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ON THE HISTORY OF DISEASE-CONCEPTS: THE CASE OF PLEURISY

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It is, I believe, uncontentious to suggest that concepts of disease — both in general and with respect to particular ailments — have changed and developed historically in the long history of the Western medical tradition. And it is obvious, too, that diseases (taking that term in its widest sense, to embrace illnesses at large) were-and-are precisely the distinctive concern of medicine. On principle, therefore, we might expect that *the history of disease-concepts* plays a central part in the historiography of medicine, just as the histories of celestial, physical and vital concepts do in the historiographies of the respective natural sciences; yet paradoxically, this is far indeed from being the case. That strand of medical history which focuses on medicine's cognitive content has devoted far more attention to anatomical and physiological knowledge than to pathology; the history of medical practice is often written without reference to the disease-categories by which past practitioners apprehended the illnesses of their patients; and as we shall see, histories of actual diseases have tended to treat their objects as timeless entities, thereby blocking off the very possibility of considering disease-concepts historically.

This paper proposes that the history of disease-concepts deserves far more attention than it has traditionally been accorded, not least because this theme is relevant to the full range of medical history's existing concerns, from anatomy to medical practice. I shall proceed in three stages. Part 1 sketches the historiographic state of play with regard to disease-concepts. Part 2, which makes up the bulk of the paper, seeks to illustrate the possibility of treating such concepts historically by means of an example, namely the disease of "*pleuritis*" or "*pleurisy*"; I shall trace the shifting meanings of this term first amongst the ancients and then, on a very selective basis, in the early-modern period. Finally Part 3 will meditate briefly on the results of this case-study, returning to the general historiographic theme and suggesting some points of wider application.

1. HISTORIOGRAPHY

There is already a substantial tradition of scholarship which treats concepts-of-disease as historical products, and which has traced their histories in a series of particular cases. That tradition could be said to begin in the 1930s and '40s, with three pioneering studies: Ludwik Fleck's *Genesis and development of a scientific fact*, which stressed that "syphilis" was a historically-changing concept;¹ Owsei Temkin's *The Falling Sickness*, a history of concepts of epilepsy from ancient

to modern times;² and Georges Canguilhem's *The normal and the pathological*, a critical dissection of the mid-nineteenth-century shift in the definition of the pathological, which was written as a doctoral thesis in the 1940s and was published in 1966.³ The tradition was renewed in the 1970s and '80s, most explicitly by the school of "social-constructivists"⁴ but also by various other medical historians working from a range of different perspectives;⁵ in the 1990s it is still alive and well, as various recent studies attest.⁶ And this now substantial body of work has consistently demonstrated the historicity of disease-concepts — not just for putatively "soft" diseases such as epilepsy, chlorosis⁷ and hysteria,⁸ but also for diseases which all observers regard as having a physical, bodily basis. This can be illustrated with reference to four examples: syphilis (arguably the classic case, since Fleck historicized it as long ago as 1935); asthma (which has often been assigned a "psychosomatic" component, but which has always been seen as thoroughly "somatic" as well); coronary thrombosis; and Bright's disease.

(1) The historicity of "syphilis" was the fundamental premise of Fleck's *Genesis and development of a scientific fact*: the book opened with a little history of "how the modern concept of syphilis originated",⁹ and Fleck went on to argue that every relevant development in knowledge entailed a change in the very definition of what we know as "syphilis". This impact of knowledge upon disease-concepts was particularly marked in the cases of the two great discoveries of the early twentieth century — the microorganism *Spirochaeta pallida* (first detected in 1905) and the Wassermann reaction (first reported in 1906, gradually refined into a serological test).¹⁰ Of the Wassermann reaction, which was the central concern of his study, Fleck wrote that it "*redefined* syphilis",¹¹ and indeed he argued that the very success of that reaction as a diagnostic test required such redefinition of the disease.¹² And with respect to the discovery of the spirochaete, he remarked:¹³

The statement, "Schaudinn discerned *Spirochaeta pallida* as the causative agent of syphilis" is equivocal as it stands, because "syphilis as such" does not exist. There was only the then-current concept available on the basis of which Schaudinn's contribution occurred, an event that only developed this concept further. Torn from this context, "syphilis" has no specific meaning....

The reason that Fleck thus historicized "syphilis" was that he was seeking to problematize scientific knowledge and its making, using the Wassermann reaction as his case-study. I shall shortly have occasion to look more closely at Fleck's conception of knowledge; for the moment, what commands attention is the fact that he depicted the "modern concept of syphilis" precisely *as* a concept, that is, as a human product.

(2) Asthma, John Gabbay showed in a paper of 1982,¹⁴ began in the ancient world as a symptom; it changed its nature in the Renaissance, becoming a disease in its own right, and one located in the abdomen; subsequently, in the eighteenth century, it moved its abode to the thorax; it was repeatedly reinterpreted throughout the

nineteenth century; and it went on being transmuted until the mid-1970s. In short, the entire course of asthma's long history was a story of change, to the very eve of the time that Gabbay was writing.

(3) Coronary thrombosis has a history of a very different kind, since this disease was only discovered, or invented, in the 1920s; but from Christopher Lawrence's study it emerges that coronary thrombosis was and is just as much a constructed and contingent "entity" as asthma.¹⁵ For instance, coronary thrombosis was stabilized only with great difficulty, if indeed it was stabilized at all;¹⁶ its relation to ECG findings (its supposed clinical test) was consistently problematical;¹⁷ and Lawrence went on to suggest that it was disappearing as a clinical category in the 1990s. It seems, then, that coronary thrombosis is perhaps a disease-concept with an end as well as a beginning; and just this pattern is revealed by my final example.

(4) What was called "Bright's disease" — that is, a particular kind of dropsy which Richard Bright distinguished in 1827 — has been elegantly interrogated by Steven Peitzman.¹⁸ We thus learn that "Bright's" was not one but three diseases; that its signs and symptoms were always variable, as were its putative causes (cold according to Bright himself, microbes according to a later generation, immunological changes still later); and that in the twentieth century "Bright's disease" went into terminal decline. By about 1950 "Bright's" had given up the ghost, ironically yielding place to the older category "nephritis", which was succeeded by "renal failure", which in turn gave way, in the age of dialysis, to "end-state renal disease";¹⁹ and each of these new names designated a different clinico-pathological concept, for all that these various concepts overlapped.

The collective import of these and other such studies has been that concepts-of-disease, like all concepts, are human and social products which have changed and developed historically, and which thus form the proper business of the historian. Yet this insight has been held in check by another approach, in which diseases *throughout history* have been identified with their *modern* names-and-concepts. Under this very different rubric, *responses to* diseases, both on the part of individual patients-and-doctors and on a larger scale in the form of such political measures as quarantine, are permitted to vary historically; but this historiographic permission is withheld from *diseases themselves*, since those diseases are all taken to coincide with their respective modern concepts. And the effect of this approach is to construct a conceptual space in which the historicity of all disease-concepts, whether past or present, has been obliterated. Past concepts of disease have simply been written out of existence; and the historicity of modern disease-concepts (or what are taken to be modern ones) is effaced, because those concepts have been assigned a transhistorical validity.

These two approaches have now been running side-by-side for some decades; yet the tension between them has attracted remarkably little comment.²⁰ Since one chief purpose of the present paper is to bring that tension to light, it will be worth

comparing the different ways in which these two traditions have approached a single disease. The most convenient example to choose is the disease which came to be known as “syphilis” — for this was precisely the disease-concept which Fleck historicized in his *Genesis and development of a scientific fact*, and it is also the theme of a highly-regarded history-of-disease study, namely Claude Quétel’s *Le mal de Naples* (1986), translated in 1990 as *History of syphilis*.

Quétel’s *Le mal de Naples* is (as its introduction puts it) a study of “syphilis as a cultural phenomenon”.²¹ Seen strictly within its own terms of reference this is an admirable work, which beautifully captures many of the cultural responses evoked over the centuries by what was variously called the “*mal de Naples*”, “great pox”, “*morbus Gallicus*”, “*morbus venereus*”, “*lues venerea*”, or “syphilis”. But Quétel’s terms of reference are precisely what is at issue here, for his inquiry rigorously excludes from consideration the various *concepts* associated with this motley welter of names. The reason for this exclusion is that one such concept, namely the modern concept, has been privileged as the factual mirror of Nature: *Le mal de Naples* is entirely premised upon retrospective diagnosis. Early-modern disease-descriptions are consistently read through the conceptual grid of the modern concept of “syphilis”; as Quétel depicts it, Schaudinn’s *Spirochaeta pallida* was (or rather, is) simply “the syphilis microbe”, and the Wassermann reaction yielded a diagnostic test for an implicitly unchanged “syphilis”. And as a result, the little that emerges concerning disease-concepts presents a series of paradoxes which are in radical tension with the book’s premises and are accordingly left entirely unresolved. For instance, the very name “syphilis” presents a riddle: coined by Fracastoro in the early sixteenth century, it lay dormant until the eighteenth century, when it somehow displaced “the great pox” and other terms which had been used for the previous two hundred years; yet this profound shift of terminology emerges only in asides.²² Again, the “great pox” included, as one of its symptoms, a “gonorrhoea” (that is, a flux-of-seed),²³ which in the early nineteenth century came to be seen as a separate disease;²⁴ yet Quétel barely notices that this puts in question his own identification of the “great pox” with “syphilis”.²⁵ In sum, disease-concepts lie beyond the conceptual horizon of *Le mal de Naples*; they enter Quétel’s discourse only to be instantly dismissed from consideration.

Ironically enough, it turns out that this very approach to the history of diseases had already been described, criticized and explained in Fleck’s *Genesis and development of a scientific fact*. In order to bring out this point, it will be necessary to outline Fleck’s larger theses concerning the nature of scientific knowledge.

We saw earlier that Fleck sought to problematize both the content and the making of scientific knowledge. What we must now notice is that in the pursuit of this aim, Fleck consistently depicted the development of such knowledge as a social process. “Not only the principal ideas but also all the formative stages of the syphilis concept”, he wrote, “are the result of collective, not individual, effort”;²⁶ and he went on to portray scientific knowledge in general as embedded in a “thought style”, associated with a specific “thought collective”. Thus a “scientific fact” such

as the Wassermann reaction was “a stylized signal of resistance in thinking ... by the thought collective”.²⁷ Equally important, the subsequent *maintenance* of any such fact, that is, its historical stability, required “social reinforcement”;²⁸ and Fleck proceeded to elaborate the social mechanisms by which historically-produced knowledge was maintained and stabilized.²⁹ Here Fleck’s key concept was what he called “popular science” or “science for nonexperts”: that is, a drastically-simplified version of scientific knowledge, whose aim is “the apodictic valuation” of “a certain point of view”.³⁰ In “popular science”, the uncertainty which is characteristic of scientific research has been replaced by certainty; the need for arduous proof is eliminated by creating a “vivid picture”; and *the historicity of concepts is necessarily eliminated*, for certain of those concepts are assigned the quality of eternal truths. This erasure-of-history, Fleck explained, has its own particular rhetorical mechanisms: in particular, the collective character of knowledge-making is obliterated and is replaced with a mythical, individualized picture,³¹ involving a Manichean conflict between “the ‘bad guys’, who miss the truth, and the ‘good guys’, who find it”.³² But the key point is that the historical development of concepts is thereby erased. And in the particular case of *disease*-concepts, this means that the modern concept is extended backwards in time: the disease as presently conceived is seen as a permanent entity,³³ and it is assumed that it can be diagnosed retrospectively on the basis of earlier texts which in fact conceived that disease in radically different terms from those underlying the modern concept.³⁴ In sum, Fleck not only demonstrated the historicity of “syphilis”; he also showed that the contrary picture — the view which eternalizes the modern concept of “syphilis” — is intelligible as a necessary by-product, within “popular science”, of the modern concept itself.

Le mal de Naples is obedient in every particular to the “popular-science” view of syphilis and its history, exactly as Fleck sketched it. As we have seen, the very foundation of Quéтел’s approach is the assumption that the “mal de Naples”, the “great pox”, and so on, are all to be identified with one another under the sign of the modern concept of “syphilis”, and this entails that the actual meanings of these various disease-concepts are suppressed. Inevitably, therefore, Quéтел’s study scarcely considers the making of medical knowledge.³⁵ And to the extent that the development of medical knowledge is brought into view, it is depicted in strictly teleological terms (for instance, as a “step in the right direction”³⁶) and is rigorously individualized³⁷ — precisely in line with the dictates of the “popular-science” conception of knowledge as delineated by Fleck.

The profound and pervasive contrast between the approaches of Fleck and of Quéтел stems from a radical difference in conceptions of the cognitive, of the social, and of their interrelationship. Specifically, Fleck saw knowledge as socially-constituted, thereby placing the cognitive *within* the social, whereas Quéтел has drawn a rigid boundary around the realm of the social and has placed knowledge *outside* that boundary. It should also be observed that Fleck went to some trouble to argue for his conception, whereas Quéтел simply assumes his conception — and

symptomatically, Quéтел nowhere mentions Fleck's work. The final irony is that this self-limitation radically restricts the scope of the responses-to-syphilis which Quéтел allows himself to depict, thereby impoverishing the very argument which he was striving to develop, namely the vision of "syphilis as a cultural phenomenon". But what is significant in the present context is that Quéтел's founding act of excluding the cognitive from the social has systematically obliterated the historicity of concepts-of-disease.

This contrast is by no means confined to the individual figures of Fleck and Quéтел, or to the specific case of syphilis: on the contrary, it runs through the whole historiography of medicine. We might well describe it, using Fleck's own terms, as a difference in "thought styles" — specifically, "thought styles" concerning the nature of disease-concepts. By way of a convenient shorthand, I shall term these two thought styles 'the historicalist-conceptualist approach' and 'the naturalist-realist approach'. Their defining characteristics can be summarized as follows:

(a) The *historicalist-conceptualist* approach takes concepts of disease as objects of historical study: this is the tradition which I have illustrated by the studies of Fleck on syphilis, Gabbay on asthma, Lawrence on coronary thrombosis, and Peitzman on Bright's disease.

(b) The *naturalist-realist* approach excludes disease-concepts from historical investigation, because it takes modern disease-concepts as the mirror of natural reality: this is the countervailing tradition of which I have taken Quéтел's study of syphilis as a specimen, and which is also illustrated by many other studies of responses-to-diseases and of historical epidemiology.

Now as I have already remarked in passing, the tension between these two approaches has scarcely been noticed.³⁸ And the reason for this, I suggest, is simply that *the naturalist-realist approach to diseases has played and continues to play a hegemonic role in the historiography of medicine*. That is to say, there has been no confrontation between the two approaches, because the naturalist-realist approach has been dominant. This makes intelligible the otherwise paradoxical fact that historians of medicine — in contrast to historians of science, from Thomas Kuhn onwards — have shown very little interest in Fleck's *Genesis and development of a scientific Fact*.³⁹ The indifference of the historiography of medicine to Fleck's work, then, reflects a deeper problem: that the historicalist-conceptualist approach to the history of diseases has remained subordinate in medical history.

But perhaps the most telling index of the dominance of the naturalist-realist approach is that its assumptions pervade even some of the leading attempts to historicize medical knowledge in general, and apprehensions of disease in particular. I shall illustrate this by considering four classic works, spanning almost half a century; remarkably enough, we shall see in each of these cases that the historicity of disease-concepts has actually been written out.

(1) My first example is the very book which is usually cited as the paradigmatic example of the history of a disease-concept:⁴⁰ Owsei Temkin's *The Falling Sickness*, published in 1945. Now Temkin's approach to the history of epilepsy was admirably and rigorously historical, for he wrote in his preface that "we must above all find out what was meant by epilepsy", and "we must take the past seriously and try to understand it". But in the very act of defining this approach, Temkin placed severe boundaries upon its application — for he confined his vision of the historiography of disease-concepts to those diseases whose bodily riddles medicine had not (in his view) solved:⁴¹

A history of epilepsy seems a premature, perhaps even a doubtful enterprise. There is no unanimity about the range of the concept of epilepsy, and the nature of the disease is yet obscure....

All these doubts and questions suggest that a history of epilepsy cannot be written in the manner in which, for instance, a history of tuberculosis might be approached. In the latter case, we have definite and well-founded knowledge of the nature of the disease. *This knowledge we can apply as a critical standard to the past and can separate the true from the false.* At present such a procedure is not possible with regard to epilepsy, for we may easily decree as false what the future will prove to be true....

This being the case, there seems but one way left. We must above all find out what was meant by epilepsy, what symptoms were attributed to it, how it was explained and how treated. That means we must take the past seriously and try to understand it.

That is to say, the attempt to "find out what was meant by epilepsy" and to "take the past seriously" was *only* appropriate *because* epilepsy was still not adequately understood. And conversely, Temkin assumed that when it came to such a disease as "tuberculosis", its history should be written strictly in naturalist-realist terms — defining "tuberculosis" along the lines which Koch had put forward in 1882,⁴² and thereby obliterating the history of all the relevant concepts, including the concept of "tuberculosis" itself. This of course was precisely the anachronistic approach which Fleck had explained and criticized ten years earlier.

(2) A classic of a very different kind was Foucault's *Naissance de la clinique*, which was published in 1963 and was translated into English in 1973 as *Birth of the clinic*.⁴³ It may seem strange to suggest that *Birth of the clinic* blocked off the possibility of historicizing concepts-of-disease, for that work has widely been perceived precisely as having historicized medical knowledge, and specifically clinical knowledge,⁴⁴ and in one sense this was indeed the thrust of the book — as was only to be expected from a pupil of Canguilhem's. Yet in fact Foucault so constructed his theme as to erase the very possibility of writing the history of disease-concepts, and this in two ways. In the first place, the threshold which he depicted medicine as crossing with the "birth of the clinic" was a transition not within knowledge, but on the contrary from non-knowledge to knowledge.

