

7 Research articles in English

I have divided this chapter into several sections. It opens with a necessarily brief and episodic account of the history of the research article over the last 300 years or so. The purpose of this section is to place the main synchronic analysis against a diachronic framework, for, like all living genres, the RA is continually evolving, and future developments may find part of their explanations in present or previous rhetorical practice. The second section examines and tries to inter-relate several case studies of the processes whereby research articles were actually constructed. Both the first and the second sections are therefore evolutionary, although in very different ways. Sections 7.3 to 7.6 constitute a textual analysis of the genre itself, taking each of the main sections of the RA (Introduction, Method, etc.) in turn, while 7.7 attempts to summarize our current state of knowledge. Despite considerable research activity, conclusions must at present be considered tentative, partly because the enormous size of the genre means that the number of texts examined represents extremely minute proportions of the whole, and partly because we still experience considerable difficulty in making well-validated decisions about how that whole should be divided up.

7.1 Episodes in the history of the research article

The scientific RA emerged, albeit in embryonic form, contemporaneously with the establishment of the first scientific periodical, *The Philosophical Transactions of the Royal Society*, in 1665. According to Ard (1983), the genre of the scientific article developed from the informative letters that scientists had always written to each other – and still do. Thus, many of the early contributions to the *Transactions* took the first person descriptive narrative form associated with letters, some even having the salutation ‘Sir’ at their outset. However, as the *Transactions* and subsequent journals began to assume a role of providing a regular arena for discussion, the new and recurring rhetorical situation that emerged led to the creation of a new genre increasingly distinct from its letter-writing origin. In Bazerman’s words:

By talking to each other in a specific format scientists were figuring out how to talk to each other and changed the format according to what they were figuring out.

(Bazerman, 1983:1)

Another powerful force that shaped the early scientific article came from the existing tradition of published scientific treatises; most immediately, from the efforts of Robert Boyle and his fellow experimentalists in the decade preceding the appearance of the first issue of the *Transactions* to establish a proper foundation for scientific knowledge (Shapin, 1984). According to Shapin, Boyle and his colleagues sought to transform claims and speculations into generally-accepted knowledge by way of the experimental *matter of fact*. In order to achieve this transformation, Boyle would appear to have developed a largely self-conscious and highly complex set of strategies. Some of these strategic elements are as follows:

- a) The key apparatus for his pneumatic experiments was the air pump. At that time air pumps were very expensive, elaborate and temperamental; they were thus rare and well beyond the means of the great majority of potential users. Boyle presented his machine to the Royal Society to ease the problem of access and to pre-empt objections that might be based on traditional opposition to alchemical secrecy or to aristocratic aloofness. (Boyle was a son of the Earl of Cork.)
- b) In Boyle's program of work the capacity of experiments to yield matters of fact depended less on getting the apparatus to do certain things than on securing the agreement of the relevant community that these things had, in fact, been done. He needed witnesses, the more the better and the better qualified the better. Experiments were performed before an audience at the Royal Society and members were encouraged to sign a register as witness that they had seen what they had seen.
- c) Boyle also recognized that witnesses could be multiplied by encouraging others to replicate experiments. Although he strongly advocated this practice, he came soon enough to realize that many attempts at replication would fail.
- d) According to Shapin, Boyle's most important way of trying to establish facts was by what Shapin calls *virtual witnessing*: 'the technology of virtual witnessing involves the production in a reader's mind of such an image of an experimental scene as obviates the necessity for either its direct witness or its replication' (1984:491). Boyle set out to achieve this objective by a variety of methods:
 - i) If there were to be illustrations of apparatus in his published work, Boyle was insistent that these should be realistic, exact and detailed.

- ii) He wrote deliberately elaborate and prolix accounts of his experiments so that the reader would be encouraged to believe that he was getting a full and honest account.
 - iii) He offered his readers circumstantial accounts of *failed* experiments.
 - iv) He deliberately avoided philosophical speculation.
 - v) Boyle wrote very cautiously and made much use of what today have become known as 'hedges' (e.g. Lakoff, 1972). As Boyle himself put it, 'in almost every one of the following essays I ... speak so doubtingly, and use so often *perhaps, it seems, it is not improbable* and other such expressions, as argue a diffidence to the truth of the opinions I incline to ...' (quoted by Shapin, 1984:495).
- e) A further important aspect of Boyle's contribution to the rhetoric of science was his attempts to regulate scientific disputes; in particular he insisted that disputes should be about findings and not about persons. In this way he stood out against the common *ad hominem* style of arguing at that time. As he elegantly puts it, 'I love to speak of persons with civility, though of things with freedom' (Shapin, 1984:502).

Of course it is sometimes thought that the facts 'speak for themselves'; that is, a scientist's description of natural reality, if it is carefully and competently done, is simply a reflection of that reality. However, if this were to be the case, then Boyle's complex strategy would have been unnecessary. Rather, even the foregoing short summary of Shapin's analysis seems to show clearly enough how hard Boyle and his collaborators had to work to *make* a rhetoric – to *develop* a convincing style for the research report. It would appear that phenomena only acquire fact-like status by consensus and that consensus may not be achievable without rhetorical persuasion. The art of the matter, as far as the creation of facts is concerned, lies in deceiving the reader into thinking that there is no rhetoric, that research reporting is indeed 'writing degree zero' (Barthes, 1975) and that the facts are indeed speaking for themselves.

Boyle was a great researcher, and at least a small part of his greatness lay in the subtle and imaginative perceptions he developed about the relationships between doing research and writing about that research. We can see what happened to the research article on a more mundane level by taking up Bazerman's study of developments in the *Transactions* during the period 1665–1800, which focuses particularly on changes in the presentation of the experiment. This study is neither simply a piece of literary nor a piece of scientific history. As the author says:

I hope to show that the story of the experimental report is not one of simple accretion, but an evolving response to an evolving

discourse situation. As literature and knowledge represented by the literature accumulated and locuses of discussion emerged, the discussion itself changed in character.

(Bazerman, 1983:4)

Bazerman's first finding was that relatively few early items in the *Transactions* were actually experimental reports, and even by 1800 such reports still amounted to less than 40% of all the articles published. In the early days, the majority of the items were reports of noteworthy natural events such as earthquakes, or of observations made by means of telescopes or microscopes or by anatomical dissection. Further, Bazerman is able to show that 'the definition of experiment moves from any made or done thing, to an intentional investigation, to a test of theory, to finally a proof of or evidence for a claim' (1983:5). In this process of evolution, the scientist's relationship with nature gradually changed from a view that the nature of things would be easily revealed by direct or manipulated observation to a view that nature was complex, obscure and difficult to get at. Inevitably enough, this changing view also meant that more care began to be taken in describing how experiments were done, in explaining why particular methods were chosen, and in detailing precisely what results were found. All this was necessary because it was becoming more and more clear that minor differences in procedure could produce major differences in findings. The supposedly modern concept of controlling the variable is not far away.

By the end of the eighteenth century, these developments (and others such as the demise of communal witnessing) had led to a reconfiguration of the RA:

As phenomena began to be treated as more problematic, articles began to take on a different organization, opening with an introduction to the problematic phenomenon, often substantiated with the story of an experiment that did not go as expected. With the problem established, the article would chronologically describe a series of experiments aimed at getting to the bottom of the mystery. Transitions between each two experiments would draw conclusions from the previous experiment and point to the rationale or need for the subsequent one. In the highly developed continuity we see the experimenter gradually come to an adequate understanding of the phenomenon, which would then be pulled together in a concluding synthesis or explanation of the phenomenon, as in Hewson's investigations into the nature of blood (60:368–83).

(Bazerman, 1983:16–17)

This description of a typical RA offers, I would suggest, a striking vindication of Boyle's attempts 100 years earlier to establish a rhetoric

for experimental description. There are, of course, certain quite marked differences between the eighteenth and the twentieth century RA; among them a decidedly more casual approach to the previous literature and some continuation of the epistolary convention of first person narrative. This kind of writing has almost totally disappeared from the contemporary RA, although it exists in other scientific genres such as Nobel Prize acceptance speeches (Ard, 1983). According to Ard, the use of 'I' in early scientific discourse is related to the fact that the observer played a more central role at that time, at least partly because observations, especially with untried apparatus, relied much on individual skill. (And we could well remember that early instruments were handmade and were far from being the standardized products in use today.) However, as we have seen, a further – if connected – reason for the continuing reliance on first person pronouns derives from the insistence of Boyle and other pioneer experimentalists for a style of writing that would project both personal honesty and modesty. This style should avoid the presumptions which the passive voice would bring that others either could or (worse) could not replicate the methods and results with ease. However, the issue of impersonality in scientific writing, despite its apparent innocent simplicity, turns out to be complex and vexatious, and is yet to be fully understood.

The only substantial study known to me that traces the *textual* development of the RA in the present century is Bazerman (1984a). Bazerman investigated a selection of Spectroscopic articles in the *Physical Review* from its founding in 1893 to 1980. As he says, 'this period marks the rise of American physics from backwardness to world dominance, reflected by the journal's rise from a local university organ to the primary international journal of physics' (1984a:166). Some of his main conclusions are summarized below:

a) ARTICLE LENGTH

From 1893 to 1900 the average length of articles fell from around 7,000 words to around 5,000. With some fluctuations they continued to average about 5,000 words until 1940. Thereafter average article length has steadily increased and reached about 10,000 words by 1980. So much then for the common belief that scientific articles have become more compact during this century.

b) REFERENCES

Referencing trends over the 1893–1980 period reveal an intriguing story. In the early years references were quite common (about 10 per article) but rather general, rarely relating to specific findings or to the specific topics investigated by the authors. As Bazerman says, these characteristics 'weaken the sense of a coherent, moving research front' (1984a:173). By

1910, the number of references had become severely curtailed, but the very few that remained were all recent, had dates and were of direct relevance to the research being reported. From then on, the number of references has trended upwards, whilst maintaining specific relevance to the work at hand. Thus, new work becomes increasingly embedded in the spectroscopic literature. A further sign of this is that references are no longer concentrated in the Introduction but are distributed throughout the RA, so that every stage of the document both relies on and relates to the work of others. And it is this development that provides at least part of the explanation for the doubling of average article length over the last 40 years.

c) SYNTACTIC AND LEXICAL FEATURES

Bazerman found no important variations in sentence length. The averages of sentence length of around 25 words accord closely with those of other investigators (Barber, 1962; Huddleston, 1971). He did find, however, that relative clauses declined in frequency, whilst both noun clauses and temporal and causal subordinate clauses have become more frequent. The rise of the latter (also corroborated by Huddleston, 1971) indicates a shift from description to explanation, thus suggesting increasing intellectual complexity. On the lexical level, subjects of main clauses have over the period become more abstract. Concrete subjects like *substance*, *apparatus*, and so on, have tended to give way to nouns of process or quality such as *ionization* and *correlation*. Significant changes in the function of the main verb were also found:

The decrease in reporting verbs (for example, 'Smith reports ...') and increase in active verbs (for example, 'temperature increases ...') suggest that the finding or theory has increasingly been brought into the central grammatical position, whilst the publishing scientists have been given a back seat, thus adding density to the discussion and integrating source material into the continuity of the argument.

(Bazerman, 1984:177)

d) NON-VERBAL MATERIAL

The same trends towards abstraction and integration can be seen in the changing nature of the graphic material. During the period, there was a decrease in the number of apparatus drawings and in the number and size of tables. In compensation, there were increases in the number and complexity of both graphs and equations.

e) ORGANIZATION

Before 1950 only about 50% of the articles were formally divided into section titles; after 1950 section headings became a regular feature. Up

till 1930, if sections were used they usually ended with Results thus implying that the findings could stand alone without further comment. Since then, Discussion and Conclusion sections have not only become much more common but they also have greatly increased in length and complexity. On the other hand, the proportion of space given to Method and Apparatus sections has generally declined (c.f. Huckin, 1987).

In this pioneering study, Bazerman assembles considerable discoursal evidence for a number of general trends: growing abstraction, the deepening integration of present work within the relevant literature, the increasing foregrounding of research as opposed to researcher, the increasingly uphill struggle to incorporate more and more information, and a steadily more focused argumentation. The finer rhetorical and linguistic detail of these outcomes will be discussed in later subsections.

The fact that Bazerman's investigation is pioneering means that we are faced with very real problems of extrapolation and generalization. Is the *Physical Review* typical of other important American physics journals? Would the findings apply to comparable journals from elsewhere? What about chemistry, psychology or sociology? Physics is a large, central and long-established field – would similarities be found in a field that had none of these characteristics?

In an attempt to throw a little light on at least the last of these questions, I analyzed main articles in the first 20 years of the *TESOL Quarterly*, the flagship publication of the US-based association of Teachers of English to Speakers of Other Languages (Swales, 1988b). From the early 1970s, average article length – of the main text – has remained relatively stable, centering around a mean of very approximately 5,000 words. However, the articles look longer because of steady upward trends in the amount of non-textual material (principally tables) and in the number of references. The average number of references had grown from four in 1968 to 34 in 1986. In addition, the period was also characterized by a proportional decline in citations of books (particularly ESL textbooks) and a rise in citations of shorter works (particularly articles, and chapters in scholarly edited collections). Further signs of the adoption of a social science (as opposed to humanities) paradigm have been the consistent subsectioning of articles, increasing co-authorship and a wider employment of statistics. In addition, the fact that authors in *TESOL Quarterly* have increasingly tended to cite previous work published in it may be taken to imply the existence of a number of coherent and established research fronts. On the other hand, there was no sign of the drift towards graphs that Bazerman noted from the *Physical Review*, and I eventually abandoned my efforts to trace increasing lexical abstraction due to a lack of firm evidence. (Syntactic features were not investigated.)

Overall, this vignette of RA history in the embryonic ESL field evinces a determination to professionalize – a particularly pressing and under-

standable concern given the folkloristic belief that anybody who knows a language well can teach it. The clearest rhetorical evidence of this lies in an increasingly standardized main article product which meets a number of requirements, such as the formulation of research questions sited within a rhetorically-established framework of previous work, and the presentation and discussion of data. One consequence of all this can, especially in the light of the comments made in Chapter 6, be seen as unfortunate. Although the teaching of English to speakers of other languages has become a major global activity, the leading journal in the field has remained one which publishes contributions from authors based in North America – even by 1986, articles from outside the US and Canada constituted less than 20% of the total.

7.2 The constructing of research articles

The continuum from a gleam in the researcher's eye to the distribution of the published paper may not be easily breakable into segments, but one possible staging is into the processes of writing prior to submission to a journal (internally-moderated changes) and into those that may occur subsequently (externally-moderated changes). Myers' (1985a) study – already mentioned at the opening of Part III – of the struggles of two biologists to get their papers published would fall into the latter category. For Myers all researchers are faced with decisions about the level of claim they might wish to make. The higher the level of claim, the more likely that it will involve contradicting large bodies of the relevant literature and will challenge assumptions embodied in important ongoing research programs. On the other hand, the lowest level claims may contradict nothing, but may also add very little to what is accepted and established within the given research field. Thus, high-level claims are likely to be important but risky, whilst low-level claims are likely to be trivial but safe. Both of Myers' biologists consistently sought to make the highest-level claim that they could persuade a particular journal to accept, but in both cases they eventually had to settle for the publication of much more limited and lower-level claims than they had originally hoped for (and perhaps still hope for). Concomitantly, they had to settle also for a more limited and more specialized readership; in aggregate they made no fewer than six attempts at publication in *Science* and *Nature* before abandoning these two highly visible and widely-read journals.

If we turn to the earlier stages of the composing process, the preparation of a manuscript prior to review, we can also find impressive evidence that any vision we may have of the scientist-researcher working away in the lab or in the field and then retiring to a quiet place to type up quickly the experimental report according to some stereotyped format is

decidedly at odds with reality. Evidence for what really happens can be gathered from three recent book-length studies that are largely concerned with the construction of research papers. Two are case studies of important US laboratories (Latour and Woolgar, 1979; Knorr-Cetina, 1981), the third is an analysis of a controversy in biochemistry (Gilbert and Mulkay, 1984). All three books are significant products of a relatively new school within the sociology of science in which discourse is topic rather than resource. As Gilbert and Mulkay put it, the approach concentrates 'on describing how scientists' accounts are organized to portray their actions and beliefs in contextually appropriate ways' (1984:14).

