's waveforms, which he says has been in the Department for many years ('the thing is falling to pieces'). It almost certainly corresponds to the figure on page 197 of Curry (1936), and was perhaps the original from which that figure was produced. DJ and Fry were the joint editors of the Proceedings, so presumably they must have handled originals for all the figures in the volume.

Among the artefacts which the author recovered from UCL are what appear to be the film records from that occasion. They are only the known original output from Curry's oscillograph camera and probably the earliest surviving examples anywhere of extended speech waveforms photographed with a cathode-ray device**.** A small cardboard box has been fitted with glued dividers yielding 10 compartments. These contain twenty-eight tight coils of developed 16 mm film. A neatly handwritten card lists the contents and identifies the speakers. There are recordings made by Fry as well as by DJ. The coils of film have been tightly rolled for approximately 80 years and will need professional conditioning if they are to be unrolled and examined.

Fry does not mention the surviving film records or his own recordings of the Cardinal Vowels contained in them. He does recall an occasion when he and DJ were comparing their respective Cardinal Vowel waveforms on an oscilloscope. DJ had (with a 'chuckle' to use Fry's word) observed that Fry's Cardinal 1 appeared 'more cardinal' than his own. Fry interprets this as arising from the visibility on the wave of the strong high-frequency components in his own voice.[22] He remembers the episode as having taken place at UCL after SJ's retirement, though it is not impossible that it was really at Newcastle. If that Newcastle visit was the first occasion when either of them had had the opportunity to examine the speech waveform displayed in real time, it would have been natural for them to make comparisons—indeed, the question of whether two speakers' waveforms of the 'same' sounds would be alike is an obvious one to ask.

In principle, Curry's Cardinal Vowel oscillograms can be turned back into sound, and there is some interest in attempting to do so, since they predate the earliest electrically-made disc recordings of the Cardinal Vowels by 7 years. One might start either from the surviving films (in which case they would have to made suitable for scanning), or from the illustration which Curry made from them. The print has the advantage of being flat for scanning, though it may be expected to have lower resolution. Trials have been made from the print, using a conversion program kindly developed by Mark Huckvale. The program accepts a jpeg file of a waveform, together with a specification (in seconds) of the duration of the sound represented, and a desired sampling rate. The output is a wav file. An analysis of DJ's Cardinal Vowel 4 obtained this way is shown in Figure 8.7. The estimated formant frequencies obtained are F_1 = 963 Hz, F_2 = 1628 Hz; for comparison, the values obtained from a direct analysis of DJ's 1950s recording were F_1 =881 Hz, F_2 = 1614 Hz.

The attempt reveals some engineering shortcomings in Curry's first (handmade) camera. Firstly, the trace is relatively unsharp, perhaps partly on account of an imperfect optical system, and perhaps from spreading of fluorescence on the screen, especially in regions where the deflection velocity is low and the beam dwells relatively longer on one place. Secondly, the vertical deflection axis on the tube face was evidently not perfectly perpendicular to the direction of travel of the film, with the result that the waveform excursions are skewed. The traces can even become multi-valued (the trace occupies two positions on the y-axis at the same time). This has to be remedied by adding a ramp to the waveform—hence the reconstructed signal shown in Figure 8.7 appears to go uphill. A better engineered version of the camera, incorporating several improvements, was later constructed in Ohio (Curry 1937).

