

Oliver John Shaw

From process to product: a text-
based ethnographic study of ten
biomedical research articles by
Spanish authors

Departamento
Filología Inglesa y Alemana

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Tesis Doctoral

FROM PROCESS TO PRODUCT: A TEXT-BASED ETHNOGRAPHIC STUDY OF TEN BIOMEDICAL RESEARCH ARTICLES BY SPANISH AUTHORS

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Doctoral thesis

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List of Abbreviations

EAP	English for Academic purposes
ELF	English as a lingua franca
ELT	English language teaching
ERPP	English for Research and Publication Purposes
ESP	English for Specific Purposes
FJD	Fundación Jiménez Díaz
IMRD	Introduction, methods, results, discussion
IR	Intercultural rhetoric
NES	Native-English-speaking
NNES	Non-native-English-speaking
RA	Research article
TH	Text history

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Introduction

1.1 English as the putative language of science: implications and issues for non-native-English-speaking academics

The position of English as the language of science and the academy has been widely documented (Ammon, 2001; Crystal, 2003; Kirkpatrick, 2007), especially in the natural (Ammon, 2001; Ferguson, 2007; Gea-Valor, Rey-Rocha, & Moreno, 2014) and social sciences (Ferguson, 2007; Lillis & Curry, 2006a, 2010). Although the inroads made by English has raised alarm among those who see monolingualism as a threat to local languages, research by a number of authors (e.g., Benfield & Howard, 2000; Ferguson, 2007; Hanauer & Englander, 2011; Tardy, 2004) indicates that this trend shows few signs of slowing down.

The status of English as the undisputed “language of Science” (Benfield & Howard, 2000) means that most all who wish to contribute to scientific advancement must do so in English, whether they are native-English-speaking (NES) or non-native-English-speaking (NNES) scholars. This linguistic status quo, raises a number of issues concerning the ways in which an English-dominated culture of international science may affect different stakeholders around the globe. One such issue is that of equity, and more specifically, the treatment given to NNES researchers who seek to contribute to academic communities by submitting written papers to international English-medium journals. Several authors have taken up the matter of fairness in the English-dominated world of science through various theoretical and empirical means (Ferguson, 2009; Lillis, 2004; Lillis & Curry, 2006a; Phillipson, 1992). Although not the only genre of scientific inquiry, having a paper published in an academic journal is a particularly valued indicator of membership in the scientific community (Swales, 1990). Of all the types of written academic output, the research article (RA) is widely considered the most prized (Canagarajah, 1996; J. Flowerdew, 1999b; Gosden, 1992; Moreno, 2010; Swales, 1990). Some studies on global communities of academic publishing highlight the lopsided presence of journal articles by authors from Anglophone-center countries compared to authors from other parts of the world (Benfield & Howard, 2000; Salager-Meyer, 2008), while others (Benfield & Feak, 2006; Burrough-Boenisch, 2003; Ferguson, 2007; Ferguson, Pérez-Llantada, & Plo, 2011; Iverson, 2002; Kourilova, 1998;

Tribble, 2017) have found bibliometric and other empirical evidence suggesting that certain non-Anglophone countries have a high number of scientific publications per capita, and that scientists from peripheral and semi-peripheral countries are publishing more and more frequently.

Equitable or not, academic publishing has been the subject of a number of studies over the last few decades. Although analyses predicated on a dichotic relationship between NES and NNES academics have been called into question (Baker, 2009; Dewey, 2007; Hyland, 2016; Leung, 2005; Tribble, 2017; Wood, 2001), scholars of English for Academic purposes (EAP) and English for Research and Publication Purposes (ERPP) have examined a number of variables that may make academic writing in English a tedious and demanding task (for a review, see Uzuner 2008).

One apparently obvious factor that may limit NNES academics' ability to publish their work in English-language journals is the very fact that English is not their native language, thus hindering their ability to express their ideas on the page. In order to understand how these researchers' comparatively limited ability to write in English may put them at a disadvantage relative to their NES peers, researchers in EAP and ERPP have highlighted a number of issues, most of which involve the discoursal and rhetorical traits of academic writing rather than mere grammar and other surface-level errors (J. Flowerdew, 1999a).

Aside from the additional time invested in English-language classroom hours over the course of their educational lives to the detriment of other academic pursuits (Burgess, Gea-Valor, Moreno, & Rey-Rocha, 2014; Tardy, 2004), once NNES scientists have become practicing professionals, they must devote substantial time and energy to reading and writing (Curry & Lillis, 2004; Huang, 2010; Swales, 1990), and some believe that this added time constitutes one of the disadvantages they face (J. Flowerdew, 1999a). When preparing their manuscripts, NNES authors with access to either paid or unpaid editing services may send their work to these "language brokers" (Lillis & Curry, 2006a, p. 4), also prolonging the total time to submission (Burrough-Boenisch, 2003; St. John, 1987). In addition, once they have submitted their papers, requests for revision due to language issues also slow down the process (J. Flowerdew, 2000; Y. Li, 2006), which may handicap their potential output. Faced with substantial pressure to publish in international journals in

order to further their careers, secure additional funding for research, become tenured professors, etc. (Curry & Lillis, 2004; Johns, 2013; Y. Li & Flowerdew, 2009; Pérez-Llantada, Plo, & Ferguson, 2011; Sheldon, 2011), the language factor has been found by some to have an appreciable effect on NNEs scholars' academic lives (Huang, 2010; Uzuner, 2008).

Despite the findings of these analyses, other researchers point to difficulties inherent to the nature of academic discourse as the most formidable hurdle. Indeed, a number of prominent studies have stressed that contributing to scientific discourse communities is a craft that must be learned by NES and NNEs academics alike (J. Flowerdew, 2001; Mauranen, Pérez-Llantada, & Swales, 2010; Swales, 2004; Wood, 2001), thus challenging the notion of native language as the primary stumbling block for NNEs researchers. While this may be true, cultural differences in rhetoric may cause NNEs authors to formulate their arguments in ways that are seen as inferior by Anglophone-center gatekeepers (Bennett, 2007b; Uzuner, 2008), and the advantages for NES academics that come with carrying out their daily lives in English cannot be considered negligible (J. Flowerdew, 1999a; Salager-Meyer, 2008).

In addition to the added task of mastering a foreign language and becoming conversant in their field of science in English, many NNEs academics are from countries that are considerably less economically advantaged than such countries as the United States, the United Kingdom, and other nations where English is the dominant language of communication, if not the countries' official tongue. This comparative deficit of resources may manifest in a number of ways, including limited access to the tools they need to access information and produce their work (Canagarajah, 1996, 2002; Salager-Meyer, 2008). Also, scholars in these "periphery" (Canagarajah, 1999) or "off-network" (Belcher, 2007) settings may not have the same resources available to a "semi-periphery" (Sousa Santos 1994, cited in Lillis & Curry 2006) academic so as to afford the services of a language broker (Burrough-Boenisch, 2003; Kerans, 2001; Lillis & Curry, 2010; Shashok, 2001). Together, these factors may make the goal of successful academic publication an increasingly difficult one, causing the number of scientific publications by country to mirror the economic discrepancies between them (Ferguson, 2007; Salager-Meyer, 2008).

Another difficulty working to the detriment of NNES scholars' ability to publish is their physical and social distance from prestigious scholars who live and work in the Anglophone center countries where many scientific journals are edited and published. Curry & Lillis (2010) found evidence that underscores the importance of accessing international networks for NNES researchers, and Belcher (2007) also stresses the importance of networks, suggesting that by encouraging novice off-network scholars to enter into more frequent contact with researchers in other locales, they may be more effectively socialized into the discourse community. In fact, Cho (2004) and Tardy (2004) both reported that NNES PhD students studying at a university in the United States felt more confident, mainly because of their closer connection with center scholars, with Flowerdew (1999b) producing similar findings in his survey of Cantonese-L1 academics living in Hong Kong after having spent time abroad.

As who and what gets published in academic journals depends on the decision of editorial boards and the referees recruited to vet manuscripts for quality and value, these two types of gatekeepers wield substantial power over the field of science. NNES-submitted papers may include non-standard genre or discourse patterns, and are sometimes not well-received by journal gatekeepers. In a recent study, Tribble (2017) found numerous examples of unconventional language on the clause level in an international medical journal, noting however that the move structures in the published papers were highly consistent across his corpus, which suggests that publications by NNES scholars may be different on the texts' surface, although the discourse structure of these papers tends to reflect the conventions of the discipline.

Aside from the particular aspects of NNES manuscripts that reviewers and editors may find inappropriate, a number of reports raise the issue of possible bias against manuscripts submitted by multilingual scholars (J. Flowerdew, 1999a, 1999b, 2008; Huang, 2010; Iverson, 2002). Some of this evidence is based on author views, which can be challenging to confirm. However, lack of impartiality may be manifested semi-consciously (Ferguson, 2007) or be more an issue of differing views of science culture (Gosden, 1992) between authors and gatekeepers.

As shown by the works cited above, the topic of equity in academic publishing is a controversial one. Issues of written language in its many facets (e.g., surface-level accuracy, discourse, rhetoric), material and non-material resources, and access to professional networks overlap in complex ways to make the enterprise of international academic publishing a challenging one. As the number of NNES speakers has mushroomed in recent decades, several prominent applied linguists have brought attention to the disjuncture in English language teaching (ELT) between the types of competence learners of the language need and the idealized NES model present in a large part of ELT pedagogy. Rather than carrying out an actual assessment of what constitutes communicative competence in the lives of NNES individuals, much of ELT remains in thrall to idealized native speakers, thereby ignoring the multifaceted, individual, and situationally dependent nature of language knowledge. As a result, researchers like Leung (2005) have challenged the effectiveness of the NES model, urging linguists and teachers to explore the use of English as a lingua franca (ELF) by deploying ethnographic techniques. Motivated by a similar conviction on the possible misconception of what English is used to accomplish in individuals' actual experiences, a number of studies have criticized the unfairness of academic journals who conflate quality writing with adherence to NES rhetorical conventions (Jenkins, 2011; Seidlhofer, 2001). Certain signs indicate that journals are moving toward greater recognition of international contributors and their ways of writing (J. Flowerdew, 2001; Jenkins, Cogo, & Dewey, 2011; Mauranen et al., 2010; Swales, 2004). Indeed, some publications have abandoned the practice by which journal gatekeepers request that NNES authors have their writing revised by a native speaker (Jenkins et al., 2011), although others (Salager-Meyer, 2008) see less cause for optimism given the multi-faceted process of making one's mark within the international scientific community. Salager-Meyer eloquently summarizes her view of this interrelationship as follows:

The problem is, firstly, that non-discursive factors frequently go hand in hand with poor linguistic skills (at least in non-English speaking periphery countries) and, secondly, that poor linguistic skills frequently go hand in hand with paper rejection. (p. 125)

As the scientific world becomes increasingly monolingual, local languages (i.e., non-English) may undergo a gradual loss of prominence both on the international scene as well as the countries where they are the majority language. In recent decades, national publications formerly

appearing in the local language have transitioned to an English-only policy, leaving NNES scientists with fewer and fewer forums in which to institute and utilize new terminology and specialized registers in their L1, thus raising the risk of other languages losing their places as vehicles of scientific inquiry (Bennett, 2007a; Kirkpatrick, 2007).

With the shift to English as *the* language of Science, and in times when gatekeepers of academic journals expect submitted manuscripts to adhere to English rhetorical and discourse styles, minority languages face the threat of losing their rhetorical uniqueness (Ostler, 2002). Indeed, the spread of English as a *language* may have less of an impact on local languages than the encroachment of the English *linguistic culture* (Bennett, 2014; Uzuner, 2008). Bennett (2007a, 2007b) shows how the differences in rhetorical style that may cause NNES-authored texts to be considered inappropriate for journals published in the academic center are a reflection of deep-seated cultural identities fashioned over centuries and resulting from historical currents that have shaped the way ideas are developed in writing. In her analysis, Bennett compares the anthropocentric writing style that predates the Scientific Revolution but is still popular in the semi-peripheral countries of Portugal and Spain to the style that has gained currency in other nations, where positivism and the Scientific Revolution helped usher in the “plain authoritative style” (Venuti 1995:5, cited in Bennett 2007a, 2007b) that continues to hold sway in the English-dominated world of academia because of its purported transparency and objectivity. In this fashion, Portuguese and Spanish scholars sometimes struggle to adapt—through writing in English or in translation—their unique, culturally-bound perspectives on science to a rhetorical culture that is foreign to them, and these same Anglophone rhetorical traits have the potential to dominate the epistemological underpinning of scholarship on the Iberian Peninsula (*ibid.*) and, conceivably, beyond, triggering a phenomenon that Bennett (2007a) refers to as “epistemicide”.

A similar but distinct view on the subject of the advance of English and its impact on other peoples and languages is that of Phillipson (1992), for whom the English language and the burgeoning field of ELT are the tools of English linguistic imperialism, whereby material and cultural inequalities between English-speaking countries and non-Anglophone countries are ushered in and perpetuated by the language. In his influential and scathing analysis, Phillipson argues that

English linguistic imperialism is an act of linguistic imperialism, defined as “ideologies, structures, and practices which are used to legitimate, effectuate, and reproduce an unequal division of power and resources (both material and immaterial) between groups which are defined on the basis of language” (ibid, p. 47). More recently, critics of this theory have questioned the orchestrated nature that underpins the notion of linguistic imperialism as well as the status of non-Anglophone cultures and institutions as victims of the encroachment of the English language (Ferguson, 2007), while House (2003) sees ELF as a “language for communication,” one that is clearly different from multilingual scholars’ “language for identification” used in other pursuits. Others, meanwhile, challenge the view of center and periphery cultures from a “large” or macro perspective, indicating that such theories fail to account for how linguistic imperialism manifests in actual scenarios (Canagarajah, 1999) and negate the variability and dynamism of human behavior (Holliday, 1999). Teachers of academic writing who find injustice in this status quo have sought to empower their NNES students by equipping them to reproduce the dominant Anglo-American discourse norms, by encouraging students to critique existing structures and bring about change, or by taking a middle-of-the-road approach, helping students to master the mores of their target discourse communities while recognizing and engaging with the power structures that perpetuate orthodox discourse patterns (Harwood & Hadley, 2004). Such challenges to the inner-outer dichotomy see culture as something that is yet to be conceptualized to its fullest, requiring greater sensitivity toward the many different configurations of culture at play when people interact within social systems, with global academic publishing representing a particularly complex arrangement of “cultures” in their many manifestations (Atkinson, 2004; Baker, 2009).