The most directly relevant of the three is Knorr-Cetina, for in one of her chapters she presents an extensive textual study, including facsimiles and additionally supported by direct observation and interview, of what transpired between the first rough notes for and the final draft of one paper produced at a large government-financed research center in Berkeley, California during 1977. The subject of the paper is the recovery of protein from potatoes, a process of some significance for the food industry.

The first significant point to emerge is that the public story as told in the drafts is a reversed, rather than revised, version of what actually took place within the confines of the laboratory. In the lab, the scientists responded opportunistically to an incidental finding, rather than consciously planned to try and solve a particular problem. (The problem would, in fact, turn out to be finding a method of extracting valuable potato protein which would require less energy than the current acid/heat treatments and which would also increase nitrogen solubility.) As one of the main researchers commented:

No. I think I was not clever enough originally to see that it would be better to recover protein without applying heat treatment. I probably first read about the ferric chloride ...

(Knorr-Cetina, 1981:101)

In the laboratory realizing that ferric chloride coagulation could occur without heat eventually led to the establishment of an alternative method, whilst *in the paper* the story opens with the need to produce a better method and then offers ferric chloride coagulation as a resolution of this need. Of course, this reversal of the research dynamic is *in its context* neither deceitful nor misrepresentative – although it might be thought so if the *laboratory notes* themselves had been revised in this way. This is because the *research paper* is a quite different genre to the laboratory record and has its own quite separate conventions, its own processes of literary reasoning and its own standards of argument, within

all of which one powerful shaping paradigm is that of the problem-solution text type (Hoey, 1979).

But the story of the Introduction does not end here, because there is a further world of difference between the first full version and the final version. In the first there is a clear succession of increasingly specific paragraphs starting with observations about the large quantities of valuable potato proteins available in the world and how these are under-utilized. A description of current recovery methods follows with considerable emphasis on their drawbacks. The Introduction ends with a discussion of a major alternative coagulant (ferric chloride), which would turn the disadvantages of the current methods into advantages. The final sentence then generalizes the method search:

The aim of this work was to find an alternative precipitation method resulting in a yield comparable to that of protein recovered by means of the most commonly used acid/heat treatment method, while achieving a more acceptable quality of the PPC needed for the application in human foods.

(Knorr-Cetina, 1981:157)

As Knorr-Cetina observes, the switch at the close to the Past tense suggests that the method was actually found, although it is not identified at this point in the paper.

Several months and drafts later, a final version emerged after considerable discussion with and comments from colleagues, including the Director of the Institute. The straightforward and somewhat dramatic unfolding of the first version has almost entirely disappeared. The general-specific structure (zeroing in on the solution) has been abandoned for a series of paragraphs that discuss various topics at approximately equivalent levels of detail, thus producing a more discursive and less goal-directed text. Further, many 'dangerous' claims have been eliminated; for instance, only one of the several first-version statements about the 'disadvantages' of the prevailing protein recovery method has survived. There has also been a considerable increase in 'hedging'; *should* becomes *could*, *is* becomes *has been suggested as possible*, *good solubility* becomes merely *enhanced* and so on. The cautiousness and rhetorical diffuseness of the final version is neatly illustrated by the very different closing sentence of the Introduction:

The purpose of this study was to compare the effectiveness of HCl, FeCl₃, and HCl combined with heat, as precipitants of potato protein in the laboratory, as well as under pilot plant conditions, and to evaluate some compositional, nutritional and functional characteristics of the protein concentrates recovered by these three methods.

(Knorr-Cetina, 1981:165)

Thus, the pre-announcement of a new method has been toned down to a comparative analysis, and the early exuberance of the primary researchers has become the careful understatement of a wider group. In Myers' terms, the level of knowledge-claim has been reduced, perhaps partly in order to limit damage to the Institute's reputation should subsequent work go awry. Throughout, rhetorical considerations have had a pervasive role – first in reconstructing events in the laboratory, and secondly in the long process of generating the final draft. However, it is not quite as though the lab notes could not have been built on in a linear manner, nor that the first version is clearly unpublishable (its main author had already published 40 papers). Rather we seem to see a process of technical critique and social control operating both in the particular research setting and in a wider half-imagined world of 'what other scientists will think'. Knorr-Cetina herself offers a stronger version of this observation: 'the published paper is a multilayered hybrid *co-produced* by the authors and by members of the audience to which it is directed' (1981:106, original emphasis).

Unlike the Introduction, the Method and Materials section remained virtually unchanged in succeeding full drafts except for the eventual deletion of one or two statements of purpose. A sample of Method discourse is given below:

Methods for analysis and functional properties

The standard AOAC methods (AOAC, 1975) were used for the determination of total solids, nitrogen, crude fat, ash and vitamin C. Total sugars were determined by the method of Potter et al. (1968) and the total carbohydrates (in terms of glucose) were assayed according to the procedure of Dubois et al. (1956). The method of Kohler and Patten (1967) was followed for determining amino acid composition.

(Knorr-Cetina, 1981:167)

The above text is a bald Past tense narrative with agentives realized by the method rather than by the protagonists (the contrast with Boyle's reporting style as described in the previous subsection is patent). There are no problems, no matters of discussion, no questions of choice (even though some of the procedures involved several months of testing and modification), no evidence of failure, and no statements of rationale. The contrast with the Method sections in the 'soft' social sciences is also patent. As Knorr-Cetina observes, 'compared with the relevant work in the laboratory, where the *making* of selections dominates the scene, the paper offers a curiously *residual* description, constituted by what is *not* at stake in the research (such as the brand names of devices, or the origins of a technique) than by what is *is*' (1981:115). In fact, the Method sections of RAs often seem increasingly not to be 'reports' in any normal sense;

rather, they are highly abstracted reformulations of final outcomes in which an enormous amount is taken for granted.

This conclusion inevitably belies the common belief that the purpose of Method sections is to permit replication. As it happens, Knorr-Cetina's informants – as well as numerous others – deny that replication is really possible. On one occasion, Knorr-Cetina asked whether a reader could work out the reason for an unglossed change in method. The reply she received was as follows:

He could in principle ... but it would require a lot of thinking.
And he would have to presuppose that I did a lot of thinking too
... In practice, he simply would not know.

(Knorr-Cetina, 1981:129)

Apparently, there are many virtually indescribable matters of technique (which require 'know-how', 'laboratory skills', 'a good pair of hands') that ostensibly make up much of the difference in the way laboratory events turn out.

The Results and Discussion section of the paper also creates a different reality to that observed by Knorr-Cetina in the laboratory. In the laboratory the rhetorical division into the various article sections was, to all intents and purposes, non-existent. For instance, Knorr-Cetina noted that 'methodological constructions' were continuously interpreted and discussed; she also found that the researchers recognized that method and result were mutually inter-dependent. And yet in the published account, the Method section is a 'listing of procedural formulae' whereas the Results is largely taken up with statements of similarity and difference. Interestingly, in the rewriting of the Discussion a comparable process took place to the one we have already described for the Introduction. Evaluation was gradually squeezed out, and no speculations were attempted beyond those previously adumbrated in the opening section. Although the final version of the Discussion, extrapolating from the described results, implicitly argues for a change in the existing practice of recovering potato protein, no longer are there any explicit proposals.

In all then, at the close of this commentary on a splendid but individual case study, we can carry forward two main empirical findings: first, we have seen a long process of rhetorical construction leading to the drafting of the first full version; secondly, an equally long process of rhetorical reconstruction leading to the published paper. In this way we have seen once again how the ultimate published product attempts to create a reader-environment in which the tentative facts can be allowed to 'speak for themselves'. However, we can also see, on some occasions at least, that the creation of such a linguistic artifact is neither simple, nor short, nor particularly natural.

The 1979 Latour and Woolgar study of the Salk Institute in California is somewhat less concerned with rhetorical processes and more with the role of language itself in the scientific enterprise. They note, *inter alia*, that the denizens of the Institute spend the greater part of their days making or reviewing inscriptions: they code, mark, correct, read and write. The aim of all this documentary activity is not to preserve administrative records, but to make contributions to the research front in the form of published papers:

Firstly, at the end of the day, technicians bring piles of documents from the bench space through to the office space. In a factory we might expect these to be reports of what has been processed and manufactured. For members of this laboratory, however, these documents constitute what is yet to be processed and manufactured. Secondly, secretaries post off papers from the laboratory at an average rate of one every ten days. However, far from being *reports* of what has been produced in a factory, members take these papers to be the *product* of their unusual factory.

(Latour and Woolgar, 1979:47)

Of course, this laboratory also produces other things; most obviously, small quantities of rare and valuable natural and synthetic substances. However, these are not sold (Latour and Woolgar estimate that their market value would about cover the Institute's expenses); rather, they are exchanged as part of various kinds of deal or in return for various kinds of favor. Thus their real value lies, as we might now have come to expect, in their *potential* for generating further papers.

Latour and Woolgar then point out that the oral discussion in the laboratory also is largely taken up with discussion of documents: 'almost without exception, every discussion and brief exchange observed in the laboratory centered around one or more items in the published literature ... In other words, informal exchanges invariably focused on the substance of formal communication' (1979:52). For Latour and Woolgar, then, the laboratory is constantly performing operations on statements; citing, borrowing, criticizing, making stronger or weaker knowledge-claims in respect to prior statements. In addition, 'members of our laboratory regularly noticed how their own assertions were rejected, borrowed, quoted, ignored, confirmed or dissolved by others' (1979:87). Therefore, for Latour and Woolgar the laboratory is no longer so much in confrontation with recalcitrant *nature*, but in open competition with other research groups. However, these kinds of argument lead them to adopt the extreme subjectivist position that reality is the outcome of the settlement of a dispute, that facts are always constructed, and that – at least by implication – substances, physical mechanisms and so on do not exist until they have been identified.

Naturally enough this view has been challenged. Bazerman (1980) in particular has observed that *laboratory life* conflates fact with statement of fact. The documentary world of Latour and Woolgar rather conveniently ignores the real substances (and animals) left behind as description moves progressively forward from raw data to the Results sections of papers. In the end, experimental reporting in science is not a collective flight of the imagination, nor a mere matter of shooting down the opposition, but is tethered, however tenuously and obliquely, to an experiential world of substance.

In contrast to the two other books, Gilbert and Mulkay (1984) offer an analysis of the various ways in which a major controversy in biochemistry is described and discussed by the leading protagonists. The accounting for 'the facts' seems to vary along two major dimensions. The first relates to where a particular researcher stands *vis-à-vis* the currently fashionable position. More specifically, Gilbert and Mulkay are able to show the tension between a need to recognize good work by others – however unpalatable – and a need on the researcher's behalf to protect his or her 'investment' in time, equipment, money, effort and kudos. The second major variation in accounting relates to public and private statement – more specifically to the difference between what is said in formal published papers and what is said in informal interviews with the two sociologists. Thus, Gilbert and Mulkay argue that the ordered variability of scientific discourse can be explained by recognizing the existence of two repertoires: the *empiricist* and the *contingent*. The former is typically used in the research literature:

As we have seen, in research papers experimental data tend to be given chronological as well as logical priority. Neither the author's own involvement with or commitment to a particular analytic position nor his social ties with those whose work he favors are mentioned. Laboratory work is characterized in a highly conventionalized manner, as instances of impersonal, procedural routines which are generally applicable and universally effective. Although the content of experimental papers clearly depends on the experimenters' actions and judgments, such papers are overwhelmingly written in an impersonal style, with overt references to the author's actions and judgments kept to the minimum. By adopting these kinds of linguistic features, authors construct texts in which the physical world seems regularly to speak, and sometimes to act, for itself. Empiricist discourse is organized in a manner which denies its character as an interpretative product and which denies that its author's actions are relevant to its content.

(Gilbert and Mulkay, 1984:56)

This depiction of the empiricist repertoire has, on the one hand, much in common with Knorr-Cetina's description of the RA product, while, on

the other, it contrasts sharply with the accounts that scientists produce when they discuss their work informally. In interviews, a *contingent* repertoire was manifest in which the impact of a range of factors not directly concerned with the world of biochemical phenomena was admitted. In interviews 'scientists presented their actions and beliefs as heavily dependent on speculative insights, prior intellectual commitments, personal characteristics, indescribable skills, social ties and group membership' (1984:56).

Further, Gilbert and Mulkay engagingly demonstrate that one kind of humor in the research world depends on playing off one repertoire against the other, a *locus classicus* being those lists pinned on research students' walls which contrast what is written with what 'really' happened. A version perhaps appropriate for readers of this book is given in Figure 6.

Empiricist	Contingent
A sample of 139 reprint requests was assembled.	My own and Bob's down the corridor.
The return rate was only 34%.	Actually a bit higher but two forms were returned about a year later, long after I had done the analysis.
An intermediate group of students was chosen as especially suitable.	We were already teaching them.
There is evidence that NNS graduate students are concerned about academic correspondence.	At least some of mine either panicked or indulged in avoidance strategies.
Short introductions were used for the preliminary analysis.	I wanted to be able to spread out single-page texts.
It has long been known that ...	I haven't been able to remember where I read it.

Figure 6 The two repertoires

The Gilbert and Mulkay study is itself artificially restricted (Halfpenny, 1988) since the division into just two repertoires is a direct consequence of the dual nature of their investigation: formal papers and semi-structured informal interviews. However, as linguists and rhetoricians

know, language is always relatable to context, and we would therefore expect that other settings would give rise to additional repertoires. Indeed, Mulkay has gone on to do this by considering other genres such as scientific correspondence and Nobel Prize speeches (Mulkay, 1985). Another problem is that, as Gilbert and Mulkay observe but Latour and Woolgar do not, eventually 'the truth will out'; for instance, the relevant discourse communities will eventually agree that 'cold fusion' does or does not occur. A third is that sociologists of science not unnaturally need for the growth of their discipline to find – and perhaps stress – *sociological* aspects of both the processes and products of hard science experimentation. Thus, the accounts we have may not themselves be immune to the encroachment of contingent elements; we have yet to have in this area an ethnography of an ethnography (cf. Lury, 1982).

Despite these minor caveats, the three major studies reviewed in this section indicate in their slightly different ways the strength of the genre-specific conventions that constrain and shape the research article. Consequently, and despite appearances to the contrary, we find ourselves far away from a world in which it is expected that researchers will 'tell it as it happened'. Despite the conventional sectioning of the research article, we are far away from a world in which the research itself is comparably compartmentalized. Despite an objective 'empiricist' repertoire, we are far away from a world in which power, allegiance and self-esteem play no part, however much they may seem absent from the frigid surface of RA discourse. And yet we find the research article, this key product of the knowledge-manufacturing industry, to be a remarkable phenomenon, so cunningly engineered by rhetorical machining that it somehow still gives an *impression* of being but a simple description of relatively untransmuted raw material.

There is one other kind of rhetorical transformation that needs to be considered at this juncture: the translation of the RA into various kinds of more popular account. Dubois (1986) has traced the process whereby articles in journals like the *New England Journal of Medicine* (which we have already met at the outset of Part III) appear as news items in local papers through the mediation of science journalists working for the Associated Press wire service. As might be expected, the very clear differences in anticipated audience have profound rhetorical effects. Not only do we find expected changes such as the removal of jargon and the diminution of qualification, but the structure of the medical news item takes on the organizational form of journalistic genres. The main conclusions now occur in the opening sentences, while summaries of method are placed at the end (where they may be cut by local editors without causing the story as a whole to lose coherence). Further, 'publication of the scientific article is treated itself as a news event, with

the result that the status of the scientific information may appear to be elevated to that of unalterable fact' (Dubois, 1986:243).

Fahnestock (1986) has also studied 'the fate of scientific observations as they pass from original research reports intended for scientific peers into popular accounts aimed at a general audience' (1986:275). She again traces translation into ordinary language, loss of careful qualification and a greater concern to capture human interest. She also observes:

With a significant change in rhetorical situation comes a change in genre, and instead of simply reporting facts for a different audience, scientific accommodations are overwhelmingly epideictic: their main purpose is to celebrate rather than validate. And furthermore they must usually be explicit in their claims about the value of the scientific discoveries they pass along. They cannot rely on the audience to recognize the significance of information.