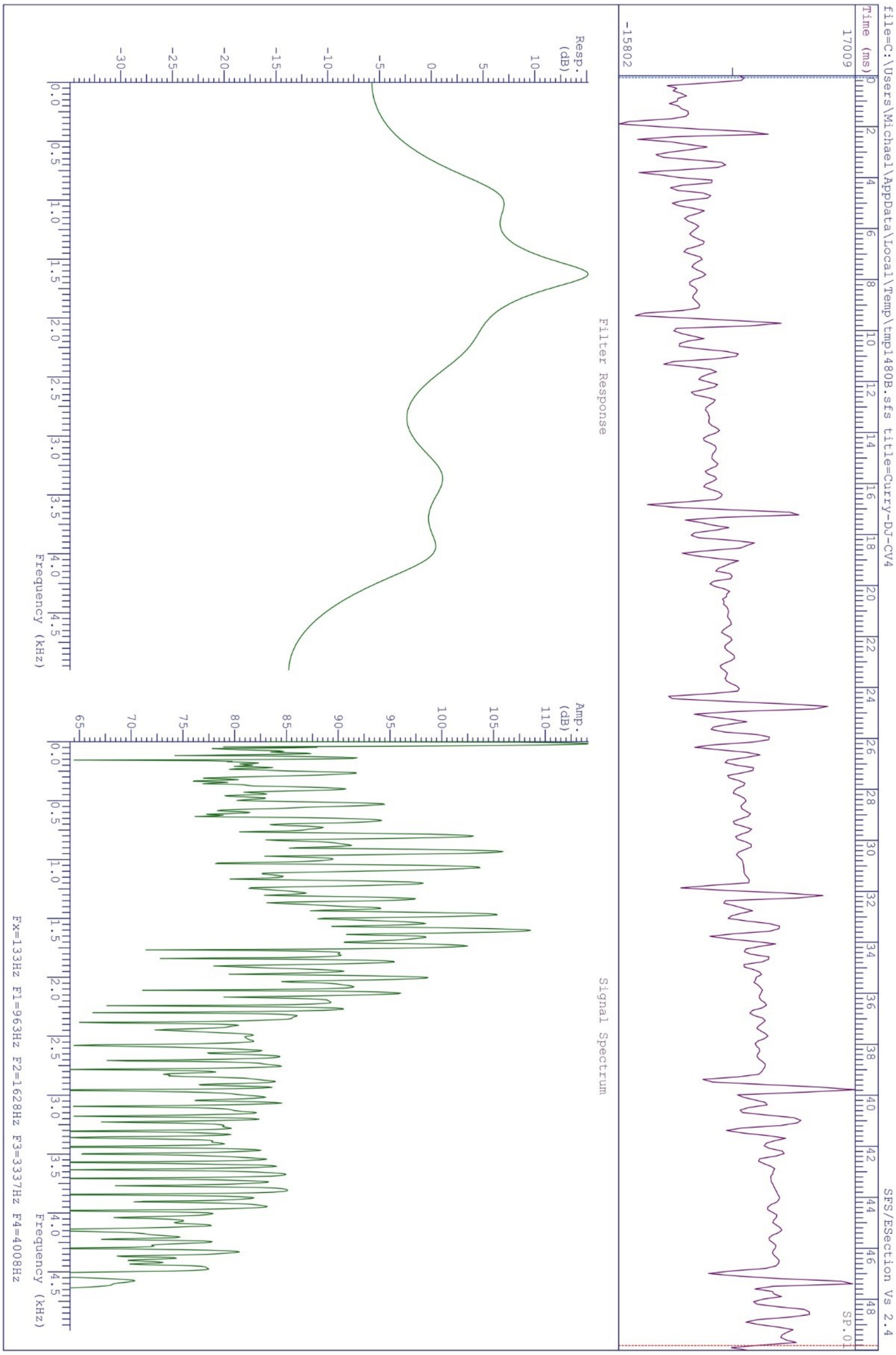


Figure 8.7 Illustrating (top) the reconstructed signal from a printed image of the waveform of DJ's Cardinal Vowel 4 [a] (Curry 1936: 197), with analysis of the result (lower panels).