1.2 Brief introduction to the study: Aims and hypotheses

The setting for the present study is the Health Research Institute of *Fundación Jiménez Díaz* (IIS-FJD, derived from its initialism in Spanish). The IIS-FJD is the research arm of a tertiary hospital located in Madrid, Spain, and providing institutional support for the medical doctors, nursing staff, and biomedical researchers employed by the hospital and its affiliated hospitals and specialist-care centers. Spain is a country where the multifarious issues affecting NNES scholars are particularly relevant. While the relative prosperity and comparatively abundant funding available for research in Spain may have qualified the country as a member of the “inner” circle

(Kachru, 1985) prior to the global financial crisis that began in 2007–8, the budget cuts instituted by the governing Popular Party coupled with a widespread lack of English language skills makes the prospect of communicating science through the language a somewhat daunting one. In this sense, Spanish researchers can be placed somewhere between “inner” and “outer” (Lillis & Curry, 2006a). Thus, this combination of relative macroeconomic wealth and historic disadvantage in the English-language skills among the Spanish population may enable the variable of language and its diverse ramifications to be brought into sharper focus.

I have held a part-time position as in-house editor for the IIS-FJD since 2009. At the time I initiated this study, I had held the post for four years. After successfully conducting previous research that analyzed the rhetorical traits of the discussion section in a sample of my researcher-clients, I then endeavored to explore the writing practices and experiences of a similar sample of researchers, albeit taking the entirety of the RA into consideration and broadening the scope of my analysis in ways which will be detailed in the methods section below.

This study exploring the phenomenon of academic writing at the semi-periphery addresses three main research questions:

1. What are the circumstances in which English-language texts are produced in the local setting? Put in another way, the study describes the *processes* inherent to scientific writing.
2. What are the primary traits evidenced by the writing produced by these scholars? In answering this question, I look to understand the written *products* of their efforts at the point in which they were assessed by journal gatekeepers, that is, the pre-submission stage.
3. How were these textual artifacts received at the post-submission phase by journal editors and gatekeepers? Data related to this question can shed light on the interaction between the expectations of the discourse community and how the researchers in my sample were either meeting these expectations or failing to do so.

As I began the study, my hypotheses were informed by my previous years of experience as an in-house editor. Numerous exceptions notwithstanding, my work revising the papers written by researchers affiliated with the IIS-FJD in both the pre- and post-submission stages led me to surmise the following:

- A large part of the authors' attention would be focused on the content behind the writing rather than on the process of creating the text itself;
- The type of textual product resulting from this conception of writing would make the texts rhetorically unconventional; and
- This concentration on the concepts being conveyed would give rise to criticism on the part of both editors and reviewers due to a perceived failure to adhere to conventional genre practices, thus impeding publication, at least until these issues were addressed and the standards of the discourse community were met.

1.3 Literature review

To best situate my investigation within the ongoing scholarship and in order to incorporate insights from similar studies carried out in the past, I draw from four approaches to the phenomenon of NNES writing for the academy, namely, intercultural rhetoric, genre studies, corpus studies, and ethnography. As will become clear in the following review of the literature, there is substantial overlap between these scholarly traditions. From a historical perspective, they have grown out of and enriched each other. Also, many of the most prominent scholars in EAP have made contributions within more than one analytical line. This multiplicity of analyses in the literature makes a strong case for situated research that understands academic writing as a multifaceted endeavor. In the following section I review some of the guiding principles and salient research within these four research frameworks, giving prominence to those studies that are most pertinent to my research.

1.3.1 Intercultural rhetoric

Originally termed contrastive rhetoric, intercultural rhetoric (IR) is a field of study that seeks to identify problems NNES authors face when writing in English and then explain these problems

through the prism of the rhetorical strategies inherent to their native language (L1) (Connor, 1996). Throughout its history it has undergone a substantial degree of change, evolving into a highly complex approach; however, IR has continued to be primarily concerned with three central issues: writing (by NNES scholars within academic and professional contexts), learning and using second and additional languages, and culture (Atkinson, 2004). Both in its theoretical underpinning and methods of inquiry, it has drawn from a number of sub-fields within Applied Linguistics and beyond, including second language acquisition, composition and rhetoric, anthropology, translation studies, linguistic discourse analysis, and genre studies (Connor, 2004).

Indeed, the very multifaceted and derivative nature of IR has sparked a good measure of criticism and debate, especially as concerns the way scholars define it, that is, as a method or a theory. It has been called one of the four methodological subdivisions for research on academic discourse, alongside genre analysis, corpus-based studies, and ethnographic approaches (Flowerdew 2002, cited in Connor 2004), thus placing it on equal footing with some of the most dominant approaches to the realities faced by NNES scholars when writing for professional and academic purposes. Connor, however, challenges the status of IR as a method, calling it:

not a specific method, but rather a substantive approach or goal, which itself employs various methods. These methods include text analysis, genre analysis, corpus linguistics, and ethnographic approaches. All are needed; each produces different kinds of information (2004, p. 294).

Critics of IR have seen this lack of a clearly identifiable methodology as a weakness, while its proponents believe the multiple perspectives offered by the approach to be one of its strengths (Connor, 1996), opening up different ways of examining a given phenomenon. Notwithstanding the differences of opinion from both the very researchers who contribute to the field as well as those who call into question its soundness, controversy surrounding its theoretical basis, methodological tools, and the interplay between the two has driven considerable change in IR throughout the more than four decades since its conception.

1.3.1.1 History of intercultural rhetoric

What is today commonly referred to as IR began with the seminal paper written by the American applied linguist Robert Kaplan in 1966. Kaplan argued that learners of second languages use the culturally specific logic and rhetorical patterns that are characteristic of their L1, thereby suggesting that language and writing are cultural phenomena. Although his theory came under considerable criticism with time, even to the point in which Kaplan himself (1987, cited in Atkinson 2004) revised his theory in recognition of its shortcomings, Kaplan's work was credited for expanding the focus of ELT scholarship from one that was almost entirely trained on spoken language to include written language. With this addition of the written mode, the scope of teaching and research was also widened from the sentence to the paragraph, examining how students from different L1 backgrounds structured paragraphs in argumentative essays (Connor, 1996).

In the 1970s, the focus on writing in English by people of NNES backgrounds gave rise to a number of studies on the composing process, followed in the next decades by a conception of writing as a socially situated phenomenon, the investigation of which required greater sensitivity toward purpose, setting, and audience. During this time, studies within IR began analyzing texts produced within professional and academic contexts (Connor, 2002), and in doing so incorporated the notion of writing as an activity that takes place within the socially constructed milieu of the discourse community (Connor, 1996), thus bringing IR into close contact with the research tradition known as genre studies (Ostler, 2002). According to this situated perspective, novice scientists not only had to meet the challenge of learning to write in English; they also had to produce generically appropriate texts and become conversant in the "accepted world view" of the discourse community they endeavored to join (Connor, 1996, p. 77).

Coinciding with the rise of postmodernist thought and its radically different views of culture, Kaplan's original view of culture fell out of favor among those who saw culture as multifaceted, discontinuous, in flux, and reflective of social inequality. The previously held notions of culture based on societies, such as "Anglo-American" or "Spanish," culture came to be known as "received culture," or a system of classifying individuals solely by nationality and ethnicity. Before

new ways of conceptualizing culture were developed, most analyses in EAP, Applied Linguistics, and second language education took a received view of culture, which theorists such as Atkinson (2004) found to limit the richness of descriptions of what NNEs writers draw from. As a result, this new, highly problematized conception of culture gained favor, and many in the tradition began to refer to the field as IR (Baker, 2013; Canagarajah, 2013). Compared to previous research perspectives, IR takes greater interest in the writing processes that give rise to textual products so as to offer a fuller account of the political, social, and economic forces involved (Atkinson, 2004).

1.3.1.2 Big vs. small culture

One particularly influential response within the reconceptualization of culture is the “small” versus “big” cultures dichotomy. Under this paradigm, received culture came to be referred to as big culture, while small cultures were taken to represent any of the numerous cultural allegiances that come into play when groups of any type engage in shared activity, including gender, generation, profession, and many other attributes and affiliations. In characteristic postmodern fashion, these cultural alignments are characterized by their constant state of flux and renegotiation and are defined by agents’ actions rather than other, purportedly intrinsic features (Holliday, 1999). Cultural alignments vary from situation to situation, though some defining characteristics of these small cultures are the same as those of big cultures, including shared sets of values, more or less durable norms, well-defined roles, certain hierarchies, and symbolic and material artifacts (Atkinson, 2004).

The small culture approach grew out of concerns that big culture, which classifies culture along ethnic, national, or international lines (Holliday, 1999), is insufficiently critical in their conception of culture and intercultural communication and texts, and such limited views perpetuate stereotypes of how languages and cultures operate (Baker, 2013; Connor, 1996) and also fail to incorporate sensitivity to issues of power, dominance, and inequality (Ostler, 2002). In fact, critics of the type of big culture views that previously held sway in IR believe that by applying the big culture perspective when describing the global ecosystem of academic publishing using broad categories such as “inner,” “outer,” and “expanding” circles, Kachru (1985) and other theorists

conceive of academics from other L1 backgrounds as “other,” thereby perpetuating certain perceptions among members of the academic periphery (Holliday, 1999).

For some proponents of the small culture paradigm, culture is taken to be the interaction of small cultures (Holliday, 1999), while others see a complex interaction between small, medium-size, and large cultures (Atkinson, 2004). Regardless of how cultures big and small are brought together to analyze writing for intercultural audiences, there is certain consensus that observation of any unfamiliar social grouping will perceive “discernible sets of behaviours and understandings connected with group cohesion” (Holliday, 1999, p. 248).

1.3.1.3 Research methods in intercultural rhetoric

As the concept of culture has evolved in step with the dominant modes of thought in the social sciences, the methodological underpinning of IR has undergone a similar degree of change and has been equally challenged by critics. In keeping with the early concern for textual products rather than the forces impinging on the text-making process, IR research originally consisted of decontextualized linguistic analysis to describe such aspects of writing as cohesion, coherence, and discourse structure (Connor, 2002). This quantitative descriptive approach has been a mainstay in the field, with many studies seeking to establish frequencies of certain text features in order to describe writing produced in different cultural contexts (Carcu, 2009; Lorés-Sanz & Murillo-Ornat, 2007; Moreno, 1997, 1998; Murillo-Ornat, 2012, 2016; Sheldon, 2011).

Studies in this vein have had to grapple with a number of challenges inherent to research design and execution, however, especially as scholarly inquiry broadened the range of factors it examined. One challenge that has received a great deal of analytical attention has been the concept of *tertium comparationis*, or a “common platform of comparison or shared similarity between texts and analyses” (Connor, 2004, p. 292; Golebiowski, 1998). Great care has gone into compiling corpora that lend themselves to rigorous analysis using clearly defined linguistic models. As IR researchers began to include NNES scholars’ writing in their L1 alongside descriptions of their L2 writing in English as well as NES-authored texts in English (Golebiowski, 1998; Lorés-Sanz, 2011a, 2011b; Pérez-Llantada, 2008; Taylor & Tingguang, 1991) so as to gain a

fuller understanding of how they adapted their rhetorical style when changing languages (Connor, 1996), this preoccupation with comparability has taken center stage.

Despite the findings of quantitative studies, a number of doubts have been raised concerning the limitations of quantitative approaches to culture in writing. One concern warns of the disadvantages of over-reliance on corpora inasmuch as such research causes a detachment between the text on the one hand and culture and perceptions on the other (Atkinson, 2004; Ostler, 2002). As a result, authors have called for studies on IR to include such data as the authors' level of L1 literacy, certain basic bio-data, the setting in which authors compose their work, and the purpose of the writing (Ostler, 2002), thus echoing to some degree other researchers' emphasis on *tertium comparationis*. Since the 1990s, these social and cultural influences have been given increased attention, answering calls from within IR as well as other related disciplines to account for the link between texts and the social and intellectual activity of which they form part (Bazerman, 1988; Bazerman & Prior, 2003; Berkenkotter & Huckin, 1995; Connor, 2004; Gosden, 1996). As a result, a number of studies have been designed to pinpoint the contextual factors that impact intercultural text production (e.g., Mur-Dueñas, 2012), and others have opened analyses up to a wider range of genres (Connor, 2002).

While the overlap with genre has enriched the study of IR considerably by leading IR investigations toward narrower cultural perspectives of disciplinary culture, this greater sensitivity toward academic and professional groupings has created a need to widen the lens to such small culture configurations as age, gender, experience, and others, which in turn has necessitated an evolution toward more ethnographically inflected research (e.g., Atkinson & Sohn, 2013; Gosden, 1995, 1996; Hyland, 2000; Mauranen et al., 2010; Mur-Dueñas, 2013; Tardy, 2004). However, small cultures and discourse communities are related but not interchangeable denominations (Holliday, 1999), due in part to textual variation within genres (Samraj, 2002a).

Context-based approaches to texts do not come without pitfalls, however. Taylor and Tingguang (1991) stress that author accounts must not be taken at face value, as individuals' behavior may differ from what they report. When approaching textual products and processes, observers must

also maintain an awareness of and reflect on of their own cultural perceptions, as analysis of cultural phenomena needs to be interpreted through the analyst's perspective (Atkinson, 2004; Canagarajah, 2013). One such culturally bound view of the world is our very belief of what constitutes rhetorically sound and convincing prose. Despite the many ways Kaplan's seminal study has had a positive impact on the field of EAP, closer analysis has revealed that his original "squiggles" diagram was based on a cultural bias that favored Anglo-Saxon rhetoric as "straight" and others, not so (Kaplan 1987, cited in Atkinson 2004). Some researchers espousing the ELF approach caution against using NES standards as a measuring stick when describing the language used by NNEs writers when communicating interculturally (Baker, 2009; Dewey, 2007; House, 2003; Mauraenen, 2003; Mauraenen et al., 2010; Seidlhofer, 2001). Also, the methodological rigor of studies comparing writers' textual production in their L1 with their ELF writing has been subjected to scrutiny by theorists such as Baker (2013), who finds it more fruitful to look for rhetorical patterns that emerge in texts written by and for multilingual and multicultural readerships.

1.3.1.4 Existing studies

Numerous contrastive studies have been carried out over the years, and the RA has received particular focus. Some have studied the RA in its entirety (Carciu, 2009; Lafuente-Millán, 2010; Moreno, 1997, 1998; Pérez-Llantada, 2008; Salager-Meyer, Alcaraz Ariza, & Zambrano, 2003; Valero-Garcés, 1996), while work with a narrower focus has looked into the abstract (Burgess, 2002; Lorés-Sanz, 2006, 2009; Martin-Martin, 2005; Martín-Martín, 2003) and introductions (Golebiowski, 1998; Sheldon, 2011; Taylor & Tingguang, 1991). Specific analyses on the other sections of the RA have been somewhat more scarce. Some studies have combined ethnographic and text-based elements (Atkinson & Sohn, 2013; S. Cho, 2004; Mur-Deñás, 2012). Other research threads have explored author attitudes toward English as a vehicle for intercultural communication (Gosden, 1996; Tardy, 2004) and gatekeeper perspectives on (Gosden, 1992) and reports resulting from (Mungra & Webber, 2010) NNEs-authored RAs.

