If I had entitled this paper "Footnotes, Quotations, and Name-lists," readers would, I fear, pass rapidly to the next contribution. But since I am going to discuss footnotes, quotations, and name-lists, I owe some explanation as to why I chose so odd and apparently incoherent a set of subjects, linked only by the fact that historians — but not only historians — use them.

I have long been interested in what goes on between the time an historian says to himself: "Well, I guess I understand this matter about as well as I ever will, so I may as well start writing," and the time he lays down his pen ruefully beside a stack of scrawled pages and says: "Well, it's a damned bad job, but it's about as good as I can do, so that's that." In the intervening time the historian has been trying to find suitable ways to impart to others what he believes he understood; he has been writing history. This activity once had a name; it was called historiography. In recent years that name has been ruthlessly misappropriated, and its meaning drastically altered. Historiography has been used to designate the history of what at one time or another historians have written about the past. Thus understood, it is a special kind of intellectual history or a branch — or perhaps a twig — of the sociology of knowledge. I am simply reappropriating the term to its original use, since there is no other single term to describe the process of writing history.

On the face of it, the outcome of writing history, the rhetoric of history, is unmistakably different from the rhetoric of, say, physics or lyric poetry. Whatever an historian may profess that he is up to when he writes history, the result of his activity, an article, say, in the American Historical Review, does not look at all like an article in the Physical Review or an epic poem or a sonnet. Indeed if it does look like either a physicist's paper or a poet's sonnet, its chance of publication in the American Historical Review is fairly slim. And since the serious commitment of craftsmen is better revealed by their common activity than by their often idiosyncratic individual professions of intent, from the look of what they write it appears that historians are committed to writing in a different way, committed to a different rhetoric, from what science as exemplified by the physicists, or belles lettres as ex-
emphaced by the poets, is committed to. By carefully examining and analyzing the differences can we discover anything about the infrastructure, the bone and gristle, of the trade we historians follow? What do the manifest gross differences between historiography and the rhetoric of the natural sciences on the one hand and of belles lettres on the other tell us about the historian’s often inarticulate conception of his vocation? Are the differences casual and trivial, or do the peculiarities of its rhetoric mark history as a unique and separate domain of human knowing, in important respects incommensurate with the other two? In this paper I shall focus on the relations between historiography and the rhetoric of the natural sciences. One difference becomes manifest in the divergent attitude of the historian and, say, the physicist to the lowly item in their common repertoire — the footnote. It is so lowly, indeed, that it may seem unworthy of notice; but we must remember that the lowly and humble things of the earth may be more instructive than the great and mighty — after all, geneticists learned a good deal more about genetics by considering the fruit fly than they could have learned in an equal span of time from a contemplation of the somewhat more impressive elephant.

Suppose both physicists and historians were prohibited from using footnotes for any purpose except citation to the so-called literature of their subjects. The physicists, I suspect, would regard such a prohibition as a minor nuisance. But because it would bar them from citation to the records of the past, most historians would regard it as a major calamity. Citation to those records is the way an historian makes his professional commitment clear in action, as the report on the experiment is the way a physicist makes his commitment clear. In both instances it is a commitment to maximum verisimilitude. For the physicist it is a maximum verisimilitude to the operations of nature as glimpsed through consideration of the experimental cluster; for the historian, verisimilitude to the happenings of the past as glimpsed through consideration of the surviving record. Experiment and citation to the record, then, are activities which more significantly than any theoretical pronouncements indicate the actual common commitment of physicists and historians to exploration, understanding, and rendering the best possible account of reality: for the physicist, the reality of the operations of nature; for the historian, the reality of what happened in the past.

As noted above, men’s actual commitments are much more accurately revealed by what they do in the practice of their calling than by their quasi-philosophical excursions into methodology. The well-nigh universal use of footnotes to the records by historians indicates that no matter what form of intricate epistemological fancy work they fiddle around with in their spare time, when they actually get down to writing history they all still commit themselves to trying to write about the past, as Ranke put it so
very long ago, "Wie es eigentlich gewesen," as it really happened. Today we might put that old and much derided aphorism in a somewhat more sophisticated language. We might say that historians are concerned and committed to tell about the past the best and most likely story that can be sustained by the relevant extrinsic evidence. Still we would only be saying what Ranke intended in a form hopefully more satisfactory to a generation acutely conscious of linguistic niceties. Abandoning such niceties in the interest of brevity, let us call this statement about the historian's commitment the "Reality" Rule.