(Fahnestock, 1986:278–9)

Fahnestock's genre shift is dramatically underlined by Myers (forthcoming), for he is able to establish major differences between original versions and their appearance in such prestigious science magazines as *Scientific American* and *New Scientist* – periodicals of course often believed by English teachers to represent contemporary scientific writing. Myers argues that the popularizations tell a different story and have a different view of science to the originals:

These two views of science are apparent in textual differences in narrative structure, in syntax, and in vocabulary. The professional articles I study create what I call a *narrative of science*: they follow the argument of the scientist, arrange time into a parallel series of simultaneous events all supporting their claim, and emphasize in their syntax and vocabulary the conceptual structure of the discipline. The popularizing articles, on the other hand, present a sequential *narrative of nature* in which the plant or animal, not the scientific activity, is the subject, the narrative is chronological, and the syntax and vocabulary emphasize the externality of nature to scientific practices.

(Myers, forthcoming:1–2)

Myers then goes on to show how this dichotomy works its way through differences in titles, abstracts, overall organization, introductions and illustrative material.

There are a number of pedagogical messages to be drawn from this review of what we know about the processes of constructing (and reconstructing) research articles. For example, the findings of Dubois, Fahnestock and Myers strongly suggest that we need to be aware of what we are doing if we are to introduce texts of the *Scientific American* type into our classes. We need to recognize that they constitute a different

genre to the original RAs from which they derive. While, as Fahnestock (1986) argues, there may be a place for engaging students in exercises of technical journalism of this kind, they must be recognized as such and not seen as helping students to enter primary discourse communities of researchers. Indeed, there may be gain in getting students to test out Myers' two narratives proposals with material from their field or to follow controversies that arise from the over-simplifications of original conclusions.

Other easily communicable messages include the sheer importance of the writing aspect of research activity, its rhetorical complexity even within well-established genres, and the often elaborate nature of the revision process. Many of the longer quotes cited in this subsection are directly suitable for discussion, comparison, analysis and matching tasks, as I attempted to show in Swales 1987b.

7.3 Textual overview of the research article

The immediately preceding section offered several case histories of the processes whereby research articles get constructed. These case histories may in fact be somewhat one-sided in that they consistently point to the long-drawn-out and complex nature of these processes. Unfortunately I do not know of studies which document instances where RAs were 'dashed off', even though the very high productivity of certain researchers would indicate that such instances very probably occur and recur. Perhaps all we can conclude at the present time is that it would be erroneous to assume that the writing of a RA is *necessarily* a straightforward task even for full and established members of discourse communities.

The reason for disassociating process and product is encapsulated in Murray's celebrated dictum that 'process cannot be inferred from product any more than a pig can be inferred from a sausage' (Murray, 1982:18). However, while the admonitory strength of this is obvious enough for those who would study the ways and means of the compositional process, the analogy does not affect the fact that the world itself evaluates the end product. The sausage succeeds or fails on its own merits. Although there may well be some correlation between the quality of the materials (plus the time and care given to their processing) and the acceptability of the outcome, the consumer makes a judgment on the thing itself. Similarly, editors and reviewers evaluate the product they receive, and do not encourage accompanying accounts of how long or how agonizing the construction processes were. In the RA genre the amount of effort is ultimately deemed not material, as it is similarly deemed in a book like this. This is not to say, of course, that matters are

always of an open or shut character. We may, with varying degrees of formality, distribute or even submit *drafts* for comment – as the prefaces to this and many other books signify – but these are subsequently viewed by their recipients as being drafts and treated accordingly. Accompanying commentary will tend to refer to the draft as being ‘rough’ or ‘incomplete’ rather than refer to the amount of time or effort expended. At the end of the day, it is the product that counts.

I believe that sections 7.1 and 7.2 should have convinced many that the RA is anything but a simple genre. It is thus quite surprising to find that there have been very few attempts to define an appropriate procedural methodology for approaching texts of this kind (unlike in spoken discourse, e.g. Ellis and Donohue, 1986; Potter and Wetherell, 1987). The most important set of proposals is probably that of Bley-Vroman and Selinker (1984) in which they advocate the following five-step approach to ‘an optimum research strategy’:

1. Practical problem areas and current tools
2. ‘Quick and dirty’ analysis
3. Explicit initial assumptions
4. Highly-valued texts
5. Subject-specialist informants

(Bley-Vroman and Selinker, March 1984:4)

As far as I am aware, the first three steps have received general assent. In Step 1 Bley-Vroman and Selinker advocate that research in the grammatical/rhetorical tradition – as pertaining to academic genres – should be grounded in a perceived inadequacy in extant teaching materials, in a perceived dissatisfaction with current methods of applied discourse analysis, or in a perceived dissatisfaction with our state of knowledge of a student-relevant text type. Under Step 2 they recommend a rough-and-ready survey of relevant texts in order to ascertain the incipient problems of procedure, sampling, text-division and so on. For Step 3 they state that it is necessary ‘to establish and *explicitly state* the initial series of assumptions which will guide the researcher studying the unit in the text(s) under observation’ (January 1984:2) – and this concern for validating assumptions is both emphasized and extended in Crookes (1986a). So far so good.

However, Bley-Vroman and Selinker then suggest in Step 4 that the analytic work be limited to an apparently restricted number of ‘highly-valued’ texts that can be established – by inquiry, interview and so on – as central to a given field, either by virtue of content or by virtue of form. There are, I believe, a number of difficulties with this notion. First, important papers may often be written by powerful luminaries who are consequently able to ride rough-shod over many of the accepted linguistic and rhetorical conventions in their given field; Ard (1983), for instance,

has pointed out that Chomsky's later writings (written when he had already achieved a considerable degree of fame) display very much greater use of first person pronouns than his early publications. Therefore, there is a very real danger that the 'content' version of a highly-valued text may be stylistically atypical, thus offering a misleading role-model to those trying to embark upon a career as research writers. Apparently, Bley-Vroman and Selinker take 'form' in the somewhat abstract sense of referring to standard 'argument' or representative rhetorical organization. However, as the two authors nowhere make any reference to corpus-building or establishing a representative sample, this method of ascribing 'high value' seems prone to circularity. After all, we do not know what a model in terms of 'form' may be *unless* and *until* some survey has established that a particular text is generally representative of exemplars of the genre. It seems to me that the concept of *highly-valued* texts may have considerable significance in studies that trace the intellectual and/or rhetorical history of an area; however, in our attempts to develop genre-specific skills among apprentices we may do better to operate on and with texts that are not extraordinary by virtue of import, authorship or whatever, for then they are more likely to be *prototypical* exemplars.

Step 5, eliciting the help of a specialist informant in text-selection, category-establishment and interpretation, offers at first sight a convincing way out of the subjectivist dilemma. However, if we are to give credence to the findings of Gilbert and Mulkay and others we have to recognize that discussions with specialist informants may fall squarely within the *contingent repertoire*. Those discussions will hence be subject to all the subjective features of personality, allegiance, status and so on that this repertoire exhibits. Although Selinker (personal communication) is clearly right to point out that without specialist informants genre analysts may be in danger of 'not knowing what they don't know', over-reliance on specialist informants may invite the opposite danger of analysts 'believing all that they hear'. Further problems are that specialist informant work can be very time-consuming (Huckin and Olsen, 1984), and that it raises uncertainty when comparing RAs from different disciplines (where instability is inevitably created by relying on *different* informants for each discipline). We might conclude, then, that the role of the subject specialist informant in RA genre analysis remains, given the current levels of evaluated experience, somewhat controversial. Certainly the evidence is mixed. There is at least one purely textual study that has been subsequently confirmed by re-analyzing the data with the aid of a specialist informant (Selinker, Tarone and Hanzeli, 1981:52); and yet there must be myriad instances of language specialists 'misreading' specialized texts when working on their own. Even more confusingly, there is one case (Huckin and Olsen, 1984) in which the original author

offered a rather different interpretation of his text to that provided by the subject specialist informant in Selinker (1979).

If there are discernible figures in this carpet they would suggest the following. Firstly, there is always a partial and supportive role for the specialist informant: useful for certain types of analysis, for certain parts of texts, for testing formulated hypotheses and findings. Secondly, the general need for a specialist informant may be in inverse proportion to the degree of relevant experience possessed by the genre analyst. Thirdly, there is a strong suggestion in the literature that the value of specialist informants increases when they are not only conceived of as sources of information and insight, but also as objects of ethnographic study themselves as they negotiate textual material within their own environments (Pettinari, 1982; Bhatia, 1983; Zumrawi, 1984; Huckin, 1987). It remains the case, however, that the field still lacks effective studies that evaluate the use of informants in terms of the trade-off between the rewards of 'behind the scenes' insight and professional confirmation, and the penalties of extra time and narrowed scope.

The literature that explores the textual properties of RAs is quite extensive. Moreover, there is considerable variation amongst the papers in the scale of research, in the level of analysis (from the straight counting of surface linguistic features to the search for underlying structure), and in the methodological and linguistic approaches devised or drawn upon. Useful part characterizations of this variety are provided by Widdowson (1979) and Jarvis (1983). In fact, some of this research attempts to describe scientific writing in general: research articles, textbooks, specialized reports, and sometimes various kinds of scientific journalism. Those investigations that have conflated genres in such a way that information specific to the RA is irreconstructible will not be considered further in this section. This may seem arbitrarily dismissive, but I believe it is a not unreasonable position to adopt in a book whose major premise is that genre differentiation provides the opening strategy in elucidating the characteristics of recurring and regularized communicative events. Some of the more important pieces of work that have been 'lost' in this way are Barber (1962), Huddleston (1971), Selinker et al. (1976) and a series of articles on lexis such as Salager (1983).

Because of the complexity of the remaining literature, I will first offer a summary overall listing of studies before discussing detailed findings. Thus, in Table 3 the studies are principally arranged by *coverage* – all of the RA or one or more of its parts – and secondarily by main *feature* selected for study (tense, voice, etc.). Under the latter heading I have used the cover-term 'structure' to encompass investigations that deal with a wide variety of *patterns* of rhetorical, informational and conceptual organization. I have also tried to show wherever possible both the size of corpus and the discipline or disciplines covered, the asterisks indicating

TABLE 3. OVERVIEW OF THE TEXTUAL STUDIES OF THE ENGLISH RA

a) *The RA as a whole*

<i>Author(s)</i>	<i>Date</i>	<i>Feature(s)</i>	<i>Corpus (no. of RAs)</i>	<i>Field(s)</i>
Lackstrom et al.	1972	tense, etc.	*	engineering
Lackstrom et al.	1973	paragraph development and tense	*	science and engineering
Inman	1978	types of lexis	40	range/ undifferentiated
Lackstrom	1978	modals	*	general science
Ewer	1979	modals	18	range/ undifferentiated
West	1980	<i>that</i> -nominals	15	biological science
Tarone et al.	1981	voice	2	astrophysics
Tomlin	1981	voice and clause	*	*
Wingard	1981	verb forms	5	medicine
Ard	1982	tense and aspect	*	*
Darian	1982	definitions	*	*
Dubois	1982	NP-development	5	zoology
Heslot	1982	tense	16	plant pathology
Hill et al.	1982	structure	1	psychology
Ard	1983	personal pronouns	*	*
Een	1982	tense	9	geotechnical engineering
Adams Smith	1984	authorial comment	6	medicine
Stanley	1984	structure	1	engineering
Weissberg	1984	paragraph development	60 (paras)	agriculture/ botany/ engineering
Adams Smith	1987	variation	6	medicine
Jacoby	1987	citation patterns	6	literary research
Malcolm	1987	tense	20	medicine
Popken	1987	topic sentences	35	range/ differentiated
Tinberg	1988	variation	2	economics

TABLE 3. OVERVIEW OF THE TEXTUAL STUDIES OF THE ENGLISH RA

b) *Introductions*

<i>Author(s)</i>	<i>Date</i>	<i>Feature(s)</i>	<i>Corpus (no. of RAs)</i>	<i>Field(s)</i>
Hepworth	1979	structure	*	*
Oster	1981	tense	2	engineering
Swales	1981	structure, etc.	48	range/ differentiated
Trimble and Trimble	1982	tense	*	science/ engineering
Zappen	1983	structure	*	engineering
Kinay et al.	1983	concluding sentences	50	range/ differentiated
Cooper	1985	structure	15	electronics
Hopkins	1985	structure	5	agriculture engineering
Crookes	1986	structure	24(96)	range/ differentiated
Swales and Najjar	1987	results statements	110	physics/ educational psychology

c) *Methods and Results*

Wood	1982	structure	10	chemistry
Bruce	1983	structure	*	medicine

d) *Discussions*

Belanger	1982	structure	10	neuroscience
McKinlay	1984	structure	30	medicine
Peng	1987	structure	10	chemical engineering
Hopkins and Dudley-Evans	1988	structure	12	range/ differentiated

that information is not available. The use of 'range' in the final column refers to a spread of fields or disciplines; the *range* may be undifferentiated (i.e. the findings are consolidated) or differentiated (i.e. similarities and differences between fields can be traced).

Before we examine some of the more interesting findings, a few general

observations on Table 3 may be useful. In some of the studies, there are comparisons between genres, such as Heslot's study of experimental and review articles or Adams-Smith's 1987 search for differences and similarities between original RAs and their popularization. Typically, I have excluded the data that is less prototypical of the RA. Secondly, it is easy to see that some fields are much less well represented than others; for example, there is very little on disciplines such as economics and sociology. Third, I have not thought it worthwhile to incorporate in Table 3 papers that have already been quite extensively discussed for other purposes (Bazerman, 1984a; Huckin, 1987), although relevant aspects of these fine studies will not be neglected. Finally, the listed papers vary considerably in their analytic perspective. Bruce, Dubois, Weissberg and Wood have been influenced by the *Functional Sentence Perspective* of the Prague school; Stanley and Jacoby by the *Clause Relations* of Winter (e.g. Winter, 1986); and Oster, Tarone and others by the *Rhetorical-Grammatical Approach* associated with Lackstrom, Selinker and Trimble. A number of other studies have attempted to develop an analysis which reflects the characteristics of the genre itself – a posture already recommended more than once in this volume.

A number of papers have attempted an account of the conceptual macrostructure of the research article: Stanley (1984) proposes a problem–solution structure, Bruce (1983) suggests that the Introduction–Method–Results–Discussion format follows the logical cycle of inductive inquiry, and Hutchins (1977) offers for the RA a modification of Kinneavy's cycle of *Dogma–Dissonance–Crisis–Search–New Model* (Kinneavy, 1971). However, little textual evidence is put forward to justify such configurations and, as the previous section has made clear, there can often be considerable distance between research actuality and its formal presentation. Perhaps then a more manageable starting-point for a discussion of shape of macrostructure is the hour-glass diagram (Figure 7) proposed by Hill et al.

Although this schematic diagram is apparently derived from but a single paper in psychology – and one with an off-beat content at that – it intuitively accords with much of the discussion in the previous two sections. As the authors say, 'research papers make the transition from the general field or context of the experiment to the specific experiment by describing an inadequacy in previous research that motivates the present experiment' (1982:335). The Method and Results sections (subsumed under Procedure in Figure 7 overleaf) then continue along a narrow, particularized path, whilst the Discussion section mirror-images the Introduction by moving from specific findings to wider implications.