Concepts-of-disease before *la clinique* were consigned to “the language of fantasy”; concepts-of-disease inaugurated by *la clinique* “made it possible ... for the patient’s bed to become a field of scientific investigation”; the shift from the one to the other consisted simply in the fact that “language has turned into rational discourse”.⁴⁵ Thus the historicity of disease-concepts consisted in one event and one event only, namely the transition itself — a transition which was neither more nor less than the move from darkness into light, from invisibility to visibility, from “fantasy” and “imaginary investments” to “scientific investigation” and “rational discourse”.⁴⁶ And on neither side of this divide was there any further story to be told.⁴⁷

Secondly, in line with the larger “archaeological” vision which he was then in the process of constructing, Foucault conceived this transition in language, this “mutation in discourse”,⁴⁸ as strictly *syntactic* in nature: that is, as concerned not with the meanings of words but purely with their mutual arrangement. Having asked at the outset “From what moment, by what *semantic or syntactical* change, can one recognize that language has turned into rational discourse?”,⁴⁹ he went on to argue — at the climax of his preface — that the semantic dimension was irrelevant and that the “mutation in discourse” could be identified at the level of syntactics alone:⁵⁰

But is it inevitable that we should know of no other function for speech (*parole*) than commentary? *Commentary* questions discourse as to what it says and intended to say ... in other words, in stating what has been said, one has to re-state what has never been said. In this activity ... is concealed a strange attitude towards language: to comment is to admit by definition an excess of the signified over the signifier; a necessary, unformulated remainder of thought...

To speak about the thought of others, to try to say what they have said has, by tradition, been to analyse the signified. But.... Is it not possible to make a structural analysis of discourses that would evade the fate of commentary by supposing no remainder, nothing in excess of what has been said, but only the fact of its historical appearance? ... The meaning of a statement would be defined not by the treasure of intentions that it might contain ... but by the difference that articulates it upon the other real or possible statements....

Now in fact this formulation rested upon an “attitude towards language” no less “strange” than that of commentary itself: for in invoking “what has been said” as his object, Foucault simply reintroduced the very problem which he had been attempting to evade, namely the problem of meaning. The paradox of language, then, could not be resolved — for all of Foucault’s efforts — by taking a stand on the side of syntax, of structure, of arrangements of words, and against meaning, intention, interpretation; on the contrary, Foucault’s own claim that *la clinique* “turned” language into “rational discourse” rested precisely on a posited semantics, on the notion that language was “open[ed] up ... to a whole new domain”, on the conception of “a new alliance *between words and things*”.⁵¹ Thus the purely

structural analysis of language which Foucault claimed to envisage was in the strictest sense a contradictory enterprise.⁵² Yet for all this, his conception carried powerful messages: that a line could be drawn between the meanings of words and the modalities of their arrangement, and that the former (semantics) was strictly subordinate to the latter (syntactics). And this formulation precluded any history of actual disease-concepts — for such a history would necessarily take the meanings of those concepts, that is, the semantic dimension of language, as its theme.

(3) In 1978 there appeared what was to become perhaps the best-known essay in the genre of “social constructivism”: Karl Figlio’s study of chlorosis in nineteenth-century Britain.⁵³ Subtitled “the social constitution of somatic illness in a capitalist society”, Figlio’s paper argued that the disease of chlorosis, or the “green sickness”, was anchored in class relations. At first glance it seems that Figlio here historicized and relativized the concept of “chlorosis”, and that he did so by presenting a narrative account of the rise and fall of that concept; yet these appearances are highly deceptive. In fact the paper offered no such narrative at all; it paid only the most cursory attention to the meaning of “chlorosis”; and it left wholly in suspense the question as to whether what changed historically was the *concept* of chlorosis or *the disease itself*. Indeed, as we shall see, the effect of Figlio’s approach was to obscure the very distinction between what I am calling the historicalist-conceptualist approach and the naturalist-realist approach.

Chlorosis was classically a disease of young women. Figlio began by arguing that chlorosis was “a physical illness”,⁵⁴ which was “limited to a definite life-span”: it “emerged in the sixteenth century; it was first described clearly in the early eighteenth century and became common in the nineteenth century; it peaked around mid-century and was rare by the 1920s”. In 1928, when it was being recognized that chlorosis had strangely disappeared, Henry Sigerist had argued that “the history of chlorosis is the history of young girls in society”;⁵⁵ and Figlio’s interpretation was an elaboration and refinement of Sigerist’s view. Figlio summarized his own analysis as follows:⁵⁶

Capitalism developed increasingly by calling on youthful female labour. To the extent that the working-class girl was drawn into the labour process, the characteristics of the non-working girl were exaggerated, first by defining adolescence as a new child-like stage corresponding to the age of intensive labouring in the working class, and then by throwing into ever sharper relief the image of a-sexual, non-working, delicate femininity. Chlorosis reinforced this now polarized, dual nature of the youth become adolescent.

Despite these various narrative gestures — “a definite life-span”, “developed”, “first”, “then”, “now” — a narrative content was precisely what Figlio’s essay *lacked*. The rise of chlorosis was not discussed at all. The period of its flourishing was depicted as entirely homogeneous, both with respect to the disease itself (examples were plucked indiscriminately from the 1780s, the 1830s, the 1860s, the 1880s and 1906)⁵⁷ and with respect to nineteenth-century British capitalism

(specific capitalist industries were scarcely identified, and there was no mention of, for instance, Chartism, empire, or compulsory education). But the most remarkable anti-narrative feature of the essay was that the decline of chlorosis — the very feature of its history that Sigerist had emphasized — was explicitly bracketed off from consideration at the very outset of the argument; and the form of this move was highly revealing. Here is how Figlio disposed of the issue (but with my emphases):⁵⁸

Explanations of its disappearance vary, but authors did reject the possibility that chlorosis vanished simply because it had received another name.... To go into these reasons in detail would get us into the risky business of retrospective diagnosis. But more important even than the explanations proposed to interpret its decline is the fact that the need met by attaching a medical label, with all the consequences that followed, was now *either* met in some way, *or* no longer existed.

Thus Figlio left it strictly indeterminate (“either ... or”) whether concepts of disease had changed, or whether the physical disease of chlorosis had disappeared. The same ambiguity recurred in the body of the essay: sometimes Figlio hinted that capitalism had led to the very category “chlorosis”,⁵⁹ sometimes he suggested that biological changes were involved,⁶⁰ and he never attempted to resolve the tension between these two interpretations. Indeed if anything, Figlio inclined towards the biological explanation — since in the passage just quoted, “the risky business of retrospective diagnosis” was invoked as the reason for not discussing the decline of chlorosis.

Although this ambiguity seems surprising, it is intelligible in the light of Figlio’s purposes. All that mattered was that chlorosis could somehow be tied to “capitalist society”: it was a matter of indifference whether this putative link was constructed by way of the concept or by way of the disease itself. And precisely for this reason, Figlio’s essay had very little to say about the actual concept of chlorosis, or about its history. The ironies are considerable, for in fact chlorosis had undergone a dramatic mutation in the early nineteenth century (that is, at the beginning of the period with which Figlio was concerned), and this was linked to a larger and highly consequential shift of medical concepts. In the eighteenth century, chlorosis was a “cachexia”, that is, a corruption of the blood;⁶¹ in the early nineteenth century, it became an “anaemia”; and what brought about this transformation, or at least made it possible, was that the concept of “anaemia” itself — a concept which, unlike that of “chlorosis”, is still with us — was invented at that time.⁶² Thus attention to the concept of chlorosis rapidly opens a much wider horizon of historical inquiry. And conversely, the reason that Figlio failed to disclose such possibilities was precisely that he never brought chlorosis into focus *as a concept*.

(4) In 1992 Charles Rosenberg and Janet Golden published a collection of essays entitled *Framing disease*.⁶³ The volume was notable for including two papers which I have already cited as leading examples of the historicization of disease-concepts:

Lawrence's study of coronary thrombosis and Peitzman's essay on Bright's disease. Yet as Rosenberg's introduction made clear, the book's very title was designed to marginalize, indeed to erase, the significance of those two essays and of the historicalist-conceptualist tradition as a whole. In a pivotal passage, Rosenberg spelt out the reasons for the collection's title, making it clear that diseases were to be seen as "framed" precisely in order that that they should *not* be seen as "constructed":⁶⁴

In the following pages I have, in fact, avoided the term social construction. I felt that it has tended to overemphasize functionalist ends and the degree of arbitrariness inherent in the negotiations that result in accepted disease pictures. The social-constructionist argument has focused, in addition, on a handful of culturally resonant diagnoses — hysteria, chlorosis, neurasthenia, and homosexuality, for example — in which a biopathological mechanism is either unproven or unprovable. It invokes, moreover, a particular style of cultural criticism and particular moment in time — the late 1960s through the mid-1980s — and a vision of knowledge and its purveyors as rationalizers and legitimators, ordinarily unwitting, of an oppressive social order. For all these reasons, I have chosen to use the less programmatically charged metaphor "frame" rather than "construct" to describe the fashioning of explanatory and classificatory schemes of particular diseases.

The effect of this passage was to identify the metaphor of "construction" with its particular instantiation as "social construction", that is, with the genre to which Figlio's study of chlorosis belonged — whence the allusion to that disease. And this move obliterated from view the essays of Lawrence and of Peitzman in the volume itself, not to mention Gabbay's study published ten years earlier and Fleck's *Genesis and development of a scientific fact* — all concerned with diseases which are generally regarded as involving "biopathological mechanisms", each of them demonstrating the contingent and constructed character of the disease-concept in question, none of them advancing "a vision of knowledge and its purveyors as rationalizers and legitimators ... of an oppressive social order". Rosenberg concluded:

Biology, significantly, often shapes the varieties of choices available to societies in framing conceptual and institutional responses to diseases: tuberculosis and cholera, for example, offer different pictures to frame for society's would-be framers.

An accompanying footnote explained what such "different pictures to frame" meant: "The very different modes of transmission imply different relationships to relevant ecological and environmental factors."

Despite what Rosenberg was suggesting, the new metaphor "frame" was every bit as "programmatically charged" as the discarded metaphor "construct". For of course the purpose of this "framing" imagery was to do away with the notion of construction, and to depict diseases as natural entities — tuberculosis and cholera

being taken as examples. The historically-constructed categories “tuberculosis” and “cholera”, complete with their “modes of transmission”, that is, strictly following the post-Kochian definition of these diseases,⁶⁵ were assigned a timeless existence. Consequently, the scope of the historical was drastically restricted, in the standard manner of the naturalist-realist approach: “responses to” diseases were permitted to vary historically, but concepts of disease were not.⁶⁶ (And most of the studies in the book implicitly echoed this conception: that is, rather than exploring the historicity of disease-concepts in the manner of Lawrence or Peitzman, they treated diseases as real entities identical with their names,⁶⁷ and limited themselves to the various cultural “responses” which diseases, so construed, had evoked.) Yet ironically enough, the metaphor of pictures-and-framing undermined itself; for tuberculosis and cholera were depicted as “offer[ing] ... pictures” — and “pictures” are of course human-made. Thus Rosenberg’s own rhetoric inadvertently conceded just what he was trying to deny:⁶⁸ that diseases are not simply “biological” entities in Nature but human constructs, as of course are the very concepts of the “biological” and the “biopathological”.

To sum up, the naturalist-realist approach to the history of diseases has been remarkably tenacious: it comfortably survived the translation of Fleck’s book into English, and it has even pervaded some of the major attempts to historicize medical knowledge. Consequently, the history of most disease-concepts still awaits investigation, as indeed does the history of the concept of “pathology” itself. And we have still to take the measure of Fleck’s point, that the supposed timelessness of diseases, and the associated conception of medical knowledge, are themselves precisely human constructs.⁶⁹

2. THE CASE OF PLEURISY

To adapt to the present context what Steven Shapin once wrote about the sociology of scientific knowledge,⁷⁰ one can either debate the possibility of the history of disease-concepts or one can go out and write such a history. In the previous section I sought to demonstrate the possibility; here I shall attempt to do it, albeit within certain limits. By means of a case-example I hope to show that investigating the history of disease-concepts yields rich rewards, and also that such an exercise does not require a technical medical competence, but simply calls for a modicum of exegetical care. The example I have chosen for this purpose is *pleurisy*, or to give it its original and ancient name, *pleuritis*.

“Pleurisy” — in inverted commas, that is, *concepts of pleurisy* — is a particularly eligible subject for such a study, for several reasons. In the first place “pleurisy”, like “asthma”, belonged and belongs to the entire history of the Western medical tradition. Its birth coincided with the birth of Western medicine itself, for it featured (as “pleuritis”) in several of the Hippocratic texts; it was considered by all the subsequent leading medical authors of Antiquity; it reappeared in the medical writings of the Renaissance and the Enlightenment; in the epoch of *la clinique* it

was discussed extensively by Laennec, who found for pleurisy its own distinctive auscultative sound; and people are diagnosed as suffering from “pleurisy” today. Thus pleurisy offers an example which should speak to historians of Western medicine working on any and every period. Secondly, it so happens that the first episodes in the history of “pleurisy” have already been charted by Wesley D. Smith, whose pioneering study of “pleurisy” (strictly speaking, “pleuritis”) in the ancient texts considerably facilitates my own task.⁷¹ Thirdly, “pleurisy” — *unlike* “asthma” — has always been seen (at least, in all the texts I have consulted) strictly as a bodily ailment, never as involving any “psychosomatic” component. Thus if, as I hope to show, “pleurisy” changed historically throughout the Western tradition, this mutability of the concept cannot be ascribed to any supposed “softness” on pleurisy’s part: it always was as definite and bodily a disease as any ailment from which patients were seen as suffering. And yet a fourth reason for taking an interest in pleurisy concerns the particular modes of anatomical localization. Specifically, one school of thought held that pleurisy was located in the membrane lining the ribs: that is to say, not in an organ but rather in what would come to be designated, in the “age of Bichat”,⁷² as a tissue. Thus in one dimension of its history, pleurisy serves as an example — and as we shall see, a remarkably precocious one — of precisely that kind of localization which supposedly defined *la clinique*.

In line with the programmatic aims of the present paper as a whole, my discussion of “pleurisy” will be indicative rather than definitive. I shall not be telling a connected story, but merely depicting some salient episodes; thus what follows is to a history-of-”pleurisy” much as a series of snapshots is to a film. Further, the approach adopted here will be strictly “internalist” and textual. That is to say, I shall make no attempt to explain the various shifts of meaning which will emerge; rather, those shifts will be presented as offering a series of problems for further investigation. In short, the aim of this section is not to write the history of “pleurisy” — for as will become clear, that would require an entire book — but simply to demonstrate that “pleurisy” *had* a history of which an account *could* be written.

I shall begin with “pleuritis” in the ancient texts; here, thanks to Wesley Smith’s help, it will be possible to give a more or less comprehensive overview. My discussion will then leap forwards to the Renaissance, from which point it will be necessary to be much more selective: I shall focus on just three authors — Vesalius, Baglivi and Morgagni — who between them will conduct us from the early stages of the medical Renaissance to the eve of *la clinique*. My emphasis throughout will be upon the *definition* of “pleuritis” or “pleurisy”, even though several of the texts which I shall be considering were chiefly concerned with other matters: for instance, Vesalius’s *Venesection letter* (the only work, amongst those to be discussed, which was devoted specifically to pleurisy) dealt with therapeutics. In different texts, therefore, we shall find the definition of “pleuritis” variously foregrounded and taken-for-granted; and it will emerge that this variety of focus is revealing in itself.

2.1. ANCIENT CONCEPTIONS OF PLEURITIS

We can schematically but conveniently distinguish three epochs within the ancient roots of Western medicine:

- (i) the Hippocratics of the fifth century B.C., of whose works some have survived;
- (ii) the Alexandrians of the third century B.C. and their “dogmatic” followers, whose writings have survived only in quoted “fragments”; and
- (iii) the authors of the early Christian era, from whom (as from the Hippocratics) we have a number of extant works: principally Soranus (relayed through Caelius Aurelianus), Aretaeus, and above all Galen.

In his important and path-breaking essay on this subject, Wesley Smith has surveyed the accounts of “pleuritis” in (i) and in (iii), and has reconstructed the doctrines that were advocated in (ii).⁷³ What follows is much indebted to Smith’s incisive study.

(i) In the Hippocratic writings, although there was no single picture of “pleuritis”, the various different accounts had this in common, that they defined the disease — if they defined it at all — *symptomatically*. Many of the texts simply took the definition of “pleuritis” for granted, concentrating instead on prognostics,⁷⁴ on therapeutics,⁷⁵ or on speculative humoral pathology.⁷⁶ But pleuritis was described in *Affections*,⁷⁷ in *Diseases II*,⁷⁸ and in *Diseases III*,⁷⁹ and these three works gave closely-overlapping definitions of its characteristic symptoms. We may take as an example the description in *Diseases III*:⁸⁰

When pleuritis arises, a person suffers the following: he has pain [οδυνη] in his side [πλευρην], fever and shivering; he respire rapidly, and he has orthopnoea [that is, he can breathe only when in an upright posture].⁸¹ He coughs up somewhat bilious material the colour of pomegranate-peel, unless he has fissuring, then he coughs up blood too, from the fissures. In sanguinous pleuritis the sputum is charged with blood....