8.3.3 Curry's work in Ohio

Beginning with (1937a), Curry's publications deal with work he accomplished in the much better equipped laboratory at Ohio. They employ high-quality still X-ray images and linked oscillograms. The papers on soprano, baritone and contralto voices do not necessarily indicate any special concern with singing as such. It was still necessary to sustain the sound which was to be analysed, preferably on a steady pitch, and those with at least some training in singing were thus the most promising subjects. Curry's papers are typical of a widespread effort in the 1930s aimed at producing synchronised multi-channel registrations of running speech (Russell & Palomo 1934). Those authors recognised that 'Prolonged or sung vowels are not characteristic of normal speech' (1934: 223), but until high speed cine X-ray could be developed, the best that could be done was to use X-ray stills corresponding to precisely identified points in sound recordings. Part of Curry's work in Ohio was devoted to the design of circuits to trigger the X-ray exposure automatically—for example, at the first voiceless interval following a vowel within a spoken word. What he produced (1937b: 384) was effectively an analog speech-recognition processor driving relay logic at the output (though none of his published papers seem to report results obtained with these advanced capabilities).

8.3.4 DJ and Curry

Though relatively concise, Curry (1940) is a comprehensive and impartial survey of the whole field of speech, with extensive bibliography. It is an extremely valuable snapshot of the situation at the conclusion of the period covered in this thesis. That the book is not better known is probably a consequence of its appearance in the early days of the Second World War—a fate it shares with Chiba and Kajijama's great work *The vowel* (1941).

Curry (1940: Chapter 5) gives scant (and very critical) coverage of descriptive phonetics. The IPA chart is reproduced (pp. 88–89) and briefly discussed, not as *the* phonetic alphabet but as a 'good example' of 'the type of phonetic alphabet [that has been] produced'. Curry calls the articulatory basis of speech sound classification which underlies the IPA 'quasi-theoretical, quasi-empirical' (p. 87), says that the practice of inferring articulatory activity from what is heard is subjective and unreliable, and that segmentation is arbitrary: 'the division of a given period of speech into units is arbitrary and depends upon the principles of classification' (p. 86). The 'so called phonetic alphabet' (p. 91) fails to take into consideration pitch, intensity and duration—and besides, members of a speech community must share common auditory percepts rather than patterns of articulatory control.

It is a fairly comprehensive list of shortcomings, and would hardly be expected to meet with approval from someone who had devoted most of his life to the IPA and to phonetic description. Nevertheless, DJ's review of Curry (Jones 1945) is very positive. He says the book is a 'conspicuous success' and 'a mine of useful information', and praises the 'copious references'. He can 'recommend it warmly' and concludes with the hope that 'it will not be long before more appears from Dr Curry's pen' (Jones 1945: 31):

> He covers an immense number of topics, including phonation, muscles, nerves, sound-waves, resonance, quality, intensity, duration, pitch, the singing voice, registers, vibrato, hearing, deafness, speech disorders, judgements of personality by voice, experimental techniques...even the phoneme—usually the Cinderella of experimental phoneticians—is not ignored, though we should have liked to see more on this subject.

'We should have liked to see more' is very different from saying that Curry is wrong about the phoneme. Similarly, in relation to Curry's Chapter 5, in which the very critical account of the classificatory basis of the IPA appears, DJ does not voice disagreement—on the contrary, he calls for 'considerable expansion' of the chapter.

Overall, the review reads as if DJ comfortably acknowledged that the IPA, and much of his own work, formed merely a part of one limited area of the broad field of phonetic science.

Notes to Chapter 8

[1] https://www.rcseng.ac.uk/museums/archives/documents/mrcs-exam-factsheet (23 July 2016).

[2] *The Courier-Mail* (Brisbane, Queensland), Friday 29 December 1933, page 2. No explanation has been found for why this item appeared in an Australian newspaper.

[3] Her father, Brees van Homan (1879–1932) was admitted to the Institution of Mechanical Engineers (Graduate 1897, Member 1908) and is then described as being a partner in Messrs. Homan and Rodgers. (He is however, identified as a 'constructional' engineer, and Homan and Rodgers seem to have been architectural engineers.) Frieda appears to be the only family member with the initial F. No explanation has so far come to light for the choice of the name Janvrin. Before her marriage to Brees van Homan, Frieda's mother was Gertrude Tims.