Historians employ the footnote for a host of residual matters other than citations to the record—lists of names, minor qualifications of assertions made in the text, polemical criticisms of other historians, short statistical tables, suggestions for future historical investigation. And these are but a beginning of the tasks to which historians have turned that versatile tool of their trade; even if one allowed them the footnote for citation to the records, they would be loath to forego its use for these many other jobs. And this confronts us with two questions: 1) amid the apparent chaos of "residual" footnotes can we find any rule at all regulating their use? 2) what is the relation of any rule we find to the first rule that emerged from our examination of the peculiarities of the footnote as a device of historiographic rhetoric—the "Reality" Rule?

As to the first question, the application of any rule about footnotes requires an act of judgment in each case, and among historians judgment about the uses of residual footnotes differs. It might seem that in matters of judgment, as in those of taste, there is no disputing. But is this so? Let us consider an example:

At Shilbottle, in the case of three separate parcels of meadow, 31, 20 and 14 acres respectively, the first rendered 42s. in 1415-16 and 30s. in 1435-6, the second 28s. in 1420-1 and 23s. in 1435-6, and the third 24s. in 1422-3 and 14s. in 1435-6. At Guyzance 6½ husbandlands each rendered 13s. 4d. in 1406-7, but 10s. in 1435-6. At Chatton and Rennington, on the other hand, the situation was more stable. At Rennington the clear revenues were £17. 8s. 3d. in 1435-6 and £17 in 1471-2 and at Chatton £40. 18s. 7d. in 1434-5 and £36. 18s. 7d. in 1472-3. At Chatton the decline was due to a fall in the value of the farm of the park, from £6. 13s. 4d. to £2. 13s. 4d.

This dashing passage is imbedded in the text of a study of the wealth of a magnate family in the fifteenth and early sixteenth centuries and of the effect on it of concurrent changes in the economy, the military apparatus, and the political situation in England. Can it be suggested that the young man who inserted it in the text instead of quarantining it in a footnote did not commit an error of judgment? But to say he did commit one is to imply a rule from which his erroneous judgment made him deviate. Can such a
rule—a “law” of historical rhetoric or historiography, if you will—to cover this case be stated? I think so. As a rough approximation, the rule might go: “Place in footnotes evidence and information which, if inserted in the text, diminishes the impact on the reader of what you, as an historian, aim to convey to him.”

So although in the matter of the use of residual footnotes judgment is inescapable, we are not at all confronted with chaos or anarchy, but with a reasonably precise rule or law. We may name it the Maximum Impact Rule. Inevitably there are marginal situations on which there will be divergent views among historians as to how to achieve maximum impact or whether a particular rhetorical presentation has in fact achieved it. The existence of such marginal situations, however, does not mean that all situations are marginal, and that therefore there is no rule, or that any rule is as good as any other. Lawyers have a saying that hard cases make bad law, but they do not feel impelled thereupon to take a deep dive into a non sequitur and argue that there are no easy cases and no good law. Because there are some matters both substantive and procedural concerning which they are very uncertain, historians somehow have permitted themselves to be nudged into accepting the notion that everything about the past and about writing about it is infected with a total uncertainty. This is not so. Specifically, as we have just seen, it is not so in the case of the residual footnote. There without difficulty we found a rule not heavily infected with uncertainty.