Of these various symptoms, the *pain in the side* was the critical element — which is just as we would expect from the etymology of the word “pleuritis”. “Pleuron” meant “side” or “rib”; the suffix “-itis” seems to have meant an “affection”, “affliction”, “disease” or “trouble”.⁸² Thus “pleuritis” was a disease named after its chief symptom, namely pain-in-the-side — whence the fact that this symptom alone was named in those other Hippocratic texts which mentioned pleuritis without explicitly defining it,⁸³ while yet another work (*Regimen in acute diseases*) used the phrase “pain in the side” and the word “pleuritis” more or less interchangeably.⁸⁴ Yet while this was the common core of the various usages in the *Corpus Hippocraticum*, those usages varied in at least two ways. In the first place, one description, namely that in *Diseases II*, located the pain of pleuritis not in the side but instead “along the spine and in the chest” — as if the “pleur-” was here designating “ribs” rather than “side”. However, as we shall see in a moment,⁸⁵ even *Diseases II* went on to revert to

the normal usage, that pleuritis was pain in the side. We may take it, therefore, that pain in the side (rather than in the ribs) was the standard meaning.

A second and more significant source of variety within the Hippocratic texts was that “pleuritis” was seen sometimes as a single disease, and at other times as comprising, in effect, a family of different though related diseases. Indeed *Diseases III* itself, whose description of pleuritis I have quoted, deployed both of these usages in succession:⁸⁶ for that description was already in the process of distinguishing between “bilious” and “sanguinous” forms of pleuritis, and the text went on to speak of further “varieties” of pleuritis: first “dry pleuritis without expectoration”; then “pleuritis in the back”; and finally still further “pleuritis”, in which “the sputum is clean, but the urine bloody” and “sharp pains extend along the spine to the chest and groin”. Similarly *Diseases II*, having located the pain of pleuritis “along the spine and in the chest” as we have seen, immediately described “another pleuritis” involving “pain in the side and sometimes around the collar-bone”, followed by yet “another pleuritis” in which the patient has “pain” in “his side” — thereby, we may notice in passing, transferring the pleuritic pain from the patient’s “spine and chest” to its more typical location in the patient’s side.⁸⁷ Although this pattern of usage seems inconsistent, it simply reflected the very origin of the term “pleuritis”; for different “pleuritis” were different constellations of symptoms which had in common pain in the side (or perhaps, taking *Diseases II* into account, in the ribs). Thus the term “pleuritis” was symptom-based: a “pleuritis”, we might say, was what the patient reported to the doctor in the first place.⁸⁸

(ii) A very different conception of pleuritis was advanced by the various so-called “dogmatic” medical writers who first appeared around 300 B.C., and chiefly in Alexandria: that is to say, the anatomists Herophilus and Erasistratus together with their teachers⁸⁹ and followers. For all of these authors — so we learn from the much later testimony of Soranus, as recounted still later by Caelius Aurelianus — defined pleuritis not symptomatically but *anatomically*.⁹⁰ Yet while the various “dogmatics” agreed over this radical shift of the mode of definition, they diverged as to its practical content; for amongst them (so Soranus/Caelius informs us) two very different opinions were advanced. Some, including Herophilus, saw pleuritis as affecting the *lung*; others, including Erasistratus, placed that affection not in the lung but instead in “the *membrana hypezocota* which girds the sides internally” — that is, in the “undergirding membrane” which lines the ribs. Those of the former opinion were presumably faced with the question as to how pleuritis was distinguished from that affliction of the lungs called *peripneumony*;⁹¹ and at least one of these authors — namely Herophilus — offered an answer to that question: pleuritis, he suggested, arose when peripneumony was complicated by fever.⁹² It seems that these two different anatomical interpretations of pleuritis, the one locating it in the lung, the other placing it in the *membrana hypezocota*, generated lively debate in the ancient world; for Soranus/Caelius gave an extended account of the arguments which had been advanced on each side. In addition, there was

probably also a third party in this debate, a party which Soranus/Caelius did not report — namely the “empirics”, who argued that the location was irrelevant and that only therapeutics mattered.⁹³ We might well guess that the “empirics” defined pleuritis just as the Hippocratics had done, but unfortunately this information is lost to us. Nevertheless this much is plain: that the “dogmatics” all conceived pleuritis anatomically (while locating it variously in the lung and in the *membrana hpezocota*), in sharp contrast with the symptomatic conception of the earlier Hippocratics.

Further, there was at work here another shift; for in at least one respect the dogmatics gave pleuritis a new *symptomatic* content, over and above what was included in the Hippocratic writings. Specifically, the dogmatics observed that pleuritic patients characteristically have difficulty in lying on one particular side — not, curiously enough, on the side of the pain, but rather on the unaffected side.⁹⁴ Strikingly, the two contending anatomical schools of thought agreed on the presence of this symptom⁹⁵ — though they described and interpreted it in different ways, corresponding to their respective conceptions of the seat of the disease. Those who placed pleuritis in the lung (Herophilus and others) depicted the trouble-in-lying-on-the-unaffected-side as “difficulty in breathing”;⁹⁶ but those who located pleuritis in the “undergirding membrane” (Erasistratus and others) portrayed the trouble-in-lying-on-the-unaffected-side as “pain”.⁹⁷ And each school had its own distinctive anatomical account of the cause of that symptom — which was precisely why Soranus/Caelius, writing some centuries later, included it in his summary of the debate between them. Now what we must observe is that this symptom had *not* been mentioned in any of the accounts of pleuritis within the Hippocratic Corpus — despite their detailed descriptions of symptom-patterns, of prognostic signs, of different varieties of pleuritis. Moreover, there is no particular reason to assume that the much later testimony of Soranus/Caelius has exhausted the catalogue of such additional symptoms. It is therefore entirely possible that the “dogmatics” added to “pleuritis” yet further symptoms — perhaps, like the difficulty-in-lying-on-the-unaffected-side, common to both anatomical interpretations, perhaps specific to one interpretation or the other. This is of course entirely conjectural; yet we should not on that account rule it out as a possibility.

(iii) From the early Christian era we have discussions of pleuritis by at least three authors: in the first century by Soranus (relayed, as has been mentioned, through Caelius Aurelianus);⁹⁸ in the second century by Galen, in several of his works,⁹⁹ and by Aretaeus of Cappadocia in his treatise on acute diseases.¹⁰⁰ Although Soranus, Galen and Aretaeus were writing largely independently of one another, they were all heavily influenced by the writings of the preceding dogmatists; and while their orientations and emphases differed, they reveal a clear common core. For all three of them interpreted pleuritis anatomically, and specifically, they all followed what we may call the “Erasistratean” line, that is, that pleuritis was seated in the

membrane lining the ribs. True, the anatomical orientation was much less prominent in Soranus/Caelius than in Aretaeus or Galen, for Soranus regarded the state of the whole body (“stringent” or “relaxed”) as the key issue in the interpretation of diseases,¹⁰¹ and he deployed this whole-body conception in his description of pleuritis just in his accounts of other acute diseases. Nevertheless Soranus also gave some weight to anatomical localization, to different degrees in different diseases;¹⁰² and as it happened, he was particularly sympathetic to this approach in the case of pleuritis. As he summed up this point (from the Latin text of Caelius Aurelianus):¹⁰³

But when we see a case of *pleuritica*, let us view it not as a simple affection of the side but as an affection of the whole body accompanied by acute fever. For fevers involve the whole body. We shall be right, however, if we say that the *membrana hypezocota* is more particularly affected, for it is there that the pains are centred.

Thus even Soranus accepted a measure of anatomical localization for pleuritis — whence the fact that his account of the competing *definitions* of the disease did not mention Hippocrates,¹⁰⁴ whereas his subsequent discussion of competing *treatments* began with Hippocrates.¹⁰⁵ Further, the particular localization which Soranus endorsed was in the *membrana hypezocota*, not in the lung: thus the “Erasistratean” line had come to prevail over the “Herophilean” line. And in each of these respects Aretaeus and Galen, writing in the next century, were to take the same view as Soranus before them, simply going further than Soranus in their commitment to anatomical localization. In short, as Wesley Smith has well put it, there was by this time “a general understanding” as to the nature of pleuritis:¹⁰⁶ it was to be interpreted anatomically, and its particular seat was in the *membrana hypezocota*, the “undergirding membrane” which lined the ribs.

I shall consider the content and significance of this anatomical conception in a moment; first, however, we must pause to notice a further development on the symptomatic front. We have already seen, when considering stage (ii) of our three-epoch schema, that the anatomical approach inaugurated by Herophilus and Erasistratus brought with it a new symptom of “pleuritis”, namely difficulty in lying on the unaffected side; and I suggested that other symptoms might well have been added to “pleuritis” at that time. Now that we have reached stage (iii) of our schema, another such symptom has indeed emerged; and as we shall see, it is possible that this, too, had been added at the time of Herophilus and Erasistratus.

The new symptom is the *quality* of the pain associated with “pleuritis”. With one minor exception, which I shall note in a moment, the Hippocratic texts had all described pleuritis merely as pain-in-the-side, without distinguishing that pain in any way save by its location; but as Wesley Smith observes, the pain of pleuritis has now, in the writings of the early Christian era, become a pain of a particular kind.¹⁰⁷ Soranus/Caelius describes it as “pricking, throbbing, and burning”;¹⁰⁸ Aretaeus mentions “acute” pain (specifically in the clavicles) and “acid heat”;

Galen describes the pleuritic pain as “nygmatodes”, that is, “pricking”,¹⁰⁹ or in Professor Smith’s gloss, as “sharp or piercing as though from needle jabs”. Now there are some further complexities here, for Soranus, Aretaeus and Galen described and interpreted this pain in slightly different ways.¹¹⁰ Nevertheless they agreed in assigning to the pleuritic pain a particular quality, a certain sharpness or intensity; and this was almost wholly new as compared with the discussions of “pleuritis” in the Hippocratic texts. True, *Diseases III* had mentioned “sharp pains” [ὄδονα τε ὄξεα].¹¹¹ But even there, this sharpness of the pain was by no means seen as characteristic of “pleuritis”, since these sharp pains were located not in the side but “along the spine to the chest and groin” — and this type of pain was associated only with one variety of “pleuritis”, not with “pleuritis” in general. In short, even if the quality of the pleuritic pain as described by Soranus, Aretaeus and Galen was somehow derived from this little passage in *Diseases III*, it had radically changed its status since the days of the Hippocratics, for it had now been installed as a defining characteristic of pleuritis.

Thus the sharpness of the pain as a definitive symptom of “pleuritis” was added *after* the writing of the Hippocratic corpus. But just *when* was “pleuritis” endowed with this new quality? On this question, as with so many issues concerning ancient medicine, we have no clear evidence; and much depends on what we make of the precise description of the pleuritic pain. Galen’s “nygmatodes” was apparently inherited from Archigenes, the leading Pneumatist who was active around 100 A.D.;¹¹² thus if Galen’s term is taken as definitive, then this quality of the pleuritic pain may well have been a relatively late discovery.¹¹³ But if we adopt a looser criterion, assimilating together the various descriptions of that pain supplied by Soranus, by Galen and by Aretaeus, there are grounds for assigning its discovery to the earlier dogmatics, and specifically to those who placed pleuritis in the *membrana hpezocota*.¹¹⁴ It is therefore possible that the quality of the pleuritic pain dated from the era of Herophilus and Erasistratus, just as the difficulty-in-lying-on-the-unaffected-side did — though this is merely a hypothesis.

Although it is frustrating that this new symptom, the sharpness of the pleuritic pain, is so difficult to pin down historically, there is one sense in which this hardly matters. For whether we ascribe it to the earlier dogmatics or to the medical writers contemporary with the Roman empire — that is, to stage (ii) or to stage (iii) of our schema — the adding of that symptom confirms and extends what we already learnt from considering the difficulty-in-lying-on-the-unaffected-side: namely, that the shift from a symptomatic definition of pleuritis to an anatomical definition was accompanied by changes in the symptoms themselves. And as we shall see, this is intelligible enough, once we focus on what the anatomical conception of pleuritis entailed.

For a closer look at that anatomical conception in late Antiquity we may turn from Soranus to Aretaeus — for as we have seen, Aretaeus and Galen were more closely wedded to anatomical localization than was Soranus, and it happened that Aretaeus’s treatise on acute diseases offered a more detailed and explicit account

of pleuritis than did any of Galen's extant works. It is a fair presumption that the earlier Alexandrians, the originators of the anatomical approach — from whom we have no extant writings on the subject — had written along analogous lines to what we shall be seeing in Aretaeus. For our purposes, therefore, Aretaeus's interpretation of pleuritis can stand in lieu of that of those dogmatics (Erasistratus *et al.*) who had first placed this disease in the *membrana hypezocota*, though of course we have no such surrogate for the alternative (Herophilean) interpretation, that pleuritis was seated in the lung. Here are the opening sentences of Aretaeus's description of pleuritis:¹¹⁵

Under the ribs, the spine and the internal part of the thorax as far as the clavicles, there is stretched a thin strong membrane, adhering to the bones, which is named *hypezokos*. When inflammation [*phlegmone*] occurs in it, and there is heat with cough and parti-coloured sputa, the affection is called pleuritis. But all these symptoms must harmonize and conspire together as all springing from one cause; for such of them as occur separately from different causes, even if they occur together, are not called pleuritis. It is accompanied by acute pain of the clavicles; acrid heat; lying down on the inflamed side easy ... but on the opposite side painful.... It is attended with dyspnoea, insomnolency, anorexia, florid redness of the cheeks, dry cough, difficult expectoration of phlegm, either bilious, or deeply tinged with blood, or yellowish; and these symptoms observe no order, but come and go irregularly....

This picture of pleuritis, when compared with the descriptions of pleuritis in the Hippocratic texts, reveals several interlinked changes which together amount to a profound transformation of medical categories. First and fundamentally, although pleuritis is still *identified* symptomatically (for of course only symptoms can be observed), it is now *defined* anatomically: specifically, as an inflammation in the membrane called the "*hypezokos*". That is to say, a word which had originally referred to *an experience of the patient* — pleuritis, pain in the side — is now being used to designate *a change in an anatomical structure*; and significantly, that structure is the starting-point of Aretaeus's entire description of pleuritis.

Second, the pain-in-the-side has strangely receded from view: Aretaeus assumes that pleuritis is an affection of the *side* (as becomes clear when he remarks that lying on the inflamed side is easier), and furthermore that it involves *pain*, yet he does not mention the pain-in-the-side itself. And the reason is that he has absorbed this, the experience of the patient, within the category "inflammation" [*phlegmone*]. Hence the fact that Aretaeus implicitly refers to that inflammation as a "symptom":¹¹⁶ although this denomination is strictly illogical — for such inflammation of an interior structure is a medical inference rather than a presenting symptom — it is in fact entirely consonant with Aretaeus's redefinition of pleuritis. The patient's experience (pain) has been transformed into a process (inflammation), and has been remapped from a part of the experienced body (the side) onto an internal structure (the membrane called *hypezokos*); as we shall see in a moment, it has

thereby changed its status, becoming the cause of the illness; and yet, for all this, it also retains traces of its original nature, namely the fact that it is a pain, and is thus a symptom.

Third, precisely because the pain-in-the-side has ceased to be definitive, “pleuritis” no longer embraces several different diseases, or a family of diseases; rather, it is a single condition. In this new conception it would be inconceivable to speak, as some of the Hippocratic texts had done, of “another pleuritis” (*Diseases II*), or of “some pleuritises” (*Diseases III*), each with different clusters of symptoms.

Fourth, as the corollary of this shift, the core symptomatic content has expanded: pleuritis is now characterized not by a single symptom (pain in the side) with which other and variable symptoms are loosely associated, but on the contrary by a precise constellation of symptoms. As we have seen already, this has involved the introduction of new and hitherto unsuspected symptoms: the piercing quality of the pain, the difficulty in lying on the unaffected side.

Finally, it is not the mere *concatenation* of these symptoms which establishes that unity which is “called pleuritis”; on the contrary, Aretaeus insists that this unity derives from the (supposed) *common causation* of those symptoms. And this completes the conceptual circle within which the new concept is inscribed; for of course the cause is neither more nor less than the defining anatomical event, namely the inflammation of the *hypezokos*.

In short, the entire conception of disease has been radically redefined. The installation of the new, anatomical element, and above all its privileging as the site of definition — these have necessitated a rearrangement of the old, symptomatic elements; or to use a different metaphor, what seem to be the same “moves” (the identification of certain symptoms) are in fact being played within a new and different “game”.¹¹⁷ Indeed, it is perhaps only now that the word ‘symptom’ is appropriate, or that it gains the sense which we today confer upon it: that is, as something which reveals the disease, yet which is not that disease, and which thus in fact reveals the disease only obliquely.¹¹⁸ It would seem that the new ‘game’ involves a decisive shift of power from the patient to the doctor; for pain-in-the-side was an experience of the patient, whereas “inflammation of the *hypezokos*” is not something which the patient could conceivably experience or describe. Yet the patient’s experience has by no means been swept aside; indeed, quite the contrary. For the anatomically-oriented writers have observed pleuritic symptoms which were precisely within the realm of the patient’s experience, yet which the Hippocratics had ignored: the stabbing quality of the pain, the difficulty in lying on the unaffected side.

In view of this sharp disparity between Hippocratic “pleuritis” and the “pleuritis” of late Antiquity, we may well wonder what Galen had to say about “pleuritis”; for Galen adhered to the anatomical definition of that disease, yet he was concerned throughout his writings to portray his own medicine as *continuous* with the medicine of Hippocrates, repeatedly eliding the awkward problem that the Hippocratic texts were largely silent on matters anatomical.¹¹⁹ Thus pleuritis was one of many themes

which required Galen somehow to efface the differences between Hippocratic medicine and “dogmatic” (that is, anatomical) medicine. How did he tackle this task in the particular case of pleuritis? Within Galen’s various commentaries upon Hippocrates, there were two particular points at which pleuritis was discussed: with respect to the *Aphorisms* and *Regimen in acute diseases*. In each case I give first the pertinent Hippocratic passage and then Galen’s commentary upon it as translated by Wesley Smith.