[4] Scripture's paper misspells 'Thomson' as 'Thompson'. It is unclear whether he or the editors are at fault. Another British physicist of the period was Silvanus P[hillips] Thompson (1851–1916). To add to the possible confusion, Thompson was the first biographer of Thomson (Thompson 1910). Scripture also gives the reference as 1899: 73. Presumably this refers to a later edition.

[5] Donelan, James (1886–1922), Surgeon, Throat Department, Italian Hospital, London, and to Royal Society of Musicians of Great Britain and Ireland; Lecturer, Diseases of Nose and Throat, Medical Graduates College, London; President, Section of Laryngology, Royal Society of Medicine, 1918–19; Permanent Organiser of Surgical Staff, Hôpital Régional, Villeneuve-sur-Lot, 1914–18.

Source: *Who Was Who*. Published online April 2014 | e-ISBN: 9780199540891

[6] In his later writings on vowels, Scripture places increasing emphasis on the idea that vowels are characterised by the free (ringing) response of the vocal tract cavities to 'puffs' of excitation, and have nothing to do with 'filtering' or 'resonance'. In consequence, he seems to have believed that voice quality is completely unaffected by vocal tract resonance, and thus that the quality of a singer's or speaker's voice is entirely determined by the larynx (and, presumably, therefore, he must have thought that it would be heard just as well in the *unmodified* larynx tone). He gives no consideration to the form or acoustic spectrum of the excitation pulse.

[7] Chief among the unpublished sources is Paget's autobiography, 'Hits and misses' (Cambridge MS Add. 9671), from which much of the material in Blunt (1962) appears to be directly drawn. The typescript of more than 300 pages is dated internally to May 1955. Paget died in October that year, probably before final revision was completed. In addition, there is a large collection of Paget papers at UCL and further material relating to 'NSL' (New Sign Language) held at Cambridge. A 1936 Soviet-made sound film ('MEKHANICHESKAYA RECH', British Film Institute catalogue 17262) has also been examined; it shows Paget

delivering a lecture on speech and demonstrating various synthesis models and techniques. In addition to his numerous publications (see References), Paget is named as inventor or co-inventor in at least 12 British patents, two of which directly concern speech synthesis. One patent (on marine depth sounding) was obtained jointly with William Duddell. It seems probable that if Duddell had lived (he died in 1917 before Paget's interest in speech sounds developed) the two might have collaborated very profitably on speech research.

8 Paget (1943: 80) actually says that the proposal to employ a systematic sign language as an international language had been made by Daniel Jones 'nearly twenty years ago', though no published proposal from DJ has been traced. Similarly Paget (1955: 281) mentions 'the Utopian suggestion of Professor Daniel Jones that a systematic Sign Language might be the ideal Universal Language of mankind'.

9 Paget seems to have taken an interest in existing sign systems from about 1928. Thus (1928b) includes an illustration of a deaf signer being filmed. The 'Paget-Gorman' system that was promoted in the postwar period is an invented sign system, distinct from both BSL and 'Signed English'. At first it was called New Sign Language (NSL).

10 The slightly skewed appearance of the figure is a feature of the original, which was evidently drawn or reproduced out of true. The angle at top left is 89.12 degrees

11 Bell tapped on the larynx to elicit a response dominated by the lower resonance, and on the teeth or cheek to make the upper resonance dominant. His findings (Bell 1879) anticipate Paget's in that he is quite clear that *all* vowels have double resonance, and that a systematic sequence of vowels exhibits an orderly arrangement of resonances: 'For instance, commencing with the high-front vowel (ee), assume successively the positions for the other vowels of the front group. A double series of resonances will be obtained, so arranged that the tones of one series fall in pitch while the tones of the other rise' (1879: 167). He does not, however, provide quantitative estimates of the resonance frequencies. A further similarity (specifically with Paget 1922a) is that Bell likens the higher resonance to a 'whistle'.