The findings of West (1980) and Heslot (1982) can now be fitted into this scheme. West studied the occurrence across sections of *that-nominals* (elements of the type in italics in the following example sentence): 'We

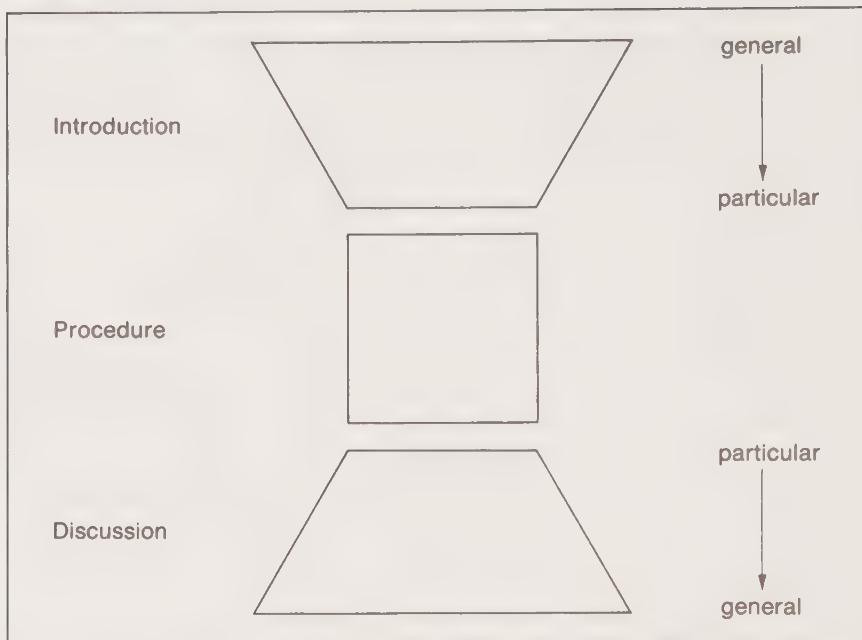


Figure 7 Overall organization of the research paper (Hill et al., 1982).

have shown that genres have considerable generative power'. He found 306 such structures in the 15 biology RAs he examined. As might be expected, they were distributed differently across the Introduction–Method–Results–Discussion (IMRD) structure; very rare in Methods, fairly uncommon in Results, frequent in Discussions and most frequent in Introductions. He provides the following explanatory comment:

Because that-nominalization relegates statements to secondary positions in other statements, that-nominals are used when making claims about other statements rather than simply making statements. The Introduction section, as the rhetorical section that motivates the study, normally includes a review of previous research. The fact that a primary function of the Introduction section is to make claims about statements from other research accounts for the high density of that-nominal constructions. Similarly, the Discussion section, as the rhetorical section whose primary function is to explain the statistical findings in non-statistical English, makes many claims about the research findings and therefore contains many that-nominals. The Results section, which describes the process of manipulating the data obtained from the Methods section and makes only limited claims about the statistical tests, has fewer that-nominals. The Methods section, which simply describes the process of obtaining the data, rarely

makes claims about other statements and consequently has almost no that-nominal constructions.

(West, 1980:486-7)

Of course, commenting on the work of others can be expressed by a variety of syntactic devices other than that-nominals (West himself mentions the infinitive), and at this stage we might not want to accept too readily West's descriptions of the purposes of the various RA sections; nevertheless, the statistical differences in regard to this linguistic feature are both striking and indicative.

Heslot (1982), in a useful paper, examines tense, voice and 'person markers' across the four sections in 16 RAs from the journal *Phytopathology*. Again the distributions are highly significant (percentages have been rounded where not 0.5).

TABLE 4. TENSE AND VOICE PER SECTION IN 16 RAS (after Heslot)

	I %	M %	R %	D %
Simple Present	48.5	4.0	6.0	48.5
Simple Past	35.0	94.0	94.0	39.0
Active	67.0	17.0	72.0	83.5

Both the procedural M and R sections in this sample are almost exclusively 'past', but the former strongly favors the passive whereas the latter equally strongly favors the active. The I and D sections are similar in showing a smallish preponderance for the present and in having combined percentages for the two main tenses in the 80-90% range (as opposed to 98-100% for M and R), thus suggesting that other verb forms, such as the Present Perfect and modal complications, tend to occur principally in these two sections.

Heslot also comments that first person forms did not occur in M and R, but did so in I and D, although she does not give any figures. This finding conflicts with that of Tarone et al. (1981) who showed that *we* was widely distributed across the (more complicated) sectioning of two astrophysics RAs – indeed, if anything, it occurred less frequently in the opening and closing sections. If there is a simple explanation for this anomaly, it may lie in the fact that astrophysics papers have no obviously procedural sections but largely consist in descriptions of and arguments for an increasingly refined set of equations to account for astrophysical phenomena.

Leaving aside the Tarone results, further support for the emerging

conclusion that the different sections perform different rhetorical functions and thus require different linguistic resources to realize those functions is provided by Adams Smith, who investigated 'author's comment' in (amongst other genres) six medical research papers. The relevant part of her figures is given in Table 5 below.

TABLE 5. INSTANCES OF AUTHOR'S COMMENT PER RA SECTION
(simplified from Adams Smith, 1984)

	<i>Instances of author's comment</i>	No. of lines	<i>Comment/ line ratio</i>
I	58	173	1:3
M and R	18	385	1:21
D	202	436	1:2,2

Again we see a marked contrast between I and D on the one hand, and M and R on the other; in this particular case a difference close to an order of magnitude. According to Adams Smith, the main way in which I- and D-clustered authorial comment is introduced is by modal auxiliaries, of which *may* and then *should* are the most frequent. (In Adams Smith's sample *can* is rare, although Huddleston (1971) remarks that it is common in physical science papers.) In fact, modality accounted for about half of the instances of *comment*, the other half being expressed by adverbs and adjectives of 'probability' such as *possible*, *certainly*, and so on (about 15%) and by a wide range of 'attitudinal markers' (the remaining 35%) such as adverbs like *surprisingly*, marked choice of noun (*view v. hypothesis*), switch to first person, and unusual use of metaphor or analogy. Functionally, by far the most common type of authorial comment in the six RAs in the sample is epistemic, that is, relating to the probability (from 0 to 100%) of a proposition or a hypothesis being true. The three other uses that occurred with any frequency were recommending, emphasizing and evaluating. If we put these preliminary findings together we get an overview as in Figure 8.

The evidence thus suggests a differential distribution of linguistic and rhetorical features across the four standard sections of the research article. By and large, we have seen evidence for a two-way division into 'simple' M and R and 'complex' I and D – and at this juncture we could bear in mind Knorr-Cetina's observation that in her case study it was the Introduction and Discussion that were serially redrafted whilst the M and R drafts survived to publication virtually unchanged. The following sections will examine each of the four parts in more detail by considering, *inter alia*, the rationale behind such findings, exceptions to them, and cross-disciplinary variation. This, however, is a suitable point

Feature	I	M	R	D
Movement (Hill et al.)	outside-in	narrow	narrow	inside-out
Reporting statements (West)	high	very low	low	high
Present Tense (Heslot)	high	low	low	high
Past Tense (Heslot)	fairly low	very high	very high	fairly low
Passive Voice (Heslot)	low	high	variable	variable
Authorial comment (Adams Smith)	high	very low	very low	high

Figure 8 Features across the IMRD sections

to indicate one or two linguistic and functional features that occur very rarely, if at all, in *any* section of a RA. Progressive or continuous forms are extremely rare (Barber, 1962; Wingard, 1981). Second person pronoun forms are absent (in contrast to textbooks), except for the occasional imperative verb in comments on non-verbal data or in footnotes. Explicit definitions (again unlike textbooks) are very rare (Darian, 1982; Swales, 1981a).

7.4 Introductions

Introductions are known to be troublesome, and nearly all academic writers admit to having more difficulty with getting started on a piece of academic writing than they have with its continuation. The opening paragraphs somehow present the writer with an unnerving wealth of options: decisions have to be made about the amount and type of background knowledge to be included; decisions have to be made about an authoritative versus a sincere stance (Arrington and Rose, 1987); decisions have to be made about the winsomeness of the appeal to the readership; and decisions have to be made about the directness of the

approach. If we add to the above brief catalogue the assumption that first impressions matter (especially in an era of exponentially-expanding literature), then we are not surprised to note that over the last 10 years or so there has been growing interest in the introductory portions of texts. As far as the RA is concerned, part of this literature has been listed in Table 3.

One possible approach is to view RA introductions as encapsulated problem-solution texts. This, for instance, is the position adopted by Zappen (1983) who, following Toulmin (1972), argues that researchers in their writing need continuously to address the context of the intellectual discipline wherein they are located. More specifically 'the researcher addresses the goals, current capacities, problems, and criteria of evaluation that derive from and operate within that discipline' (Zappen, 1983:130). As Figure 9 shows, Zappen's analysis follows this series of sub-contexts: *goal* in the first paragraph, *current capacity* (the best we can do at the moment), *problem* (However, ...), *solution* (In the present work, ...) and *criteria of evaluation*.

While the emphasis on the disciplinary audience in this characterization is both salutary and necessary, and while the five-part rhetorical division is itself plausible, the labeling of those divisions suggests a rather flat and certainly sunny world in which the empiricist repertoire of logic, objectivity and reason strongly predominates. However, if we examine a little more closely the Figure 9 text that Zappen himself chose to illustrate his approach, we can see that this short introduction is firmly embedded within the localized field of the researchers' previous work. Eight of the nine references cite previous papers by the first author. All but one of the self-citations are positive, the exception being (9), while the solitary outside reference (8) is considered to make a less 'practical' proposal than the authors' own. The exception to the positive evaluation of their own work lies in Zappen's Problem section where the authors need both to motivate their present work and to justify its publication by showing that their contribution to the discipline, whilst previously established as significant and reference-worthy, is as yet incomplete. In addition, the story that Neelakantaswamy and Hong tell is not without its rhetorical interest. They open their account with the bold claim that they have developed not a 'number' nor a 'series' but a 'class' of instruments – and one that has been given a class name. These are 'compact' and 'simple', have 'practical utility' and are 'non-invasive'. Unfortunately, this class of instrument does produce a 'significant amount of spherical aberrations', but in the latest version as presented in the current paper 'the spherical aberration effects are relatively minimized'. (But not eliminated completely.)

Thus, this 'simple' and short engineering introduction is rich in evaluative commentary that not only reveals the authors addressing the

GOAL

In the recent past, Neelakantaswamy et al. (1-4) developed a class of microwave radiators termed as 'Gaussian-beam launchers' to produce a focused exposure field in biological experiments for partial-body irradiations. These compact and simple structures with their ability to focus the microwave energy in a very small region indicate their practical utility, in the areas of biological researches and medical applications of microwaves, such as for selective heating of diseased/cancerous tissues. These launchers can also be used in noninvasive beam-wave reflectometric and spectrometric instrumentations for measuring complex permittivity of biological material at microwave frequencies, as indicated by Neelakantaswamy elsewhere (5-7).

CURRENT CAPACITY

When compared to the microwave beam-launching system described in (8), which consists of a plane-wave irradiated dielectric sphere (lens), the launcher formed by combining a scalar horn and dielectric sphere (1) is a more practical source of microwave Gaussian beam. However, the use of a dielectric sphere as the focusing lens results in a significant amount of spherical aberrations in the focal field, as indicated by Neelakantaswamy et al. in (9) ...

PROBLEM

In the present work, a Gaussian-beam launcher is formed by placing a dielectric hemisphere (instead of a full sphere) at the aperture end of corrugated circular waveguide (scalar horn). This enables a reduction in the path length of the ray in the lens-medium, and hence the spherical aberration effects are relatively minimized. Further, by using a hemisphere in the place of a full sphere, the launcher structure becomes less massive and smaller.

SOLUTION

CRITERIA OF EVALUATION

(from P. Neelakantaswamy and F. Hong. 1979. Dielectric Hemisphere-Loaded Scalar Horn as a Gaussian-Beam Launcher for Microwave Exposure Studies. *IEEE Transactions on Microwave Theory and Techniques. MTT*, 27:797)

Figure 9 A problem-solution model of article introductions (Zappen, 1983)

expectations of the discourse community (as the Toulmin-Zappen model suggests) but also addressing the development of their research area particularly as it relates to their *own* contributions, past, present and future. Although the level of self-citation in the text may be abnormally high, scientist colleagues consistently respond to my queries with comments like 'you cannot avoid citing your previous work in science'. This suggests, amongst other things, that part of the difficulty with fitting a problem-solution schema onto introductions is the fact that 'problems' or research questions or unexplained phenomena are the life-blood of many research undertakings. Adams Smith (1987) cites her informant as follows:

Biomedical research, he said, is not a matter of problem-solving. Rather it is the observation of something interesting that does not seem to fit the pattern, followed by the observation of this phenomenon over a period of time, and the recording and explanation of the findings. It is common for a piece of research to answer the question it has set out to clarify while at the same time it raises other questions to be accounted for in the course of further investigation.

(Adams Smith, 1987:19-20)

Perhaps it is not therefore surprising that Adams Smith (1987) found that the medical RAs she examined either failed to contain a recognizable *problem* or tended not to foreground it. In contrast, the derived popularizations emphasized problem or controversy, partly by placing the issue early (presumably for reasons of 'newsworthiness' discussed in the previous section).

My earliest attempt to offer an alternative *sui generis* model to account for the rhetorical movement in article introductions was Swales, 1981b. Although the '4-move' model presented in that monograph has had some little influence (sometimes more than I would wish), certain defects have become increasingly apparent. Several analysts (Lopez, 1982; Bley-Vroman and Selinker, 1984; Crookes, 1986a) have commented on the difficulties of separating Move 1 and Move 2. The fact that the original corpus was deliberately restricted to *short* introductions led to the creation of a separate citational category (Move 2 – Summarizing Previous Research) clearly at odds with the increasing practice of spreading references throughout the introduction (Jacoby, 1986). A further consequence of the corpus choice was neglect of the recycling possibilities in longer introductions. In addition, the range of options in the final two moves was overly restrictive (Jacoby, 1987; Cooper, 1985).

In the revised *Create a Research Space* (CARS) model (Figure 10) I have taken the ecological analogy rather further than hitherto, because it seems to me that it adequately captures a number of characteristics of RA

Move 1 Establishing a territory

- Step 1 Claiming centrality
and/or
Step 2 Making topic generalization(s)
and/or
Step 3 Reviewing items of previous research

Declining
rhetorical-
effort

Move 2 Establishing a niche

- Step 1A Counter-claiming
or
Step 1B Indicating a gap
or
Step 1C Question-raising
or
Step 1D Continuing a tradition

Weakening
knowledge
claims

Move 3 Occupying the niche

- Step 1A Outlining purposes
or
Step 1B Announcing present research
Step 2 Announcing principal findings
Step 3 Indicating RA structure

Increasing
explicitness

Figure 10 A CARS model for article introductions

introductions: the need to re-establish in the eyes of the discourse community the significance of the research field itself; the need to 'situate' the actual research in terms of that significance; and the need to show how this niche in the wider ecosystem will be occupied and defended. It follows that the amount of rhetorical work needed to create such a space depends on the existing ecological competition, on the size and importance of the niche to be established, and on various other factors such as the writer's reputation.

If we apply this model to the Zappen text (Figure 9) we can see that Move 1 is coterminous with *goal* and *current capacity*, Move 2 with *problem* and Move 3 with *solution of criteria of evaluation*. We would ascribe Move 1 as containing only a Step 3 because there are no opening generalizations of a type to be discussed shortly. However, the Step 3 (literature items 1–8) is, as we have already seen, quite marked by upbeat expressions of significance and relevance, presumably because of the self-citational nature of the review. The single sentence Move 2 can be identified as a Step 1B (indicating a gap). A possible alternative would be to take Move 2 as a Step 1D (continuing a tradition) but the use of the adversative *however* suggests otherwise.

The four options for Move 2 can be illustrated as with the following versions of the Figure 9 text:

Step 1A (Counter-claim)	However, the use of ... results in such a degree of spherical aberration that radical design changes have become necessary.
Step 1B (Gap)	However, the use of ... results in a significant amount of spherical aberrations ...
Step 1C (Question)	However, it is not clear whether the use of ... can be modified to reduce spherical aberration to acceptable levels.
Step 1D (Continuation)	The remaining issue is to find a way of better controlling spherical aberration.

Finally, we can assign the third paragraph to Move 3, the first sentence being Step 1B, and the final two being Step 2.