(1) *Aphorisms*:¹²⁰

Hippocrates: Pleuritis that does not clear up in fourteen days results in empyema.¹²¹

Galen: In pleuritis, acute fever with laboured breathing and cough and stabbing pain in the side are the essential signs by which we recognize the disease. Extension of the pain to the hypochondria or the collarbone are accessory, as is the greater ease of lying on the affected side rather than the other.

At first glance Galen here appears to be as “Hippocratic” as one could wish, for his description of pleuritis has no explicit anatomical content: it is confined to the realm of symptoms. Yet in fact his account is far removed from that of the Hippocratic texts, in two related respects. In the first place, the actual symptoms he describes are not Hippocratic but “dogmatic”, for they include the very symptoms which the dogmatists had added to pleuritis, namely *stabbing* pain and the “greater ease of lying on the affected side”. Secondly, Galen conceives these symptoms as “signs by which we recognize the disease”, dividing these moreover into “essential signs” and “accessory” indications; and this conceptual grid, with its semiotic language, had no precedent in the Hippocratic writings.¹²² No doubt it is the anatomical conception of pleuritis, positing as it does that the disease is an internal and invisible process rather than a reported experience of the patient, which has required and called into being this semiotic conception. And although the crucial anatomical element is wanting here, it will be supplied in another of Galen’s commentaries, as we are about to see. What we should notice at this point is that by presenting this compact description of pleuritis within the context of his commentary on the *Aphorisms*, Galen has managed to identify the “pleuritis” of the later dogmatics, whose constellation of symptoms he is echoing, with Hippocratic “pleuritis”.

(2) *Regimen in Acute Diseases*:¹²³

Hippocrates: ... Moreover, suppose the pain in the side continues and does not yield to the fomentations, while the sputum is not brought up, but becomes viscid without coction; should gruel be administered in these conditions without first relieving the pain, either by loosening the bowels or by venesection, whichever of these courses is indicated, a fatal termination will quickly follow.

Galen: Let us recall that Hippocrates wrote the discussion of pleuritis as an example, and that this was shown to occur through the inflammation of the membrane lining the ribs. If one give barley broth [i.e., gruel] at the height of the inflammation, he will harm the patient greatly.

Here Galen has elegantly stitched together Hippocratic medicine and anatomical medicine. By the end of his first sentence he has established rhetorically that the “pleuritis” or “pain in the side” of Hippocrates was identical with the “pleuritis” of which Erasistratus and others wrote, that is, with “inflammation of the membrane lining the ribs”. Then, in the next sentence, he rephrases the advice of Hippocrates in appropriate anatomical language: where Hippocrates had written that gruel should not be given to patients in whom “*the pain in the side continues*”, adding a humoral gloss (“the sputum is not brought up, but becomes viscid without coction”), Galen reiterates the same warning but locates the time of danger — the danger of giving gruel — at “the height of *the inflammation*”. And thus does Galen establish a putative Hippocratic lineage for the anatomical conception of pleuritis.

These little pieces of commentary by no means exhaust what Galen had to say about pleuritis; on the contrary, he adverted to it in several other works.¹²⁴ Although it is beyond my present scope to review all of Galen’s discussions of the disease, we must take note of the way he considered it in his writings on the pulse — for in these treatises Galen added a further diagnostic sign of pleuritis, one which he did not mention in his commentaries on Hippocrates. Specifically, Galen taught that pleuritis is characterized by a “hard” pulse, its hardness arising not from strength (for it is not in fact a strong pulse, even though it seems to be) but rather from the fact that it is fast and frequent (*celer ... et creber*).¹²⁵ It is not clear whether Galen discovered this himself or whether he had derived it from earlier authors; the “hard” pulse as a diagnostic sign of pleuritis seems to have been unknown to the earlier dogmatists¹²⁶ — even to Herophilus, the effective discoverer of the pulse and inventor of its diagnostic use¹²⁷ — but it may have been Archigenes rather than Galen who made this innovation.¹²⁸ At all events, the “hard” pulse was now added as yet a further characteristic of pleuritis. Amongst the implications of this was that pleuritis was readily distinguished from peripneumony; for the latter disease was associated with a very different kind of pulse, namely one which was “large”, “slow” and “soft” (*magnus, languidus, mollis*).¹²⁹ Characteristically, Galen had a rationale for the pleuritic pulse: the reason that it was “notably hard” was that in pleuritis, “it is a nervous part which is affected”.¹³⁰ Characteristically, too, he berated those “vulgar physicians” who were ignorant of the diagnostic use of the pulse,¹³¹ while conveniently omitting to mention the fact that this “ignorance” was shared by Hippocrates himself.

Galen’s persistent elision of the differences between Hippocratic medicine and dogmatic medicine serves as a fitting ending to the strange tale of pleuritis in Antiquity. To reiterate, pleuritis began as a pain in the side, defined by that symptom; subsequently it acquired an anatomical seat, and this necessitated not just a different suite of symptoms but also, and more fundamentally, a different

conception of the very nature of diseases and of symptoms; those who agreed in giving pleuritis an anatomical location nevertheless differed for several centuries as to whether it was seated in the lungs or in the *hypezokos* membrane; eventually it was placed in the membrane, though the reasons for this consensus remain obscure. And finally, in Galen's hands, pleuritis changed yet again, in at least two ways. It now acquired a new sign, the hard pulse; and the very shift from a symptomatic conception to an anatomical one was obliterated, for an imaginary continuity was set up between Hippocratic pleuritis and the anatomical conception of pleuritis.

2.2. EARLY-MODERN CONCEPTIONS OF PLEURITIS

2.2.1. *Vesalius*

It happened that pleuritis played a critical role in the life and work of Vesalius — for it was the treatment of pleuritis which was at issue in the book which marked his first significant break with the authority of Galen, namely his *Venesection letter* of 1539. The controversy over bleeding in pleuritis had begun a generation earlier, in 1514, when Pierre Brissot launched a typically Renaissance attack upon “Arab” bleeding practices in the name of Hippocratic and Galenic purity.¹³² Vesalius's intervention of 1539 arose from his discovery as to the anatomy of the “azygos vein” (that is, the “unpaired vein”) and its branches: specifically, Vesalius had found that this single vein, which branched off from the vena cava on the *right* side, supplied the ribs of *both* sides (apart from the top three ribs). From this anatomical arrangement, and from the theory of phlebotomy as interpreted by Vesalius, there followed — so Vesalius argued — a new and decisive solution to the problem of where to bleed in pleuritis. For whether the pain was on the right side or on the left, and whether “derivative” or “reversionary” bleeding was desired, the pleuritic patient should always be bled from the *right* arm, since this was adjacent to the origin of all the costal veins.¹³³ At least, this was where bleeding should be carried out if the patient's pain was located in the lower nine ribs — and such was indeed usually the case in pleuritis, as Vesalius went on to argue, once again using anatomical reasoning.¹³⁴ Thus Vesalius's novel practice of dissecting for himself and of seeing (as he thought) with his own eyes had yielded its first decisive fruit: it had resolved an important therapeutic problem which had troubled physicians for a generation.

But what was “pleuritis” itself? That is, what was the condition whose treatment was at issue here? *A priori*, we should surely expect to find that Vesalius defined “pleuritis” anatomically: not only in view of his vehement convictions as to the importance of anatomy in general, but also and more particularly because the entire rationale of his argument in the *Venesection letter* itself was anatomical. Yet we are in for a surprise, for in fact Vesalius deployed a *symptomatic* definition of pleuritis, and moreover he was at pains to argue *against* the anatomical definition of that illness.¹³⁵

It does not worry me that perhaps someone more contentious might contend that where I have included under pleuritis, pain in the loins or ilium I am using the expression “pleuritis” incorrectly. To him I shall reply that the name of this disease belongs to the category of those which are derived from the position of the primary lesion such as nephritis, peripneumonia, ophthalmia and *coxendix*. The name pleuritis will signify to me an affection of the whole side and not of the membrane lining the ribs alone, as many who ignorantly call that membrane the pleura believe.

Here Vesalius at first appears to class pleuritis amongst diseases which are anatomically-defined — for what counts is “the *position* of the primary *lesion*”, and the analogues he cites are all seemingly anatomical: nephritis was doubtless located in the kidney (*nephron*), peripneumonia in the lungs, ophthalmia in the eyes, *coxendix* in the coccyx or its vicinity. Yet it immediately becomes apparent that the meaning of “position” and of “lesion” here is in fact not anatomical at all. For whatever we make of “nephritis” *et al.*, Vesalius is locating pleuritis not in any particular organ or structure but on the contrary in the “side” of the patient, and in “the whole side” at that — that is, at a site defined in terms available to the patient, and localized only diffusely. And Vesalius goes on to contradict the alternative view, inherited as we have seen from the ancients, that pleuritis was an affection “of the membrane lining the ribs alone”.¹³⁶ With respect to pleuritis, then, Vesalius has in effect gone back not, as we might expect, to Erasistratus or to Herophilus (who became his models for anatomy once he had broken with Galen),¹³⁷ but instead to Hippocrates,¹³⁸ that is, to a pre-anatomical conception; and accordingly his preferred terms for pleuritis were *dolor lateralis* (pain in the side)¹³⁹ and *morbus lateralis* (disease of the side).¹⁴⁰ Finally, as the finishing touch to his rigorously symptomatic definition of pleuritis, Vesalius specifically contests the use of the word “pleura” for the membrane-lining-the-ribs. That is, it would seem that Vesalius, precisely because he defined “pleuritis” as pain-in-the-side rather than an inflammation of the membrane in question, wanted to restore or reserve the term “pleura” as a name for the side of the body, rather than as a term for that membrane. (And he further asserted that “pleuron” as a term for a “primary position” did not mean “rib”; rather, he suggested that the Greeks had called the ribs *pleurai* merely “because they form the side”,¹⁴¹ which meant that “the side” was the true and original meaning of the Greek πλευρον.)

Now this terminological issue has taught us something more: that by the time of Vesalius, the membrane lining the ribs has acquired a new name. In the ancient world, that membrane had been called the “*hypezocota*” (or in Latin, the *membrana costas succingens*); but at some point since that time — we do not know when — it has come to be called, by some, the “*pleura*”. Further, Vesalius indicates that those who have thus re-named that membrane are just those who interpret the disease “pleuritis” as an affection of that same membrane. As we have seen, the act of locating pleuritis within the “*membrana hypezocota*” was the work of a vast ancient tradition, from Erasistratus in the third century B.C. to Aretaeus and Galen four

or five hundred years later; but the renaming of that membrane as the “*pleura*” has apparently happened since Galen’s time. (I have not established who it was that proposed or adopted this new term; in view of Vesalius’s hostility to it, we may presume that the “ignorant” re-namers of the membrane are men who do not know their Galen and do not dissect for themselves, but this leaves many possible candidates.)¹⁴² Of course all acts of anatomical naming, throughout anatomy’s complex and troubled history, are highly significant: to a large extent anatomical discovery amounted to anatomical nomenclature, for anatomy was inherently concerned with a domain of experience which went beyond the bounds of ordinary language.¹⁴³ But there is something very special about this particular re-naming: in this instance, the name of the *part* has apparently been influenced by the name of the associated *illness*. For the term which had originally referred to the patient’s side or rib, namely “*pleura*” (Greek *pleuron*) has so to speak migrated inwards, into the “*hypezocota*” — and this is the very path which had previously been traced by “pleuritis” itself. That is to say, the name “*pleura*” for that membrane is a piece of anatomical nomenclature which has followed in the footsteps of pathology.

True to the argument of the *Venesection letter*, Vesalius’s *Fabrica* of 1543 gave the membrane its ancient name, that is, in its Latin form, the *membrana costas succingens*.¹⁴⁴ However, this was one battle which Vesalius lost; for by 1605, when Caspar Bauhin’s *Theatrum anatomicum* was published, the term “*pleura*” had become the standard name for the membrane.¹⁴⁵ Just how and why this occurred I have not established; at least two alternative hypotheses suggest themselves. First, it is possible that the anatomical conception of pleuritis somehow came to prevail in the course of the late sixteenth century, and that this shift carried with it the name of the membrane: in other words, that Vesalius was defeated on both fronts. But second, there was in the late sixteenth century another and different development, which might also have led to the term *pleura*’s being preferred as a designation for that membrane. Specifically, by Bauhin’s time that membrane had acquired another layer, a layer which invested the lungs (just as the peritoneum had long been seen as investing the abdominal organs);¹⁴⁶ and it is conceivable that this doubling of the membrane made the term “*succingens*” (undergirding) seem inappropriate. Whatever the reason, the anatomical aspects of the matter seem to have become consensual by the seventeenth century; for the double character of the membrane, and its naming as “the *pleura*”, have been routinely accepted ever since. But what of pleuritis? As we shall see, consensus was to prove much more elusive in pathology than in anatomy.

2.2.2. *Baglivi*

Vesalius surprised us by defining pleuritis symptomatically when we expected him to define it anatomically; a century and a half later Giorgio Baglivi, in his *De praxi medica* (*On the practice of medicine*) of 1699, will spring upon us a complementary surprise. As it happened, pleuritis — in the English translation of 1723, “pleurisy” — was the very first disease which Baglivi used to illustrate what he called the

“aphoristical way” of describing diseases.¹⁴⁷ Thus in order to appreciate what Baglivi had to say about pleurisy, we need to take note of this “aphoristical way” and of its place in Baglivi’s larger argument. *De praxi medica* was just as much a Renaissance project as Vesalius’s earlier anatomical investigations had been,¹⁴⁸ for Baglivi’s central argument was that although the moderns had now excelled the ancients in medical *theory*, chiefly thanks to “experimental philosophy” and to the use of “geometrico-mechanical principles”,¹⁴⁹ the ancients — which meant Galen as well as Hippocrates — were far superior to the moderns in all aspects of medical *practice*.¹⁵⁰ And in line with this orientation, Baglivi presented his “aphoristical way” as a revival of the literary practices of Hippocrates.

Baglivi described the “aphoristical way” as consisting of “short sentences, tied up to no rules of method or scholastic subtlety, but clearly and openly delivered”¹⁵¹ — this in contrast with what he called the “methodical way”, which “consists in tying it [physic] up to methods, and in digesting and adorning it with abstracted and useless notions”.¹⁵² The point of the “aphoristical way” was that it was a “loose” form of description, which precisely because of its unsystematic character left “void spaces ... for the insertion of the new and ever multiplicable voices of Nature”.¹⁵³ Baglivi associated the “aphoristical way” not only with Hippocrates but also with Bacon;¹⁵⁴ and he presented it as the appropriate mode of recording for what he called the *Medicina Prima* — a concept which was surely meant to echo Bacon’s “*Philosophia Prima*”.¹⁵⁵ The *Medicina Prima*, Baglivi explained, was “a pure history of diseases, obtained by sole observation at the sick man’s bedside, and related by the patients themselves”.¹⁵⁶ Baglivi argued strenuously that this *Medicina Prima* — the very foundation of medicine — was “not to be met with in books”, because “the descriptions of diseases” to be found in books “are for the most part taken not from observation and matter of fact, but from the Authors’ Brains”.¹⁵⁷ As Baglivi defined the *Medicina Prima*, in a passage which also introduced the accompanying *Medicina Secunda*, the “curative part”:¹⁵⁸

The *Medicina Prima* is a particular science of a peculiar form, which does not owe its principles or improvements to other sciences, but ... depends upon a diligent and patient description of all such things as the learned observator has marked down concerning the invasion, progress and exit of diseases, and committed to writing, with the same simplicity and sincerity that he used in observing them, without adding any thing of his own, or of the doctrines of books or other sciences.... In effect, all that part called the *Medicina Prima*, which is the basis of the *Medicina Secunda* or curative part, ought to be treated of so as that it be derived from the true nature of things and not from the nature of our thoughts, as many have done....

Thus the *Medicina Prima* was to be developed by setting aside all theoretical conjectures and by constructing instead “a pure and exact History of Diseases, I mean such as shows from the very Nature of things and is described” — Baglivi reiterated — “by the patients themselves”.¹⁵⁹ In short, the basis of the *Medicina*

Prima was empirical observations, of a kind which specifically and explicitly privileged *the experiences of the patient*; and the “aphoristic way” was the appropriate way of recording such observations.

Having extolled the virtues of the aphoristic method, Baglivi proceeded to illustrate it from his own experience. “Now the way of setting down the solid and reported observations of diseases in a short and aphoristical style”, he remarked, “will sufficiently appear from the succeeding diseases, which were examined by patient and repeated observations made in the Italian Hospitals”.¹⁶⁰ There followed brief accounts of some twelve or so diseases, of which “a pleurisy” was the first. From what has been said so far, we should surely expect that Baglivi defined pleurisy symptomatically (since “a pure history of diseases” is “related by the patients themselves”), and that he did so strictly on the basis of his own experiences (that is, his “patient and repeated observations made in the Italian Hospitals”). Yet in fact Baglivi’s conception of pleurisy was straightforwardly anatomical — and his opening remarks followed Galen! Here is the first paragraph of Baglivi’s account:¹⁶¹

Of a Pleurisy

If you would discover a pleurisy, place your chief care in observing the nature of the pulse. The hardness of the pulse is almost an infallible sign of all pleurisies; and while the pleurisies are sudden, or complicated with other diseases of the breast, if you observe a hardness in the pulse, i.e. too great a distension or vibration of the artery, tho’ the other signs are absent, you may assure yourself that the patient is under a pleurisy; for a hard pulse is an inseparable companion of all inflammations upon the nerves or membranous parts.