1.3.2 *Genre studies*

Genre studies and IR are two research traditions that have been intricately linked since IR began to broaden its analytical scope from argumentative student writing to writing for the professions and for academic purposes (Connor, 2004). One study that illustrates the commonalities between the two is that of Moreno (1997), which tests findings of culturally linked rhetorical differences such as those reported by Mauranen (1993), doing so by using two parallel subcorpora of L1-authored RAs: one subcorpus in Spanish and another in English. Moreno found that the rhetorical similarities imposed by the genre of the RA are stronger than any intercultural dynamics inherited from the authors' L1. Moreno's study is of particular interest in that it places the two methodological frameworks on equal footing, demonstrating how textual products can be examined under either IR or genre analysis, or, in her case, both points of view.

1.3.2.1 *Genre as a concept*

Genre has been defined in numerous ways throughout its history. Like Miller before them, Berkenkotter and Huckin's conception of genre is rooted in the typification of social and rhetorical action, although the consistency these linguists observe exists in constant interplay with the dynamism of the social networks that use these genres (Berkenkotter & Huckin, 1995; Hyland, 2000). For Martin, genre is a way of acting in the world when actions must be carried out using language (1985, cited in Biber, Connor, & Upton, 2007) and "regularities of staged, goal-oriented social processes" (1993, cited in Bhatia, 2002a, p. 5). In similar fashion, Swales (1990) and Bhatia (1993) consider genre to be that which is characterized by regularities of communicative purposes.

In general, genre analysts have tended to examine discourse in search of emerging patterns that can be used to describe the features of the discourse culture, although in doing this genre can also uncover a great deal about the context in which they are constructed. Rooted in the textual products of particular discourse communities, close analysis of discourse patterns is a rich source of information on the epistemologies, shared values, and norms of the groups that use them (Candlin & Hyland, 1999). As Bazerman eloquently states:

Genres are not just forms. Genres are forms of life, ways of being. They are frames for social action. They are environments for learning. They are locations within which meaning is constructed. Genres shape the thoughts we form and the communications by which we interact. Genres are the familiar places we go to create intelligible communicative action with each other and the guideposts we use to explore the familiar (1997, p. 19).

This quote highlights the way in which the linguistic features and internal structure that make up genres are characteristic not because of their uniqueness on the surface or other aspects that can be measured quantitatively; rather, what makes genre a rich field of inquiry is the link between language and the sociopragmatic goals and the routes taken toward those goals through the vehicle of language.

Genre is an integral part of academic pursuits and a necessary link between laboratory results and recognized scientific achievement. Research findings and knowledge claims can only be brought into the larger community after they have been given the seal of approval of journal gatekeepers, that is, individuals entrusted to ensure that the work of scientists adheres to the discourse community's recognized means of deriving knowledge as well as the proper articulation of this knowledge within the framework established by the genre. Despite this tenet of genre studies, some studies have portrayed authors' dismay at having to rhetorically package their work in ways accepted by the discourse community, with these (often novice) scientists feeling a certain loss of essence as their lab results are transposed into genre-appropriate knowledge during the peer-review process (Berkenkotter & Huckin, 1995). This apparent disconnect between the scientist-as-individual with findings to report and the rhetorical demands made by gatekeepers shows how science and rational argument are socially constructed activities (Hyland, 2000; Wood, 2001) that both adhere to and perpetuate the community-specific norms of the disciplines and establish paths for new scholars to enter their ranks.

Within the typification and representativeness found by genre analysts in their effort to explain the institutionalized ways in which individuals pursue social goals (Bhatia, 2004), genre also provides space for change, agency, and manipulation of tools for novel purposes. Recognized and

aspiring members of discourse communities such as those profiled in this doctoral thesis can use genre and interdiscursive rhetorical strategies to further their individual agendas. Genres are assets that belong to discourse communities, and these discourse communities are free to make adjustments to conventional genre patterns as the sociocognitive needs of the group evolve (Atkinson, 1999; Bazerman, 1988; Berkenkotter & Huckin, 1995) and as rhetorical cultures interact with one another (Salager-Meyer et al., 2003), possibly as a result of increasing textual consumption by NNEs authors, whose L1 writing increasingly resembles the rhetorical characteristics of English-language texts (Moreno, 1998).

1.3.2.2 Approaches to genre

Genre approaches to discourse can be subdivided into three different branches: Systemic Functional Linguistics, English for specific purposes (ESP), and the American New Rhetoric. All of these branches see conventions in discourse, and this conventionalization of communication is thought to justify the study of genres. Though having much in common, they differ in the degree of predictability and dynamism they find, as well as the importance given to social context (Handford, 2010).

Set in motion by the writings of Halliday (1978) and Halliday and Martin (1993), systemic functional linguistics (SFL) found there to be an integral link between how language is structured, the function of the language, and the context in which it is produced (Bawarshi & Reiff, 2010). Lexicogrammatical choices made by writers are studied to shed light on the topic being dealt with as discourse progresses and also to describe the writer-reader relationship (Handford, 2010). As they parse texts for analysis, certain linguists in the SFL tradition such as Paltridge (1994) find that genres are sometimes staged for reasons driven by non-linguistic, rather than linguistic concerns. When drawing boundaries between rhetorical moves, researchers use cognitive judgment rather than purely linguistic criteria (Bhatia, 1993; Kwan, 2006). Despite its proponents, SFL has also been criticized for failing to take into account the degree to which genres not only reflect ideology and social purpose, but also reproduce these dynamics (Bawarshi & Reiff, 2010).

Another branch of genre studies is the tradition that has followed the work of John Swales and his contemporaries, who are often placed under the category of ESP (Bawarshi & Reiff, 2010). Swales was the first to examine academic writing as a series of rhetorical moves, or sections of text that perform a specific communicative function (1981). Unlike genre analysts who espouse the SFL understanding of the interplay between texts, what they look like, and what they do, the Swalesian tradition privileges communicative purpose within discourse. This, however, was not always the case: in its origins, Swalesian genre analysts explored the linguistic properties of language varieties, although as the scholarship matured, it shifted toward increasingly specific genres produced by a growing number of discourse communities and centered analyses on the communicative purposes at play within moves. Subsequent work done by Swales and others refined his conception of rhetorical moves, further subdividing these functional components into steps and also searching for common realizations of moves occurring in cycles (Swales, 1990, 2004). As scholarship applied the theory to new types of communication, the notion of genre as archetypal writing patterns was reassessed, and linguists began to find ways in which writers manipulate genres in their pursuit of personal goals (Hyland, 2000).

Lastly, the American New Rhetoric is an approach to genre that uses texts to illuminate social dynamics, thus distinguishing itself from other traditions that begin with the social milieu and then work toward the texts produced within it. One of the more influential scholars was Carolyn Miller, whose 1984 paper inspired a number of researchers after her (Berkenkotter & Huckin, 1995). One of the primary concerns of the New Rhetoric was the way in which discourse was seen to be socially constructed (Biber et al., 2007), thereby paying less attention to textual products than to the processes that go into them. Writing and reading are seen as inextricably linked to individual and social processes at play within a given community. This writing-in-the-world approach is elegantly summed up as follows:

Writing is a social action; texts help organize social activities and social structure; and reading is a form of social participation; thus, saying something about writing is saying something about sociology (Bazerman, 1988, p. 10).

Genres and the meaning-making of writing are explored through a variety of ethnographic approaches, often with one approach complementing another (Bazerman & Prior, 2003).

1.3.2.3 Genre in academic publication

Although genre-analytical research has not been limited to academic discourse, the RA has received a great deal of attention in Applied Linguistics. Some studies have described the entire body of the RA (Biber et al., 2007; Gosden, 1993; Nwogu, 1997; Swales, 1990, 2004), while others have focused on sub-sections. Of these, the introduction has received a great deal of attention, possibly due to the privileged position given to it in the work of John Swales (Swales, 1990, 2004). Much of the analysis of the introduction has aimed to pinpoint interdisciplinary variation (Gledhill, 2000; L.-J. Li & Ge, 2009; Samraj, 2002a, 2002b; Taylor & Tingguang, 1991), although other aspects have also been the subject of analysis. Some noteworthy investigations of the generic traits of methods sections include the in-depth studies by Nwogu (1997) and Peacock (2011). The results section has been described in the fields of sociology (Brett, 1994) and medicine (Williams, 1999), while the discussion section has also been analyzed in a number of fields (Berkenkotter & Huckin, 1995; Dudley-Evans, 1994; Lewin, Fine, & Young, 2001; Peacock, 2002). As for the abstract, there has been debate about whether it should be considered a part of the RA proper, and some analysts have argued that it is not (Gosden, 1993; Hyland, 2000). Despite this, Samraj (2005) found presence of rhetorical moves traditionally regarded as belonging to the introduction's repertoire in the abstract and also concluded that rhetorical leanings in abstracts could also be seen in their respective introductions.

1.3.2.4 Intercultural research into academic genres

In somewhat more specific fashion, analysts of IR have applied the theoretical premise envisaged by Swales of section-specific communicative purposes realized by discourse-appropriate rhetorical moves as a basis for investigations into the rhetoric of RAs written in English by NNEs and NES authors, with some studies contrasting academic discourse in other languages with NES-authored texts. Work done in this realm has contrasted rhetorics from a number of big-culture backgrounds. Research concerning similarities and differences between Anglo-American discourse and writing by Spanish authors has focused on the presence or absence of rhetorical

moves (Lorés-Sanz, 2006, 2009; Martín-Martín, 2003; Martín-Martín & Burgess, 2004; Sheldon, 2011), academic criticism (Martín-Martín & Burgess, 2004), evaluative language (Bellés-Fortuño & Querol-Julián, 2010), authorial presence (Carcu, 2009; Lorés-Sanz, 2011a, 2011b; Mur Dueñas, 2007), and the writer-reader relationship (Mur-Dueñas, 2011). As a result, applied linguists have developed deep knowledge of not only what marks the differences between Anglophone-center texts and writing by NNES academics either in their native language or when writing in English, but also of the particular rhetorical and discursive tools employed by academics from one section of the IMRD structure to the next as they attempt to achieve their goals. These findings are then used as a platform upon which to form hypotheses on why these differences may exist, thus feeding into further scholarship at the crossroads between genre studies and IR.

One such potential explanation comes to us from analysts such as Carciu (Carcu, 2009, p. 88), who remarks that intercultural differences in rhetoric may be more appropriately seen through the lens of written ELF, as certain small-culture realities faced by non-center academics (e.g., the need to foreground “newsworthiness” in research) may constitute communicative purposes in their own right, and thereby be reflective of rhetorically fit-for-purpose writing. Others also approach the phenomenon of NNES rhetoric from a similar angle. These efforts have resulted in the creation of the Written Academic ELF Corpus by researchers and collaborators at the University of Helsinki (<http://www.helsinki.fi/englanti/elfa/wrelfa.html>) and numerous academic publications (Mauranen 2005, 2011, cited in Johns 2013: 14; see Jenkins 2011 for a review of ELF from a broad perspective).

1.3.2.5 Toward “thicker” descriptions of genre

Despite the many ways genre studies has helped us understand professional and academic communication and the many currents that underlie academic publications, certain myths about the perspective taken by genre analysts have pervaded. One such belief is the misconceived notion that genre analysis serves to reproduce discourse forms that understand the world in simplified terms, and another is that genre takes a static view of discourse at the expense of author agency (Bhatia, 2002a). These claims are refuted by genre analysts, who insist that this notion of genre is outdated, and that developments in the field have opened up generic inquiry

toward “thicker” descriptions (Bhatia, 2002b, p. 21) of language use, including particular awareness of the context-specific forces that motivate discourse (Bhatia, 2002a, 2002b, 2008). Genre analyses would benefit from greater attention to small culture, as such broad criteria as NES or NNEs status, and even disciplinary culture may obfuscate relevant dynamics at play in text production (Gledhill, 2000; Mur-Dueñas, 2011; Mur Dueñas, 2007; Taylor & Tingguang, 1991).

1.3.3 Corpus studies

A corpus is a sample of naturally occurring language that can be used to study a number of features about language and language use. These collections of texts can range in size from small compilations of discourse to multimillion-word databased, and both types lend themselves to qualitative and quantitative study (Hunston, 2002) and are amenable to study with any linguistic theory (Handford, 2010; Kennedy, 1998). Corpora are often used to establish frequencies of many types, and to explore phraseology and collocation. When designing corpora, close attention must be paid to the purposes that the corpus will serve, as this will have implications on issues such as the required size, the contents of the corpus, its degree of balance and representativeness, and also its continued validity and usefulness as time passes between the moment the corpus is assembled and the time it is used for investigation (Hunston, 2002).

1.3.3.1 Corpus-driven vs corpus-based approaches

Despite the challenges of analyzing discourse using corpora, corpora can reveal different issues about language in action. First, we can mine corpora to examine phrase and clause structure and linguistic variation. Second, aspects of language beyond the sentence can come to light when we look at extended sequences of utterances or sentences in an effort to learn more about the logic that explains the systematic configuration of utterances in texts. Third, corpus data can teach us more about the social practices and ideologies at work in language, showing why particular actors use language in particular situations and what person-centered factors are brought to light through discourse (Biber et al., 2007, pp. 2-3).

According to Biber (2007), discourse analysis using corpora is either done by applying a bottom-up or a top-down approach. In a way that mirrors to some extent the dichotomy between corpus-

driven and corpus-based approaches (Tognini-Bonelli, 2001), top-down approaches begin the analysis with an analytical framework that is decided beforehand and then applied to the task of segmenting all the discourse units called for by the framework. Once this is accomplished, the discourse units are classified, starting with the linguistic components (lexis/grammar) contained in each unit, and then using these observations to arrive at broader and broader conclusions on the discourse categories, text structure, and organizational tendencies exhibited by the genre. When taking a bottom-up approach, however, the analytical framework emerges from the analysis and is not fit into a preconceived framework. A comparison of two highly influential studies illustrates this dichotomy to some extent: on the one hand, Ken Hyland's (2000) *Disciplinary Discourses* takes a bottom-up approach to genre inquiry within a broad range of academic genres and disciplines, arriving at conclusions which are then triangulated using other methodological tools. Meanwhile, John Swales's (1990) *Genre Analysis* is top-down in that it starts from the perspective of texts' macrostructure as it is realized through the deployment of rhetorical moves; once these rhetorical moves are described both in their components and configuration within discourse, Swales goes on to describe the genre and common patterning of moves.