Figure 11 offers a sample Move–Step analysis of a slightly longer, 14-sentence introduction, but again from the hard sciences area. Both the text itself and the issues it raises for the genre analyst are quite intriguing. In the first place, the establishment of territory in Move 1 utilizes all three step options, again suggesting that we would be unwise to come to any swift conclusion that science and engineering RA introductions tend to avoid (or do not need) the more rhetorical options. In fact, the opening two sentences represent a prototypical instance of both the 'narrowing' effect and of what I have called *claiming centrality*:

I Introduction

	(1) The increasing interest in high-angle-of-attack aerodynamics has heightened the need for computational tools suitable to predict the flowfield and the aerodynamic coefficients in this regime. (2) Of particular interest and complexity are the symmetric and the asymmetric separated vortex flows which develop about slender bodies as the angle of attack is increased. (3) The viscous influence on the separation lines and the unknown three-dimensional (3D) shape of the vortex wake are some of the main flow features that must be modeled in the construction of a computational method to properly treat this problem.	STEP 1
MOVE 1	(4) Among the many potential flow methods developed in attempting to solve body vortex flows are early two dimensional (2D) multivortex methods, ²⁻⁴ 2D time-stepping vortex models that include boundary-layer considerations, ⁵⁻⁸ and a quasi-3D potential flow method ⁹ that uses source and vortex elements. (5) Linear, unseparated potential flow models as well as purely viscous models, are not mentioned here. (6) A survey of the various methods may also be found in Ref. 10. (7) The potential flow methods are of special interest because of their ability to treat 3D body shapes and their separated vortex flows using a simple and relatively inexpensive model. (8) However, the previously mentioned methods suffer from some limitations mainly concerning the treatment of the vortex wake formation and its interaction with the body. (9) The first group of methods ²⁻⁴ cannot treat 3D flows and is limited to very slender bodies. (10) The second group of computational methods ⁵⁻⁸ is time consuming and therefore expensive, and its separation prediction is not sufficiently accurate. (11) Both the methods in this group and the method in Ref. 9 suffer from the dependency on too many semiempirical inputs and assumptions concerning the vortex wake and its separation. (12) The steady, 3D nonlinear vortex-lattice method, ¹¹⁻¹² upon which the present method is based, eliminates many of these limitations by introducing a more consistent model, but it can treat only symmetrical flow cases. (13) The	STEP 2
MOVE 2	present work extends the use of the last model to asymmetric, body-vortex flow cases, thus increasing the range of flow problems that can be investigated. (14) In addition, an effort is made to improve the numerical procedure to accelerate the convergence of the iterative solution and to get a better rollup of the vortex lines representing the wake.	STEP 3
MOVE 3	(D. Almosino. 1985. High Angle-of-Attack Calculations of the Subsonic Vortex Flow in Slender Bodies <i>AIAA Journal</i> 23 (8):1150-6)	STEP 1B

Figure 11 A sample Move-Step analysis

I am grateful to Changyu Yang for bringing this text to my attention.

S1 The increasing interest in ... has heightened the need for ...

S2 Of particular interest and complexity are ...

Centrality claims are appeals to the discourse community whereby members are asked to accept that the research about to be reported is part of a lively, significant or well-established research area. Some typical examples of the linguistic exponents – and signals – of centrality claims are given below in abbreviated form. In all these cases, and in subsequent ones, the examples have been taken from actual RA texts, unless indicated otherwise.

Recently, there has been a spate of interest in how to ...

In recent years, applied researchers have become increasingly interested in ...

The possibility ... has generated interest in ...

Recently, there has been wide interest in ...

The time development ... is a classic problem in fluid mechanics.

The explication of the relationship between ... is a classic problem of ...

The well-known ... phenomena ... have been favorite topics for analysis both in ...

Knowledge of ... has a great importance for ...

The study of ... has become an important aspect of ...

The theory that ... has led to the hope that ...

The effect of ... has been studied extensively in recent years.

Many investigators have recently turned to ...

The relationship between ... has been studied by many authors.

A central issue in ... is the validity of ...

As the above list indicates, authors of a RA can make a centrality claim at the introduction's outset in a number of ways. They can claim interest, or importance; they can refer to the classic, favorite or central character of the issue; or they can claim that there are many other investigators active in the area. In the two corpora that I have examined in detail (Swales, 1981b; Swales and Najjar, 1987) the exercise of the Step 1 option was comparatively common, averaging a little under 50% for the combined sample of 158 introductions. It also seems quite widely distributed across various disciplinary areas, although exercised somewhat less in the physical sciences. Possible rationales for utilizing or avoiding a centrality claim remain an unexplored but interesting research area. Among the variables that might turn out to be relevant are the disciplinary area itself; some felt sense of the expectations of particular journals; the nature of the research itself (as when authors might want to

enhance with centrality claims a particular piece of research or scholarship that others might conceive of as marginal); or individual rhetorical predispositions for or against marked rhetorical activity of this type.

Centrality claims are typically communicated in a single sentence, but can, as Figure 11 shows, extend over two or more sentences. They are also typically, but not inevitably, introduction initial. In the following case, for instance, Steps 1 and 2 have been reversed:

- S1 An elaborate system of marking social distance and respect is found in the morphology of Nahautl as spoken in communities of the Malinche volcano area in the Mexican States of Tlaxcala and Puebla. (Step 2)
- S2 The complexity of the morphology involved, the semantic range of the elements, and variation in the system of use *raise questions of considerable interest* for our understanding of the form and function of *such systems*, both in Nahautl itself and in other languages. (Step 1, my emphases)

(J. H. Hill and K. C. Hill. 1978. Honorific Usage in Modern Nahautl. *Language* 54:123)

This introduction opens with a topic generalization of a Step 2 type, and as such does little to appeal to any but those with a specialized interest in Mexican languages. The 'repair', if one is needed, immediately follows, for the elements that I have italicized make a strong claim that the about-to-be-described findings are of central interest to sociolinguists of whatever areal specialty. It would doubtless have been possible to reverse the order in some way, or to have incorporated the substance of S1 into S2, although at the probable cost of increased syntactic complexity. My reading of the introduction as a whole suggests that the unusual order may be relatable to the authors' concern to establish early that their study was based on very extensive field work. This would explain the early circumscription to 'as spoken in communities of the Malinche volcano area', which in turn would explain the difficulty of initiating the introduction with Step 1.

The more general point raised by this particular text is that there are good general and applied reasons for assigning numerical sequence to textual elements that occur in suitably robust preferred orders. In the case of RA introductions the three moves occur at a high frequency in their assigned order. Swales and Najjar (1987) found, for instance, only 10 out of 110 introductions beginning with a Move 3. An anomaly percentage of under 10% is well within acceptable bounds in discoursal and textual studies for, even more than syntax, discourse is a phenomenon of propensities. Discourse generalizations are permeable to exceptions, and are not consequently falsified by limited numbers of counter-instances. Further, the occasional recurrence of minor dispreferred structures is

itself a phenomenon of interest both intrinsically and in terms of what it may reveal about the rationale behind the major preferred ones.

Step 2 has been labeled as *making a topic generalization* and represents a more neutral kind of general statement than Step 1. Step 2s can take a variety of forms, but generally fall into two categories: statements about *knowledge or practice*, or statements about *phenomena*. Representative authentic but abbreviated examples of the first group are:

The aetiology and pathology of ... is well known.

There is now much evidence to support the hypothesis that ...

The ... properties of ... are still not completely understood.

A standard procedure for assessing has been ...

Education core courses are often criticized for ...

Typically, these Step 2s express in general terms the current state of the art – of knowledge, of technique, or as in the case of the Figure 11 text, of current requirements for further progress. The second group of topic generalizations refers to phenomena, such as:

... is a common finding in patients with ...

An elaborate system of ... is found in the ...

English is rich in related words exhibiting 'stress shifts'.

There are many situations where ...

As these examples indicate, there is a strong tendency for *phenomena* topic generalizations in particular to establish territory by emphasizing the frequency and complexity of the data – indeed I suspect that if territory cannot be easily established in this way a Step 2 of this type would be dispreferred. Consider these alternates:

a1) English is *rich* in related words exhibiting 'stress shifts'.

a2) English is *poor* in related words exhibiting 'stress shifts'.

b1) There are *many* situations where examination scripts are marked and then re-marked by another examiner.

b2) There are *few* situations where ...

In the case of (a2) the bald announcement of the relative absence of the phenomenon would seem, *prima facie*, to surrender territory; we would therefore be more inclined to expect that the author would attempt to regain ground with something like:

a3) English is *surprisingly poor* in related words ...

for we can now expect some interesting account of the *reasons* for the impoverished phenomenon. The (b) example, which is discussed in detail

in Swales (1987b), is the opening sentence from an experiment in exam re-marking. In that context, it would make little rhetorical sense to imply (by using 'few') that the phenomenon under investigation is of marginal status. On the other hand, territory might be established via the insertion of a couple of small words that carry implications of being 'state of the art':

- b3) There are *as yet* few situations where ...

The author may then be in a position to advance a knowledge claim that the innovation is provisionally promising.

Indeed, we see the precise mirror-image of the frequency requirement in studies that purport to be case reports. Here is a skeletalized version of an introduction in a medical journal:

- S1 Primary malignant ... lymphomas of ... are very rare.
- S2 In the Department of ..., this diagnosis has been made in only ten patients during the last fifty years.
- S3 Retrospective analysis showed that one of these cases showed a very uncommon ...
- S4 A review of the recent literature yielded only two reports of ...: both of these reports paid little attention to clinical details.
- S5 L... reported fourteen cases, ... but in this report, too, clinical data are not given.
- S6 In the present report we shall describe the chemical ... findings in a case of ... lymphoma ... with an uncommon clinical course.

It is often believed that straightforward research reports begin with a straightforward thesis statement or statement of purpose. While it might have been possible for the authors of this article to have so begun by opening with S6, study of the above text shows clearly enough why they chose to establish both a territory and a niche before S6. Most obviously, given the fact that they had one case to report – and presumably one dug out of the archives at that – they were under some rhetorical pressure to establish its rarity and possibly near uniqueness. After all, the chances of being able to publish in a RA (as opposed to a textbook) a description of a single standard case of a standard disease are likely to be very slim. Hence, we are not surprised to find the authors' endeavoring to establish in the first three sentences the rarity of the phenomenon in their own medical institution, and then to go on to note that the mere 16 cases found in the literature lack clinical details. The complex establishment of the first five sentences thus prepares the discourse community to accept that there is indeed a niche being occupied in the introduction's final sentence.

The third step in establishing a territory is the review of one or more items deemed by the authors to be relevant to that establishment. Apart from at least one exceptional disciplinary area, minimal reference to previous work is the obligatory step in Move 1, while the other steps, from a corpus perspective, are discretionary. One exception to a strong literature-citation requirement was discovered by Cooper (1985) in her study of IEEE publications dealing with advances in computer technology. Four of her 15 introductions did not contain a Step 3, and she suggests that there may be a number of special circumstances that account for the light referencing: the fact that the field is relatively new and has little accumulated research tradition; and the fact that there is heavy commercial involvement in the field; and the fact that work tends to be product-related rather than concerned with hypotheses *per se*. Cooper's findings are interesting because they suggest that evolving discourse communities on the periphery of the academic world may be developing alternative conventions for their central genres.

The Step 3 is one of the main occasions where the RA author needs to relate *what has been found* (or claimed) with *who has found it* (or claimed it). More precisely, the author needs to provide a *specification* (in varying degrees of detail) of previous findings, an *attribution* to the research workers who published those results, and a *stance* towards the findings themselves. My earlier attempts (e.g. Swales, 1981b) to provide a useful account of the attribution variables – and their typical tense correlates – have not fully withstood the test of critical commentary (particularly by Jacoby, 1987) and I now offer a modified position. The basic distinction I would wish to make is between *integral* and *non-integral* forms of citation. The distinction has the merit of being easily applicable because it depends merely on recognizing surface features of text. An integral citation is one in which the name of the researcher occurs in the actual citing sentence as some sentence-element; in a non-integral citation, the researcher occurs either in parenthesis or is referred to elsewhere by a superscript number or via some other device. The main patterns are illustrated with constructed examples in Figure 12.

The integral citations show the name of the researcher as subject (Ia), passive agent (Ib), as part of a possessive noun phrase (Ic and d) and as what Tadros (1985) calls 'an adjunct of reporting' (Ie). The non-integral citations show three parenthetical citations and two superscripted ones. In Figure 12 the citations all in fact occur at sentence-final position, but scrutiny of technical RA introductions will reveal instances of other placements, especially when groups of researchers and related topics are introduced (as in sentence 4 of the Figure 11 text). The final type of non-integral citation listed in Figure 12 (Nf) was, to my knowledge, first discussed in the discourse analysis literature on RAs by Jacoby (1986).

Integral	Non-integral	
Ia Brie (1988) showed that the moon is made of cheese.	Na Previous research has shown that the moon is made of cheese (Brie, 1988).	
Ib The moon's cheesy composition was established by Brie (1988).	Nb It has been shown that the moon is made of cheese (Brie, 1988).	+R
Ic Brie's theory (1988) claims that the moon is made of cheese.	Nc It has been established that the moon is made of cheese. ¹⁻³	
	Nd The moon is probably made of cheese (Brie, 1988).	
	Ne The moon may be made of cheese. ¹⁻³	
Id Brie's (1988) theory of lunar composition has general support.		
Ie According to Brie (1988), the moon is made of cheese.		-R
	Nf The moon may be made of cheese (but cf. Rock, 1989).	

Figure 12 Integral and non-integral citation

She labeled such references as 'contrastive' because they go against the drift of the conclusions being reached in the sentence itself. Contrastive references seem very unevenly distributed in academic writing. They are, for example, very uncommon in the scientific areas, but quite common in scholarly legal commentary. In the humanities, they seem to form part of some academics' writing style, but rarely, if ever, occur in the writings of others. Of Jacoby's six texts dealing variously with literary research, only two made much use of contrastive references but these two texts employed them quite consistently. They are worth further study.

Jacoby (1987:55) also proposes a category of reference which she calls *summary*: 'In these references no particular research predecessor is named, as a rule, but clear reference to the state of previous research as a whole or to the state of consensus knowledge can be identified'. I have not adopted this particular proposal. In cases where no previous researchers are specifically cited, I see no reason not to assign the text to Step 2 (topic generalization). In cases where specific previous researchers are cited, text elements can usually be assigned to one of the categories illustrated in Figure 12. As Jacoby implies, the problematic cases occur when the writer refers to groups or 'schools' of researchers and scholars. Compare the following:

- a) Generative grammarians have recently modified their position.
- b) Generative grammarians influenced by Chomsky have recently ...
- c) Chomskyan grammarians have recently ...
- d) Chomsky and his co-workers have recently ...

Even in the case of (d) the most workable assignment procedure would seem to be one that asks whether there is an actual citation or not. If there is, as in:

- e) Chomsky and his co-workers (e.g. Napoli, 1988) have recently ...

then it falls under one of the Figure 12 categories (i.e. Nd). If there is none, as in (d), then it is not a citation.

The final column in Figure 12 is labeled +R or -R. The +R citations are *reporting*; that is to say the RA author employs a 'reporting' verb (show, establish, claim, etc.) to introduce previous researchers and their findings. In the lower sections of the figure, the citations are *non-reporting* (-R). The dichotomous classification works fairly well except for uncertainties that can arise with a small set of verbs, particularly *find* and *be associated with*. For example, we can give two possible readings to the sentence: 'X was found to be impaired' (Sang et al. 1972). We could read this as *reporting*:

- a1) X was found by Sang et al. (1972) to be impaired.
- a2) Sang et al. (1972) found that X was impaired.

Alternatively we could read it as *non-reporting*:

- b1) X was impaired (Sang et al., 1972).
- b2) Impairment of X occurred (Sang et al., 1972).

This existential reading has affinities with such common uses of *find* in the passive as: 'Coal is found in the ground' (i.e. coal occurs in the ground).

As we have already seen, Bazerman (1984a) noted a firm trend from reporting to non-reporting citations in the *Physical Review* during this century. However, this trend may be partly due to the fact that the *Physical Review* uses a numerical/superscript system. Such systems do not easily permit integral reporting choices:

? Reference 3 established that the moon was made of cheese.

In 16 biological and medical RAs from the 1970s, I found that the non-reporting/reporting ratio was only 40–60 (Swales, 1981b) whereas Jacoby (1987) found a 25–75 ratio among her literary critics. The survival of both integral and non-integral *reporting* structures can fairly clearly be attributed to their considerable discriminatory power. In the first place, the repertoire of reporting verbs that an author can draw on is quite large (around 50 possible candidates) ranging from highly frequent choices such as *suggest*, *report* and *show* to rarities like *asseverate*. Secondly, this class can be broadly divided into two main groups; those whose use asserts the author's commitment to the attendant proposition (*show*, *demonstrate*, *establish*, etc.) and those whose use carries no such commitment (*suggest*, *propose*, *examine*, etc.). The distinction is a powerful rhetorical tool in authors' attempts to create research spaces for themselves, because it allows them to signal early whether claims are to be taken as substantiated or not. Thirdly, the incorporation of a reporting verb concomitantly involves a choice of tense, the selection of which may be highly indicative.