12 Secretary's Annual Report for 1922, *Transactions of the Philological Society* 29, page 15. There appears to be no corresponding written paper.

13 Though he worked on numerous aspects of radio propagation and wireless communication, William Henry Eccles is now chiefly remembered for the invention, with his colleague Frank Wilfred Jordan (1881–1941), of the Eccles-Jordan trigger circuit later known as the 'flip-flop', which is the building block of digital computers.

Eccles included the vowel apparatus in a discourse at the Royal Institution on 13 April 1923, which was published in *Nature* on 7 July (Eccles 1923). It is said to have been built by his students C. F. A. Wagstaffe and E. S. Smith. A full description of various versions of the apparatus—including a version where the resonance components are not heard simultaneously, but alternated at the voice frequency, and the linked notion of 'persistence of audition'—is given in Eccles and Wagstaffe (1924). Eccles and Wagstaffe seem to have worked with the rather high fundamental frequency of 250 Hz.

14 The locus theory was evolved over the years 1952–1955, largely as a result of work with the Pattern Playback at Haskins Laboratories. Its creators make little reference to previous work—and certainly none to Paget. They do mention as an antecedent the 'hub' concept of Potter, Kopp & Green; for those authors the 'hub' was the target F_2 of *any* sound, including vowels, though (as with the locus) the hub not might be reached (Potter *et al*. 1947: 39–43; 49–51). Potter *et al*. (1947) themselves make no mention of previous work.

15 The word 'cheirophone' (presumably /ˈkaɪ(ə)rə(ʊ)fəʊn/) is composed of Greek elements meaning 'hand' + 'sound'. The *cheiro*- element is seen (though spelled differently) in *chiropractor*, etc. Paget presumably selected the spelling *cheiro*- for a direct Greek coinage (i.e., one that had not passed via Latin and possibly French). There are a few words with *cheiro*- in OED, such as *cheirokinaesthesia* (ability to feel movements of the hands). 'Cheirophone' itself is not in OED.

16 The content is a version of Shaw's piece 'Spoken and broken English', which Shaw himself recorded in 1927.

17 The plot incorporated reference frequency markers for alignment, which were later deleted. The superimposition was accomplished using a transparent layer in a PhotoShop document.

18 In both their study and the earlier ones they refer to, it is not clear whether the different formant frequencies are 'real'—in the sense that they reveal different resonance frequencies of the vocal tract in the two conditions—or whether the lower estimate of F_1 in the phonated condition results from the very different excitation spectrum. But that is irrelevant to the present argument, since both Paget's estimates and those made instrumentally appear to be affected the same way. It should be noted that Kallail and Emanuel used only female speakers.

19 It will be noticed that Paget consistently places the FOOT vowel (not GOOSE) at the extreme of the vowel scale. This is the reverse of the expected ordering.

20 Paget—probably accurately—noted at least faint nasal components in many of his early analyses of 'oral' consonants. Taking an entirely empirical approach, he quite rightly did not at first assume that they were irrelevant. Only later did he adopt the simplification of analysing oral consonants as entirely without nasal resonance (1930: 310).

21 Arthur James Balfour (1848–1930), Prime Minister 1902–1905, First Lord of the Admiralty and later Foreign Secretary during WW1. 'John' is Rayleigh—i.e., John Strutt.

22 Even in later life (the present author knew him from 1971), Fry had a noticeably 'bright' high tenor voice.

9

Retrospect and prospect

9.1 Retrospect

Chapters 1, 2 and 3 show that nineteenth-century Britain did not lack ideas, techniques or personnel in the field of speech science. Indeed, Britain was abundantly supplied with polymaths such as Herschel and Ellis, outstanding engineers such as Barlow and Duddell, and sharply-focused experimentalists such as Jenkin and Ewing. Their various contributions were incorporated into the experimental phonetics that was eventually re-imported.