Analysts who apply a corpus-driven approach tend to conceive of corpus linguistics as a theory in the sense that the corpus is a source of data from which a number of conclusions can be reached by observing patterns that emerge during analysis (Tognini-Bonelli, 2001). During these analyses, lexical items, or nodes, can be isolated within concordance lines to explore previously unknown aspects of lexis and semantic prosody and, possibly, collocation, coligation, and semantic preference. Though parameters such as semantic prosody require human input (L. Flowerdew, 2012), corpus-driven analyses tend to be more quantitatively oriented and do not use any other particular theory as a starting point.

For their part, linguists taking a corpus-based approach use corpora as a methodology (Connor, 2013; L. Flowerdew, 2012), that is, a means of exploring hypotheses outside of the corpus. Much of this research seeks to use these collections of language to further knowledge of discourse. Corpora-based analyses can benefit other qualitative methods such as text studies, case analyses,

and observations in order to gain new insight into IR (Connor, 2013); indeed, the introduction of corpora into IR has brought with it greater methodological rigor, especially inasmuch as it leads analysts to ensure *tertium comparationis* (Connor, 2004). Unlike their corpus-driven counterparts, many corpus-based studies make extensive use of tagging to analyze the issue under analysis. Tagging beyond the level of proposition is a challenge for discourse analysts, however, as it involves identifying functional and rhetorical aspects (Upton & Connor, 2001), both of which are highly dependent on contextual and generic considerations and therefore require extensive, and somewhat subjective, human intervention. In addition, tagging of discourse-level features such as rhetorical moves may be complicated by the absence of a highly conventionalized structure in the genre depicted (L. Flowerdew, 2002, 2005), thus necessitating new ways of combining the ability of computers to process more easily quantified aspects of language such as lexico-grammar and more situationally sensitive analyses such as those performed by genre analysts (L. Flowerdew, 2005; see Upton & Connor 2001 for an example).

Despite the fact that corpus-based scholarship appears to examine language use with a broader lens, some critics have challenged the utility of corpora to further knowledge of discourse. One such critique calls into question its absence of context, culture(s), agency, and processes, that is, many of the pragmatic forces that lie beyond the word or sentence level. As stated by Hunston, (2002), a corpus is essentially a decontextualized collection of language, and “can show nothing more than its contents” (p. 22). Such uprootedness runs contrary to the acute sensitivity of social action that is central to certain approaches to discourse analysis, as it essentially ignores the social dynamics that motivate discourse in the first place. Also, by understanding a text as a product rather than the physical artifact that results from a process of meaning-making in society (Biber et al., 2007), corpora require additional points of analytical entry to serve as effective tools for discourse analysis.

1.3.3.2 *Specialized corpora*

Specialized corpora are corpora that represent a given type of text. The boundaries used to limit the texts included can be broad or narrow, and also may be set to reflect a given time frame or social setting. Example of large-scale specialized corpora include the Michigan Corpus of

Academic English (MICASE), which gathers spoken-register English in a U.S. academic setting, and the Cambridge and Nottingham Corpus of Discourse in English (CANCODE), a collection of informal British English (Hunston, 2002). Specialized corpora are generally considered to be more useful for the study of discourse structure (Biber et al., 2007) because of their sensitivity to writing requirements and contexts; indeed, they are more suitable for analysis of academic and professional discourse than general corpora because a) general corpora, which strive to represent the language as a whole, have a comparatively low presence of certain genres; b) retrieval of specialized realms of discourse within a specialized sub-corpus is often made difficult by the fact that general corpora were not designed for these types of inquiries; c) certain types of discourse are not widely available for collection; and d) some general corpora only include texts fragments of a limited length, thereby obscuring certain aspects of text production which could have implications for the overall understanding of the text. One of the advantages of certain specialized corpora is the fact that in many cases, the corpus analyst is also its compiler, who has privileged information on the socio-pragmatic circumstances in which the texts were produced, thus ameliorating the problem of the disjuncture between concordance lines and context (L. Flowerdew, 2004). Also, the type of fine-grained discourse analyses that specialized corpora are used in generally cause them to be smaller (Handford, 2010) and more amenable to qualitative rather than quantitative research approaches (L. Flowerdew, 2004).

Corpus-based and corpus-guided research has been used widely within genre studies. Large-scale studies have investigated the generic features of direct mail letters (Upton, 2002), job application letters (Upton & Connor, 2001), RAs (Biber et al., 2007; Davis, 2015; Gledhill, 2000; Luzón-Marco, 2000), RAs and book reviews (Groom, 2005), scientific discourse and its development over time (Atkinson, 1999), and literature reviews in doctoral theses (J. Flowerdew & Forest, 2009). With the exception of the preliminary study by Upton (2002), these and other studies like them are of particular interest to genre analysis in that they harness the ability of concordancing to perform quantitative searches so that data on frequencies can be mapped against the rhetorical moves in which these words occur, thereby shedding light on not only the rhetorical macrostructure of these texts but also the lexico-grammatical tools used to realize this structure.

1.3.3.3 Limitations of specialized corpora

Some linguists have found specialized corpora and genre analysis to be incompatible. One such point of criticism questions the size and degree of representativeness of specialized corpora. When populating a corpus with specialized discourse, representativeness may be compromised by a reliance on an expert's opinion of which text source to investigate, a lack of criteria for the selection of texts, the inclusion of heterogeneous genres in the sample, and use of different publications, possibly with different guidelines for contributions (Biber et al., 2007, pp. 73-74). Decisions on corpus size, source, and the ease with which the data can be obtained often depend on issues of practicality (Biber, 1993; L. Flowerdew, 2004), although one guiding principle for the design of suitable specialized corpora is the availability of the linguistic feature or pattern within the corpora to the extent needed to test hypotheses (L. Flowerdew, 2004). Some scholars believe that studies of genre should account for culture (e.g., L. Flowerdew, 2002), and by combining interculturally sensitive data with corpora from the genres, analysts will be better able to study texts in their intercultural context (Connor, 2013). Indeed, some advocate a more pluralistic view of global academic writing, urging other analysts not to limit their investigations to NES writers' discourse, but rather to perform IR studies based on ELF corpora (Baker, 2013, p. 23).

A second point taken up by critics of corpus use in genre analysis comes from Swales (2002, cited in Flowerdew, L. 2005), who finds corpus analyses to preclude perspectives on genre that originate with the concept of the macrostructure. In defense of corpus use in genre analysis, however, Lynne Flowerdew mentions a number of effective studies that make use of tagging to explain genres as well as some that use other means (ibid.).

Third, linguists who use corpora across different discursive settings have stressed that concordance lines are incomplete if they are not coupled with some way of contextualizing the discourse (L. Flowerdew, 2002; Mur-Dueñas, 2011). Situational gaps can be filled in by including ethnographic aspects in research design, even as concerns the sample used in the corpus (Gledhill, 2000) or as a way to both confirm and contextualize the linguist's findings (e.g., Berkenkotter & Huckin, 1995; Hyland, 2000).

1.3.4 *Ethnography*

Attempts to explain the phenomenon of writing for the academy, the constraints posed by the genres of academic discourse and the way users manipulate these constraints in pursuit of their objectives, and the specific circumstances inhabited by NNES authors attempting to publish globally have led some scholars to highlight the importance of more contextually sensitive analyses. Despite sophisticated analysis into linguistic patterns made possible by IR, genre analysis, and corpus studies as well as work that combines these approaches, descriptions that obviate the personal, cultural, and political forces that give shape to writing may be incomplete. Indeed, theorists in all three of the aforementioned theoretical and methodological traditions have raised doubts about studies that frame analyses around texts as products, suggesting that understanding texts as processes may be more appropriate.

Scholars who advocate situationally sensitive accounts of writing find that the principles of ethnography can help researchers better triangulate textlinguistic studies (Bhatia, 2004; J. Flowerdew, 2002; Mur-Dueñas, 2011). It has been argued that investigations rooted in a genre-analytic approach stand to benefit from text-external data sources to complement rhetorical analyses (Bhatia, 2008; Connor & Mauranen, 1999; Mur Dueñas, 2007). Use of a range of ethnographically-inspired tools can more fully depict the background and history of final published products (Bhatia, 2008; Connor, 2004). The limitations of writing that is extracted from its procedural and social context is eloquently portrayed in the following quote:

Writing as text is [thus] not usefully separated from writing as process and interpretation, and neither can easily be divorced from the specific local circumstances in which writing takes place nor from the broader institutional and socio-historical contexts which inform those particular occasions of writing. (Candlin & Hyland 1999: p. 2)

Here, the two authors underscore the way in which written artifacts conceal a great deal of their reasons for being. In separating our gaze from textual analysis, we quickly encounter the way in which the text is both meant to be construed by the discourse community and how it is actually received by scientific peers. And beyond the consumption of texts, Candlin and Hyland point out the many situational and cultural traces that can be discovered if we peer beneath the textual surface.

1.3.4.1 Challenges to ethnographic analysis

Enthusiasm for ethnographically leaning research contrasts somewhat with some scholars' skepticism of the appropriateness of the approach and also the challenges posed by the methodological demands of ethnography proper. Eager to increase the situatedness of discourse analysis, Swales (1998) embarked on what he termed a "site-based textographic study" (p. 2) to portray a certain cohort of authors' writing within the context of their careers, discourse communities, and local setting in a North American university. Swales, however, approaches ethnography at arm's length, highlighting the differences between true ethnography and his field of expertise as well as a possible disconnect between work whose essence is focused on texts and writing on the one hand and the type of engagement demanded by ethnographic work on the other. Another influential theorist, Ken Hyland, also remains faithful to his academic forte of detailed text analysis as he explores the options afforded by ethnography. Hyland (2000) notes how textlinguistic analysis dematerializes texts, studying interaction through the lens of groupings of language, while ethnographic studies use texts as the starting point as they conceptualize the actual human interactions they represent (pp. 132-3). This dichotomy sets up a rather clear boundary between text and context, which may limit the situational depiction sought by discourse analysts. This delimitation causes studies to position themselves along a continuum: text-focused studies on one end, and writer-focused studies on the other (Lillis, 2008).

As remarked by Swales (1998), the methodological rigor of ethnography poses another challenge for those who seek to adapt it to EAP and other related fields of study. For ethnographic findings to be reliable, they must be the result of prolonged engagement with the subjects and cultures studied, triangulation of data, and to some extent, participant verification (J. Flowerdew, 2002, p. 237). By using these three guidelines, researchers ensure the exhaustiveness of their observation, test hypotheses that arise during the study from different methodological angles, and, when applied, look to participants to confirm their findings (e.g., Mur-Dueñas, 2012). In recognition of the possible shortcomings of his and other studies from the lens of ethnography, Flowerdew makes the careful distinction between "qualitative/naturalistic methods" on the one hand, and "ethnography proper" on the other (ibid.). As shown by Hyland, Swales, and

Flowerdew, textually rooted analyses can adopt part of the sensitivities of ethnography, although true ethnography demands a more involved approach (for an overview of the vibrant field of linguistic ethnography, see Maybin & Tusting 2011).

Lillis (2008) outlines the ways in which studies of writing can harness the power of ethnography while recognizing the extent of methodological rigor in each and classifying the studies accordingly. Studies that feature minimal engagement with authors, their surroundings, and their processes are categorized as *ethnography as method*. Some research uses “talk around texts,” whereby the analyst uses texts to elicit writer perspectives and therefore arrive at a more in-depth understanding of the choices writers make and the reasons behind those choices. Investigations under this category can either be more text-focused, or *etic*, using the author’s viewpoint to inform the researcher’s observations and queries, or can be more *emic*, foregrounding the writer’s perspective and using texts as an additional lens to portray their circumstances. The second category, *ethnography as methodology*, bases findings on multiple data sources and is characterized by greater researcher engagement, thus foregrounding the ethnographic component to a greater extent than ethnography as method, seeking “thick description” (Geertz 1973, cited in Lillis 2008: p. 367) of the phenomenon. Studies such as these can draw analysis away from the *etic* and more toward a situationally faithful account that has the writer as the locus. Last, *ethnography as deep theorizing* addresses the text-context divide in research on academic discourse and failures of EAP and academic literacy studies to integrate the reality of writers and the texts that they write using, among other tools, “indexicality,” or the way what is spoken or written point to certain aspects of social context, and “orientation,” the way in which users of language take in and process what they hear and read (ibid.).

1.3.4.2 *Studies performed*

When exploring the agents, processes, and relationships that converge when writers seek to participate in discourse communities, one natural point of departure is the viewpoint of authors, be they NES or NNEs. As I will show, however, the exercise of distilling the writer’s perspective is often closely entwined with such issues as English language proficiency, differences between disciplines, the genres of the discourse community, the influence of other members of the

discourse community, and also certain non-members who contribute to academic text production. In an attempt to address the issue of what is at stake for NNES academics who attempt to contribute to international English-medium journals and how they fare in this process, some context-sensitive studies have been conducted to more fully decipher what is meant when journal gatekeepers criticize “the English” without providing sufficient explanation on the particular aspects of the language in a paper they found to be needing improvement. To achieve this, analysts with access to authors resort to the “occluded genre” (Swales, 1996) of the correspondence that takes place between submitting authors and journal reviewers and editors (Belcher, 2007; S. Cho, 2004; Englander, 2006; Gosden, 1995, 2003; Huang, 2010; Kourilova, 1998; Mungra & Webber, 2010; Mur-Dueñas, 2012, 2013). These comments can be used to examine the type of changes requested and deficiencies found, pinpoint the success factors that aid in texts securing publication, and describe the functional thrust of gatekeeper content, among others.

Ethnographic studies have been used to explore a number of cultural and situational factors that shape the experiences of NNES authors seeking to publish. Connor (2000) used text-based interviews to complement a genre-analytic study of IR, querying authors in an attempt to confirm or challenge genre-analytical frameworks in a corpus of grant proposals. Of interest, she found salient differences between writer perceptions based on gender, especially as concerns male and female writers’ insights on the rhetorical move in which researchers assert their competence to the reader. This and others like it have foregrounded the perspective of learners of or novices to writing in English for academia (Gosden, 1996). By undertaking a qualitative case study, two authors examined the notion of culture as experienced by one of them, attempting to illustrate how culture is co-constitutive her daily actions. The authors found that identity is not uniform, but rather variegated (Atkinson & Sohn, 2013). Other work has specifically targeted author attitudes on the endeavor of writing in English within the broader question of access and equity in global academic production, contributing more detailed knowledge on the specific needs of Spanish scholars as well as the motivations, experiences, difficulties, attitudes, and strategies of these academics and the degree to which these perceptions may be influenced by the discipline within which they publish (Burgess et al., 2014; Gea-Valor et al., 2014; Lorés-Sanz, Mur Dueñas,

Rey-Rocha, & Moreno, 2014; Martín-Martín, Rey-Rocha, Burgess, & Moreno, 2014; Moreno, Rey-Rocha, Burgess, López Navarro, & Sachdev, 2012; Mur-Dueñas, Lorés-Sanz, Rey-Rocha, & Moreno, 2014).