Obviously enough, both the hard pulse and its rationale echoed Galen (albeit with a slight modulation, from “a nervous part” to “the nerves or membranous parts”),¹⁶² and I shall attend to this seeming paradox in a moment. But what is just as important is the massive depth of taken-for-granted assumptions built into Baglivi’s remarks. To begin with, Baglivi simply assumed that pleurisy is an inflammation, and that his readers would share that premise; indeed, so tacit was this knowledge that he did not even trouble to spell out what he doubtless had in mind, that pleurisy consisted specifically in an inflammation of the pleural membrane. (We may surely infer that this was Baglivi’s meaning, for he classed pleurisy as one of the “diseases of the breast” — and significantly, this appeared merely in an aside, which is another indication of how much he was taking for granted.) Similarly, he made no attempt to describe pleurisy’s characteristic suite of symptoms — “the other signs”, as he put it — for these too were common knowledge. Instead Baglivi concentrated upon just one sign, namely the hardness of the pulse, which he was privileging on account of its diagnostic power; and so too it emerged in the next sentence that “the difficulty of breathing” was a typical symptom of pleurisy, but this was mentioned specifically because of its special prognostic significance. It seems, then, that Baglivi had no need to enumerate the full list of pleurisy’s symptoms,

for those symptoms were taken as implied by the very word “pleurisy”. Sure enough, Baglivi’s subsequent discussion,¹⁶³ which was devoted to prognostics¹⁶⁴ and to therapeutics,¹⁶⁵ mentioned several of those symptoms — fever, spitting, and (obliquely) pain-in-the-side — but always in passing, as things which were already well-known.¹⁶⁶ In short, just as Baglivi could take it as read that pleurisy consisted in inflammation of the pleural membrane, so also he could assume that his readers were familiar with its characteristic signs and symptoms.

Thus Baglivi’s discussion of pleurisy exemplified several times over the utter impossibility of his own project of pure observation, of what he called “diligent and patient description”, uncontaminated by “the nature of our thoughts”.¹⁶⁷ In the first place Baglivi, like any observer, was of course necessarily observing through inherited *categories*, categories which were so ingrained that he did not realize that they *were* categories: in this instance, the category “pleurisy”. Secondly, that particular category as Baglivi deployed it embodied the conviction that pleurisy consists in an inflammation of what had now come to be called the pleural membrane — a conviction which by its very nature derived not from “sole observation at the sick man’s bedside”, but on the contrary from “our thoughts”. It was of course this very conception of pleurisy, inherited from the later ancients, which Vesalius had contested in his *Venesection letter*. Evidently Vesalius’s attempt to restore the symptomatic conception of pleuritis had failed, at least in Baglivi’s particular setting; for the anatomical conception had somehow — we do not know how — attained the status of a convention. Indeed, so conventional had it become that Baglivi deployed it *malgré soi*: in the very attempt to illustrate his argument that medicine should be based on “a pure history of diseases, obtained by sole observation at the sick man’s bedside, and related by the patients themselves”, he actually defined pleurisy in anatomical terms.

Thirdly, precisely the same was true of the *diagnostic* claim which Baglivi was putting forward — that “the hardness of the pulse is almost an infallible sign of all pleurisies”. For Baglivi justified this aphorism not by practical experience (either his own or that of others), but instead by appealing to underlying mechanisms: “a hard pulse is an inseparable companion of all inflammations upon the nerves or membranous parts.”

Last but not least, this rationale was itself derived from Galen, yet Baglivi was seemingly unaware of the fact. He could have acknowledged Galen as his source, and indeed this would have been grist to his mill: for *De praxi medica* bracketed Galen with Hippocrates as laudable ancients, accurate observers, and Baglivi was also at pains to detach the virtues of Galen himself from the vices of the “Galenists”.¹⁶⁸ Yet as a matter of fact he did not mention Galen here; and the simplest explanation for this apparent oversight is that Baglivi had so internalized this particular Galenic message — perhaps derived from his teachers, rather than directly from Galen himself — that he really believed that it was his own observation. On this reading, it would seem that when Baglivi *felt* a hard pulse, he also and in the same moment *felt* an “inflammation upon the nerves or membranous parts”, and

specifically an inflammation of the pleural membrane. In other words, Baglivi had this particular doctrine of Galen's literally in his fingertips.

To sum up so far: In respect of pleurisy the medical men of the Renaissance inherited from the ancients two distinct conceptions, the one symptomatic (in the Hippocratic texts), the other anatomical (conveyed to them especially in Galen's works). These two approaches were surely in tension with one another, and this is indeed what we have found; yet that tension expressed itself in very surprising ways. In the case of Vesalius, we would expect to find the anatomical conception at work; yet in fact Vesalius defined pleuritis in symptomatic terms, that is, as pain-in-the-side, and explicitly argued against the anatomical interpretation of the disease. And in the case of Baglivi, we would expect to encounter a deliberate application of the symptomatic definition; yet Baglivi actually defined pleurisy anatomically, and moreover he did so quite unwittingly. In short, the relation between symptomatic and anatomical conceptions was remarkably subtle and complex — and this for just a “single” disease.

Nevertheless there seems to be a certain logic to the story of “pleuritis”: for to judge by all that Baglivi was taking for granted, it would appear that Vesalius had been defeated over the definition of pleuritis just as he was over the naming of the membrane. That is to say, it seems that the anatomical conception of pleuritis — in its “Erasistratean” form, with the Galenic addition of the hard pulse — had prevailed in the course of the Renaissance just as it had in later Antiquity. Once again, however, we are in for a surprise, as we shall see by turning from Baglivi to Morgagni.

2.2.3. *Morgagni*

Giambattista Morgagni's *De sedibus* of 1761 was perhaps the most important work, and certainly one of the most original and profound works, in the whole Western medical tradition.¹⁶⁹ What characterized that great book was not only the painstaking correlation of symptoms with post-mortem findings, for, in addition, *De sedibus* was a vast meditation upon the entire corpus of Western medical writings, from Hippocrates to Morgagni's own day. Further, Morgagni's historical sense was far more subtle and sophisticated than that of, say, Baglivi (with his naïve assertion that “Hippocrates speaks in the words of Nature, rather than those of man”)¹⁷⁰ or Boerhaave (with his Galen-like claim that Hippocrates was an anatomist).¹⁷¹ Indeed, Morgagni dissected texts just as skilfully as he anatomized dead bodies and interpreted symptoms; that is, he was a master of analytical exegesis in three distinct domains: symptomatic, anatomical and textual. And the signal achievement of *De sedibus* was that it *united* these three layers or dimensions: that is, Morgagni systematically correlated (i) symptomatology with (ii) anatomy, meanwhile interweaving (iii) previous discussions of the disease in question. Consequently, *De sedibus* was a treasury not just of new observations but also of commentary upon old ones, stretching throughout Western medicine's history. Yet the vast riches of the book remain largely untapped by medical historians, at least those writing

in English.¹⁷² In part this neglect reflects the inherent difficulty of the text, for Morgagni's prose is at first sight rebarbative and forbidding. At the same time, that neglect can be taken as a "symptom" (to use the obvious metaphor) of the very point which I am trying to argue: that medical history has strangely overlooked the history of medicine proper. Certainly there is a vast and troubling disparity between the recognized importance of *De sedibus* and the amount of attention that the book has received in English.¹⁷³

But let us turn to Morgagni's discussion of pleurisy, which appeared — along with peripneumony — in Letters 20 and 21 of *De sedibus*.¹⁷⁴ (Letter 20 was concerned with the cases, dissections and observations of Morgagni's erstwhile teacher Antonio Maria Valsalva; Letter 21 with those of Morgagni himself.) It will be helpful to begin by comparing Morgagni's working definition of pleurisy with that of Baglivi, which we have just been examining. On the one hand, Morgagni differed radically from Baglivi in that he conceived pleurisy in rigorously *symptomatic* terms. On the other hand, Morgagni curiously echoed Baglivi in that he too *took for granted* the defining symptoms of pleurisy: that is to say, he never listed those symptoms in a formal way, but instead let them emerge piecemeal in the course of his case-histories and discussions. Thus the implied premise of his account was that there existed a consensus as to pleurisy's characteristic symptoms. We shall see in a moment what those allegedly-consensual symptoms were; first it is important to notice why Morgagni defined pleurisy symptomatically.

The point of Morgagni's symptomatic definition of pleurisy was this: that the very theme which Baglivi had *assumed* (at least, in the particular case of pleurisy), namely the anatomical seat of the disease, was what Morgagni wanted to *investigate*. In this respect Morgagni's approach to pleurisy was identical to his approach to diseases in general: he always defined diseases symptomatically and then tied them to anatomical seats as indicated by dissection — accomplishing this link by means of an intricate logical operation which rested on the comparison of different individual case-histories.¹⁷⁵ Hence the fact that Letters 20 and 21 of *De sedibus* were described as "the discourse of pain in the breast, sides, and back", that is, as being concerned with *experiences of the patient*. And this gives us a good indication of the nature of Morgagni's enterprise in general, and of his approach to pleurisy in particular. We have seen that pleurisy had been tied in Antiquity to the "*membrana hpezocota*", that is, to the membrane which later came to be called the "*pleura*", and that Baglivi had uncritically echoed this conception. But Morgagni, while being highly favourably disposed to anatomical localization, wanted to *reopen* the whole question as to the site of that localization. And for this purpose it was essential to begin with a strictly symptomatic definition, for only in that way could anatomical localization be investigated *de novo* as Morgagni was seeking to do.

The central symptoms-of-pleurisy which Morgagni was assuming, as best I can reconstruct them, were as follows (in purely arbitrary order):

(1) a "pungent" pain in the side, (2) a fever, (3) a hard pulse, and (4) difficulty in lying on the unaffected side. In fact the last symptom on this list was attended

with certain complexities; for some patients, paradoxically, experienced difficulty in lying not on the unaffected side but, on the contrary, “on the pained side”. This anomaly occasioned a typically subtle and fruitful Morgagnian digression;¹⁷⁶ but for present purposes we can ignore it, since Morgagni acknowledged that the difficulty was indeed usually in lying on the unaffected side, just as the ancient dogmatics had claimed.¹⁷⁷ Now this occasional qualification aside, Morgagni’s cluster of pleuritic symptoms was entirely conventional. Or to be more precise, Morgagni was here echoing those before him — from the later ancients to Baglivi — who had seated the disease in the “*membrana hyezocota*” / the “*pleura*”; for these were the very symptoms which had been identified by that anatomical tradition. Thus Morgagni’s implicit definition of pleurisy overlapped with both Vesalius’s conception and Baglivi’s conception, while differing from them both. Like Vesalius, Morgagni defined pleurisy symptomatically — but Vesalius’s symptomatic conception had been Hippocratic (pleuritis is pain-in-the-side), whereas Morgagni’s conception followed that of the dogmatic tradition (pleuritis is a constellation of symptoms, of which pain-in-the-side is only one, and that pain is specifically “pungent” in character). And like Baglivi, Morgagni drew his suite of pleuritic symptoms from the dogmatic tradition, including the “hard pulse” which Galen had added — but Baglivi had defined pleuritis anatomically, whereas Morgagni defined it by its symptoms.

In short, Morgagni *accepted* that definition of pleurisy’s symptoms which had been constructed by the ancient “dogmatic” tradition stemming from Erasistratus and culminating in Galen; but he *put in suspense* the anatomical localization which had been decreed by that same tradition, precisely because his purpose was to *reconsider* the site of pleurisy’s localization. And in effect — to oversimplify brutally what was an immensely complex and subtle discussion — Morgagni took up again the very argument which had raged between Erasistratus *et al.* and Herophilus *et al.*, that is, the question as to whether pleurisy is seated in the membrane lining the ribs (which had now, of course, become a double membrane named the *pleura*)¹⁷⁸ or in the lungs. To this end he carefully compared the symptoms and post-mortem findings of pleurisy (traditionally seated in the membrane) with those associated with peripneumony (traditionally seated in the lungs). And he came up with the answer that pleurisy and peripneumony were not distinct diseases at all; rather, they were two aspects of a single disease, which Morgagni called *pleuripneumony*. Yet he did not present this conception as his own invention; rather, he ascribed it to his Paduan predecessor of the early seventeenth century, Vincent Baronius.¹⁷⁹ In short, the effect of Morgagni’s argument is that the very category of pleurisy is a mistake, and that this has been known — albeit forgotten — for over a century! Notice Morgagni’s rhetorical tactics: he could, of course, have begun with Baronius, but instead he chose to end with him. What he actually began with was the symptomatic content of the standard definition of pleurisy; and the point of this vast opening concession was that it rendered all the more forceful his eventual demolition of that definition.

In Morgagni's hands, then, pleurisy has literally disappeared: it is no longer a distinct disease at all. Rather, it is a mistaken interpretation of certain symptoms, symptoms which commonly (but not always) go together, and which are sometimes (but by no means always) associated with inflammation of the pleural membrane. So too peripneumony as conventionally conceived has to be re-thought, for the distinction between pleurisy and peripneumony is invalid. The real disease, pleuripneumony, is an inflammation of the lungs which is associated with various and irregular complications, such as the exudation of fluid, adhesion of the pleural membranes, and — as just one such complication amongst many others — inflammation of those membranes. Depending on the precise course of the disease in the particular patient, different patterns of symptoms are observed; but those patterns called “pleurisy” and “peripneumony” are merely arbitrary, for the supposed differences between them are often contradicted in particular cases. And danger to the patient arises not from the involvement of the pleura but solely from the inflammation of the lungs. Furthermore, these seemingly heretical doctrines are in fact consistent with all that has been learnt about pleuritis and peripneumony, both in Antiquity and since — as Morgagni shows through a careful contemplation of the medical writings of the ancients and moderns alike.¹⁸⁰

Incidentally we should notice that by introducing us to Baronius, Morgagni has also muddied the waters of the previous stage of our story. We saw earlier that between the lifetimes of Vesalius and Baglivi, the meaning of “pleuritis” apparently stabilized much as it had done in later Antiquity: that is, it became an inflammation of the “pleura”, the two-layered descendant of the *membrana hypezocota*. But now that we have come across Baronius, it is clear that the history of “pleuritis” between about 1550 and 1700 involved further complexities. Was Baronius responding to a previous debate? By what methods did he arrive at his concept of “pleuripneumony”? Was his work simply forgotten, was it debated and explicitly rejected, or was it in fact received more positively than Baglivi and Morgagni have led us to assume?¹⁸¹ Such questions give concrete form to what we already know on grounds of principle: that an adequate history of “pleurisy” would require us to investigate a host of intermediate figures between Vesalius and Baglivi (not least Baronius), and for that matter, between Baglivi and Morgagni (for instance, Boerhaave).¹⁸²

So too we could, of course, pursue the history of pleurisy after Morgagni. And if we were to take up the later history of pleurisy, we would find that Morgagni's discussion was by no means decisive; for in fact both pleurisy and peripneumony survived his putative dissolution of the distinction between them. Subsequently, in the hands of Laennec, pleurisy changed yet again, seemingly remaining within the pleural membrane yet with several novel twists. As the inventor of the stethoscope, Laennec found for pleurisy a distinctive auscultative sign, namely “egophony”, a bleating sound as of a goat;¹⁸³ he therefore accorded special importance to the physical state which he took that sound to reflect, namely an effusion of fluid in the thoracic cavity;¹⁸⁴ further, pleurisy's other signs and symptoms were rather

different from those which Morgagni had deployed;¹⁸⁵ and last but not least, Laennec distinguished between “pleuritis” and “pleurisy”, for *pleuritis* was inflammation of the pleural membrane, whereas *pleurisy* was the larger disease-condition which such inflammation produced.¹⁸⁶ Nor did pleurisy in this new form achieve stability; for as we learn from Russell Maulitz’s study of pleurisy in the Paris *clinique* of the 1830s,¹⁸⁷ Laennec’s immediate successors such as Auguste François Chomel redefined pleurisy/pleuritis yet again.¹⁸⁸ Although they still regarded inflammation of the pleural membrane as definitive, it seems that they placed particular diagnostic emphasis upon the pleuritic pain;¹⁸⁹ they certainly loosened the tie between that pain and the pleural effusion,¹⁹⁰ thereby diminishing the pathognomonic significance of Laennec’s egophony;¹⁹¹ and they linked pleurisy with phthisis (tuberculous consumption),¹⁹² whereas Laennec had made a point of separating pleurisy from phthisis.¹⁹³ (Notice too that this link between diseases of the pleural membrane and of the lungs was quite different from that which Morgagni had sought to establish — for Morgagni had connected pleurisy with peripneumony, not with phthisis,¹⁹⁴ and there is no indication that Chomel and his colleagues were seeking to dissolve pleurisy as Morgagni had done.) And meanwhile the clinicians of the 1830s were applying to pleurisy, as to other diseases, the new statistical methods introduced by Pierre Louis,¹⁹⁵ thereby adding a novel analytic dimension which Laennec had barely envisaged,¹⁹⁶ and which was of course quite alien to Morgagni’s focus upon individual case-histories.¹⁹⁷

Thus throughout the three centuries from the Renaissance to the Paris “clinic”, the history of pleurisy reveals a repeated *refusal of closure*. Vesalius sought to restore a symptomatic definition of the disease, but he apparently failed in this just as he did in his attempt to preserve the Hippocratic meaning of “pleura”/“pleuron”; Baglivi took it for granted that pleurisy was an inflammation of the pleural membrane (now membranes, plural), but Morgagni demolished this conception and dissolved the very being of pleurisy; Morgagni’s account, for all its rigour, did not persuade his successors; Laennec tied pleurisy to the “egophony” revealed by his stethoscope, but this bond was immediately weakened. Indeed, it is doubtful whether pleurisy ever stabilized; but that is a question for another study, as is the associated and equally intriguing issue as to the meaning, or meanings, of pleurisy today. What is clear is that in the first two thousand years and more of pleurisy’s history, any such stability of its meaning was always temporary and contingent.