But the 'experimental phonetics' which came into being on the Continent around 1890 was not just a re-branding of the centuries-old cross-disciplinary science of speech to which figures such as Thomas Young, Erasmus Darwin and Robert Willis had contributed. It was an academic fashion—a faction within phonetics which began almost as soon as academic phonetics itself got established.

Experimental phonetics was 'the one right way' of doing things, and it wanted autonomy. Its name became a buzzword. It attracted zealous devotees (such as Scripture)—and made enemies (such as Sweet). It had an agenda. It founded laboratories, started journals, established university posts, organised lectures, congresses and summer schools, enrolled students and appointed examiners. It enhanced the prestige of the institutions that hosted it.

Obviously, autonomous experimental phonetics could not take root in Britain ahead of autonomous phonetics itself—and that had been delayed for decades by the hidebound conservatism of British universities that Alice Herbage Edwards bewailed. Even modern language faculties and physical science laboratories faced stubborn opposition. The public addresses of James Clerk Maxwell (Maxwell 1890)—the first Cavendish Professor in Cambridge, and the most significant physicist between Newton and Einstein—show him accommodating to the stylistic and cultural expectations of an audience brought up on the classics who suspected that there was 'more than a whiff of the factory floor' about any laboratory (Morus 2009: 227).

Mathematical physicists of the stature of Maxwell or Rayleigh could in principle have advanced the acoustic theory of speech production enormously had the right research questions been asked, while on a practical level the ingenious experimentation with simple equipment which both favoured—and which is encapsulated in the handbook *Practical Physics* by two of Rayleigh's associates (Glazebrook & Shaw 1885)—could profitably have been imported into the early phonetics laboratory. But the experimentation which became fashionable in the manner of Rousselot was commonly done with unnecessarily elaborate apparatus of unquantified behaviour, and often with rather poor attention to precision and proper controls. Scripture continued to draw audacious conclusions from inadequate data to the end of his career.

As for the phonetics laboratory itself, it was seen principally as an establishment, a physical asset, with its hoard of instruments on display in the glass-fronted cabinets which typically lined the room. But once established, a collection of laboratory apparatus soon begins to go out of date. The UCL laboratory was largely obsolete almost as soon as it began, and by the mid 1920s was forced to put a brave face on limited resources. In many ways it would have been better, both there and elsewhere, to make common cause with medicine, engineering or physics, and share the use of expensive hardware such as the Duddell oscillograph. Rousselot proudly showed a visitor such as Jespersen around his *domaine*, but if research—rather than impressing one's visitors—is the purpose, apparatus need not be owned. It can be shared, or even housed in a central location and used remotely. Frieda Janvrin has a remarkably prescient comment. The work she and Scripture were doing depended on sound-tracks made with studio film equipment. They did not own it, and it wasn't portable, but as she remarks 'It would be quite feasible to arrange for recording over the telephone system from a hospital to a studio' (Janvrin 1933: 642).

But Rousselot's proprietorial model of the phonetics laboratory prevailed, and thus every new laboratory contained the seeds of its own obsolescence. Rousselot's own laboratory in Paris—founded with such fanfare—was soon outmoded, and the energy and impetus to replenish it were lacking (Scripture 1925: 164–165). His forgotten successor, the abbé Millet, describes a laboratory equipped with museum exhibits (Millet 1925).

The dramatis personae introduced in the preceding chapters form a gallery of polymaths, inventors, amateurs and eccentrics, many with remarkable gifts, who contributed in diverse ways to the science of speech—but hardly anyone who qualifies primarily as an 'experimental phonetician'. Stephen Jones comes closest, and he struggled to think of anyone else in Britain who might be recruited to the International Society of Experimental Phonetics. But his career seems to have been stunted (no doubt unintentionally) by the predominance of Daniel Jones. SJ had hardly any scope to function as an autonomous researcher at all.

Meanwhile, from the late 1920s, particularly in the Netherlands and the USA, a new kind of phonetic science was establishing itself, in which the sole British participant before World War II was to be Robert Curry.