To a large extent, the way NNEs authors respond to the feedback they receive during the peer-review process is built upon the type of feedback they receive. A number of ethnographic studies have been carried out to show how the work of academics meets the demands of these gatekeepers, as doing so can indicate some of the deficiencies editors and reviewers found with the originally submitted product. One way of conducting such a study is to compare the first and final draft, as done by Knorr-Cetina (1981), classifying the types of changes that seven novice authors' texts underwent within the review process. In work by other authors, revisions made between early drafts and final versions have been used to gain insight on the textual practices, experiences, and viewpoints of NNEs authors, yielding interesting data on the real changes that are made to texts to bring them from so-called deficient English to acceptable final products (Belcher, 2007; Englander, 2006; J. Flowerdew, 2000; Gosden, 1995; Petersen & Shaw, 2002). Studies that follow these lines have been designed as case studies (J. Flowerdew, 2000; Gosden, 1995), "submission histories" (Belcher, 2007), and collections of papers from authors from the same home country (Englander, 2006).

Ethnographic approaches can also move the scope of analysis beyond the author to include other individuals who influence texts as they progress from early drafts to finished products. Work in this vein has been carried out on NNEs students (J. Flowerdew, 2000; Y. Li & Flowerdew, 2007) and professional academics (Curry & Lillis, 2010; Lillis & Curry, 2006a, 2010; Ventola & Mauranen, 1991). Some of the studies have taken up the many roles, remits, and degrees of expertise that language professionals can bring to bear on texts (Burgess & Lillis, 2013; J. Flowerdew, 2000; Lillis & Curry, 2006a, 2010; Mur-Dueñas, 2013; St. John, 1987), while others include the accounts of academic supervisors along with their students, whom they are teaching to navigate the processes of writing for the academy (D. W. Cho, 2009; Gosden, 1995; Y. Li & Flowerdew, 2007).

Also, ethnography can be used to portray the attitudes held by the members of discourse communities on a number of issues, including opinions on genres, the ways these specialized forms of communication affect researchers' lives, and the teaching of genres to novice scientists (Atkinson & Sohn, 2013; Bhatia, 2002a; Moreno et al., 2012; Mur-Dueñas et al., 2014) or to test theories on the impact of the dominance of English by basing studies on empirical evidence (Canagarajah, 1999; Hanauer & Englander, 2011). One of the earliest such studies was conducted by St. John (1987), who analyzed the problems, strategies, and attitudes of scientists in Spain, finding a certain degree of resentment toward the need to publish in English.

As indicated by Lillis (2008), many attempts to contextualize research have been influenced by a formalist perspective on text production, therefore causing them to be more *etic*. Within this general framework, however, scholars have employed a range of tools. A good example of this is the aforementioned work by Hyland, who applies ethnology as method to a broad study of academic writing across different genres. To situate his findings on the genres of academic writing, he uses a number of tools such as interviews with subject experts on their communication practices, unstructured interviews with members of the discourse communities from which the texts in his corpus were taken, and also discourse-based interviews to obtain expert feedback on particular uses of discourse within the professions. Other analysts have combined linguistic analysis with questionnaires (Kourilova, 1998), while Uysal (2008) conducted stimulated recall interviews to ask authors why they made particular decisions in their papers. One limitation of approaches like the one used by Hyland, which first compile large corpora of representative texts within a genre based on expert opinions of what constitutes a relevant journal, is the subjectivity of this selection criterion (Biber et al., 2007). In addition, the remove in space and time imposed by using international scientific journals as a source makes true contact with the authors unlikely. However, as membership in the discourse community is granted through these publications, it can be argued that successful academic publishing is an adequate methodological filter (Connor, 2004). Research that circumvents the problem of limited access to the contexts of creation such as the one advocated by Lynne Flowerdew (2004) are effective in reducing this distance, especially when a large-scale survey (J. Flowerdew, 1999b) is followed by micro-level interviews with selected scholars called on to contextualize data by

expressing their perceptions, problems, and strategies (J. Flowerdew, 1999a). Despite these attempts on the part of researchers to sound out NNES authors, their condition as outsiders to a disciplinary committee means they do not have the same expertise as its members (J. Flowerdew, 2000).

The ongoing work of Theresa Lillis and Mary-Jane Curry, working both as coauthors and individually, has adopted a particular form of ethnographically inspired inquiry in the authors' attempts to answer the question of what goes into successful English-medium academic production (2010). Using what they refer to as a "text-oriented ethnographic" approach (Lillis & Curry, 2006a, p. 7), they developed a series of tools with which they have followed a population of 50 scholars from non-Anglophone contexts working and publishing within the fields of education and psychology. Consciously avoiding the "textualist stance" (Lillis & Curry, 2010, p. 21) taken in much of the EAP literature, their research is based largely on what they term "text histories," or the arc followed by texts as they progress from early drafts, through different coauthors and also as they are influenced by literacy brokers, the many individuals who, while not being named authors on the manuscript byline, are nonetheless part of the text-shaping process. Literacy brokers include all editors, reviewers, academic peers, and any English-speaking friends, colleagues, or professional assistance that can influence an academic text along its trajectory (Lillis & Curry, 2006a). Insights from their scholarship have shed light on a number of attributes of successful academic work, including the extent to which certain well-connected individuals in non-Anglophone countries can catapult more novice scholars into the international arena by brokering their entry into national and then international networks of scientists, which often is a substantial windfall for these up-and-coming NNES scholars (Curry & Lillis, 2010).

Methods

1.1 Chapter overview and context for the study

I will begin this chapter by briefly describing the career events that led me to conduct this study. I will then explain how the study is designed and how it draws from previous research in Applied Linguistics. Following this, I conclude with an account of the steps taken before, during, and after my field work, including a number of general observations on the methodology used and other findings that may be of use for subsequent studies taking a similar approach. My decision to pursue a doctorate by researching NNES academic writing in English has been marked by the turns my career has taken and the ways I have gone about adapting to the challenges of translating and editing in highly specialized realms of technical discourse. In 2009, I took on the role of translator, editor, and English language instructor at the IIS-FJD. This opportunity involved working with a wide range of medical texts, and I soon came to realize that by familiarizing myself with the research on the genres of these texts, I would be better equipped to meet the expectations of the institution's research staff.

1.2 The in-house editor

To arrive at an accurate description of the work I do for the biomedical researchers, medical doctors, and nurses affiliated with the IIS-FJD is not an easy task. Colleagues of mine and academics who study the profession have struggled to arrive at a universal description for the type of work I do, and studies on the involvement of language professionals in NNES academic writing in English have assigned a number of titles to the figure, including correctors, revisors, local editors, language professionals, language service providers, and authors' editors (for a brief account, see Burrough-Boenisch 2003: 225). This absence of a standardized terminology can likely be attributed to the diverse roles, remits, and relationships that language professionals take on (Burgess & Lillis, 2013; Koyalan & Mumford, 2011; Shashok, 2001), although recently the professional association Mediterranean Editors and Translators have published the second edition of their guide to choosing appropriate language support, which contains a glossary of terms (<https://www.metmeetings.org/en/how-to-choose-an-editor-translator-interpreter-other-language-professional:799>). Initiatives like this one, which received input from a range of

experienced translators, editors, interpreters, and other language specialists, are a step toward aligning clients' needs and the range of skills different language professionals can offer to assist them.

I will begin this description of my professional context by making some key distinctions between my circumstances and those of others like me. First and foremost, I am a salaried employee of my institution, and as such receive a stable income independently of the demand for my services, my output over time, or the degree of satisfaction with my work. Unlike self-employed professionals, the authors with whom I collaborate have much broader access to my services, which come at no cost. There are no restrictions on the genres of text they may submit, and over the years I have worked with a number of academic genres and a wide range of other texts, both for publication and for other purposes. Authors may send me their work in any state they wish, and some send me early drafts so that I can improve the text before the paper is sent on to their coauthors, and sometimes I also edit a later version that includes these post-revision contributions. The circumstances of my relationship to my employer (the institution) and my clients (NNES authors employed by the institution) shape the way I orient to their texts and the strategies I employ to satisfy their demands, causing my work to differ from that of a freelance. In particular, I generally devote a shorter amount of time to each text, am less likely to research problematic passages, and the margin comments I write for the authors in my institution display less concern for diplomacy (Shaw & Voss, in press).

My setting is most likely unique for the reasons I have touched on. However, the situation of other language professionals within institutional settings varies considerably. Differences stem from the characteristics of the organization, the priority given to editing services with respect to other duties, and the presence or absence of a defined work flow. Many like myself are employed by universities (J. Flowerdew & Wang, 2016; Harwood, Austin, & Macaulay, 2009; Koyalan & Mumford, 2011; Ventola & Mauranen, 1991; Willey & Tanimoto, 2012); indeed, the literature contains fewer accounts situated in research institutes like my own (cf. Nygaard, 2015), although I have met others employed by teaching hospitals, biomedical and other research institutes, and similar organizations operating within the knowledge economy.

People who revise NNES academic texts in institutional settings commonly lack specific training for the task, so it would appear that revision services are given low priority; many are English teachers first and foremost (J. Flowerdew & Wang, 2016; Willey & Tanimoto, 2012), though some institutions have a writing center set up to provide these additional services (Harwood et al., 2009; Koyalan & Mumford, 2011; Ventola & Mauranen, 1991). My work as editor of academic texts is also ancillary to other responsibilities. In addition to editing academic papers, I also translate both academic and non-academic texts; serve as liaison interpreter for top-level meetings, visits of the facilities, and external assessments performed for a variety of reasons; and teach English to a range of students, both researchers and executive-level employees. As a result, I must meet a number of differing expectations in my day-to-day work. Within my editing function, my job lacks an official framework of obligations, priorities, and procedures, thus marking the difference between my position and that of an institutional editor (Shashok, 2001). In an effort to best adapt to my circumstances, the services I render for IIS-FJD researchers have evolved over time as I have become familiar with the institution, my researcher-clients, the types of texts they send to me for revision, and the needs manifest in their work.

In this doctoral thesis I will refer to my role as that of an “in-house editor.” As I have shown, this title omits a number of the aforementioned factors that influence the time I spend on editing tasks, my level of expertise with academic genres and NNES writing, and the expectations placed on my work. However, I believe it to be the most accurate description. While it is true that my interventions are often at a greater depth than proofreaders, time constraints and other factors prevent me from providing the type of developmental (Salager-Meyer, 2008) or substantive (Kerans, 2001) approaches taken by others.

1.3 Genesis of the study

My decision to perform this study can be traced back to my exposure to research-informed training I began to receive years ago to strengthen my skills as an editor and translator of academic texts. The published literature in Applied Linguistics has different research aims, including that of describing academic writing in its many generic manifestations and across numerous disciplines, using this knowledge to design teaching materials to equip writers to

produce effective texts, and also to use empirical evidence to inform language editors and translators about the texts that they aim to produce and the many dynamics at play behind those texts. Insights from the literature have added to the understanding of how language brokers contribute to the text-production process (Burrough-Boenisch, 2003; Lillis & Curry, 2006a, 2010), editing strategies used by these collaborators (Mur-Dueñas, 2013; St. John, 1987; Willey & Tanimoto, 2012), and the limits and limitations of language brokers (Harwood et al., 2009; Y. Li & Flowerdew, 2007; Ventola & Mauranen, 1991). Other studies have explored the difficulties that NNES authors encounter throughout the writing and publication process (Mur-Dueñas, 2012; Murillo-Ornat, 2012), the burden that these and other difficulties may introduce (D. W. Cho, 2009; Hanauer & Englander, 2011; Huang, 2010), and the different attitudes that NNES academics display toward the inclusion of language brokers (J. Flowerdew, 2000; Mur-Dueñas, 2013; Mur-Dueñas et al., 2014; St. John, 1987; Ventola & Mauranen, 1991). Thanks to the work done in the field, editors like me can resort to published research to gain a fuller understanding of the factors behind successful publishing by NNES academics (Gosden, 1995, 1996) and compare their experiences with data derived from editor surveys (J. Flowerdew, 2001; Gosden, 1992; Iverson, 2002) and studies that analyze the referee comments received by NNES scholars (Gosden, 2003; Kourilova, 1998; Lillis & Curry, 2015; Mungra & Webber, 2010). Lastly, editors' and translators' output can benefit from genre-sensitive analyses of collocation patterns (Gledhill, 2000) and collocational frameworks (Luzón-Marco, 2000), and those wishing to advise clients on trends in academic publishing can learn from the findings in Kerans et al. (2016) on the conventions of titles or even use these authors' approach to perform their own corpus-based analyses.

In the hope of comparing my work with that found in other accounts and furthering knowledge on NNES academic writing in the medical sciences in Spain, in 2011-2012, as part of a master's degree in Applied Linguistics, I undertook a study of the rhetorical structuring of the discussion section in a sample of ten different RAs, each by a different author from my institution. As my research progressed, I began to see how the study design held potential for broader analysis, especially as my conversations with the authors of the texts segued into talk about their experiences when writing in English for international publication and the strategies they used.

What had originally been planned as more specific interviews with these specialist informants to verify my move and step assignments as part of my rhetorical study soon took on a more ethnographic nature.

Having completed the original study, I endeavored to perform a similar analysis, though one with greater breadth and depth. As in my previous work, I focus on the RA, as this is widely considered to be the genre of academic research *par excellence* (Canagarajah, 1996; J. Flowerdew, 1999b; Gosden, 1992; Moreno, 2010; Swales, 1990). Unlike my earlier research, in this study I include all sections of the RA in my text analysis and use an ethnographic approach for deeper study of authors' writing and the processes, agents, challenges, and other experiences that shape this group's ability to publish internationally. I track the progress of 10 RAs—each written by different researchers affiliated with the IIS-FJD—noting how these publications evolve across multiple drafts and sources of input. I pay particular attention to rhetorically significant changes that take place along the texts' trajectories and describe the circumstances in which this rhetorical shaping occurs, that is, which agents cause these changes to take place and what kinds of transformations are required for the authors' RAs to secure publication. My hope in portraying the publication process from this wide-angle lens is to broaden the knowledge of the numerous factors inherent to NNES writing, as greater awareness of these factors can inform the work of designers of EAP materials, instructors of writing courses for NNES academics, and the language brokers who are called on to assist these writers.