But this turns our attention to the second question we foresaw earlier, that of the relation of the two rules—the Reality Rule and the Maximum Impact Rule—to each other. Note, first, that in our example the data that ought to be withdrawn from the text and consigned to a residual footnote, are informative and relevant with respect to the substantive historical argument the historian in the case is presenting, and that they are as complete, as accurate, and as exact as possible. Note, second, that what the historian, applying the second rule, is committed to seek to convey to the reader with maximum impact is his conception and understanding of the past as it actually happened, the “Reality” of the first rule. And thereupon we run head on into a paradox, for the clear implication of the two points we have just made is that in the interest of conveying historical reality to the reader with maximum impact, the rules of historiography may sometimes require an historian to subordinate completeness and exactness to other considerations. If this is so, it indeed differentiates historiography from the rhetoric of the sciences as currently conceived. A look at our next rhetorical device of historiography may help not to resolve our paradox but perhaps to transcend it. The device is the quotation in the text. Let us again note a difference between the historians and the physicists. Suppose the editor were to issue an edict that in the text of the Physical Review neither quotation
marks nor their equivalents would henceforth be permitted. Contributors would probably be annoyed, but with respect to the advancement of knowledge of the natural world they would not feel that much was at stake. Suppose the editor of *American Historical Review* were to issue such an edict. At the very least he would promptly be fired. A luxury for physicists, quotation is a necessity for historians, indispensable to historiography.

The kind of quotation that historians deem indispensable is quotation from the record. And again we may ask two questions: 1) is there any rule governing quotation from the record? 2) how does the rule relate to the Reality Rule?

Let us start with a purely imaginary case of inept quotation. Suppose in writing the history of the Civil Rights Act of 1964, a historian were to quote verbatim from the *Congressional Record* the entire debate on the Act in both the House and the Senate. The result would be undeniably relevant and exact — and the historian who perpetrated it would find his sanity under grave suspicion. Again our paradox: maximum completeness and exactness are not always essential, and they are not even always desirable in the historian's work of trying to tell the reader what really happened. Perhaps we can escape the paradox by way of an adept quotation. It comes from the late Professor Harbison's *The Christian Scholar in the Age of the Reformation*. He says:

Erasmus had absorbed [Lorenzo] Valla's historical perspective, his sense of the historical discontinuity between pagan antiquity and the Christian era . . . a sensitivity to anachronism. On one occasion he ridiculed the absurdity of the practice . . . of using Ciceronian words to describe an utterly different modern world: "Wherever I turn my eyes I see all things changed, I stand before another stage and I behold a different play, nay, even a different world." The world of Cicero (or of Paul) can be understood and even in a sense relived — but only if we recognize that it had its unique existence, once, in a past now dead.

What is the function of Harbison's brief but apt quotation from Erasmus? Not mere validation or proof of his assertions; he could as well have effected that by citation or quotation in a footnote. By using Erasmus' own words in the text, he seeks and wins a response not merely of assent but of conviction, not just a "yes," but "yes, indeed!" Nothing Harbison could have said about Erasmus' sense of history could produce the conviction about it that Erasmus' own assertion of his intense feeling of distance from antiquity produces.

The quotation aims at something in addition to conviction, however. The quotation communicates the historian's own view of what happened in the past by the particular means of confrontation. It says in effect, "In my judgment the most economical way at this point to tell you what I believe Erasmus meant and to convince you that he meant it is to confront you
directly with what Erasmus said.” This provides us with a third general rule of historiography — an Economy-of-Quotation Rule: “Quote from the record of the past only when and to the extent that confrontation with that record is the best way to help the reader to an understanding of the past wie es eigentlich gewesen.”

We saw, however, in the instance of our hypothetical case of the Congressional Record that mere confrontation with the record of the past is not necessarily the best way to achieve this understanding or even to achieve confrontation. Indeed, far from being a clear glass window through which the reader may capture an image of the past, quotations from the record injudiciously used can be a thick opaque wall that cuts him off from it. Once we recognize that confrontation is one of the means by which the historian seeks to convey to the reader an understanding of what actually happened, we may be on our way to transcending the paradox which up to now has perplexed us. For it opens up the possibility that the microscopic means of historiography have to be adapted to its macroscopic ends and that it is part of the task of the writer of history to mediate understanding and confrontation by devices of the rhetoric of history less direct but more compelling, and more to the purpose than any simple maximizing of completeness and exactness. And this brings us to the word list. It is a device of both the rhetoric of history and the rhetoric of the sciences, and neither would willingly forego it. Consider the following:

An inert element will not react or enter into chemical combination with any other element. In order of increasing atomic weight the inert elements are helium (4), neon (20), argon (39), krypton (84), xenon (131), and radon (222).