3. CONCLUSION

What larger implications might follow from this all-too-incomplete survey of pleurisy’s history? I suggest that eight points stand out.

First, my findings underline once more what was demonstrated long ago by Temkin (for all the limitations of his conception), by Canguilhem and above all by Fleck, and has been confirmed by various investigations in the last twenty years or so: namely that concepts-of-disease comprise an eligible domain for historical investigation in their own right. That domain is distinct from — though by no means

independent of — such more familiar history-of-medicine themes as the history of anatomy and of theories of normal bodily function. More particularly, it is quite different from the exercise of retrospective diagnosis; for retrospective diagnosis suppresses precisely what the history of disease-concepts brings to the fore, namely the *content of past* descriptive and diagnostic categories.

Second, this particular historiographic theme is not only possible but is also uniquely appropriate: it is in the literal sense *proper* to the history of medicine, since the business of medicine was-and-is to do with human illnesses. Indeed, the history of concepts-of-disease takes us to the very heart of medicine's history: for of course it was those very concepts that determined what medical practitioners saw, and which thus defined what medicine actually *was*. In short, the history of disease-concepts is just that segment of the history of ideas which is medical history's distinctive and special concern.

Third, to judge by the example of pleurisy, the history of disease-concepts is rich with possibilities and has many unexpected twists and turns. When I embarked on this investigation, in blissful ignorance of Wesley Smith's pioneering study published over ten years earlier, I had expected to find a simple transformation of pleurisy from symptomatic to anatomical, and I was vaguely locating that putative transformation in the eighteenth century, or perhaps in the early nineteenth century, with the work of Bichat. In only one respect did those expectations prove correct: for while there was indeed a shift from a symptomatic conception to an anatomical one, it had taken place (as I learnt from Professor Smith's essay) over two thousand years earlier than I had thought, it was by no means simple, and it did not produce the closure that I was assuming. And my subsequent explorations have yielded further surprises at every point — for instance ramifying from pathology into anatomical discovery-and-nomenclature, and repeatedly raising the problem as to how medical consensus was established, if indeed such consensus was established at all.

Fourth, as has just emerged, this subject is by no means bounded by the history-of-ideas conception to which I have restricted myself here. On the contrary, both shifts and stability in the concept of pleurisy, or of any other disease, raise explanatory questions which could be addressed only by a widening of focus, a shift of attention outwards from the concepts themselves to the many other matters which might have influenced them — for instance, practices of healing and of pedagogy, corporate relationships, patronage networks, religious and philosophical allegiances. Indeed, one of the attractions of a history of disease-concepts is that it can provoke us to connect the cognitive and the social dimensions of medicine's history¹⁹⁸ — precisely in line with Fleck's conception of knowledge.

Fifth, and relatedly, the history of disease-concepts brings into play the perspectives of both patients and doctors, and what is more, it necessarily *combines* those perspectives. It is now, of course, well-recognized that medical history embraces the viewpoint of the patient as well as that of the practitioner.¹⁹⁹ And concepts-of-disease as an historiographic theme hold out the promise of enriching

this insight, for one might well say that those concepts were and are the very ground upon which those two viewpoints come to meet and merge.²⁰⁰ This is well attested by what we have seen of pleurisy's history, for while the various definitions of the disease all emanated from medical practitioners, those definitions always bore some imprint of the patient's experience, even if (as with Baglivi's conception) only in attenuated form. To recapitulate, "pleuritis" began as neither more nor less than an experience of the patient; its symptomatic content was actually *enlarged* — remarkably enough — by the adoption of an anatomical definition; and even in the Paris clinic of the 1830s, the supreme site of the anatomical conception of illness, the patient's experience in the form of pain-in-the-side retained a crucial diagnostic significance. In short, just as the sufferings of patients are the very occasion for medicine, so the history of disease-concepts entails attention to those sufferings and to the dialectic between the patient's experience and the practitioner's categories.

Sixth, the case of pleurisy has put in a very different light the picture developed in Foucault's *Birth of the clinic*. We saw at the outset that Foucault's programme was putatively constructed along strictly syntactic lines, and that this approach, were it actually possible to implement it, would foreclose the very possibility of writing a history of disease concepts; and now that we have glimpsed how complex was the development of the concept of "pleurisy", it has become only too apparent what a wealth of possibilities Foucault thereby excluded. But this is by no means all, for pleurisy's history radically qualifies the substantive story of *Birth of the clinic*. For one thing, pleurisy shows that the kind of anatomical localization pursued by Bichat — the siting of diseases not in the organs but in the "membranes" or "tissues" — had a model which began in Antiquity. For another, the shift in Antiquity from a symptomatic definition of pleurisy to an anatomical conception was associated with a transformation of medical categories just as radical as the "mutation in discourse" associated with *la clinique* itself. And finally, the relation between a medicine of symptoms and a medicine based on anatomy was far more complex than Foucault supposed: for rather than the one giving way to the other, whether in the years around 1800 or at any earlier point, the two were *in productive tension* for almost the entire history of Western medicine, that is to say, from the time of Herophilus and Erasistratus onwards. Nor was that tension abolished by *la clinique* — for as Jacalyn Duffin has recently shown, Laennec himself was deeply aware of the limitations of the very anatomico-clinical conception which he did so much to consummate,²⁰¹ and we have seen that the relation between symptoms, signs and anatomy continued to trouble his successors of the 1830s.

Seventh, the history of concepts-of-disease is a field of vast scope. To begin with, we have at our disposal literally hundreds of diseases whose history could be traced; but this, which we might call "special pathology history" (the history of concepts of particular diseases), is only part of the subject. For there also beckons the possibility of "general pathology history", that is, the history of pathological theory at large; and the latter theme, which was the very concern of Canguilhem's

The normal and the pathological, is every bit as important as “special pathology history”. As just one illustration of the many opportunities that present themselves in “general pathology history”, we may take the case of eighteenth-century *nosology*. Although Foucault rightly drew attention to nosology in *Birth of the clinic*, and although it has attracted a handful of studies since that work appeared, we still remain almost wholly in the dark as to the origins of nosology (just *why* did Sauvages invent it?);²⁰² about its relations with traditional “pathology” (how exactly did nosology differ from pathology?);²⁰³ about the sites of its success and failure (which medical schools used nosologies in their teaching, and which did not?);²⁰⁴ about its mutations and inflections in the course of the eighteenth century (how and why did Pinel’s “nosographic” categories differ from the “nosological” categories of Sauvages or of Cullen?); and last but not least, about its historical impact (was nosology, as Foucault argued, the essential preliminary to *la clinique*, or was it, as Porter has suggested, of little or no long-term significance?).²⁰⁵

Finally, the history of disease-concepts is a theme which should lend itself to *comparative* investigation. For instance, to stay for convenience with the example of pleurisy, we might ask: is there any analogue of pleurisy in Chinese medicine or in Ayurvedic medicine?²⁰⁶ Have Ayurvedic practitioners and Chinese practitioners encountered “pain in the side” at all, and if so, how have they described it, interpreted it and treated it? It is of course beginning to be recognized that “history of medicine” can no longer be narrowly equated with the history of Western medicine alone; but historians of medicine have not found it easy to bring different medical traditions within a common focus, because the cognitive structures of those traditions are so very different. But concepts of disease, with their necessary focus upon patients’ experiences, hold out the promise of establishing points of intersection between different diagnostic categories, which should shed light upon the larger conceptual styles which characterize each of the three great medical systems. And it may be ventured that such a comparative perspective will make still clearer the historically-constructed character of disease-concepts in the Western tradition itself.

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2. O. Temkin, *The Falling Sickness: A history of epilepsy from the Greeks to the beginnings of modern neurology* (Baltimore, 1945). On the limits of Temkin's vision see below, at ref. 40.
3. G. Canguilhem, *Le normal et le pathologique* (Paris, 1966); *The normal and the pathological* (transl. by C. R. Fawcett and R. S. Cohen, Dordrecht, 1978; New York, 1991). On the enduring relevance of Canguilhem's work see M. Nicolson, "The social and the cognitive: Resources for the sociology of scientific knowledge", *Studies in history and philosophy of science*, xx (1991), 347–69.
4. K. Figlio, "Chlorosis and chronic disease in nineteenth-century Britain: The social construction of somatic illness in a capitalist society", *Social history*, iii (1978), 167–97 (cf. below, at ref. 53); P. Wright and A. Treacher (eds), *The problem of medical knowledge: Examining the social construction of medicine* (Edinburgh, 1982).
5. For instance M. Pelling, *Cholera, fever and English medicine 1825–1865* (Oxford, 1978); S. Jarcho, *The concept of heart failure: From Avicenna to Albertini* (Cambridge, Mass., 1980); W. F. Bynum and V. Nutton (eds), *Theories of fever from Antiquity to the Enlightenment (Medical history Supplement no. 1, London, 1981)*; R. C. Maulitz, *Morbid appearances: The anatomy of pathology in the early nineteenth century* (Cambridge, 1987); W. D. Smith, "Pleuritis in the Hippocratic Corpus and after", *Proceedings of the Sixth International Hippocratic Colloquium, Quebec, September 1987* (Quebec City, 1989), 000–00; M. Nicolson and C. McLaughlin, "Social constructionism and medical sociology: A study of the vascular theories of multiple sclerosis", *Sociology of health and illness*, x (1988), 234–61.
6. For a helpful conspectus and a guide to recent literature see W. F. Bynum and R. Porter (eds), *Companion encyclopedia of the history of medicine* (2 vols, London, 1993), Part III (vol. i), particularly the essays by M. Pelling, L. G. Wilson, R. C. Olby, T. M. Brown, M. Worboys, D. Cantor and R. Porter (chaps. 16, 19–21, 24–25, 27). (The *Companion encyclopedia*, it should be remarked, is exceptional amongst recent textbooks in the attention it devotes to this theme.) See also R. C. Maulitz, "In the clinic: Framing disease at the Paris hospital", *Annals of science*, xlvii (1990), 127–37; essays of C. Lawrence and S. Peitzman, cited in refs 15, 18 below; and J. Duffin, *To see with a better eye: A life of R. T. H. Laennec* (Princeton, N.J., 1998).
7. On what "chlorosis" meant in relation to 20th-century categories see I. Loudon, "The diseases called chlorosis", *Psychological medicine*, xlv (1984), 27–36.
8. See for instance S. L. Gilman *et al.*, *Hysteria beyond Freud* (Berkeley, 1993).
9. Fleck, *Genesis and development of a scientific fact* (ref. 1), 1–19.
10. The corollary was that in Fleck's view, "*Spirochaeta pallida* should ... be defined by syphilis rather than the other way around" (*ibid.*, 18).
11. *Ibid.*, 14, my emphasis.
12. "The relation between the Wassermann reaction and syphilis — an undoubted fact" entailed "several adaptations and transformations of concepts": *ibid.*, 97–98, Fleck's emphases. See also the extended discussion in note 5 (pp. 178–9) to p. 102.
13. *Ibid.*, 39.
14. J. Gabbay, "Asthma attacked? Tactics for the reconstruction of a disease concept", in Wright and Treacher (eds), *The problem of medical knowledge* (ref. 4), 23–48.
15. C. Lawrence, "'Definite and material': Coronary thrombosis and cardiologists in the 1920s", in C. Rosenberg and J. Golden (eds), *Framing disease: Studies in cultural history* (New

- Brunswick, N.J., 1992), 50–82.
16. See the remarks of Paul White in 1931, quoted by Lawrence, “‘Definite and material’”, 67.
 17. At least, so far as this has been traced — for the loose end left by Lawrence’s study is the question as to how “consensus” over the ECG was reached (*ibid.*, 72).
 18. S. J. Peitzman, “From Bright’s disease to end-state renal disease”, in Rosenberg and Golden (eds), *Framing disease* (ref. 15), 3–19.
 19. *Ibid.*, 9, 11, 16.
 20. The most notable exception is A. Cunningham, “Transforming plague: The laboratory and the identity of infectious disease”, in A. Cunningham and P. Williams (eds), *The laboratory revolution in medicine* (Cambridge, 1992), 209–44.
 21. C. Quéтел, *Le Mal de Naples* (Paris, 1986); *History of syphilis* (transl. by J. Braddock and B. Pike, Cambridge, 1990). “Syphilis as a cultural phenomenon” is the title of the introduction (pp. 1–8).
 22. *Ibid.*, 53, 67, 75, 81, 108.
 23. This from the *Oxford English dictionary* — for characteristically, *Le Mal de Naples* does not mention the meaning of the term.
 24. “Thus syphilis came to be recognised as syphilis, gonorrhoea as gonorrhoea” (Quéтел, *Le Mal de Naples* (ref. 21), 111). Cf. and contrast Fleck, *Genesis and development of a scientific fact* (ref. 1), 7–8.
 25. The point is touched upon just once and in passing: Quéтел, *Le Mal de Naples* (ref. 21), 97.
 26. Fleck, *Genesis and development of a scientific fact* (ref. 1), 41. See also, for instance, pp. 15–16 (“The discovery of the causative agent, *Spirochaeta pallida*, was the result of steady, logical work by civil servants”), 22, 69–70. For a general formulation see p. 123: “the true creator of a new idea is not the individual but the collective.”
 27. *Ibid.*, 98.
 28. *Ibid.*, 99.
 29. These mechanisms involved a spectrum of communication-media, from what Fleck called “journal science”, through “vademecum science” and “textbook science”, to “popular science”: see *ibid.*, 111–25. At one extreme, “journal science”, which is precisely the “vanguard” of science (p. 123) is “provisional, uncertain, and personally colored” (p. 119); at the opposite extreme, “popular science” entails “valuation” (p. 113), “simplicity” and “vividness” (p. 115). The key intermediate category was “vademecum science” (pp. 119–24), which represents the “collective, generally valid” aspect of research science (p. 120), and “requires a *critical synopsis in an organized system*” (p. 118, Fleck’s emphasis); on this see refs 31, 34 below.
 30. *Ibid.*, 112.
 31. *Ibid.*, 122–3. Here I am simplifying Fleck’s picture, for at this point he was in fact discussing “vademecum science” rather than “popular science”. However this simplification is not inappropriate, for three reasons. In the first place, Fleck argued that popular science has a “general epistemological significance”, since its qualities of “certainty, simplicity, vividness” are precisely the goals of “the expert” as well (pp. 114–15) — whence the fact that “the conviction that there is no development of thought”, which as we see below is characteristic of popular science, is “a conviction that also influences the expert” (p. 116). Secondly, vademecum science and popular science as Fleck depicted these have many features in common: for instance, vademecum science resembles popular science in seeking to constitute a “closed system” (p. 119); in involving “exoteric” as well as “esoteric” knowledge (p. 123); and in constructing a mythical, individualised history (pp. 122–3; cf. p. 116, as quoted immediately below). Third, Fleck described vademecum science as developing out of “journal science” through processes of communication, including the stabilising of nomenclature (pp. 120–3); and

- he had already insisted that “*Every communication and, indeed, all nomenclature tends to make any item of knowledge more collective and popular*” (p. 114, Fleck’s emphasis). See also ref. 34 below.
32. *Ibid.*, 116 (here referring to “popular science” itself).
 33. On the same page Fleck reproduced a potted history of syphilis (from Gottstein’s book of 1929) and commented: “From descriptions such as this, the conviction emerges that there is no development of thought.”
 34. *Ibid.*, 120–1. Strictly speaking, this anecdote concerning retrospective diagnosis was presented not as an aspect of “popular science” but rather within Fleck’s account of “vademecum science”, and not as a characteristic of such science but rather for a specific technical reason (i.e., as an illustration of the collective, “impersonal”, origin of collectively-accepted concepts). But my appropriation of Fleck’s brief discussion of retrospective diagnosis is, I submit, consistent with the claims he had already made about the apprehension of “syphilis” in “popular science” (p. 116). See also ref. 31 above.
 35. *Ibid.*, 82–83, 165.
 36. *Ibid.*, 140; cf. p. 111 (ref. 24 above).
 37. For a rare exception whose very brevity confirms the rule, see *ibid.*, 5–6.
 38. Above, at ref. 20.
 39. For example, the main reference work on the history of science cites Fleck’s work three times, in each case as a major conceptual resource, whereas the comparable (and substantially longer) reference work on the history of medicine refers to Fleck only once, and even then as what amounts to an addendum on the history of Fleck’s own field, immunology. See respectively R. C. Olby, G. N. Cantor, J. R. R. Christie and M. J. S. Hodge (eds), *Companion to the history of modern science* (London, 1990), 64, 91, 164, and Bynum and Porter (eds), *Companion encyclopedia of the history of medicine* (ref. 6), 203. Even Cunningham’s fine essay “Transforming plague” (ref. 20 above), the central historiographic point of which is entirely consonant with Fleck’s approach, does not cite Fleck. Fleck was first noticed in English by T. S. Kuhn, *The structure of scientific revolutions* (Chicago, 1962), pp. vi–vii; the 1979 English translation was produced at the behest of historians and sociologists of science, notably R. Merton.
 40. See for instance G. Brieger, “The historiography of medicine”, in Bynum and Porter (eds), *Companion encyclopedia of the history of medicine* (ref. 6), i, 24–44, p. 31.
 41. Temkin, *The Falling Sickness* (ref. 2), Preface, p. vii, my emphasis.
 42. R. Koch, *The aetiology of tuberculosis* (German original 1882; transl. by Dr and Mrs M. Pinner, New York, 1932), 44. Tuberculosis was originally “consumption” or “phthisis” (touched upon in refs 110 and 194 below); Koch’s redefinition of the disease transformed its meaning in complex ways, precisely in line with what Fleck was to write about syphilis (above, at ref. 13).
 43. M. Foucault, *Naissance de la clinique* (Paris, 1963); *Birth of the clinic: An archaeology of medical perception* (transl. by A. M. Sheridan Smith, London, 1973).
 44. See for instance D. Armstrong, *Political anatomy of the body: Medical knowledge in Britain in the twentieth century* (Cambridge, 1983).
 45. Foucault, *Birth of the clinic* (ref. 43), pp. x, xv, xi.
 46. For “imaginary investments” see *ibid.*, p. x.
 47. *Mutatis mutandis*, the same is true of N. Jewson’s classic study, “Medical knowledge and the patronage system in eighteenth-century England”, *Sociology*, viii (1974), 369–85.
 48. Foucault, *Birth of the clinic* (ref. 43), p. xi.
 49. *Ibid.*, p. xi; my emphasis.
 50. *Ibid.*, pp. xvi–xvii.