1.4 Study design

To achieve my aim, I bring together two methodological approaches within a qualitative study design. On the one hand, I use an adapted version of text histories (Lillis & Curry, 2006a, 2010) as a means of painting a more nuanced picture of the processes behind these cases of academic writing. On the other, I bring my focus to the texts themselves by drawing upon previous work in genre studies carried out within the field of EAP, especially those that have built on the work of John Swales to describe the rhetorical structuring of RAs. Many previous studies of genre structure have taken a quantitative approach (e.g., Biber et al., 2007; Hyland, 2000; Peacock, 2011; Upton & Connor, 2001) to identify rhetorical moves, their degree of frequency, and other

aspects; I, however, opt to use my analysis of the texts' genre structure from a qualitative perspective, focusing on the rhetorical traits of the text with a view to a more general analysis of the writing process. Similar to Bennett (2014), I pay particular attention to discourse features that seem unorthodox and use these cases of apparent divergence to learn more about why the authors may have chosen to write in this fashion and how this manner of writing was received in the peer-review process.

1.4.1 Sampling method and study sample

Ten informants were recruited to participate in this study, each contributing materials from one RA which were then used to construct a text history (TH) of the RA. I used convenience sampling, which is a method of non-probability sampling in which a primary factor driving sample selection is the availability and proximity of the study sample from the perspective of the researcher (Dörnyei, 2007, pp. 98-99). At the same time, I sought to obtain the most representative sample by recruiting informants from different fields of specialism, degrees of experience, and gender. Specifically, I contacted a variety of researchers for whom I had done previous editing work. Given that the editing services I provide for IIS-FJD researchers is optional, there is an additional self-selection bias affecting my sample. Furthermore, in the years I have worked in the institution I have learned that many researchers are unaware that this free service exists, which limits the researchers with whom I have come into contact and diminished the pool from which I drew my sample.

1.4.2 Rhetorical analysis

My effort to track the rhetorically significant changes that the 10 RAs underwent along their path toward publication or abandonment was rooted in a number of influential studies that have been carried out on the different sections of the RA. All of these studies are framed within the Swalesian/EAP school of generic analysis and can be traced back to the seminal work done by John Swales (1981, 1990, 2004), which examines rhetorical structure, most notably within the introduction section. Since the studies by Swales, a great deal of EAP research has applied the central concepts of Swalesian analysis to other sections of the RA, and most all of them are either based on previous research or recognize their debt to previous authors (e.g., Biber et al., 2007;

Peacock, 2002; Samraj, 2002b; Williams, 1999). Some of these have produced move frameworks for all sections of the RA (L.-J. Li & Ge, 2009; Nwogu, 1997), although most have devoted their efforts to a single section (Nwogu, 1997).

Although the work by Nwogu (1997) and the update of the Nwogu framework by Li & Ge (2009) would have produced valid results for my study—in part because both focus on medical RAs—I opted for single-section frameworks published in a variety of studies, most of which are derived from medical RA corpora.

Having made the decision to base my move coding on a single framework appearing in the literature, I then set out to select a particular framework to use as a guide for my textual analysis. I opted to study the introductions in my corpus in light of the updated framework by John Swales (2004), as all subsidiary studies can be traced back to this and previous studies by him (1981, 1990). As the methods and results sections have been found to vary most substantially between academic disciplines (Swales, 2004, p. 219), the analytical frameworks chosen for these two sections were both carried out within the field of biomedicine. For the methods section, I coded the texts according to the moves found by Nwogu (1997), selecting this over other structure analyses (e.g., Lim, 2006; Peacock, 2011) because of its greater focus on medical discourse. When studying the results sections, I chose the framework by Williams (1999), who builds his analysis on work done in sociology by Brett (1994). As with my master's thesis on the discussion, I once again used the framework by Dudley-Evans (1994); though subsequent models have been put forward (e.g., Holmes, 1997; Peacock, 2002; Ruiying & Allison, 2003), these draw from a variety of fields and were considered to be less appropriate for this research.

In my study I also included the RAs' abstracts, this despite the fact that some genre analysts claim that the abstract is an independent discourse and one that is used to promote the RA (Hyland, 2000, p. 64), while Samraj (2005) states that abstracts lack a distinctive communicative purpose (p. 151). Indeed, Gosden (1993) excludes the abstract from his analysis on the grounds that it is "not considered to be an integral part of the RA discourse but a separate distilled version of it" (p. 62). However, variations in the move structure of the abstract have been shown, suggesting

that NNES contributions to English-medium academic publications may be bringing about a change in what is considered by journals to be acceptable discourse (Lorés-Sanz, 2016), mirroring in this regard findings from other corpora (Mauranen et al., 2010). As a result, I include analysis of the abstract, which I base on the framework by Bhatia (1993). Although Bhatia's framework is not specific to medical discourse, work by Salager-Meyer (1992) includes other genres outside of the RA and therefore was less suitable for my research.

1.4.3 Frameworks used

A summary of the descriptive frameworks appears below.

Abstract (Bhatia, 1993: 78-9)

M1: Introducing purpose

M2: Describing methodology

M3: Summarizing results

M4: Presenting conclusions

Introduction (Swales, 2004: 230-232)

M1: Establishing a territory

M2: Establishing a niche

S1a: Indicating a gap

S1b: Addition to what is known

S2: Presenting positive justification

M3: Presenting the present work

S1: Announcing present research descriptively and/or purposively

S2: Presenting research questions or hypothesis

S3: Definitional clarifications

S4: Summarizing methods

S5: Announcing principal outcomes

S6: Stating the value of the present research

S7: Outlining the structure of the paper

Methods (Nwogu, 1997: 128-30)

M4: Describing data-collection procedure

- a) Indicating source of data
- b) Indicating sample size
- c) Indicating criteria for data selection

M5: Describing experimental procedures

- a) Identification of main research apparatus
- b) Recounting experimental process
- c) Indicating criteria for success

M6: Describing data-analysis procedures

- a) Defining terminologies
- b) Indicating process of data classification
- c) Identifying analytical instrument/procedure

Results (Williams, 1999: 128-30)

Metatextual Categories

M0.1: Pointer. Indicates which data are to be discussed.

M0.2: Structure of section. Indicates the order and content of the text which follows.

Presentation Categories

M0.3: Procedural. Explains how and why data have been produced.

M1.1: Statement of finding

- a) Comparison.** Compares two or more of the subjects of the study.
- b) Time-related change.** Describes change in an object of study over a period of time.
- c) Relation between variables.** Indicates relations between variables under study.
- d) Numerical.** Expresses quantitative data which could have been presented in tabular or graphic form.

M2.1: Substantiation of Finding

- a) Other finding.** Additional results produced by other variables or measurements, which serve to support or not to conflict with the result presented in category 1.1.
- b) Same topic.** Addition of complementary details of results which support a more general result presented in category 1.1.

M2.2: Non-validation of finding. Additional results that do not support, or contradict, the finding presented in category 1.1.

Comment categories

M3.0: Explanation of finding. Suggests reasons for the finding.

M3.1: Comparison of finding with the literature.

- a) Same.** Strengthens credibility of the findings under discussion by reporting findings which concur.
- b) Neither the same nor different.** Indicates that current findings neither agree nor disagree with previous findings.
- c) Different.** Indicates that present findings diverge from previous research.

M3.2: Evaluation of finding re: hypothesis

- a) Same.** Confirms that the finding(s) match the original hypothesis.
- b) Different.** Indicates that findings are not in line with the original hypothesis.

M3.4: Implications of Finding. Author provides his/her ideas about the implications and present/future consequences of the finding.

1.5

Discussion (Dudley-Evans, 1994: 225)

M1 Information Move: The writers present background information about theory, the aim of the research, the methodology used, previous research that is felt to be necessary for the understanding of what follows in the move cycle.

M2 Statement of Result: This is frequently the first move in a cycle and is followed by one or more moves that comment on the result. A statement of result either presents a numerical value or refers to a graph or table of results.

M3 Finding: The function of a finding is essentially the same as a statement of result in that it is followed by a series of moves that comment on it. The basic difference is that a finding does not present actual figures but rather an observation arising from the research.

M4 (Un)Expected outcome: The writers make a comment on the fact that a result is expected or, much more frequently, unexpected or surprising. The fact that the result is unexpected or surprising creates a need for a comment.

M5 Reference to previous research: The writers either compare their results with those found in previous research or use the previous research as a support for their own claims or explanations.

M6 Explanation: The writers give reasons for an unexpected result or one that differs significantly from previous research.

M7 Claim: The writers make a generalization arising from their results which is their contribution to the ongoing research on the topic. This is often referred to as a knowledge claim. (...)

M8 Limitation: The writers introduce one or more caveats about the findings, the methodology followed or the claims made.

M9 Recommendation: The writers make suggestions for future lines of research in the topic, or for improvements in the methodology followed in the research reported in the article

Procedures

1.5.1 Recruiting informants

After deciding upon a research design, I began the first phase of the study in June 2013, contacting the ten candidates I had selected as informants. I held brief face-to-face conversations with each informant to describe my project and ask them to kindly take part as anonymous volunteers. Three of them declined to participate, however, citing different reasons why they would not be ideal candidates. During the course of the study, three other participants were found to be unfit to serve as informants, at least independently.

Two researchers told me that they would be unable to participate because they did not expect to publish RAs over my study period. One of these individuals, whom I will refer to as Beatriz, explained that the demands imposed by her role in clinical work had limited her ability to publish. Beatriz suggested that I contact a doctoral student she knew, as this student was at the time preparing a compilation thesis, and would therefore have a higher output. This was how I came into contact with Laura, the author of TH9. A third researcher, who was to be the informant for a different TH, declined without giving any reason, although she introduced me to Manuel (TH2), who agreed to take part.

After the study had commenced, I learned through different means that three of the would-be informants did not meet the criteria for inclusion, at which point I had to make certain modifications. Details of these unforeseen circumstances will be developed within the corresponding THs, and some of the issues that precipitated these changes are of interest to my study of the practices and perceptions of NNEs scientists attempting to publish in English. In the first of the three modified THs—the eighth in this thesis—in which I reconstruct the story of a publication by Jesús, I first invited a senior hematologist to take part in the study, after which this researcher directed me to another individual, Belén, who initially agreed to participate but was not sufficiently knowledgeable about the RA I studied to answer my questions about the text. As a result, I then focused my study on the text's primary author, Jesús. In the second modification (TH7: Juan and Rafael), the paper I found to be most appropriate for analysis was more the work of Rafael than Juan, who originally agreed to participate in the study. I therefore decided to

interview both together. Thirdly, for the TH based on Federico's text (TH5), I originally contacted Federico's collaborator, Clara, although because the RA was based on Federico's recent doctoral thesis, I decided to focus my study on him.

I believe that these turns of events are linked to the execution of my study and the sacrifices I felt were necessary to convince my informants to continue participating. Although I explained the requirements of my study both in person and in a follow-up email sent to all informants, it appears that not everyone understood the type of work that I was to embark on or did not grasp the degree of detail with which I would need to explore their texts. This may have been because they failed to read the follow-up email or did not ask me to clarify the aspects they were unsure of. It is also possible that the researchers I contacted were slightly too eager to accept out of deference to me and my years of work editing their papers. Regardless of the precise cause of this misunderstanding, future studies taking a similar approach should include measures to check informants' understanding of what is required of them, especially given the substantial time investment called for when carrying out a detailed move-step analysis like the one used here.

1.5.1.1 Characterizing the study sample

The final sample of ten informants—or, in the case of Juan and Rafael, a tandem of coauthors, although I will count only Juan for this brief overview—included five of the ten informants from the previous study I had conducted for my master's degree and five who had not taken part. Of all the ten THs, I had edited papers for 8, and for two I had not, although I knew one of these individuals (Laura, TH9), and had not previously met Manuel (TH2) or had any other contact with him whatsoever. Seven were men and three were women. Their medical specialties or research disciplines were as follows: immunology/allergy, genetics, lipid metabolism, pediatrics, nephrology, hematology, orthodontics, cardiology, neurology, and vascular pathology. One was a doctoral student, two were mid-career researchers, one was a senior researcher, one an early-career clinician, and five were senior clinicians. Regarding their employment status, there were nine full-time salaried employees and one unpaid doctoral student.

1.5.2 Text collection

The second phase of the study began on July 3, 2013 when all participants received the aforementioned follow-up email informing them of the procedures I would be following. This email contained a brief overview of the study, a description of the materials I would require from the participants, and a summary of the post-analysis interview I would hold to discuss each informant's paper and the circumstances surrounding its publication.

The materials used to construct the THs are based on the methodology employed by Lillis & Curry (2006a, 2010). These included a) any and all drafts they had from one RA that they had either previously published or abandoned, were drafting at the time, or planned to begin and complete over the following two years as well as the final published text if published; b) all pre-submission correspondence between coauthors and, where applicable, other language brokers who had intervened in the text (in cases where I had edited the text, I added any correspondence I had kept in my files); c) covering letters; and d) post-submission correspondence with journals, including all referee reports, editor decisions, point-by-point replies, and acceptance letters. Due to the high volume of texts and other materials involved in each TH and the complexity of collating these materials to establish their chronology and analyze them according to a common set of analytical considerations, the THs were compiled and analyzed in consecutive fashion. Therefore, for a given TH, I gathered the RA drafts and other documents to be included in the TH, performed the text analysis, drafted the interview questions, conducted the interview, and carried out a preliminary analysis before devoting my attention to the next TH.

I collected all texts, performed textual analyses, and held all informant interviews between July 2013 and July 2015. For each TH, I first contacted the author to discuss which RA would be most appropriate for my analysis. This decision was based solely on the availability of materials and not on any other considerations such as the authors' satisfaction with the outcome of their efforts, the number of journals that had rejected the paper before it was finally published, impact factor, or other criteria. As shown in the table below, the number of written documents ranged from 11 to 37, and the mean number of documents per TH was 18.3. This variation was influenced by such factors as the number of coauthors who produced drafts of the manuscripts or comments

on existing drafts, differences in drafting styles, the number of journals the RA was sent to before securing publication, and the number of related documents saved by the authors such as correspondence between coauthors.