In fact, EAP studies of references to previous research have tended to focus on providing an account of tense and aspect usage (Lackstrom et al., 1972; Swales, 1981b; Oster, 1981; Ard, 1982, 1985; Een, 1982; Trimble and Trimble, 1982; Malcolm, 1987). These studies have examined in particular the use of the three forms, the Past, the Present Perfect and the Present Simple, that together realize over 90% of all finite verb usages in citational statements. There are, I believe, three broad kinds of response to the issue of tense usage in this literature. One is to say that the 'general rules' are largely adequate (Ard, 1982 and 1985; Malcolm, 1987). A second approach, best illustrated by Oster (1981), proposes a special set of explanations of tense/aspect that are closely associated with the nature of the claims being made about the previous literature. The third approach (Swales, 1981b; Een, 1982) has argued that the use of tense/aspect in referenced statements is best explained in terms of where and how the reference to the previous researcher is introduced into those statements.

We might begin with the commonly-made observation that the 'general rules' for the Past, Present and Present Perfect seem to be less powerful in expository texts than in narrative ones, this being presumably due to the fact that time-lines and time-sequences, which are

important elements in the traditional explanations, are more prominent in narratives. Thus in a *story* the following three statements might easily be explained in terms of ‘general rules’:

- A disagrees with B.
- A has disagreed with B.
- A disagrees with B.

However, in the context of a report on an academic debate with, say, A being Halliday and B Chomsky, we can see, as Lackstrom et al. (1972) have observed, that the three statements are typically interpreted not in terms of increasing present-ness or increasing relevance to the present, but in terms of increasing *generality*. It is the perceived role of such concepts as *generality* and *relevance* that has led to the second approach, which so far has reached its fullest published form in Oster (1981). She proposes the following principal hypotheses:

- i) The Present Perfect tense is used to claim *generality* about past literature. The Past tense is used to claim *non-generality* about past literature.
- ii) The Past tense is used when it refers to quantitative results of past literature that are *non-supportive* of some aspects of the work described in the technical article. The Present tense is used when it refers to quantitative results of past literature that are *supportive* or *non-relevant*.
- iii) The Present Perfect tense is used to indicate the *continued discussion* of some of the information in the sentence in which the Present Perfect tense occurs.

(Oster, 1981:77)

Although Oster’s sample is small (two articles from chemical engineering), she is able to show that the above hypotheses apply quite well to her texts. The first hypothesis, of course, fits well with general accounts – and we have already seen many instances of the Present Perfect in broad centrality claims and topic generalizations. Of the other two hypotheses, the third is the more interesting because it provides a discoursal rather than a semantic/sentential explanation via its suggestion that the Present Perfect can operate as a *signal* to the reader to expect further discussion of the topic. In Swales (1981b) I attempted to validate this claim in 16 biological and medical papers, but without much direct success. However, I did find several instances of a corollary to Oster’s hypothesis: the Past tense following a Present Perfect (or series of Present Perfects) in a discussion of a particular piece of research is apparently used to indicate that discussion is *terminating*.

The more general difficulty of an account such as Oster’s, particularly from a pedagogical point of view, is that of deciding what is going to

'count as' *continued discussion* and so forth. In order to provide a simple 'safe rule' alternative (Selinker et al., 1985) I provided a correlational match that, in terms of the updated classification of references provided here, would look like Figure 13.

	Integral	Non-integral
reporting	<i>Past</i> Brie (1988) showed that ...	<i>Present Perfect</i> <i>It has been shown that ...</i> (Brie, 1988)
non-reporting		<i>Present (or modal)</i> <i>The moon may be made of cheese</i> (Brie, 1988)

Figure 13 Reference and tense

This account matches tense with an easily identifiable structural feature and thus offers a serviceable 'rule of thumb' to non-native writers. However, it only exhibits statistical tendencies: Een (1982) in a follow-up study of geotechnical texts found confirmation as far as non-reporting and non-integral reporting citations were concerned, but in integral reporting citations in introductions the occurrence of the Past fell to 50%.

Malcolm (1987) adopts the usefully eclectic position that there is need for both 'general' rules (as outlined by Celce-Murcia and Larsen-Freeman, 1983; Comrie, 1985; etc.) and 'special' ones. She proposes, for instance, that 'an adequate theory of tense usage in EST discourse needs to account not only for obligatory constraints on tense usage, but also for strategic choices that provide authors with the capability of manipulating temporal references for their own rhetorical purposes' (1987:32). Although Malcolm, unlike Swales, starts from a generalist perspective, her findings on the use of tense in citational text – based on the *Journal of Pediatrics* – turn out to be comparable. She puts forward three hypotheses:

1. Generalizations (as indicated by verbs without researcher agents) will tend to be in the Present tense. (Found to be 74% true in her sample.)
2. References to specific experiments (as indicated by a researcher agent and a footnote to a *single* study) will tend to be in the Past tense. (Found to be 61% true in her sample.)
3. References to areas of inquiry (as indicated by agents and/or foot-

notes to more than one study) will tend to be in the Present Perfect tense. (Found to be 74% true in her sample.)

As Een (1982) also found, the greatest variability occurred in the data for hypothesis 2. This is not surprising because commentary on a single paper is a key location for strategic tense choice:

- a) Malcolm pointed out that there is both constraint and choice in tense usage⁴.
- b) Malcolm has pointed out that ...
- c) Malcolm points out that ...

Ard (1985) correctly warns us – note use of tense! – against attempting to determine a rationale for such choices in specific instances, but I believe most readers would see the progression from (a) to (c) as being one of some kind of increasing proximity. The reasons for an author choosing the more remote (a) form may (or may not be) complex: choice could relate to placing the cited author's work in a chiefly historical context; it could emphasize concern with text rather than content; it could de-emphasize relevance to present concerns; or it could prepare the way for critical discussion. Whatever the reasons, the tense choice may indicate something of the author's *stance* towards the cited work, and it is probably this facility, allied to a rich choice of lexical verbs, that continues to make reporting structures attractive to RA authors.

We can now consider in a little further detail Move 2 of the CARS model (Figure 10). We can begin with the introduction illustrated in Figure 11, for this provides an elaborate example of the rhetorical work undertaken to establish a niche for about-to-be-presented research. The key signals are repeated in skeletal form below:

- S8 *However*, the previously mentioned methods *suffer from some limitations* ...
- S9 The first group ... *cannot treat* ... and is *limited to* ...
- S10 The second group ... is *time consuming* and therefore *expensive*, and its ... is *not sufficiently accurate*.
- S11 Both ... *suffer from the dependency on* ...
- S12 The ... method (upon which the present study is based) eliminates many of these limitations by ..., *but* it can treat *only* ...

As we can see, the move opens with an adversative sentence-connector. Across various samples of RA introductions, about a quarter of Move 2s are initiated with such signals, most commonly *however* but also *nevertheless*, *yet*, *unfortunately* and *but*. The type of Move 2 is clearly 1B – that of *indicating a gap*. The author does not counter-claim that the previous work is hopelessly misguided, but rather 'suffers from some limitations'. Most of the gaps are signaled lexically in the verb (*suffer*; *is limited to*) or in adjective phrases (*time consuming*; *expensive*; *not*

sufficiently accurate). There is also a case of verb negation in S9 (*cannot treat*).

In fact, the linguistic exponents of establishing a niche are extremely interesting and have not yet received the attention they deserve either from a general or an applied perspective. In a 'quick and dirty' survey of 100 Move 2 instances drawn from a range of fields (physics, geology, psychology and composition) the means of niche-establishment broke down into the following categories, which are listed in order of decreasing frequency.

a) *Negative or quasi-negative quantifiers* (28 instances)

no	12
little	7
none (of)	4
few / very few	4
neither ... nor	1

Interestingly, these quantifiers either occurred sentence-initially (or following an adversative), or were merely preceded by existential *there*, as in: 'However, there is little research that ...'. It is quite possible, therefore, that the somewhat preferred choice of the negative quantifier format may be connected, at some level of consciousness, with a wish to signal *early* that a niche is now being established.

The other common exponent employs lexical negation or quasi-negation.

b) *Lexical negation* (26 instances)

Verbs	15	(fail 5, lack 2, overlook 2, plus 6 single instances)
Adjectives	7	(inconclusive, complex, misleading, elusive, scarce, limited, questionable)
Nouns	3	(failure 2, limitation)
Other	1	(without regard for)

The subtlety of the verbal repertoire available at this juncture is probably connected with the frequency of its adoption. The RA author may opt for a trenchant *fail*, imply oversight with *neglect*, *overlook* or *underestimate*, suggest complacency with *be content to*, impute a certain narrowness of vision with *concentrate on*, *be restricted to* or *be limited to*, or offer rather more sympathetic understanding with *constrain*.

c) *Negation in the verb phrase* (16 instances)

not	14
rarely	1
ill	1

The relative infrequency of this type of exponent, at least compared to so-called 'general English', might suggest that it is somewhat contraindicated when referring to the work of others. It is possible that the use of *not* in conjunction with many verbs is seen as providing a potentially hostile depiction of previous work. Indeed, this possibility is strengthened when we note that as many as five of the 16 instances employed *we* as the subject as in 'We do not yet know ...'. The inclusive *we* obviously includes the present authors as co-members of the unsuccessful group.

The remaining minor ways of establishing a niche can be listed together.

d) *Questions* (8 instances)

Direct	6
Indirect	2 (e.g. 'A question remains whether ...')

e) *Expressed needs/desires/interests* (8 instances)

- e.g. 'The differences need to be analyzed ...'
- 'It is desirable to perform test calculations ...'
- 'It is of interest to compare ...'

f) *Logical conclusions* (6 instances)

Must	3 (e.g. 'This must represent ...')
Seem/appear	2 'One would intuitively expect ...'

g) *Contrastive comment* (6 instances)

- 'The research has tended to focus on ..., rather than ...'
- 'They center mainly on ..., rather than on ...'
- 'Studies most often contrast ..., rather than ...'
- 'Researchers have focused primarily on ..., as opposed to ...'
- 'Emphasis has been on ..., with scant attention given to ...'
- 'Although considerable research has been done on ..., much less is known as to ...'

h) *Problem-raising* (2 instances)

- 'The application presents a problem ...'
- 'A key problem in many ... is ...'

An (e) or (f) format seems to be chosen when there is a weaker challenge to the previous research, as we most typically find in a *continuing a tradition* Step 1D. Clear evidence of this is the quite frequent co-occurrence of the sentence connector *therefore* (rather than *however*) in these contexts. Formats (e) and (f) were much commoner in physics and, to a lesser extent, in geology than in psychology or composition. On the other hand, most of the instances of what I have called *contrastive comment* (g) occurred in composition research. The milder tone of the (g) examples may be connected to the fact that composition researchers

comprise a relatively small and mutually supportive discourse community and one not without its external antagonists and detractors.

The underlying theme that has linked this discussion of the RA introduction has been a felt sense that the typical introduction is a crafted rhetorical artifact. At the published textual level, the introduction is a manifestation of rhetorical maneuver. The extent of this rhetorical work can often be seen when we compare expert and non-expert products. The text presented in Figure 14 was written by a Japanese masters student as an exercise in writing research introductions for one of my courses (Swales, 1990).

	(1) The 55 mph National Maximum Speed Limit on highways was decided and became effective after the historic Arab oil embargo in 1974. (2) Though it was originally a temporary measure to conserve energy the government decided to make it permanent because of its great contribution to highway safety. (3) In 1984, after a decade had passed, the Transportation Research Board made a research about the 55mph speed limit, and recommended that the federal government continue the low speed limitations because of its safety benefit.
MOVE 1	(4) However, the low maximum speed limit imposes some burdens on drivers and social economy. (5) The additional travelling time caused by the lower speed limit increases costs of freight transportation, especially in rural states where average length of trips is longer.
MOVE 2	(6) The purpose of this research is to find out the gross national economic defects of the 55mph National Maximum Speed Limit on highways.
MOVE 3	

Figure 14 An NNS introduction

The above text is, I would suggest, a quite effective piece of writing, especially if we bear in mind that the student had had no prior English-medium academic experience and was only two months into a US degree program. Certainly it suffers from few of the orientation problems found in Scarella (1984) in NNS introductions; there is little unnecessary background information and there is adequate use of attention-getting devices. Although there are occasional 'off-register' elements such as *made a research about* in S3, the text moves smoothly, swiftly and quite authoritatively towards the announcement of the research topic in the final sentence.

However, it remains the case that the introduction remains somewhat flat in the second half. The author seems to have somehow missed an opportunity to highlight the gap between surmise and substantiated opinion, and between present qualitative judgments and potential quantitative ones. We can see this if we compare the original final three sentences with more 'modulated' variants (Latour and Woolgar, 1979).

S4-original

However, the low maximum speed limit imposes some burdens on drivers and social economy.

S4-modulated

However, *it would seem* that the low maximum speed limit imposes *a certain amount* of burden on drivers and the social economy.

S5-original

The additional travelling time caused by the lower speed limit increases costs of ...

S5-modulated

In particular, the additional travelling time caused by the lower speed limit *can be expected* to increase costs of ...

S6-original

The purpose of this research is to find out the gross national ...

S6-modulated

The purpose of this research is to *arrive at a preliminary quantitative estimate* of the gross national ...

The relatively minor alternatives that I have been proposing take fuller advantage, I believe, of the opportunity to create a research space via more precise specification of the gap and of the attempt to fill it.

The final issue with regard to Move 2 is its cyclicity. A number of investigators (Cooper, 1985; Crookes, 1986a; Hopkins and Dudley-Evans, 1988) have pointed out that niche-establishment does not necessarily occur only at the end of a literature review, but may follow reviews of individual items, so that cycles of Move 1/Step 3 and Move 2 recur. Consider the Figure 11 text for a final time. As we have seen, the author of this text opted for a composite 'chunked' Move 1/Step 3 followed by a composite Move 2. He could have opted instead for a cycling solution to his effort to create a research space (Figure 15).

In our present state of knowledge, it is not possible to do more than speculate about the factors that might predispose authors to choose composite or cycling configurations. It is likely that the length of the introduction plays some part, so that the longer the introduction the greater the probability of some recycling (Crookes, 1986a). It is also likely that choice is influenced by how the research field is perceived. If the relevant research tradition is viewed as linear and cumulative, then a composite arrangement may work well. However, if the field is viewed as

1–3	Among the many potential flow methods developed in attempting to solve body vortex flows are early two-dimensional 2D multi-vortex methods. ^{2–4}
2–B	However, these methods cannot treat 3D flows and are limited to very slender bodies. An alternative is time-stepping vortex models that include
1–3	boundary-layer considerations, ^{5–8} but these are time consuming and therefore expensive. In addition, their
2–B	separation predictions are not sufficiently accurate. A third possibility is a quasi-3D potential flow method ⁹ that uses source
1–3	and vortex elements. Unfortunately, this method, like the previous ones, suffers from the dependency on too many
2–B	semiempirical inputs and assumptions ...

Figure 15 An example of cyclicity

branching – consisting of several loosely-connected topics – then a cyclic approach may be preferred. The combination of length and divergence may contribute to the cyclicity more evident in the social sciences, and brevity and linearity to the compositeness more characteristic of the natural and life sciences and of engineering.

We can now turn briefly to Move 3, which I have labeled *occupying the niche* (Figure 10). The role of Move 3 is to turn the niche established in Move 2 into the research space that justifies the present article. The link between the moves is a strong one. Whenever a Move 2 occurs – and there is a minority of instances in which it does not (Swales, 1981b; Cooper, 1985; Crookes, 1986a) – the ensuing Move 3 variously offers to substantiate the particular counter-claim that has been made, fill the created gap, answer the specific question or continue the rhetorically-established tradition.

The obligatory element in Move 3 is Step 1. This can take one of two predominating forms:

- Step 1A The author or authors indicate their main purpose or purposes.
- Step 1B The author or authors describe what they consider to be the main features of their research.

In both cases the opening step is a kind of promissory statement, and in both cases its onset is typically marked by (a) the absence of references to previous research and (b) the use of deictic references to the present text. The more common deictic elements, in approximate decreasing order of frequency, are: *this, the present, we, reported, here, now, I and herein*. Typical examples culled from RA introductions are:

- a) This paper reports on the results obtained ...
- b) The aim of *the present* paper is to give ...
- c) In *this* paper we give preliminary results of ...
- d) The main purpose of the experiment reported *here* was to ...
- e) *This* study was designed to evaluate ...
- f) The *present* work extends the use of the last model ...
- g) *We now* report the interaction of ...