51. *Ibid.*, 196, p. xii; my emphases.
52. Thus for all that Foucault consistently distanced himself from structuralism, his archaeological *oeuvre* is open to precisely the kind of critique that P. de Man levelled at structuralism in various of his works; see for instance *Blindness and insight: Essays in the rhetoric of contemporary criticism* (first edn, 1971; revised edn, London, 1983); *Allegories of reading: Figural language in Rousseau, Nietzsche, Rilke and Proust* (New Haven, 1979); and *The resistance to theory* (ed. by W. Godzich, Minneapolis, 1986). See further the penetrating discussion of Foucault in S. Burke, *The death and return of the author: Criticism and subjectivity in Barthes, Foucault and Derrida* (Edinburgh, 1992; 2nd edn, 1998).
53. Figlio, “Chlorosis and chronic disease in nineteenth-century Britain” (ref. 4).
54. *Ibid.*, 173–5.
55. *Ibid.*, 175.
56. *Ibid.*, 193.
57. See respectively pp. 178 nn. 32, 34; 181; 185; 174 n. 21; and many subsequent citations (*Black’s medical dictionary*).
58. *Ibid.*, 174–5.
59. See for instance *ibid.*, 179: “Constructing the illness ... was the other face of discovering adolescence.”
60. Notably at *ibid.*, 177–8, invoking Laslett’s argument that the age of menarche declined in the nineteenth century.
61. See for instance F. Boissier de Sauvages, *Nosologia methodica sistens morborum classes juxta Sydenhami mentem and botanicorum ordinem* (2 vols, Amsterdam, 1768), ii, 440–3.
62. F. H. Garrison and L. T. Morton, *A medical bibliography: An annotated check-list of texts illustrating the history of medicine* (4th edn, Aldershot, 1983), 418–19.
63. Rosenberg and Golden (eds), *Framing disease* (ref. 15).
64. Charles E. Rosenberg, “Framing disease: Illness, society, and history”, *ibid.*, pp. xiii–xxvi, at pp. xiv–xv.
65. Cf. above, at ref. 42. On the shifting meanings of “cholera” and on the problems attending retrospective diagnosis of “tuberculosis”, see M. Grmek, *Diseases in the ancient Greek world* (transl. by M. Muellner and L. Muellner, Baltimore, 1989), 7, 183–4. The latter work, it should be observed in passing, has an ambiguous significance in relation to my theme. On the one hand, it rests entirely upon retrospective diagnosis and pays little attention to ancient disease-concepts — thereby participating in the naturalist-realist tradition. On the other hand, Grmek stresses throughout (for instance, in the passages just cited) that modern concepts of disease are incommensurate with ancient ones — this in harmony with the historicalist-conceptualist approach.
66. See further Rosenberg, “Framing disease” (ref. 64), p. xvi, where this anti-relativist position was reiterated. Here the argument became rather more complicated, for Rosenberg rightly argued that “the process of disease definition” merited attention, thereby seemingly abandoning the framing metaphor. See also ref. 68 below.
67. For instance an essay on silicosis, bearing the promising title “The illusion of medical certainty”, nevertheless began by asserting — with no intended irony — that “Silicosis is a chronic lung disease caused by the inhalation of silica dust”. G. Markowitz and D. Rosner, “The illusion of medical certainty: Silicosis and the politics of industrial disability, 1930–1960”, in Rosenberg and Golden (eds), *Framing disease* (ref. 63), 185–205.
68. Similarly, when introducing Peitzman’s essay Rosenberg summarized its argument by saying that “the evolving framework of pathological assumptions describing and explaining ‘Bright’s disease’ has been gradually integrated and reintegrated into a series of differently focused

- explanatory frameworks for *the same clinical pictures*" (my emphasis), and remarked that "It is precisely this process of definition and redefinition that demands scholarly attention". Here the initial assumption of clinical constancy, associated again with the metaphor of framing-and-pictures, was immediately (and commendably) undermined by the notion of "definition and redefinition". See Rosenberg and Golden (eds), *Framing disease* (ref. 63), 4, and *cf.* ref. 66 above.
69. Until recently an analogous situation obtained within the sociology of medicine, the scope of which was traditionally restricted by giving a particular twist to the supposed distinction between "illness" and "disease". "Illnesses", it was said, were the subjective experiences of patients; they fell within the realm of "culture", and were therefore seen as forming a proper theme for sociological inquiry. "Diseases", in contrast, were conceived as real pathological processes, taken to coincide with their (supposed) medical definitions; they were regarded as inhabiting the realm of "nature", and therefore as lying beyond the bounds of sociological investigation. For this point, and for a cogent argument against this framework of assumptions, see P. Atkinson, *Medical talk and medical work: The liturgy of the clinic* (London, 1995), chap. 2. The burden of Atkinson's argument is that just as diseases are the constructs of medicine, so the supposed objectivity of those diseases was the construct of traditional medical sociology itself. Like Rosenberg, Atkinson has deliberately avoided the term "construction"; but significantly, he replaces it not by "framing" but instead by "production" (*ibid.*, 45); and unlike Rosenberg, he refers to Fleck (pp. 143, 147). Similar points had already been made, from a rather different perspective (concerned with the sociology of illness rather than the sociology of knowledge), by R. Dingwall, *Aspects of illness* (London, 1976), chap. 2. As for the "illness"/"disease" distinction, both words are so elastic that it can be made in many different ways, or avoided altogether, according to one's rhetorical purposes: see A. L. Caplan, H. T. Engelhardt, Jr, and J. M. McCartney (eds), *Concepts of health and illness: Interdisciplinary perspectives* (Reading, Mass., 1981), and C. Curren and M. Stacey (eds), *Concepts of health and illness and disease: A comparative perspective* (Leamington Spa, 1986).
 70. S. Shapin, "History of science and its sociological reconstructions", *History of science*, xx (1982), 157–211, p. 157.
 71. Smith, "Pleuritis" (ref. 5). I thank Professor Smith for e-mailing me a copy of this paper and of his translations from the ancient texts, and for helpfully discussing my various queries.
 72. Foucault, *Birth of the clinic* (ref. 43), 122.
 73. Smith, "Pleuritis", quoted here from typescript.
 74. Particularly the *Aphorisms* (below, at ref. 121).
 75. *Regimen in acute diseases* (below, at ref. 123).
 76. This was the thrust of the discussions of pleuritis in *Diseases I* (the work known to Galen as *Internal suppurations*): 26 and in *Places in man*: 14. See respectively the Loeb edn, *Hippocrates*, v (transl. by P. Potter, London, 1988), 98–183, pp. 166–71, and Smith, "Pleuritis" (ref. 5), Appendix. This aspect also appeared in *Affections*: 7, which however was one of the texts that did define pleuritis, as next discussed.
 77. *Affections*: 7. See Loeb edn, *Hippocrates*, v, 6–91, pp. 15–16.
 78. *Diseases II*: 44–46. See Loeb edn, *Hippocrates*, v, 191–333, pp. 262–7.
 79. *Diseases III*: 16. See Loeb edn, *Hippocrates*, vi (transl. by P. Potter, London, 1988), 6–63, pp. 38–57.
 80. *Ibid.*, 38–39; Smith's translation, in the appendix to his "Pleuritis" (ref. 5).
 81. Loeb edition, *Hippocrates*, vi, "Index of symptoms and diseases", 336.
 82. *Ibid.*, translator's note, 334.
 83. *Diseases I* and *Places in man*, as cited in ref. 76 above; *Aphorisms*, quoted below, at ref. 121.
 84. See below, at ref. 123; Smith, "Pleuritis" (ref. 5), 3–4; and D. Jacquart, "Theory, everyday practice,

- and three 15th-century physicians”, *Osiris*, 2nd ser., vi (1990), 140–60, p. 154.
85. Below, at ref. 87.
 86. Here my reading differs from that of Smith, “Pleuritis”, who characterizes *Diseases III* as discussing “a single disease with various manifestations”.
 87. *Diseases II*: 44–46 (Loeb edn, *Hippocrates*, v, 264–7); cf. above, at ref. 85.
 88. In one text only, namely *Places in man*: 14, pleuritis was given an internal anatomical location, specifically in the lung. This work was also unique in conceiving pleuritis as the one-sided version of peripneumony: “When both sides are painful and the affections of both sides are similar, that is peripneumony; the other is pleuritis” (Smith’s translation, “Pleuritis” (ref. 5), Appendix). Although many of the Hippocratic texts discussed pleuritis and peripneumony in sequence and offered overlapping therapies for them, they usually defined them in quite different ways. In particular, pleuritis was always defined with reference to pain in the side, as we have seen; but the defining symptoms of peripneumony were fever, cough and expectoration, with pain being mentioned only erratically. In short, the entire thrust of the discussion of pleuritis and peripneumony in *Places in man* was well outside the Hippocratic mainstream. (Accounts of peripneumony appeared in *Affections*: 9, *Diseases I*: 27, and *Diseases II*: 47, all in Loeb edn, *Hippocrates*, v; see pp. 17, 171, 267–9.)
 89. Traditionally, Praxagoras is regarded as the teacher of Herophilus. With respect to the localization of pleuritis, Diocles may well have preceded Erasistratus, though this is not to say that he was Erasistratus’s teacher. The possible roles of Praxagoras and (especially) Diocles in the story of pleuritis are discussed by Smith, “Pleuritis” (ref. 5); here, however, as a convenient simplification, I shall depict the anatomical tradition as stemming from Herophilus and Erasistratus, setting aside Praxagoras and Diocles. (The views of Diocles and Praxagoras on peripneumony are quoted in ref. 92 below.)
 90. Caelius Aurelianus, *Acute diseases*, II.xiii–xxiv, in I. E. Drabkin (ed. and transl.), *Caelius Aurelianus: On acute diseases and On chronic diseases* (Chicago, 1950), 181–227, chap. 16 (pp. 189–93). In fact, my claim that the dogmatics “defined” pleuritis in anatomical terms is a projection backwards from stage (iii) of ancient medicine (see below, particularly the discussion of Aretaeus at ref. 115); strictly speaking, it is possible that the dogmatics defined pleuritis in symptomatic terms and added an anatomical seat (or rather, seats). But for convenience I am eliding this particular subtlety.
 91. On peripneumony cf. ref. 88 above.
 92. “In the case of people suffering from peripneumonia, Diocles says the veins of the lung are affected, while Erasistratus says the arteries are affected, and Praxagoras the parts of the lung which are joined to the spine. But Herophilus says the whole lung is affected. If the patients [also] suffer from fever, he says, it causes pleurisy.” Caelius Aurelianus, quoted in H. von Staden, *Herophilus: The art of medicine in early Alexandria* (Cambridge, 1989), 378 (T.215). Cf. Caelius Aurelianus, *Acute diseases*, ed. by Drabkin (ref. 90), II.xxviii (p. 231).
 93. This emerges from a passage from Galen’s *De locis affectis*, as discussed by Smith in “Pleuritis” (ref. 5).
 94. Caelius Aurelianus, *Acute diseases*, ed. by Drabkin (ref. 90), II.xiv [91] and II.xvi [96–98] (pp. 185, 189–91).
 95. With an interesting qualification: that those who placed pleuritis in the membrane, or some of them, apparently believed that “some patients find it impossible to lie on the side affected, while others, on the contrary, rest more easily on that side”: *ibid.*, II.xvi [98] (p. 191). To complicate the matter, the justifying argument does not match the claim itself, as Drabkin observes in an editorial footnote. This very issue would be revived by Morgagni: see below, at ref. 176.
 96. *Ibid.*, II.xvi [96] (p. 189).
 97. *Ibid.*, II.xvi [98] (p. 191). Also of interest is the passage which records the views of Soranus

/Caelius himself, for here there emerged an additional dimension of this symptom, one which I have not encountered in any other text: “when they turn on the opposite side they experience pain and actually *feel the inflamed organs hanging and being drawn down by their own weight*”: *ibid.*, II.xiv [91] (p. 185), emphasis added.

98. Caelius Aurelianus, *Acute diseases*, ed. by Drabkin; **above, at ref. 73**.
99. Cited below, from ref. 120 onwards.
100. Aretaeus, *On the causes and symptoms of acute diseases*, in F. Adams (ed. and transl.), *The extant works of Aretaeus, the Cappadocian* (London, 1856).
101. For the original Greek and Latin words see Drabkin’s introduction (*ibid.*, pp. xi–xxvi, at p. xix).
102. In the case of cardiac disease Soranus/Caelius was anti-localist (*ibid.*, II.xxxiv, 257–61, p. 259); with respect to hydrophobia, he adopted an intermediate position (III.xiv, 371–5, pp. 374–5).
103. *Ibid.*, II.xvi [100] (p. 193).
104. It should be mentioned, however, that Soranus’s (Caelius’s) citations of Hippocrates were somewhat haphazard and erratic: see *ibid.*, Drabkin’s note 1, p. 62 and note 10, p. 353.
105. *Ibid.*, II.xix [113–24] (203–15).
106. Smith, “Pleuritis” (ref. 5), 5.
107. *Ibid.*, 6–7.
108. “*Stimulosus ac pulsuosus et igneus*”: Caelius Aurelianus, *Acute diseases*, ed. by Drabkin (ref. 90), II.xiv [91], 184.
109. ‘Nygmatoδes’ (νυγματοδης) is so translated in R. J. Durling, *A dictionary of medical terms in Galen* (Leiden, 1993).
110. The tricky issue concerns the term ‘nygmatoδes’, used by Galen to characterize the pains of pleuritis. Aretaeus also used this word in connection with pleuritis, but he did so not in his generic description of the disease but rather as a sign that it had progressed to empyema: see Aretaeus, *Acute diseases*, ed. by Adams (ref. 100), I.x, 17 (Greek), 257 (English). Thus for Aretaeus, pains of the “nygmatoδes” type were in fact characteristic not of pleuritis, but of empyema. And it would appear that Soranus had done something similar; for in Caelius Aurelianus’s Latin rendition, he wrote that the transition from pleuritis to empyema is associated with “pungent pains” (*dolore pungenti*), as distinct from the pain of pleuritis itself, which in this particular context he described not as “pungent” but merely as “acute” (*dolor acutus*): see Caelius Aurelianus, *Acute diseases*, ed. by Drabkin (ref. 90), II.xvii [101–2] (p. 194). If (as later usages indeed imply) the Latin *pungens* was equivalent to the Greek *nygmatoδes*, then the account of Soranus/Caelius differs from that of Galen much as that of Aretaeus does.
111. See above, at ref. 86.
112. On Archigenes see further below, at ref. 128.
113. This is Wesley Smith’s interpretation. The matter is complicated by the relation between the “pricking” quality of the pain and the progression of pleuritis to empyema: see ref. 110 above.
114. Immediately after announcing the conclusion that “it is the *hypezocos membrana* which is the seat of this disease”, Soranus/Caelius added: “And this membrane is the source of severe pains (*dolorem vehementem*), for it is fibrous and attached to the side.” This came *within* a passage framed as a summary of the views of those who place pleuritis in the “hypezocos membrana”, which indicates that Soranus/Caelius was here paraphrasing Erasistratus *et al.* See Caelius Aurelianus, *Acute diseases*, ed. by Drabkin (ref. 90), II.xvi [98–100] (pp. 191–3).
115. Aretaeus, *Acute diseases*, I.x, ed. by Adams (ref. 100), 16, 255–6; word-order modified slightly.