Text history	Number of documents studied
TH1	11
TH2	25
TH3	11
TH4	14
TH5	13
TH6	24
TH7	37
TH8	16
TH9	19
TH10	13
	Total: 183

1.5.3 Constructing the text histories

Throughout this phase of my research in which I attempted to establish the overall trajectory of the ten publications, I encountered certain issues that slowed down the pre-analysis phases. As mentioned above, the authors in my sample varied in the number of drafts produced, though it also became clear in this and later stages of my study that not all of them are accustomed to saving all drafts of their manuscripts. Another challenge I faced was in interpreting the system they used to distinguish between drafts of a given RA; in cases of doubt, I asked the participants to confirm the correct sequence of the different versions, although in one case the author was unable to do this because he did not recall. Other factors that may have altered the materials I studied were cases in which the authors had annotated manuscript drafts on paper and had since misplaced these documents, and other instances in which participants explained that they had modified their texts in light of face-to-face meetings between coauthors for which there was no written evidence or audio recording.

One major aim of my analysis at this stage was to collect information on the agents who had collaborated in the drafting of the paper as well as other literacy brokers who may have altered the text in significant ways. Examining the materials I had gathered, I listed all named coauthors, all individuals who appeared in any pre-submission correspondence to which I had been given access, and also the reviewers and editors of the journals to which the manuscript had been sent. In some cases, this effort to establish the sources of input was complicated by the issues listed above, leading me to rely on the authors' recollection expressed in the informant interviews.

As the in-house editor of the IIS-FJD, I also acted as a literacy broker in nine of the ten texts. Although this aspect of the convenience sample limits the language brokering analyzed to only one broker, all of this editing work had been done before I began this study, and no texts were selected for inclusion and then edited by me afterward; therefore, these texts were not influenced by my research aims, and constitute a valid source of empirical input. For each of the nine texts I had worked on, I included both the version of the manuscript sent to me for editing as well as the draft containing my proposed edits and other comments for the author. The proposed edits were made using the track-changes function of Microsoft Word, while all other comments on the text appeared as margin comments and served to call the authors' attention to problematic passages that required rewriting by the author, requests for particularly close review of passages in which my interventions in the text may have changed the meaning, and instances in which I did not intervene directly in the text but called on the author to confirm my interpretation of their writing.

1.5.4 Move analysis

1.5.4.1 Milestone versions

Once I had reached a basic understanding of the path the texts had followed and the decisions of the different journals to which they had been submitted, and had listed the authors and non-authors who had contributed to the text, I next set out to establish a system that would simplify the move-step analyses I was to conduct in the following phase of my study. As the aim of my research was to characterize the rhetorical structure of the RAs and also track the ways in which the discourse evolved under the influence of different collaborators and pre- and post-

submission literacy brokers, I decided to focus my rhetorical analysis on a number of stages that were common to all of the RAs but that were also sensitive to the perspective on NNES academic writing inherent to the TH approach.

I did this by establishing a number of “milestone” versions of these texts. Milestone versions are versions of the text that reflect particular contributions and influences from a variety of sources. The first milestone version is the draft sent to me for editing in the pre-submission stage, reflecting the work of the participant and, where applicable, their coauthors up to that point. (It is worth recalling here that one TH, the ninth, was not sent to me for editing, and as a result this first milestone version does not appear in the TH.) The second of these is the version containing my proposals for revision and other remarks, some of which contain suggestions that, if accepted by the author, may have altered the rhetorical makeup of the text. The third milestone version is the text as submitted to the first journal. Analysis of the manuscript at this phase is valuable in that it can be assumed that the text represents the best effort by the participant throughout the pre-submission phase, including all negotiation with their coauthors and depicting the extent to which they either accepted, rejected, or otherwise engaged with my contributions to their work. If the participant was successful in publishing their paper, the fourth and final milestone version was the published RA. In addition to all other sources of input manifested in previous milestone drafts, this version of the text also depicts the ways in which the academic literacy brokers from the journal or journals that had considered the text for publication had molded the publication.

1.5.5 Coding of rhetorical moves and input from text histories

I will now describe the procedure followed to code the texts included in each TH and how the TH approach was interwoven with Swalesian-style genre analysis. To do this, I will explain the methodology used to carry out these analyses, illustrating the means employed to first arrive at a rhetorical description of the RAs and then observe any rhetorical evolution until publication or abandonment. In the process, I will also illustrate how the ethnographic and the rhetorical analyses complemented each other.

In this, the first of two core phases in my study, I performed a move analysis to describe the initial rhetorical traits of the text across all its sections and the abstract and then studied how these rhetorical characteristics evolved under different processes and actors from both within the discourse community and from the outside. I first coded milestone version 1 (the pre-submission draft sent to me for editing). Using this text, I carried out a move analysis to characterize the rhetorical characteristics of the text at what can be referred to as the text's baseline. This work brings together the writing of the participant and, where applicable, their collaborators up to that point, and as such is a valuable object of analysis for the study of these ten NNES authors' writing for English-medium journals.

1.5.5.1 Top-down move analysis

Before continuing with a description of how I proceeded to analyze the RAs contained in each TH through the use of these textual milestones, explanation of the methodology used to analyze the texts is called for. I performed a top-down move analysis of each section of the RAs (i.e., introduction, methods, results, and discussion) and the abstract in a way that was broadly based on the procedure published by Biber (2007, pp. 33-34). Unlike the system proposed by Biber, however, I omitted the steps that call for a) an initial analysis to get the 'big-picture' understanding of the overall purpose of the genre, as I was fully aware of this purpose; b) a pilot coding, alongside two other coders, as the formulaic nature of RAs in the medical discipline reduces rhetorical variety, thus allowing me to forego this step; and c) the addition of additional steps or moves, as this was outside of the aims of my study. The basis for the top-down analysis is similar to others mentioned in the literature, (e.g., Connor, 2013; L. Flowerdew, 2002, 2004).

Using these methods, I first established the boundaries that delimited the moves and then attempted to match that move with the one appearing in the framework I had chosen for that section. When moves that did not reflect the corresponding framework were detected, I first reviewed the neighboring moves and the move itself in an effort to decide on a conventional move to which to assign the discourse. When this failed, these fragments were flagged for observation in subsequent milestone versions and were included in the informant interview. Other characteristics of the texts that were monitored throughout the rest of the TH and

discussed during the informant interviews were moves that appeared to be markedly long, moves I considered to be obligatory but were absent from the texts, and cases of RA sections that contained a preponderance of a particular move and a lesser presence of others.

Returning now to a description of the text analysis, a study of the version of the paper edited by me (milestone version 2) was performed to search for direct interventions in the text or margin comments that appeared to concern rhetorical organization. These text fragments were also flagged for follow-up in subsequent milestone versions, as they indicated instances of uptake of my contributions to the text or rejection of these proposed changes. Evidence like this is particularly enlightening because it can be used to illustrate which textual levels (e.g., surface, paragraph, discourse) these NNES authors are willing to accept input from outside their discourse community and, conversely, when such contributions are less welcome.

After I had analyzed the first two milestone versions, I then compared the move assignments I had made in the first with the text as it had been sent to the first journal (i.e., third milestone version). A comparison of these three versions enabled me to reach a number of tentative conclusions about the pre-submission process, including the ways in which the participants processed the different types of changes I had proposed. As with all other rhetorical analyses of the texts contained in the different THs, all instances of rhetorically relevant modifications were noted and then, where there were intervening versions between the previous milestone and the version containing this change, I consulted these non-milestone versions in order to pinpoint the precise moment at which the change had been made and also to determine who had made the change, and why. These points of inquiry also fed into the informant interview when relevant to my analysis.

In nine of the ten THs, the final milestone version was the published text. As in earlier phases, this text was used to pinpoint the changes that had taken place and to form hypotheses as to what had driven these changes. What distinguishes this version from others, however, is not only its usefulness as a finished product and an example of writing that has met the standards of the discourse community, but also the rich input that was provided by journal gatekeepers between

this final version and the milestone that precedes it. I studied all referee reports and editor correspondence that took place between the time the text was submitted to the first journal (milestone 3) and final publication (milestone 4) or abandonment. I read this correspondence closely in search of comments that made reference to the language or the writing in the paper and also verified whether these comments targeted the fragments I had flagged in earlier stages of my analysis. All such comments were noted for inclusion in the informant interview and used to examine this final published version to document the author's response to this gatekeeper feedback.

In case of problematic move assignments, I discussed some of these passages with the participants during the interview. These exchanges were not always fruitful, however, presumably because the participants were unaware of the metalanguage used in the field of Applied Linguistics (Burgess & Lillis, 2013, p. 6).

1.5.6 Informant interviews

The second key phase of data gathering was the informant interview. These were semi-structured interviews that consisted of two parts. I explained to all authors that my study is anonymous and that steps would be taken to protect their identity; however, I also indicated that due to the nature of my research, certain data that are essential to my study (e.g., career stage, sex, department), when used in conjunction with the text fragments included in my work, could be used to identify them. In the first part of the interview, the informants were asked a battery of questions about specific issues that concerned the TH, while the second part of the interview invited the participants to describe their experiences in writing academic texts in English and discuss their opinions on the matter.

With the exception of TH7, in which there were two interviewees, all other interviews were held individually, the first in February, 2014 and the last in July of 2015. The shortest interview lasted 41 minutes and the longest, 112 minutes. The mean interview length was 71 minutes. Of note, both of the two shortest interviews (TH 2, 41 minutes and TH 10, 42 minutes) were held in the

early-morning hours at the request of the participants, both of whom explained that their busy schedules prevented them from holding the interview at another time.

All interviews took place on the premises of the Fundación Jiménez Díaz hospital. The informants selected the site in which the interview would be held. Some were conducted in physicians' offices, where doctors customarily examine patients, while others took place in meeting rooms and other spaces used for conferences and other work activities. Several interviews were interrupted at least once when patients or colleagues entered the room, although none had an appreciable impact on the interview.

The participants were not given any pre-interview training, and no pilot interviews were held; indeed, the only information these author-informants had about the interview content was the pre-study description referred to above and some basic aspects of the interview procedure included in my request to schedule our meeting. All written documents that made up a given TH were available for consultation during the interviews. Each had been assigned a letter code to indicate the order in which it had been generated. These codes were then used during the text-focused part of the interview to direct the participants' attention to particular passages for which I had prepared questions.

All participants were given the option to hold the interview in English or in Spanish. Three opted for English, and the remaining seven, Spanish. After each author expressed their choice of language, they were asked why they made their decision, and this response was included as part of the interview data.

1.5.6.1 Text-focused questions

After the authors decided which language they preferred to conduct the interview in, I proceeded to ask them the text-focused questions, the first being whether their experience in writing, revising, submitting, and, where applicable, publishing the text was representative of their experience or anomalous. By asking these two questions first—that is, whether they wished to hold the interview in English and why and also whether they felt that this RA was indicative of

their experience in English—my goal was to establish a dynamic by which concrete evidence from the RA could be used to discuss broader experiences, strategies, and attitudes. Subsequent questions had a number of aims: to confirm certain problematic move assignments by asking the authors what their intent was when writing a given passage; to obtain the informants' input on instances of rhetorically unorthodox writing; to inquire about rhetorically significant changes and the authors' reasons for making these changes; and to pursue any emerging themes that came up in the course of the interview and which may prove relevant for my study.

Despite the fact that the participants themselves had made all documents available to me in the first place, on a number of occasions these authors could not recall certain information about their texts, the motivations behind their writing, or the circumstances that surrounded the creation, pre-submission editing, submission, or peer-review process. These lacunae may be attributed to a number of issues, the most compelling being the time elapsed between an RA's publication and the interview. It is also possible that the time constraints of the interview setting may have caused the participants to feel under pressure, discouraging them from engaging with certain questions. In other instances, however, it became clear that, as an outsider to their discourse community, my understanding of and approach to academic texts is different from theirs, making communication of this sorts difficult without lengthy explanation.

1.5.6.2 Questions on beliefs and attitudes

Once the text-focused questions had been answered, the interviews turned to more general questions about the informants' experiences, attitudes, and strategies for publishing their work in English. These questions were standard for all interviewers and were as follows:

- Please describe your career and your position at this institution.
- Why do you perform research? In other words, how would your life or career be affected if you didn't do research?
- Do you believe that you reached your current position because you have produced scientific research?
- Are there explicit expectations that you will continue to research?

- When do you carry out your research? Is there time built into your work schedule for research and writing about your research?
- Was it necessary for you to publish this in English, or could you have published the article in Spanish?
- Does publishing in English require a substantial effort from you?
- Is it common for your papers to be rejected because of what reviewers or editors consider to be “deficient” English?
- Do you feel you understand what is expected of you by English-language journals concerning the type or quality of English in your articles?
- Do you believe you are at a disadvantage compared to your native-English-speaking colleagues when it comes to publishing?
- Do you believe your experience in publishing this article would have been different if you were a native English speaker? How so?
- What is your attitude toward the English language in general? And toward English within your profession?

1.5.7 Compiling the text history

In the final phase of my research I bring together the different sources of data to tell the story of the RA, its author, and the individuals and institutional factors that shaped the text. To do this, I studied the data from the move analyses and the supporting materials included in the TH (e.g., reviewer feedback, my edit of the text, correspondence), transcribed fragments of relevant exchanges that had taken place during the informant interview, and, where applicable, email follow-up questions I had asked authors in cases in which I required clarification, confirmation, etc. As a final quality-control measure, all authors were sent a near-final version of each TH for them to signal any inaccuracies and request modifications aimed at preserving their anonymity.

1.6 Structure of the text histories

The product of these efforts appears in the following section of this doctoral thesis. This section consists of ten THs presented in the order in which I completed them. Each chapter is structured in a similar way and bears the name of the pseudonym I have given for the person or persons

who kindly agreed to take part in my study. The opening section provides an overview of the TH, presenting the central findings and themes developed therein. This is followed by a section that lays out the chronology of the TH and the documents included in the TH. The third section introduces the literacy brokers and gives an account of the evolution of the text from its creation to the moment it was published or abandoned. The fourth section is a qualitative analysis of the RA's salient rhetorical aspects and how these features evolved as the text progressed toward publication. Next is a description of the participants' opinions and feelings about a number of issues affecting NNES scholars and the enterprise of publishing in English-medium international journals. Finally, the chapter is brought to a close with a concluding section named "Implications," in which I highlight the primary issues and observations that will be taken up once again in the discussion and conclusion.

Text history 1: Esteban

Overview

This informant is a senior cardiologist at the *Instituto de Investigación Sanitaria – Fundación Jiménez Díaz* who has a great deal of experience in publishing both in Spanish and in English as well as refereeing academic articles within his specialty. The article I studied was published after two rejections: the first from a highly prestigious general medical journal, and the second from a specialty journal, the latter after being sent for peer review. The manuscript was finally accepted by the third journal, though the acceptance was contingent on the author's agreement to make a series of specific edits suggested by the journal editor him or herself. The personal tone of this editorial decision and the invasiveness of the editor's proposed changes are suggestive of a highly marked power dynamic in which the editor makes no attempt to mitigate their authority. In the interview, Esteban described the endeavor to publish in a way that foregrounds the need to please journal editors despite all the effort and compromises that this might entail. All other objectives and potential readers are backgrounded.