There are a number of comments that can be made about the language of Move 3s. First, there is a strong tendency for the deictic signal to occur *early* – as the above examples show – and, in general, the only items that precede them are occasional linking phrases such as ‘In view of these observations’. Of the 48 introductions in the 1981 corpus, there was only one in which the deictic *in this paper* phrase occurred at sentence-final position. However, apprentice writers, both NS and NNS, are more prone to delay the Move 3 signal – and by doing so likely to create uncertainty in the reader. Secondly, there may be an opportunity, depending somewhat on style-sheet instructions, for using either a standard descriptive form or a *collapsed* structure:

- a) In this paper, we argue that ... (standard)
- b) This paper argues that ... (collapsed)

Although collapsed structures are quite common, there are little-understood constraints on the co-occurring verb:

- This paper utilizes the notion of ...
- ? This paper hopes to show that ...
- ?? This paper measures the extent of ...

There is also some evidence that the co-occurrence of inanimate subject and animate verb varies in its acceptability from one language to another. Kojima and Kojima (1978), for example, argue that it is dispreferred in Japanese and concomitantly produce evidence that Japanese scientists tend to avoid collapsed structures when they write in English.

A third observation concerns tense in purposive Step 1s. In cases where the deictic refers to the *genre* (paper, report, note, review, etc.) tense is restricted to the present. However, in cases where the deictic refers to the type of *inquiry* (investigation, study, experiment, etc.), authors may choose between *present* and *past*:

- The purpose of this investigation is to ...
- The purpose of this investigation was to ...

For reasons comparable to those discussed in connection with reporting previous research, there seems to be a strong preference for a Present tense copula, presumably because it encourages an impression of contemporary relevance.

Many, perhaps most, RA introductions end with a Move 3-Step 1. There are, however, two further options. One is to follow the Step 1 with a summary announcement of the principal findings. Swales and Najjar (1987) investigated this option in terms of quite a large sample (110 introductions) drawn from two very different fields: physics and educational psychology. They found that the Step 2 option was utilized 45% of the time in physics but only 7% of the time in educational psychology. We seem to see here, then, a quite marked disciplinary divergence that has not so far been attested for either type or form in Step 1.

A final option in the introduction is to indicate in varying degrees of detail the structure – and occasionally the content – of the remainder of the RA. If Step 3 occurs, it is always at the end of the introduction. Examples are:

- a) We have organized the rest of this paper in the following way ...
- b) This paper is structured as follows ...
- c) The remainder of this paper is divided into five sections. Section II describes ...

Cooper (1985) found a Step 3 in as many as 10 out of her 15 IEEE introductions and was further able to report that specialist informants in the computer technology field both expected and welcomed such indications of organization. In most other fields, the percentage of introductions closing with Step 3 seems to be much lower. The high incidence in Cooper's study may well be connected to the absence of an established schema for research reporting in a new and rapidly evolving field.

In this section I have made some fairly bold claims about the rhetorical organization of RA introductions across a range of fields. The most obvious way of validating these claims is to test them out on new data. To this end, I have examined how well the model fits with the RAs in the latest journal I received at the time of writing. That journal was *Research in the Teaching of English* for February 1988, RTE also incidentally covering a disciplinary area (composition research) little studied in the literature on introductions. The February 1988 issue contains four research articles, the first exceptionally long, the other three of normal length. The basic 'facts' about the four introductions are given in Table 6.

The introduction to the long 35-page opening article (Berkenkotter et al.) follows closely both the Move-Step sequences and the linguistic signals that previous research tells us to expect.

TABLE 6. FOUR INTRODUCTIONS FROM RTE

Author(s)	No. of paras	No. of lines	No. of refs (including repeats)
1. Berkenkotter et al.	5	71	22
2. Slater et al.	13	129	43
3. Cordeiro	4	44	7
4. Roen and Willey	9	125	39

RA 1 OUTLINE STRUCTURE

	Move-Step		Signals (my emphases)
Para. 1	1-1	(S1)	Recently there has been <i>considerable</i> interest in ...
	2-1B	(S3)	Yet there is a <i>dearth</i> of information
	2-1C	(S4/5)	(2 direct questions)
Para. 2	1-3	(S1)	The existence ... may be inferred from recent studies ...
	1-3	(S2)	The work of these researchers sheds light ...
Para. 3	1-3	(S1)	Composition scholars Bizzell (1982c, 1983b) and ...
	2-1B	(S3)	Yet understanding ... is only part ...
	1-3		...
Para. 4	1-2	(S1)	Understanding ... appears, therefore ...
	2-1C	(S2/S3)	(2 direct questions)
	1-3	(S4)	Recent studies have begun to explore these questions at an undergraduate level.
	2-1B	(S5)	Understanding ... in the context of ..., however, is as yet unexplored territory.
Para. 5	3-1B	(S1)	<i>This study attempts to enter this territory by ...</i>
	3-1B	(S2)	<i>Our focus will be on ...</i>
	3-2	(S5)	<i>The changes that ... show a skilled novice learning ...</i>

As we might expect in an introduction of around 70 lines, a considerable amount of cycling occurs, especially with regard to gap-indications. There are, in fact, four instances of a Move 2 segment in the first four paragraphs; these are of increasing specificity and it is the final one that

specifically establishes the niche that Berkenkotter et al. are attempting to fill. And I take it as serendipitous that the metaphor they choose is the geographic one of 'unexplored territory'! The final paragraph is given over to Move 3; it contains the only first person pronominal form in the entire introduction and concludes with a general comment about the main findings.

The second introduction poses rather more of a problem for the CARS model, but, as I shall hope to show, for rather interesting reasons. The Slater et al. introduction can be outlined as follows:

RA 2 OUTLINE STRUCTURE

	<i>Move-Step</i>		<i>Signals (my emphases)</i>
Para. 1	1–2	(S1)	<i>Recently, the relationships between ... have been explored by scholars from a number of disciplines.</i>
	1–3	(S2)	
	1–2	(S3)	
Para. 2	2–1B	(S1)	<i>However, the precise nature of ... has not been delineated.</i>
		(S2)	<i>... a considerable amount of research has been ... but little research ...</i>
		(S3)	<i>a considerable amount of research ... , while a minimal amount of this research</i>
		(S4)	<i>As a result, no comprehensive theory appears to exist.</i>
Paras 3–8	1–3		
Para. 9	1–2	(S1)	<i>Taken together, these studies indicate ...</i>
Paras 10–12	1–3		
Para. 13	3–1A	(S1)	<i>The purposes of the present study were two-fold:</i>
	?	(S2)	<i>The study thus extends the findings of previous work by examining ...</i>

After an opening broad paragraph, the introduction proceeds (with one exception) to review previous research until it reaches the final paragraph, the review typically consisting of a series of paired statements like 'X examined the effects of ... Results indicated that ...' The main exception is the second paragraph, which is a highly elaborate Move 2 designed to establish that 'no comprehensive theory' exists. The outline shows that there are no further *closing-in* Move 2s, especially immediately prior to the onset of Move 3 – unlike in the first introduction. A close reading of the text brings out the unusual character of this

introduction. The 'no comprehensive theory' argument turns out not to be establishing a niche to be filled, because the authors in fact never return to the issue of the need to start moving towards such a theory, nor do they ever claim that their present paper can be seen as making a contribution to theory. Rather, the second paragraph seems to be designed to justify the fact that it is appropriate in the circumstances to add a small further piece of empirical evidence to the puzzle. In other words, what we seem to have here is a new sub-type of Move 2-Step 1B which, by claiming that *the gap is currently unfillable*, obliquely establishes a continuing-a-tradition research space. This reading also brings the final sentence into focus, which was left unassigned in the outline. At first sight, the sentence looks as though it might be operating as an atypically-placed Move 2-Step 1D (finding extension). However, we can now see it as a typically closing Move 3-Step 1B for it *announces* the (limited) status of the present research.

Analysis of this kind will, on occasion, bring to light ambiguity and rhetorical uncertainty. If the above analysis is on the right lines, then it seems definitely odd that the authors did not take up the implication of their second paragraph in any of the ensuing 11. Indeed, one might even suggest that the introduction would have been tidier and easier to process if they had done so. Here is what might have been:

Para. 13 The preceding review suggests that further empirical research is necessary before a comprehensive theory can be developed. In order to develop the research base, the present study was designed with the following two purposes in mind: ... The study thus extends the findings ...

The third introduction (Cordeiro) is the shortest and has the following anomalous structure:

RA 3 OUTLINE STRUCTURE

	Move-Step		Signals (my emphases)
Para. 1	3-1B	(S1)	<i>This study ... is concerned with ...</i>
	3-1B	(S2)	Specifically, it addresses ...
	1-3	(S3-6)	
	3-1B	(S7)	<i>In the present research, ...</i>
Para. 2	1-3		
Para. 3	1-3		
Para. 4	1-3	(S1-4)	
	2-1B	(S5)	However, the terms ... were not semantically or syntactically descriptive enough to solve ...

As the outline shows, this introduction opens with a Move 3. Although this option is certainly possible, it does not seem as generally common as many might suppose. It occurred, for example, just 10 times in a corpus of 110 introductions (Swales and Najjar, 1987), and on several occasions already in this section I have alluded to both the likely concerns to *create* a research space, and the perils of failing to engage the wider discourse community by too narrowly focused an opening. Something of this peril can be imagined by considering the opening sentence in full: 'This study of the writing of 22 first graders and 13 third graders is concerned with how children learn the rules of punctuation' (1988:62). The readers of this sentence are immediately faced by specifics which, while strongly appealing to a few, are likely to disengage many with no direct interest in this research topic. Apart from the promotion of Move 3, the introduction largely follows the model; for instance, it closes with the Move 2, which seems typical of the relatively few introductions of this type that have been examined.

There are, in fact, a number of interesting research questions related to introductions that begin with a Move 3. Are they processed and composed differently? Can they be associated with less experienced writers, or with those who feel, for whatever reason, less need to establish a territory? Are they more likely to occur in situations where the RA is a result of a research grant, given the widespread expectation in research grant applications that there should be *early* indications of what will be done? And if so, are they consequently on the increase?

The final introduction (Roen and Willey) is almost as long as the second but has in fact almost no cycling. The first seven of the nine paragraphs establish the territory. The final two paragraphs have the following structure:

RA 4

	<i>Move-Step</i>		<i>Signals</i> (my emphases)
Para. 8	2-1B	(S1)	Despite the announced importance of ..., few researchers have experimentally tested ...
	1-3	(S2-4)	Of course many studies have examined ...
	2-1B	(S5)	No study, however, has used an experimental design.
Para. 9	1-3	(S1)	In the light of this absence of experimental work, we conducted a pilot study (Roen, 1985) ...
	3-1A	(S2)	We subsequently designed the present study, an experiment to test ...
	3-1B	(S3)	Our two research questions were ...

The only unexpected aspect of this introduction is the character of the opening sentence in the final paragraph. Its opening phrase evinces close links with the Move 2, and it further builds up expectations of Move 3 by the switch into *we*. On the other hand, the Past Simple and an earlier dated reference disconfirm those expectations. In effect, the opening of the final paragraph communicates a somewhat mixed message. However, this is *precisely* what we might expect when authors use, as a transition, discussion of their *own* previous work which is directly and causally related to the study actually being presented.

In general, therefore, the four test introductions usefully confirm the claims made for the CARS model, particularly in terms of the linguistic exponents used to express moves and their associated steps. In outline the first and last introductions fit well, while the third appears to be a fairly typical example of the *fronted-Move 3* subtype. The fact that 25% of a very small sample opened with a Move 3 needs a little further investigation. Accordingly, I checked the 16 RAs published in *RTE* in 1987 and found that 13 used the standard placement for Move 3, two were fronted, while the remaining article was hard to categorize as it used an anecdote from the study as an attention-getting opening. If we leave this last aside, three out of 19 *RTE* RAs were *fronted*; certainly a higher proportion than found in Swales and Najjar (1987), but still under 20%. The major anomaly occurred in the second introduction, where there was no Move 2 that could be related to Move 3 in any but the most indirect of ways. It was suggested that this mismatch did not so much represent a possible weakness in the *Create a Research Space* model as a possible weakness in the introduction itself and, moreover, one that the analytic procedures themselves assisted in revealing.

7.5 Methods

In 7.2, 'The constructing of research articles', I have already presented some general findings on the language of Method sections. Here I attempt to characterize the discourse of this part of the RA genre a little more explicitly and to draw some tentative conclusions about disciplinary variation. We can begin with the opening sentences of a biochemistry Method section cited by Gilbert and Mulkay (which is very similar to the one from Knorr-Cetina discussed in 7.2):

Heavy beef heart mitochondria were prepared by the method of Wong and stored in liquid nitrogen. Well coupled mitochondrial particles were prepared by a modification of the procedure of Madden. These particles were used to prepare inhibition-protein-depleted particles by centrifuging under energized conditions according to the method of Gale ...

(Gilbert and Mulkay, 1984:51)

We see in this fragment several of the features of Methods that have been noted by other investigators, for example Tomlin (1981) and Bruce (1983). The Past Passive is consistently chosen and the identity of the underlying agent is consistently that of the experimenters. There is also a somewhat restricted range of predicate verbs (*prepared; prepared; and used to prepare*). However, the most interesting feature of the above extract is the way in which the method is described – or perhaps in this case *not* described:

- a) ... by the method of Wong;
- b) ... by a modification of the procedure of Madden;
- c) ... according to the method of Gale.

There are a number of points worth noting. First, the method is merely labeled rather than characterized. Second, in the case of (b) if not for the others, replication would appear to be impossible given the fact that the nature of the modification is not made clear. Third, the preparation processes attributable to the three researchers are, on the one hand, simply associated with their names, while, on the other, they have not apparently reached the status of being *named* methods, that is, *The Wong Method, Madden's Procedure*. It is thus unlikely that other workers could easily look up the methods themselves in a reference work. The members of this discourse community would seem to take methodological appropriacy and rigor more for granted than we will find to be the case in other communities.

The opening fragment also illustrates the important process of NP stacking discussed by Dubois (1982a) among others:

- heavy beef heart mitochondria
- well coupled mitochondrial particles
- inhibition-protein-depleted particles

As the preparation becomes increasingly specialized it becomes increasingly complex syntactically; indeed Dubois (1982a) has argued that there may well be a coincidence of cognitive culmination and syntactic culmination. We can also note that in this particular instance, there is little NP identity across the sentence boundaries: as far as the non-specialist is concerned, 'mitochondria' have been mysteriously particulated, while the particles have been equally mysteriously transformed from 'well coupled' into ones that are 'inhibition-protein-depleted'.

Bruce (1983) makes a broadly similar point about a Method section from a medical journal:

Urography was performed in a routine manner, the patient micturating immediately beforehand. Tomography was used to detail the caliceal pattern when necessary. Abdominal

compression was avoided and the bladder not drained during the examination ...

As he says, the text 'might appear at first sight to be incoherent – completely lacking in the cohesive feature of anaphoric reference; but the coherence is, of course, supplied by the shared knowledge of these investigative procedures, and their likely sequence, that the reader brings to the text' (1983:8). For Bruce the well-known if difficult-to-apply Given–New paradigm (cf. Halliday, 1978) needs replacing in many Method sections by a Known–New paradigm. Either way then, either through the use of New Thematic Subjects (*new* on the discoursal level), or, as we saw earlier, by a process of cognitive but not discoursal development of the initial nominal group, ESP-related inquiries corroborate the sociologists' observations (e.g. Gilbert and Mulkay, 1984; Myers, 1985a) that Method sections often read like checklists.

In this context, some of Weissberg's findings are particularly interesting. Weissberg (1984) classified cohesive devices in 20 Method paragraphs drawn from a range of disciplines and found only seven uses of pronouns and three instances of superordinate expressions. On the other hand, he noted no less than 54 occasions where 'inferential bridging' – by relying on the readers' background knowledge or experience – was needed for coherence. This need for 'bridging' was much more noticeable in Methods than in Introductions and Discussions, and is thus further evidence for the 'inconsiderate' nature of Methods texts.