These two sentences exemplify the rhetoric of the scientist. In intent the words composing them are totally denotative. They cast no shadow; they evoke nothing; and their arrangement is wholly dictated by the mandates of rational order and utility. The list of the names of the elements is intended to suggest nothing more than it overtly states, that by agreed convention among chemists the word helium designates the inert element whose atomic weight is 4, neon the one whose atomic weight is 20, and so on. The arrangement of the name cluster is dictated by the simple quantitative principle of increasing weight. What the scientist wants and gets from the list is a method of labelling. He wants each entity and process he has to deal with labelled in such a way that the label denotes unambiguously and unequivocally that entity or process only. Although language can be evocative, suggestive, packed with overtones, massively and unpredictably connotative, such language is barred from the rhetoric of the sciences. For the scientist’s purposes when he is communicating what he knows, words should be as
free of connotative contamination, as sterile, as the apparatus in an operating room.

In contrast consider an example of a historiographic list to illustrate the rhetoric of history.

A vast stratum of events, the Christian Revival, lies on both sides of that conspicuous historical watershed, the year 1517. We offer the name Christian Revival for this historical structure because hitherto historians have had no single covering phrase to describe this intensification of religious sentiment and concern that began long before 1517 and extended long beyond, that in its full span had room for Luther and Loyola, the Reformed Churches and the Jesuits, John of Leiden and Paul IV; Thomas Cranmer and Edmund Campion and Michael Servetus.

The names in the paragraph above constitute a historiographic list, intended to serve the particular purposes of the rhetoric of history. It emits a signal, and what the signal says to all who hear it is: “Draw on the reservoir of your knowledge of the times in which these men named lived to give meaning to this list.” If that reservoir is altogether empty, then inevitably the list will itself be historiographically empty, meaningless, a mere collection of sounds, just as the sentences about the inert gases must have been empty of meaning to anyone who had no notion of what a chemical element or a chemical reaction or atomic weight were. The reason for this similarity is that in the present case both the historiographic rhetoric and the scientific rhetoric presuppose that the reader already possesses a body of precise and exact knowledge of the particular universes to which they refer. The scientific and the historiographical statements conform to the Reality Rule; they are meaningless unless there are such elements as helium and neon, and unless there were such men as Loyola, Cranmer, and Paul IV. Yet the historiographical list serves a rhetorical function quite different from that served by the scientific list. First, consider the order of the two lists. Given the common trait of inertness the order of the scientific list indicates the scientists’ normal preoccupation with establishing scalable differences of homogeneous traits — in this case, weight. In the historiographical list, on the other hand, no such preoccupation is discernible, yet the arrangement of the names lies at the very heart of the matter.

Note that there are three alternative ways of writing the historiographical list, all of which maintain the essential arrangement, to convey whatever information it contains.

1) Luther and Loyola, the Reformed Churches and the Jesuits, John of Leiden and Paul IV, Thomas Cranmer and Edmund Campion and Michael Servetus.

2) The first great figure of the Reformation and the first great figure of the Counter Reformation; the cutting edge of the Protestant attack and the cutting edge of the Catholic counter attack; the most fanatical prophet of the radical Reformation and the most fanatical Pope of the era of religious strife; the Protestant
martyr burned by the Catholics, the Catholic martyr beheaded by the Protestants, and the martyr who escaped burning by the Catholics only to be burned by the Protestants.

3) Luther, the first great figure of the Reformation, and Loyola, the first great figure of the Counter Reformation; the Reformed Churches, the cutting edge of the Protestant attack, and the Jesuits, the cutting edge of the Catholic counter attack — and so on through the list.