116. This with the phrase “all these symptoms”, in the third sentence.
117. Foucault, *Birth of the clinic* (ref. 43), 137; A. Cunningham, “Getting the game right: Some plain words on the identity and invention of science”, *Studies in history and philosophy of science*, xix (1988), 365–89, pp. 373–5.
118. Cf. M. Heidegger, *Being and time* (German original 1928; transl. from the 7th German edition by J. Macquarrie and E. Robinson, Oxford, 1962), section 7, p. 52, and below, at ref. 122.
119. See W. D. Smith, *The Hippocratic tradition* (Ithaca, N.Y., 1979). For a different reading see J. Longrigg, *Greek rational medicine: Philosophy and medicine from Alcmaeon to the Alexandrians* (London, 1993).
120. *Aphorisms*, V.viii (cf. also xv), in Loeb edn, *Hippocrates*, iv (transl. by W. H. S. Jones, London, 1931), 98–221, p. 159; Galen, *Commentary on Hippocrates’ Aphorisms* (17B 399 K.), as cited and translated by Smith, “Pleuritis” (ref. 5).
121. Contrast the translation in G. E. R. Lloyd (ed.), *Hippocratic writings* (Harmondsworth, 1978/1983), 222: “If sufferers from pleurisy do not cough up material within fourteen days, the inflammation produces empyema.” The “inflammation” here appears to have been interpolated by the translators (J. Chadwick and W. N. Mann). On the transition of pleuritis to empyema, compare Aretaeus’s discussion (ref. 110 above).
122. Although certain of the Hippocratic texts had speculated as to the internal events underlying pleuritis (see above, at ref. 76), they had not installed these posited internal events as definitive of the disease; rather, the disease was identified as its symptoms. Hence the fact that those symptoms were never turned into “signs”; nor indeed were they conceived as “symptoms” in our sense (cf. above, at ref. 118), for they lay on the same ontological level as the disease.
123. Hippocrates, *Regimen in acute diseases*: 16, in Loeb edn, *Hippocrates*, ii (transl. by W. H. S. Jones, London, 1923), 75; Galen, *Commentary on Regimen in acute diseases* (15.488 K.) , as cited and translated by Smith, “Pleuritis” (ref. 5).
124. Smith, “Pleuritis” (ref. 5) discusses several of these passages.
125. Galen, *De causis pulsuum*, IV.viii, in F. Blondel and A. Le Moine (eds), *Hippocratis Coi, et Claudii Galeni Pergameni archiatron opera* (13 vols in 9, Paris, 1639–89), viii, 223–5; *De praesagitatione ex pulsibus*, IV, *ibid.*, 298–9; *De pulsibus ad tyrones*, cap. xii, *ibid.*, 8–9; *Galenii Synopsis librorum suorum de pulsibus*, *ibid.*, 326.
126. So we may infer from the fact that the Soranus/Caelius passage cited earlier (above, at ref. 90) made no mention of the pulse in connection with the dispute over the localization of pleuritis.
127. See von Staden, *Herophilus* (ref. 92), 262–88. All observers credited Herophilus with the basic pulse-nomenclature, but none of them mentioned the “hard” pulse (or its opposite, the “soft” pulse) amongst the various terms he had developed.
128. It seems that the terms “hard” and “soft” pulse were introduced by Archigenes (see Galen, *De pulsuum differentiis*, III.vii, in Blondel and Le Moine (eds), *Hippocratis et Galeni opera* (ref. 125), viii, 77–79); but it is unclear whether Archigenes had applied these terms to the particular cases of pleurisy and peripneumony. On Galen’s debts to Archigenes in respect of pulse-lore, see von Staden, *Herophilus* (ref. 92), 284 n.156; on Archigenes and pleuritis see above, at ref. 112.
129. Galen, *De causis pulsuum*, IV.xii, in Blondel and Le Moine (eds), *Hippocratis et Galeni opera* (ref. 125), viii, 227; *De pulsibus ad Tyrones*, cap. xii (***ibid.*, 10**).
130. Galen, *De causis pulsuum*, IV.viii, in *ibid.*, viii, 224.
131. *Ibid.*, viii, 223.
132. J. B. Saunders and C. D. O’Malley, *Andreas Vesalius Bruxellensis, The Bloodletting Letter of 1539: An annotated translation and study of Vesalius’s scientific development* (New York, 1946), 15. See also A. Cunningham, *The anatomical renaissance: The resurrection*

of the anatomical projects of the ancients (London, 1997), 101–2, 110–11. The issue had medieval precedents: see Jacquart, “Theory, everyday practice, and three 15th-century physicians”, (ref. 84), 158.

133. Saunders and O’Malley, *The Bloodletting Letter* (ref. 132), 74.
134. *Ibid.*, 81–82; cf. ref. 136 below.
135. *Ibid.*, 70, reading “pleuritis” for “pleurisy” throughout.
136. The membrane did enter Vesalius’s discussion at a later point (*ibid.*, 81–82), but not as the seat of the disease; rather, to demonstrate that *dolor lateralis* tends most often to occur where that membrane is “less firmly attached”, that is, in the vicinity of the fifth to the eighth ribs. See further ref. 144 below.
137. Cunningham, *The anatomical renaissance* (ref. 132), 124.
138. Cf. *ibid.*, 102.
139. Cf. Saunders and O’Malley, *The Bloodletting Letter* (ref. 132), 8, and ref. 144 below.
140. This in a later text, from 1546; see ref. 144 below.
141. Immediately after the passage just quoted Vesalius remarked: “In addition the name pleurisy derived from the ribs — also called πλευραϊ, because they form the side — by no means indicates a primary position, since the ancient Latin writers called the disease *dolor lateralis*, not *dolor costalis*..” (Saunders and O’Malley, *The Bloodletting Letter* (ref. 132), 70; here I have modified the translation and punctuation for greater clarity).
142. A strong contender was Mondino dei Liuzzi: see Jacquart, “Theory, everyday practice, and three 15th-century physicians” (ref. 84), 156.
143. Cf. G. E. R. Lloyd, “The development of Greek anatomical terminology”, in his *Science, folklore and ideology: Studies in the life sciences in ancient Greece* (Cambridge, 1983), 149–67.
144. Vesalius, *De humani corporis fabrica*, VI.ii. A few years later (1546), in the course of his *Epistola rationem modumque propinandi radicis Chynae decocti* (*Letter on the China root*) Vesalius returned in passing to the topic of pleurisy, mentioning two fatal cases, each of which he had examined *post mortem*. The first case (in which the patient was said to have died of “*dolor lateralis*”) seems at first sight to embody a shift from Vesalius’s views of 1539, for this showed inflammation of the *membrana costas succingens* on the left side. But the second case (described as “*morbus lateralis*”) suggests that his conception was unchanged, for here the inflammation was not tied to that membrane, but rather “occupied the whole posterior part of the thorax” and followed the distribution of the unpaired vein. Combining the two cases, we may infer that Vesalius regarded the *succingens* membrane as just one possible seat of “*dolor lateralis*” or “*morbus lateralis*”, a view which harmonized perfectly with what he had written in 1539. Nevertheless it is worth remarking that both these post-mortems showed inflammation, whereas in 1539 Vesalius had mentioned only an “affection”; it is not clear whether this represents a change in his views or whether it was merely a matter of verbal tactics. See Vesalius, *Opera omnia anatomica et chirurgica*, ed. by H. Boerhaave and B. S. Albinus (2 vols, Leyden, 1725), ii, 664, and Morgagni, *The seats and causes of diseases* (ref. 169), 623. **vol!**
145. See G. Whitteridge (ed.), *The anatomical lectures of William Harvey* (Edinburgh, 1964), 236, 246. When William Harvey lectured on anatomy in 1616, taking Bauhin as his starting-point, he accepted this use of “pleura”. (Incidentally, Bauhin also used “pleura” in a quite different way: as a designation for fibrous bands connecting the lungs to the chest wall. Harvey contested Bauhin’s assumption that such fibrous bands were normal, but he did not dispute this further use of the term “pleura”: see *ibid.*, 275.)
146. *Ibid.*, 236; Cunningham, *The anatomical renaissance* (ref. 132), 103.
147. G. Baglivi, *The practice of physick, reduc’d to the ancient way of observations. Containing a*

- just parallel between the wisdom and experience of the Ancients, and the hypothesis's of modern physicians. Intermix'd with many practical remarks upon most distempers* (transl. anon., London, 1723), chaps. 9 and 103.
148. Cunningham, *The anatomical renaissance* (ref. 132), chap. 4.
 149. Baglivi, *The practice of physic* (ref. 147), 2, 120–1.
 150. *Ibid.*, *passim*, esp. pp. 20–21, 115–16.
 151. *Ibid.*, 60. Cf. also pp. 103, 104 (“scattered sentences”, “snug sentences”).
 152. *Ibid.*, 59.
 153. *Ibid.*, 103, 60.
 154. *Ibid.*, 105.
 155. See F. Bacon, *The advancement of learning*, I.v.5, in Bacon, *The advancement of learning and New Atlantis*, ed. by A. Johnston (Oxford, 1974), 34. On Baglivi’s admiration for Bacon see J. Martin, “Sauvages’ nosology” (cited in ref. 202 below), 115–18.
 156. Baglivi, *The practice of physic* (ref. 147), 188. Cf. also p. 22, where Baglivi offered his formal definition of the *Medicina prima*.
 157. *Ibid.*, 197.
 158. *Ibid.*, 22.
 159. *Ibid.*, 197.
 160. *Ibid.*, 60.
 161. *Ibid.*, 60–61.
 162. See above, at ref. 130.
 163. Baglivi, *The practice of physic* (ref. 147), 61–66.
 164. For instance, “A good respiration is a good omen, but a bad one is always to be dreaded.... But prognostics taken from the pulse are not so certain...”; and “Such pleuritical patients as were seized with a pain in the inner part of the ear, followed by an imposthume and pus, were all cured, pursuant to my repeated observations in the Italian Hospitals” (*ibid.*, 61). Again, “After the cessation of the pain of inflamed parts (especially in the case of a pleurisy or the inflammation of membranous parts), if the fever still continues, or increases, being attended with a low, intermitting and frequent pulse, cold sweats, etc., ’tis a fatal omen...” (p. 63).
 165. Baglivi recommended bleeding (without considering its site, and so without mentioning the controversy which had raged in Vesalius’s day) and expectoration, and argued against purgatives and diaphoretics: *ibid.*, 62, 64–65.
 166. *Ibid.*, 62, 63, 65.
 167. *Ibid.*, 22, quoted above, at ref. 158.
 168. *Ibid.*, 20–21, 114–16, 118, 126–7, 197.
 169. G. Morgagni, *De sedibus et causis morborum per anatomen indagatis* (Venice, 1761); *On the seats and causes of diseases, investigated by anatomy* (transl. by B. Alexander, 3 vols, London, 1769).
 170. Baglivi, *The practice of physic* (ref. 147), 2.
 171. See G. A. Lindeboom, *Herman Boerhaave: The man and his work* (London, 1968), 274–5, and contrast Morgagni, *On the seats and causes of diseases* (ref. 169), vol. 613.
 172. True, we have two excellent, complementary recent discussions: M. Nicolson, “Giovanni Battista Morgagni and eighteenth-century physical examination”, in C. Lawrence (ed.), *Medical theory, surgical practice* (London, 1992), 101–34, and A. Cunningham, “Pathology and the case-history in Morgagni’s ‘On the Seats and Causes of Diseases Investigated Through Anatomy’ (1761)”, *MedGG*, xi (1995), 37–61. But a great deal more remains to be learnt from the 2,242 pages of *On the seats and causes of diseases*.

173. On Morgagni see also *The clinical consultations of Giambattista Morgagni: The edition of Enrico Benassi* (1935), translated and revised by S. Jarcho, with new preface and supplements (Boston, 1984).
174. Morgagni, *On the seats and causes of diseases* (ref. 169), ii, 546–80 (Letter 20), 581–643 (Letter 21).
175. On Morgagni's logic see S. Jarcho, "Morgagni, Vicarius, and the difficulty of clinical diagnosis", in L. G. Stevenson and R. P. Multhauf (eds), *Medicine, science and culture: Historical essays in honor of Owsei Temkin* (Baltimore, 1968), 87–95; on the anatomical and case-historical dimensions of Morgagni's enterprise see Cunningham, "Pathology and the case-history" (ref. 172).
176. By explaining the anomaly Morgagni was able to advance a hypothesis as to the source of the pain-in-lying-down (*On the seats and causes of diseases*, 555), a hypothesis which fed into his larger subsequent discussion (see for instance p. 558).
177. *Ibid.*, 569–70. Here Morgagni duly noticed that the anomaly he had observed was also of ancient record, in Caelius Aurelianus's *Acute diseases* (ref. 94).
178. See above, at ref. 145.
179. Morgagni, *On the seats and causes of diseases*, 627.
180. *Ibid.*, 621–30.
181. In all probability, Morgagni had grossly exaggerated Baronius's contribution, and had suppressed the writings of many other authors, for in fact this conception went back to the sixteenth century and was well known in the early eighteenth century: see the citations under the entry for "pleuro-pneumonia" in the *Oxford English dictionary*. Nevertheless this licence on Morgagni's part was justified, for pleurisy and peripneumony were regarded as distinct diseases by Boerhaave, by van Swieten, and by all the leading nosologists, among them Sauvages, Linnaeus, Vogel and Cullen. See G. van Swieten, *The commentaries upon the aphorisms of Dr. Herman Boerhaave concerning the knowledge and cure of the several diseases incident to human bodies* (transl. anon., 18 vols, London, 1744–73), viii and ix (separate and extended treatments of peripneumony and pleurisy respectively); W. Cullen, *Synopsis nosologicae methodicae* (Edinburgh, 1769), 30, 33, 103, 105, 171, 260–1.
182. Garrison remarked: "It is said of him [Boerhaave] that he was the first to establish the site of pleurisy exclusively in the pleura...": F. H. Garrison, *An introduction to the history of medicine* (first published 1913; 4th edn, Philadelphia, 1929), 317, giving no reference. I have not identified the origin of this particular claim.
183. Duffin, *To see with a better eye* (ref. 6), 145–7, 216–17.
184. *Ibid.*, 205.
185. (1) The pain-in-the-side was described as being intensified by inspiration (J. Duffin, personal communication); this had already been identified earlier (see van Swieten, *Commentaries* (ref. 181), ix, 1). (2) The hard pulse had apparently been dropped as a diagnostic sign; I have not established when this took place. (3) Survivors of chronic pleurisy had retraction of one side of the chest; this was discovered by Laennec himself (Duffin, *To see with a better eye* (ref. 6), 161).
186. *Ibid.*, 158.
187. Maulitz, "In the clinic" (ref. 6).
188. Most of what follows rests specifically on Chomel's observations, of which Maulitz gives examples drawn from students' lecture-notes (*ibid.*, 134–5), but the link between pleuritis and phthisis was apparently characteristic of the Paris school at this time (p. 133). The quotations in the next four references are from Chomel, in *ibid.*, 134–5.
189. "The stitch in one's side [that is] worsened by inspiration and by cough is characteristic

- of pleurisy.”
190. “Phthisical patients from time to time are subject to pleuritic stitches without effusions.” See also the quotation in the next reference.
 191. “Certain cases of pleurisy are accompanied only by a pseudomembranous exudate, in which case there is neither dullness nor egophony.” Conversely, Andral had already argued in the 1820s that (as Duffin puts it) “egophony could exist under false positive circumstances, when there was no effusion at all” (Duffin, *To see with a better eye* (ref. 6), 205).
 192. See the quotation in ref. 190 above, and also: “In chronic pleurisy we have often found tubercles in the chest of those who have succumbed.”
 193. Duffin, *To see with a better eye* (ref. 6), 158–9.
 194. In the next letter of *De sedibus* (Letter 22), Morgagni discussed phthisis along with empyema; here he linked “pleuripneumony” (the concept which had now replaced both pleurisy and peripneumony) with empyema, and not with phthisis (*On the seats and causes of diseases* (ref. 169), 650, 655; cf. refs 110, 121 above). True, he suggested that some of Valsalva’s “peripneumony” cases (which he had discussed in Letter 20) actually “related rather to consumption” (p. 645); but this was as near as he came to linking pleurisy or “pleuripneumony” with consumption. It should also be observed that *De sedibus* included only a few cases of phthisis, because both Valsalva and (even more so) Morgagni were “cautious” about dissecting the bodies of consumptives, evidently through fear of contagion (pp. 645, 661).
 195. Maulitz, “In the clinic” (ref. 6), 130–1.
 196. Duffin, *To see with a better eye* (ref. 6), 247.
 197. See above, at ref. 174.
 198. Cf. L. Jordanova, “The social construction of medical knowledge”, *Social history of medicine*, viii (1995), 361–81, particularly p. 374.
 199. See for instance R. Porter (ed.), *Patients and practitioners: Lay perceptions of medicine in pre-industrial society* (Cambridge, 1985); L. McC. Beier, *Sufferers and healers: The experience of illness in the seventeenth century* (London, 1987); R. Porter and D. Porter, *In sickness and in health* (Oxford, 1989); D. Porter and R. Porter, *Patient’s progress: Doctors and doctoring in eighteenth-century England* (Oxford, 1989).
 200. For a fine illustration of this point see M. Nicolson, “The metastatic theory of pathogenesis and the professional interests of the eighteenth-century physician”, *Medical history*, xxxii (1988), 277–300.
 201. Cf. Duffin, *To see with a better eye* (ref. 6), 302–3.
 202. For two very different accounts see R. French, “Sickness and the soul: Stahl, Hoffman and Sauvages on pathology”, in A. Cunningham and R. French (eds), *The medical enlightenment of the eighteenth century* (Cambridge, 1990), 88–110, and J. Martin, “Sauvages’ nosology: Medical enlightenment in Montpellier”, *ibid.*, 111–37. For the most recent word on Sauvages, see L. Brockliss and C. Jones, *The medical world of early-modern France* (Oxford, 1997), 427–9, p. 435 and *passim*.
 203. This crucial issue has been raised by French, “Sickness and the soul” (ref. 202), who stresses the differences; but Martin, in “Sauvages’ nosology” (ref. 202), can be read as emphasizing the continuities between the two.
 204. For an overview see W. F. Bynum, “Nosology”, in Bynum and Porter (eds), *Companion encyclopedia of the history of medicine* (ref. 6), i, 335–56.
 205. Foucault, *Birth of the clinic* (ref. 43), chap. 1 and *passim*; R. Porter, “The eighteenth century” in L. I. Conrad et al., *The Western medical tradition* (Cambridge, 1995), 371–475, pp. 409–10. See also Maulitz, “In the clinic” (ref. 6), *passim*, and Duffin, *To see with a better eye* (ref. 6), 68–69, 251, who both find that nosology remained very much alive in the Paris clinic.

206. Dr Shyamsunder Rao Chepur assures me (personal communication, 1998) that there are in the Ayurvedic tradition at least two standard diagnostic categories which correspond to, or overlap with, pleurisy: (1) *Parshuka roga* (“disease of the sides”); and (2) *Utpullika* (a “respiratory disorder” associated with “painful swelling in the right hypochondrium and intercostal space”). But I have yet to discover just how those categories developed historically within that tradition.