One further aspect of Weissberg's paper is directly relevant. Whereas the 20 Introduction and 20 Discussion paragraphs tended to manifest a *linear progression* paragraph development (in other words, the 'classic' Given–New paradigm), the Method paragraphs were much more variable and indeterminate in structure. Weissberg's rather patchy results, plus the extensive reliance on inferential bridging, suggest that Method sections, like other condensed texts such as abstracts and telexes, evince in Hallidayan terms coherence but little cohesion, or in Functional–Sentence–Perspective terminology are heavy on *rheme* but light on *theme*. Therefore, it may well be the case that a different type of paragraph development needs to be established for Method – one which we might characterize as *broken linear*. In many Method paragraphs the sentences are like islands in a string, islands which only those with specialist knowledge and experience can easily jump across from one to the next.

The three fields Weissberg investigated were botany, agriculture and engineering, Gilbert and Mulkay looked at biochemistry, Bruce medicine, and Dubois zoology. These are all areas where many research methodologies are well established, indeed protocolized, and where scattered networks of specialists form active discourse communities.

These then are all fields that may favor the elliptical reporting of Method that we have come to expect. Indeed, Huckin (1987) has recently produced evidence to suggest that, in the biochemical area at least, the Method section is becoming increasingly de-emphasized. Method sections may be downgraded by being physically relocated towards the end of the paper. Additionally they increasingly occur in smaller print than that used for other sections, and, according to Huckin, the *Journal of Biological Chemistry* has recently begun to publish the Method section in print so small that it cannot be read without the aid of a magnifying glass.

However, it would be dangerous to suppose that such trends are equally detectable in 'softer', emerging or inter-disciplinary fields. Here, for instance, is the first paragraph from West's Method section in an article published in *TESOL Quarterly*, by no means a journal aimed at a narrow-band research readership (West, 1980:484). This paragraph has a clear Given–New character, which I have attempted to diagram informally in Figure 16. In so doing I have broken the paragraph into individual sentences.

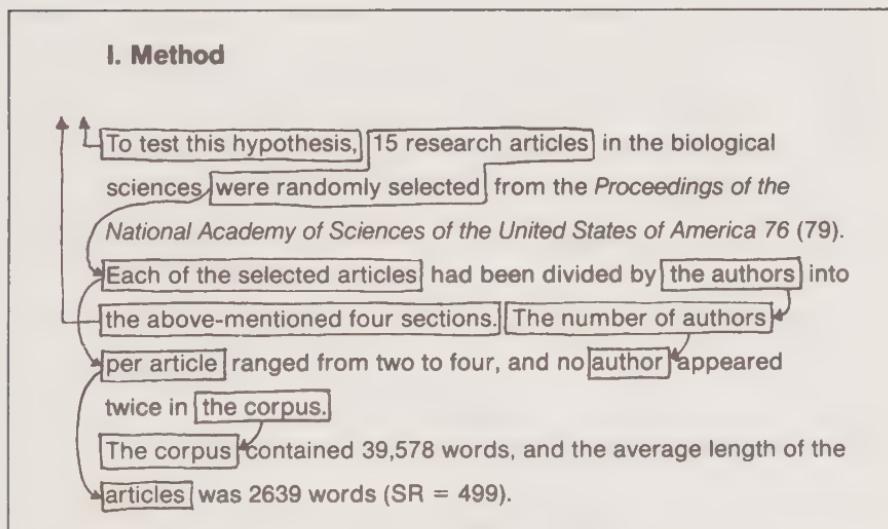


Figure 16 *Given and New in a Method paragraph*

This Method text seems to have a very different flavor to the preceding extracts from the hard sciences. The careful step-by-step description, massively supported by anaphoric reference and lexical repetition, produces the kind of explicitness that we associate with standard academic description. In contrast, Method sections in the physical and life sciences

are enigmatic, swift, presumptive of background knowledge, not designed for easy replication, and with little statement of rationale or discussion of the choices made. These differences can presumably be related to a number of sociological and intellectual phenomena, such as the nature of the discourse community, the level of agreement about appropriate methodology, the extent to which a *demonstrably* adequate methodology is deemed necessary, and the role assigned to controlled experiment in the discipline. Presumably, it is parameters such as these that explain why the Method section assumes great importance in most psychological and educational research, but can be assigned scant attention and space in an area like biochemistry.

7.6 Results, Discussions and Conclusions

In the two previous sections on Introductions and Methods, various claims have been advanced which are fairly specific as to what we might expect to find in particular sets of circumstances. Our present state of knowledge about the last two elements in the IMRD pattern is, regrettably, largely restricted to an exploratory rather than hypothesis-testing stage. One initial sign of unfirm ground is the fact that we do not yet know how matters tend to be arranged even in terms of the three section labels that provide the title for this section. Even if a majority of RAs have closing Results and Discussion sections, others coalesce the two, while even others have additional or substituted sections labeled Conclusions, Implications or Applications and so on. More particularly, there is, from the discourse analyst's viewpoint, much variation in the extent to which Results sections simply describe results and the extent to which Discussion sections redescribe results.

Consider again, for example, the four articles from *RTE* 22 (1) used to validate the CARS model for introductions. In the first article Results runs to 20 pages; in the second, Results consists of three tables and 30 lines of text. Even more significant perhaps is the way results are presented in the second RA:

- Para. 1 The ANOVA on ... indicated ...
Cell means ... are shown in Table 1.
Tukey *post hoc* tests indicated that ...
No other effects of interest were significant.
- Para. 2 The ANOVA on ... indicated ...
Cell means ... are shown in Table 2.
Tukey *post hoc* tests indicated that ...
No other effects of interest were significant.
- Para. 3 The ANOVA on ... indicated ...
The cell means ... were ...

- Para. 4 The ANOVA on ... indicated ...
Cell means ... are shown in Table 3.
Tukey *post hoc* tests indicated that ...
No other effects of interest were significant.
- Para. 5 The ANOVA on ... indicated that ...
Students' mean rating for ... was ...

The astonishing repetitive regularity of this Results section (in paragraph organization, in grammatical structure *and* in lexical choice) is presumably deliberate, especially if we bear in mind that the authors are specialists in writing. The section's style and structure seems to be designed to deny on the authors' part any associative contamination with commentary or observation. It is not Slater and Company who are talking here, but their statistical package.

The third and fourth RAs from *RTE* also present quantitative results. The third's Results section (in fact called Findings) opens with a paragraph connecting some of the present findings with the previous literature. The ensuing paragraphs deal with the results but in a much more evaluative way than in RA 2. There are also some observational asides such as '... but it must be remembered that this study does not account directly for sentence complexity ...' (1988:68). The final article deals with statistical measures similar to those in the second, but again offers greater rhetorical intervention. ANOVAS, for instance, *reveal* rather than *indicate*, and there is some justificatory commentary – 'Given these results, we applied three a posteriori tests' (1988:81).

As even authors within a single issue of a journal can apparently distribute very differently the knowledge-claims they want to make across the Results–Discussion divide, I have decided to frame the remaining discussion within the general area of results and their discussion. Even so, the fact that I know of only a handful of studies in this area means that treatment will be brief.

Belanger (1982) analyzed 10 Discussion sections from articles in the field of neuroscience. On the basis of this data, he was able to show that 'the structure of the discussion section is closely correlated to both the number and kind of research questions posed in the introduction sections of the paper' (1982:1). Belanger proposes that after a possible general introduction and before a possible general conclusion, each research question or RQ (as identified with the help of a specialist informant) is then passed through 'a cycle':

1. *Summarizing results* and stating conclusions with references to previous research;
2. *What research suggests* with references to previous research and/or to the current work;

3. *Further questions* sometimes with possible explanations and sometimes with references.

He finds that all three elements are not always present for each RQ, but any elements occurring follow the order given above; he also found that sometimes the discussion of a particular RQ was iterated through the cycle several times – an interesting parallel with Crookes' and others' observation of 'cycling' in introductions (Crookes, 1986a). It is not clear whether the nesting of 1–3 mini-discussions can be related to Weissberg's (1984) finding that linear topic development was the commonest type of Discussion paragraph. A final conclusion from Belanger's pioneering study is that the Hill et al. diagram (1982) is over-generalized. Rather than a broad move from specific to general, Belanger finds a series of small-scale expansions of scope that correspond to discussion of each research question.

Subsequent work has underlined the cyclic nature of Discussion sections. Although Introductions may also be cyclic, they equally may not be (as we saw in the Figure 11 text). In contrast, a 'chunked' composite form of Discussion seems to be a rare phenomenon. The subsequent work (McKinlay, 1984; Hopkins, 1985; Peng, 1987; Hopkins and Dudley-Evans, 1988) has largely been concerned with elaborating and refining the Move-Steps in the cycle and in searching for recurring patterns in the Move-Steps. Peng (1987) and Hopkins and Dudley-Evans (1988), for example, both offer 11-Move schemes which differ only in minor detail. The more frequent moves are glossed as follows.

1. *Background information.* This is a somewhat free-standing move that can occur at any point in the cycle. As its name implies, this move is employed by authors when they wish to strengthen their discussion by recapitulating main points, by highlighting theoretical information, or by reminding the reader of technical information.
2. *Statement of results.* If there is a quasi-obligatory move in Discussion sections it is this one. Evidence suggests, as we might expect, that it is the starting point of a cycle – and is only likely to be preceded by a Move 1. Many Discussion sections will have several cycles beginning with a Move 2; Hopkins (1985) found that three cycles were the commonest pattern in his study of papers published in the proceedings of an irrigation and drainage conference. Additionally, we might expect that the stronger results will be dealt with in an early cycle and weaker results in a later one. On this issue, Huckin (1987) makes the following interesting observation:

One of my biologists, who serves on the editorial board of a major journal in the field, said that the first paragraph of a discussion should always be reserved for the strongest claim in the study. Though he stated this as a prescription, my survey of papers in

both biology and physics showed it to be a description of actual practice anyway.

(Huckin, 1987:12–13)

3. *(Un)expected outcome.* Here the writer comments on whether the result is unexpected or not. This was, in fact, quite a rare move in Peng's chemical engineering texts, occurring in only four out of 52 cycles.
4. *Reference to previous research.* After Moves 1 and 2, probably the most common move. There are two main sub-types or steps: reference for purposes of *comparison* with present research and references for purposes of providing *support* for present research.
5. *Explanation.* This move is particularly common when the writer suggests reasons for a surprising result, or one at odds with those reported in the literature. At present the relationship between Moves 3 and 5 is somewhat obscure, particularly as to whether 5 is subsequent to 3 or an alternative to it.
6. *Exemplification.* Examples are most often used to support an explanation (Hopkins and Dudley-Evans, 1988).
7. *Deduction and Hypothesis.* This move is used to make a claim (however qualified) about the generalizability of some or all of the reported results.
8. *Recommendation.* The writer advocates the need for further research or makes suggestions about possible lines of future investigation. However, Huckin (personal communication) believes that the specific identification of interesting research questions at the end of a cycle or at the end of the Discussion section as a whole is a move being increasingly abandoned by US scientists because they do not wish to give advantage to others in an increasingly competitive market for research grants.

This distilled list of eight moves provides a useful provisional framework for much needed further work on the structure of RA Discussion sections. The existence of cycles seems well-established, as is the fact that complexity of the cycle can be related to the degree to which the results are 'compatible' with previous work and/or with the expected outcome to hypotheses or questions. The work reviewed here, and that of Huckin (1987) give support to the view that Discussions, in strict contrast to Introductions, move during a cycle in an 'inside-out' direction; they move from stating the results themselves, to placing them within the established literature, to reviewing their general significance.

However, we know little about disciplinary variation and little about the linguistic exponents of the moves. Indeed, on the latter point, a certain amount of obfuscation may have been caused by the heavy attention given to the famous article by Watson and Crick on DNA (e.g. Bazerman,

1981; Crombie, 1985; Fahnestock, 1986; Myers, 1989), and to the comments on its composition in *The Double Helix* (Watson, 1968). The celebrated penultimate sentence in the article, 'It has not escaped our notice that the specific pairing we have postulated immediately suggests a possible copying mechanism for the genetic material', is a Move 7 deduction–hypothesis. The double negative (*not escaped*), the choice of *postulated* as reporting verb, insertion of *possible*, and the use of *suggests*, all characterize a very modest knowledge claim not gainsaid by the more assertive *immediately*. However, as Fahnestock comments, 'Watson and Crick could afford to be coy' (1986:278); they knew that the scientific world would acknowledge their major breakthrough. The 'cause célèbre' treatment of Watson and Crick's closing statements has, I suggest, distracted attention away from what may turn out to be more normal practice. In more normal circumstances, authors may well feel a need to advance the significance of their work in more positive terms.

7.7 Review

In this chapter, the longest in the book, I have attempted to offer a depiction of what we know about research articles, especially those of a more experimental character. In many cases I have provided an overview of the main results of a particular investigation. The advantage of this approach is that the reader is provided with some content, and so does not necessarily have to follow up references in order to obtain substantive findings. The disadvantage is that the space available for investigating theoretical and procedural issues has sometimes been curtailed.

In Chapter 7 I have tried to bring together several distinct approaches to research writing (quantitative, historical, sociological, rhetorical, discursal, linguistic) in an effort to both broaden and deepen the perceptions of those who are concerned with the genre in practical or applied ways, as teachers of research writing and critical reading, as research writers themselves, as advisors, as editors, as abstracters, as citation analysts and so on. In particular, I have tried to illustrate ways in which genre conventions may be seen to be operating, so that applied work can be grounded in whatever sense of reality the current state of knowledge permits. On the other hand, in the second half of Chapter 7, I have given prominence to work in the EAP/Applied Discourse Analysis tradition, in the hope that the careful textual studies that this tradition usually represents can offer something in return to the wider interdisciplinary field.

As has been suggested more than once, the picture we have of the research article is far from complete. That picture suggests that there are

certain characteristics of RAs which, by and large, tend to occur and recur in samples drawn from an extensive range of disciplines. It is a moot point whether the similarities are sufficiently dominant to support Widdowson's assumption of a 'macrogenre' (Widdowson, 1983a). However, it remains the case that RAs are rarely simple narratives of investigations. Instead, they are complexly distanced reconstructions of research activities, at least part of this reconstructive process deriving from a need to anticipate and discountenance negative reactions to the knowledge claims being advanced. And this need in turn explains the long-standing (Shapin, 1984) and widespread use of 'hedges' as rhetorical devices both for projecting honesty, modesty and proper caution in self-reports, and for diplomatically creating research space in areas heavily populated by other researchers.

On the other hand, the RA varies from one disciplinary sector to another in terms of degree of standardization and of the prevalence of a nominalized impersonal style (Smith, 1982). In those areas of knowledge variously described as 'hard', 'exact' or 'physical', consensus on objectives, ground-rules and points of departure has led to textual products with regularized macro-structure and with rhetorics that follow identifiable role-models. In these fields, there is a perceivable inter-relationship between the RA as a peer-group intellectual object, the abstract nominal style, and the presence of authorial intrusion mainly in contexts thought to need persuasive support, or to need some revelation of the authors' individual cognitive processes. As is well known, certain groups in the social and behavior sciences have tried, with varying degrees of success, to adopt and adapt the hard science paradigm (cf. Bazerman, 1987). Others, such as ethnographers of various persuasions, have not. These and many in the humanities tend to align their scholarly and research products to their preferred intellectual schools and scholarly traditions rather than to disciplines as such. In general, differences between the genres of articles, books, reviews, and so on are less marked in the humanities.

Finally, there are two principal corollaries of this variation – and one unexpected outcome. First, the more established the conventions, the more articulated the genre. Thus on a superficial level, the RA text becomes increasingly divided into standardized divisions (IMRD or a disciplinary variant); on a less obvious level, the more likely we will find that different sections will have different rhetorical features (e.g. Introductions in contrast to Methods). The second corollary is that as we move towards the diffuse end of the continuum the more necessary it becomes for authors to engage in acts of persuasion that will encourage the readerships to share particular visions of the research world. The surprise is that, on preliminary evidence at least, the major differences do not lie so much in Introductions and Discussions (where I believe most

people would expect it) but rather in the Method and Results sections. Finally, there is perhaps an element of irony in a situation wherein social scientists are engaged in a cognitive and rhetorical upgrade of Method at a time when their mentors in the hard sciences are beginning, rhetorically at least, to downgrade its importance.