The persons balanced in tension with one another are the same for each version, and the arraying is identical in all three. On mathematical principles a member of any of the lists should be freely substitutable for the corresponding member of either of the other two, but in writing history this is not so. Each list must retain its integrity. On what grounds can an historian choose among the three? One might argue that the second list is preferable to the first since it explicates the rationale upon which the persons in the first list were arrayed; and that, in point of information about the past, the third is best of all, since it both names the persons and explicates the rationale of their array. Then why in the world would a reasonably experienced historian committed to communicate what he understands about the past choose the first option — the bare list of names with no indication as to his grounds for choosing them, or for ordering them as he did? Remember what we said earlier about the signal emitted by the list: “Draw on the reservoir of your knowledge of the times in which these men lived to give meaning to the list.” The writer assumed that most of his readers could and would in fact draw from their particular reservoirs the items of general information in the second and the third list. The effect of giving that information in greater detail, however, is to send another kind of rhetorical signal, a stop signal: “Stop drawing on the reservoir of your knowledge. I have already told you how I want you to think about these men.” And this stop signal is just what the writer did not want the list to emit. The third version of the list is more exact, more overtly informative, than the bare names in the first list and just for that reason it is more empty, less ample. It dams up the informed reader’s imagination instead of letting it flow freely, bringing with it the mass of connotation and association that those names have for him. Therefore to prevent such a blockage the writer chose the first list. In doing so, he made a judgment. He judged — or gambled — that the connotative evocative list would communicate a fuller meaning than the exact one, that it would more effectively confront the reader with the reality of the Christian Revival, and that therefore it was the more appropriate device for advancing the reader’s understanding of it. Whether he was correct in his judgment is immaterial. But in setting forth his findings, a scientist never needs to make such a judgment at all. Scientific rhetoric is purposefully constructed to free him of that need by barring connotative terms and evocative devices. To a scientist the idea
that he had to choose between a rhetoric of clarity and precision on the one hand and one of evocative force on the other would be shocking. The idea that the writer of history has to select between mutually exclusive ways of setting forth the same data and that the knowledge of history that he conveys in some measure depends on his judgment in selecting among alternative rhetorical devices is perhaps as disturbing and perplexing. But to the latter conclusion our investigation of footnotes, quotations, and word-lists has driven us. What is the yield of our examination of these minor devices of historical rhetoric—the footnote, the quotation, the name list?

First, that historiography is a rule-bound discipline by means of which historians seek to communicate their knowledge of the past.

Second, that the relation of writing history, of its rhetoric, to history itself is quite other than it has traditionally been conceived. That rhetoric is ordinarily deemed icing on the cake of history; but our recent investigation indicates that it is mixed right into the batter. It affects not merely the outward appearance of history, its delight and seemliness, but its inward character, its essential function—its capacity to convey knowledge of the past as it actually was. And if this is indeed the case, historians need to subject historiography, the way they write history, to an investigation far broader and far more intense than any that they have hitherto conducted.

Third, that there is an irreducible divergence between the rhetoric of history and the rhetoric of science, that the vocabulary and syntax that constitute the appropriate response of the historian to his data are neither identical with nor identifiable with the vocabulary and syntax that constitute the appropriate response of the scientist to his data. But the historian's goal in his response to the data is to render the best account he can of the past as it really was. Therefore by his resort to the rhetoric of history, regardless of its divergence from that of the sciences, the historian affirms in practice and action his belief that it is more adequate than the latter as a vehicle to convey the kind of knowledge, understanding, truth, and meaning that history achieves. Indeed, we discovered instances in which, in order to transmit an increment of knowledge and meaning, the very rules of historiography demanded a rhetoric which sacrificed generality, precision, control, and exactness to evocative force and scope—a choice entirely out of bounds according to the rules of scientific statement. And this implies that in the rhetoric of history itself there are embedded assumptions about the nature of knowing, understanding, meaning, and truth and about the means of augmenting them that are not completely congruent with the corresponding assumptions in the sciences, at least insofar as the philosophy of science has succeeded in identifying them.