Curry began to publish in the same year he completed his PhD, travelled to wherever he could find research opportunities (Ohio, Edinburgh, London), and gained a series of grants. Only a few years earlier, Negus had thanked his father for financing his monumental research, enabling him to avoid 'the possible disadvantages of the restrictions imposed by grants' (1929: ix), but Curry lists the grants he gained as marks of prestige.

It is significant that Curry was the only British researcher to publish in the *Archives Néerlandaises* (1938c). Furthermore, an examination of his bibliography will show that his seven major research papers in 1937 and 1938 are each published in a *different* journal. And although there are resemblances in technique among the various papers, they are genuinely distinct in content: Curry is not a self-plagiarist. In short, Curry's career path and publishing activity are those of a *professional* speech scientist. When compared with any other figure in this study, he belongs to a new world.

9.2 Prospect for future work

This thesis has attempted to make contributions on a number of levels, from the specific to the more general, and in further work it is hoped to pursue efforts on all of these.

(1) At the most fundamental level, the thesis presents detailed accounts of particular British contributors to phonetic science, of the instruments they developed and used, and the circumstances in which they worked. Naturally, there is extensive

scope for further additions. In fact, considerably more work has already been begun but had to be omitted from the present work for reasons of space. To give just a single example, alongside the analysis of DJ's work on *Intonation curves* reported in Chapter 5, a corresponding study was conducted of his work (with the anthropologist Northcote Whitridge Thomas, 1868–1936) applying the same method to the analysis of Igbo tones in wax-cylinder recordings made in the field (Thomas 1914). It is planned to publish this in the near future.

(2) Somewhat more generally, there are whole areas of application of experimental phonetics which are under-represented in the account presented here. We can point, for instance, to the role of experimental phonetics in foreign-language teaching, speech pathology and therapy, and hearing and deafness. Each deserves extended treatment in its own right. There is scope, too, for much more work on aspects of the history of speech technology and engineering. The relevant activity of industrial and military laboratories in Britain—especially any secret work done in wartime—remains to be uncovered. Developments in the field of speech recording and reproduction originally formed a separate section of the planned thesis, but eventually limitations of space forced the use of only small selections from the material, introduced at relevant places in the narrative.

(3) The self-imposed limitations in coverage mentioned under (1) and (2) above were partly guided by the aim—mentioned at the outset and kept constantly in mind—of throwing light on the specific artefacts that had passed into the author's care. The curation of these unique items outweighs the extension of the investigation on purely academic grounds into fields where the author has no more responsibilities and duties than any other researcher. As an example of continuing work in this direction may be

cited the author's invited visit to Japan in September 2016, undertaken in order to repatriate a collection of 89 glass lantern slides found in London but undoubtedly produced in Japan during the mid 1930s by UCL alumnus Tsutomu Chiba (1883–1959). A brief summary of the background is published in Ashby (2016). Work of this kind will continue.

(4) Where appropriate and possible, it is intended that artefacts should not merely be preserved in suitable archival homes, but also digitised and distributed. The author has already made a start on this, finding funds to digitise early film of the UCL laboratory and make it freely available (Ashby 2011). Work of this nature will continue. For instance, the year 2017 is the centenary of the Cardinal Vowel system, and it would be appropriate to mark this by making generally available the high-quality digital transfers from the early Cardinal Vowel recordings which were prepared for the present research.

(5) Finally, and at the most general level, as the brief 'Retrospect' above is designed to suggest, the countless empirical details presented in the earlier chapters do indeed fall into a 'big picture'—but it is not, as some might perhaps have hoped and expected, a big picture of the 'development' or 'evolution' of experimental phonetics. Instead, the 'big picture' turns out to be nothing less than the whole historical, social and cultural context in which the events unfolded, and the changing and interacting roles of such factors as education, religion, gender, and social class. A major but exciting challenge for future work is to enrich the understanding of that broad context in appropriate ways.

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