It is this situation that underlies a curious phenomenon of the past quarter century in the development of philosophy. During that period analytical
philosophers have turned the full power of their formidable apparatus on the investigation of history. The subject seems to exercise a fateful fascination for them. I say fateful, because after twenty-five years of that precise, accurate, and logical jabbing at which the analysts are most expert, the yield has been exhaustion, perhaps of the jabbers and certainly of at least one of the interested spectators (myself), but no perceptible modification of the object of their attentions, the writing of history, which seems to absorb the onslaught and resume its initial non-scientific shape rather like a foam-rubber pillow.

Our inquiry into the operation of three standard devices of historiography may help us to identify the cause of this philosophical furor. History has confronted the analysts with a very difficult puzzle. Most historians in theory, all in practice, treat their subject as if through their current methods and their current rhetoric they were achieving and transmitting increments of knowledge about it. That is to say they allege that when a piece of historical work is well done and properly set down, readers will know more about the past after they have read it than before. And yet historiography—the forms of statement historians adopt, their rhetoric—does not seem to fit into the sign structure suitable for scientific explanation, the classical rhetoric for communicating increments of knowledge. It is with this ostensible paradox that the analytic philosophers have tried to deal and failed to deal. This is not the place to examine the various attempts that the philosophers have made to resolve the paradox. As is well known, they have ranged from something very close to an assertion that historians practically never explain anything at all to a whole battery of modifications and remodellings of the theory of scientific explanation. The intent of the modifications is to bring the theory to terms with history by incorporating into it some of the things historians do in offering an account of the past.

It is not possible, I think, either here or anywhere else, to clear up the muddle that the analysts are in about history; but it may be possible to identify it. It seems to correspond rather closely with the sort of confusion that, according to Professor Thomas Kuhn, precedes a scientific revolution. It is marked by the same sort of distress signals, the same sort of effort to succor a sick theory by saving appearances, that preceded the collapse of the Ptolemaic system in the Copernican Revolution, the collapse of phlogiston theory in the Chemical Revolution. The ultimate outcome of such revolutions, according to Kuhn, is a paradigm shift, which, if I understand it, is not the discovery of a satisfactory way of solving nagging old problems but rather a drastic recasting of assumptions which makes the old problems seem irrelevant and uninteresting while—and in a measure by—creating a whole series of new ones.

Perhaps what we have learned about the rhetoric of history suggests the possible character of an imminent paradigm shift. It might start by asking
why anyone should take seriously the attempt to confine the activity of historians to offering explanations structured according to the pattern of the sciences, to subject historiography solely to the standard of knowing, understanding, meaning, and truth acceptable for — presumably because efficacious in — the pursuit of those ends in the sciences. The attempt has never gotten further anyhow than hacking bits and pieces out of the actual flow of historical discourse and arguing about the extent to which those bits can or cannot be made to fit the scientific model or alternately how the scientific model can be modified and redefined to encompass the selected bits. The trouble is that those bits are just that — mere disjointed fragments of the flow of historical discourse. The success or failure of the attempt to incorporate them into the scientific model therefore remains a matter of only trivial concern, since, regardless of its outcome, it leaves untouched the larger part of historical discourse, of the rhetoric of history, and of the activities of the historians who produce it.

Why should we not then simply regard those activities and the flow of historical discourse, the historiography, in which they take form, as by and large an acceptable response to the demand for an account of what went on in the past? At first sight this seems like a modest enough proposal. Indeed it is no more than we do in actual fact. Few men after all can seriously doubt that in skillful hands historical investigations which are formulated and communicated in the sometimes unexact and evocative rhetoric of history provide viable explanations of the past, increase our knowledge of it, enhance our understanding of it. On the face of it such a suggestion seems less outrageous than its contrary. Yet the suggestion is radical; it involves a drastic paradigm shift, and we have seen why. It assumes that in their writing at its best historians make an appropriate response to the demand for knowledge, understanding, and truth about what happened in the past. But according to our science-oriented notions they make their response in a rhetoric that is inappropriate to the attainment of knowledge, understanding, and truth. And so we have a choice. We can say that by and large history is bunk; but I really do not think that this will wash. Or we can say that our science-oriented notions of meaning, knowing, understanding, and truth and especially of the lines of access to them need a thorough overhauling. This would indeed be a revolution, and one in which I think I would enjoy taking a place at the barricades.

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