The quality of the English of the article did not play a role in the first two attempts to have the text published; indeed, one of the reviewers of the second journal remarked that the text was well-written. When, during the informant interview, I asked Esteban about the few passages in which his revisions evidenced changes in rhetorical structure, he downplayed these changes. The questions on his attitude toward English and publishing in English fed into rich commentary and insightful metaphor on the interactions between authors and academic literacy brokers. The interview was held in English on February 23, 2014 and lasted 62 minutes.

Chronology and texts

This TH is quite straightforward in its composition, comprising a comparatively low number of materials. Fortunately, I was able to access all of the milestone documents appearing below (in bold) as well as the journals' decision letters. With the exception of the first draft (Document A), all other drafts included are the versions sent to the different journals. These texts do not contain notes, revision marks, or correspondence between coauthors.

1. **Document A: Draft 1 of the manuscript sent to me for editing**
2. **Document B: My edit the manuscript**
3. **Document C: Version of the article as sent to the first journal**
4. Document D: Rejection letter from the first journal
5. Document E: Version of article as sent to the second journal
6. Document F: Rejection letter from the second journals
7. Document G: Version of the article as sent to the third journal
8. Document H: Hand-written “edit” received from the editor of the third journal. This text was received at the same time as the reviewer comments.
9. Document I: Point-by-point response letter
10. Document J: Final version of the manuscript as sent to the third journal
11. Document K: Final published version

Evolution of the text and the role of literacy brokers

Based only on the number of changes that took place along the text’s trajectory, the article was modified extensively from the first to the last version. A detailed, line-by-line comparison of each successive version revealed numerous changes, although Esteban assured me that “the news” being reported in the paper remained the same. Seeking to reconcile the many differences between the first and final version of the RA, I asked Esteban to talk about why the paper looked so different on the surface. In response, he mentioned such practical matters as length limits and terminological standards of the different target journals, although at times during the interview the author spoke at length about the dynamics of the author-gatekeeper relationship.

Though aided by his coauthors and other collaborators, Esteban wrote this RA mostly himself. He makes a habit of taking the reins like this, and during the informant interview he seemed to downplay the burden of writing papers in isolation.

Esteban sent the paper to me for editing, and my version appears as Document B. The revision process was more difficult than average, especially because of the high concentration of biomarkers and the important role played by sophisticated statistical operations. In addition, when academic texts use synonyms to vary the style of the paper, it is more difficult to follow the argument, and when such arcane concepts are expressed unclearly, editing can be cognitively trying. Assignments like these pose a substantial challenge when I have limited time to devote to

each one, and when analyzing my edit for this doctoral thesis, I detected a number of errors I made when interpreting Esteban's intended meaning.

Esteban first submitted the RA to a highly prestigious general medical journal, though his paper was rejected by the editor. In what appears to be a boilerplate response, the journal wrote, "It was evaluated by members of our editorial staff. After considering its focus, content, and interest, we made the editorial decision not to consider your submission further." Following this initial disappointment, Esteban made minor changes to his paper before sending it to the second journal. Some of the most noteworthy of these include a lengthier title, changes in terminology, slight additions to the description of methodology, and a number of sentence-level changes that did not effect the rhetorical patterning of the RA, and the addition of a Limitations subsection within the discussion.

Despite these changes, Esteban's work was rejected by the second journal following peer review. The journal's decision was based on the reports of two reviewers. The first questioned the methodological underpinning of the study, casting doubt on whether a clinical risk score resulting from a larger study carried out in Australia and New Zealand should be applied to a Spanish patient population due to the different characteristics of these patients. The second reviewer begins the report with a positive assessment, writing, "In principle, the paper is novel well-written and of interest. The illustrations are good and the paper is in general well referenced." This is then followed with another positive assessment on the value of the study design. The third comment is highly peculiar, however, and reads, "The reviewer is not an expert on lab techniques so I can not judge if storage etc has been done according to standards of care. I presume it is." This remark strikes me as odd, as the reviewer prefaces their remark with an admission of their lack of expertise. Though not a criticism *per se*, it seems that this skeptical attitude may be a manifestation of the reviewer's doubt as to the rigor of research performed outside Anglophone-center contexts. From what I have gathered in other THs constructed here, some gatekeepers suspect that research conducted in certain non-center countries is not reported faithfully. The fourth and final comment—and the only one that is clearly critical of the paper—requests more detailed definitions of the system used to classify patients.

After Esteban sent the paper to the third and final journal, he received a request to revise and resubmit his work. Two reviewers each issued a report. The first reviewer was very brief, and the only request this gatekeeper made was for clarification on a particular component of the study data. The second reviewer, on the other hand, was much more exhaustive, though none of the issues raised in this report had to do with the language of the paper. Of interest, Reviewer 2 suggested that the title of the study needed revision to indicate whether it was a prospective or follow-up study.

This publication is an extreme case of journal editors influencing texts. After the peer reviewers had evaluated the RA, the editor listed a series of demands for Esteban, predicated acceptance of the text on the fulfillment of these requirements. These conditions appear as handwritten notes made on the manuscript and bear the initials of the editor-in-chief of the journal. These instructions included eliminating entire paragraphs, reducing the number of tables from seven to four, omitting all sub-section headings, and even changing the title of the article. The editor writes the following: “I altered the title of your manuscript (see above) in an attempt to better describe the contents of the manuscript.” What is unique about this intervention is the highly personal tone used by the editor, underscoring the relationship of power they occupied in this transaction. To my surprise, Esteban told me he was pleased at this result, as it meant he had come within a stone’s throw of securing publication.

As our conversation segued into this matter of interacting with journal editors, Esteban spoke of these figures in markedly personal terms. Indeed, in the second question of the text-based interview in which I intended to elicit insight on what appeared to be a substantial change in the rhetorical characteristics of the text, Esteban quickly shifted the focus of discussion from the meaning of the text to the interpersonal dynamics between himself and the editor:

Oliver: In this same section, you say “Also, high-sensitivity troponin was studied.” In 2.E, you have a sim...

Esteban: Irrelevant change (...) Nowadays, we’re having some problems with publications because in the journals you have first of all the editor who takes a look at your paper and then decides whether to send your paper to reviewers or not. In many journals, the editor simply rejects the paper if they’re not interested in what you’re saying. They don’t mind

if your work has been done properly. The news you're reporting is not interesting for them. They want to have a high impact factor, they want to have the highest number of readers possible. And this is a problem. Then, if they think that it could be suitable for publication, they send it to reviewers so they can check if the work has been done properly and also they can confirm if it is relevant. But there are many journals where the editor is of key relevance and he or she rejects the paper without further review. The authors we sometimes have a lot of doubts. What would be the best way to say this so that the editor can enjoy this or like this? (emphasis added)

The editor is cast here as a subjective figure who has the potential to pose “problems” for authors because of their ability to reject contributions because of lack of interest (“they don’t mind”) and someone who needs to “like” or derive enjoyment from a manuscript.

The reviewers, on the other hand, are seen in a totally different light, and their advice can be heeded or ignored to differing degrees in light of the decision of the editor to allow the article continued consideration for publication in his or her journal. Once again, an author’s reaction to reviewer comments is an attempt to separate the valuable contributions of reviewers from the remarks that are perceived as not adding value to the text:

Nevertheless, when you receive the editor’s comment, you can have two situations: the first situation the editor tells you that if you are able to answer the editor... the reviewer queries, they are going to consider your manuscript. But what we have seen in the previous submissions is that the paper was rejected independently of the fact that you were able or not to answer correctly the reviewer’s queries. Ok, so in that case, if you are going to send it to another journal, you don’t make all the changes requested by the reviewers. You try to guess what [sic] these suggestions are really important and what [sic] of them are not important because many times you see that now where the paper is rejected in several journals, the edit...the reviewer comments are very different. In some cases they are completely wrong. You see people that they are not experts in that area. In other case [sic] you see people that are experts in another area and they ask you a lot of information about their area, so if you are not going to send it to the same journal, then you don’t make most of these changes.

Esteban here describes the “guessing” involved in journal submission, with great variation seen between the reviewer comments of different journals. As he sees it, reviewers are often ill-prepared to judge the value of a manuscript, and their remarks sometimes belie their ignorance. Other times, they appear eager to shift the discourse toward their particular area of expertise rather than assess the paper on its own terms.

On several occasions, the text-focused questions I had prepared for the interview led Esteban to talk in detail about his thought processes when writing. When preparing a manuscript for its first submission or its third, he is keenly aware of his perceived audience: the editor first and foremost, with peer reviewers coming in a not so close second. Esteban made this hierarchy clear when he stated

You must take into account that when you write a paper, the first purpose is that the editor accepts your paper. The second purpose is that the readers may find it attractive, but the first purpose is that the paper is accepted.

The editor is clearly considered the dominant figure in peer review, and Esteban works with this imagined person in mind.

And what does an author have to do to woo these editors? According to Esteban, editors' agendas are dictated entirely by impact factor, and in order to raise their impact factor, they look for specific kinds of contributions, although what exactly the characteristics of these contributions are is somewhat of a mystery. In his words, Esteban anticipates the priorities of the editor, "exploring [his] mind," "guessing" what their are for the journal, and engaging in a process of "trial and error" with this enigmatic figure. This poses quite a challenge for Esteban. As he puts it:

You try to guess what he's thinking about at that moment. Because the editor is also a person who needs papers in such area [sic]. And in that moment the editor is thinking of increasing the impact factor of his journal. You try to guess what he's thinking about.

As seen here, the "personhood" of the editor is present when this participant drafts and revises his manuscripts.

Esteban described how he writes defensively, seeking to avoid rejection of the editor and reviewers. This causes him to adopt a "more is better" policy when drafting texts: to describe his studies and findings, he includes unnecessary or superfluous information so as not give the reviewers or editor reason to reject the manuscript. In his own words:

...when I'm writing then I use to say [sic] that this information may be deleted but I think that the information will be better for the editor and the reviewers so they can understand it better and then can accept it better. I also review papers. The reviewers are not always experts in that area. They often have less knowledge than the author about this specific area. You must avoid the reviewers saying that I don't accept this because of this because perhaps you are right but you are fighting against him and you must reply to him and say that he is wrong. This is not a good thing. So you must explain things very well. Many times you suspect that some of them could be deleted, and I think this part of the text could be important for the editor to accept the text in the end, once the editor has made this interpretation of the paper, he says ok this is not strictly necessary mainly because of space limitations.

The author anticipates that part of his text will be pared down by the literacy brokers who represent the interests of the journal, and leaves it to them which parts of the article are unnecessary. Publishing in international English-medium journals poses a dual challenge for Esteban: on the one hand, authors must win the approval of unknown individuals with unclear—and, at times, questionable—agendas and, on the other, must safeguard their texts from what the journal brokers may perceive as mistakes that warrant rejection.

Although I edited Document B, Esteban did not ask me to edit subsequent versions. These later versions contain a number of surface-level errors that would have easily been spotted, although it appears these were unimportant for the journals. During our conversation, Esteban told me that earlier in his career he had very good results when working with another language editor: the first two papers he sent for publication were accepted in prestigious journals, and since then he always uses a professional editor because he believes it improves the quality of his writing. When asked whether any of his papers have ever been rejected by a journal because of deficient English, he replied that this has never happened, adding, “[Rejection because of poor English] has never happened, because if they like the idea, they help you even to write the paper.” This willingness by journals to assist authors in writing their articles can be seen clearly in this TH, although as described above, there is a trade-off.

Genre considerations

With a few small exceptions, none of the academic literacy brokers found the English to be lacking, and most comments were aimed at the characteristics of the study itself and not how

the study is depicted in the article. Based on this apparent seal of approval from the gatekeepers of the journals, it appears there were no linguistic impediments to the successful publication of the manuscript. Some of the adaptations made to the text were motivated by factors extraneous to the genre itself, such as length limits and other requirements of the target journals. Changes to the content of the text brought about a number of revisions, although the revised versions did not veer from what appears to be Esteban's rhetorical style. As analyzed in the previous section, both the person and the style of the third and final journal's editor loom large in this TH.

Unlike other sections, the abstract of this RA remained largely unchanged throughout the trajectory of the text. Though minor, the revisions that Esteban did make concerned the content of background information (Move 1) at the opening of the abstract and the prominence given to this information. Between submissions to the second and third journal (Documents E and G), this opening Move evolved from a statement on the lack of prognostic biomarkers and clinical risk scores to a more general account of the medical conditions that create a need for the study. This change is apparent when we compare the two:

Document E

Objectives

To test the prognostic value of a panel of plasma biomarkers related to atherothrombosis, renal disease or cancer in patients with chronic coronary artery disease (CAD).

Background

No prognostic biomarkers or clinical risk scores are used in chronic CAD.

Document G

Patients with coronary artery disease (CAD) may develop not only ischemic events. Also, previous myocardial damage may lead to heart failure and death. The purpose of this study was to test the prognostic value of a panel of plasma biomarkers related to vascular (...) and myocardial damage (...) in 707 patients with chronic CAD followed during 2.2 ± 0.99 years.

The less specific point of entry in Document G is entirely absent from the previous version, and by changing the opening in this fashion, the abstract draws the connection between the two types of biomarkers researched and the clinical relevance of the study's findings. (Curiously, these first two sentences are virtually identical to the opening of the third paragraph of the

introduction, suggesting that this is a central observation giving rise to the study.) The evolution of the abstract appears to have been guided by the format of the second journal (Document E) and the elimination of this constraint by the third (Document G).

The introduction to the RA was pared down substantially at the suggestion of the editor of the third and final journal, eliminating Moves 1 and 2 in their entirety and leaving only a presentation of the present work (Move 3) in the final published version. As evidenced in the handwritten notes made by the editor on the manuscript proper (Document H) and the editor's comments (Document I), a primary goal behind this drastic reduction in the introduction was to shorten the manuscript from its original length of 14 pages to the 10-page limit allowed by the journal. In spite of this circumstance, the fact that Moves 1 and 2 were eliminated from the paper does suggest that they are of lower priority within the section, at least within this discipline or for this particular journal.

Before this substantial reduction in length, Esteban appears to have devoted substantial effort in situating his study. The three paragraphs eliminated in Document J use a cyclical rhetorical structure like the one described by Swales (2004) to introduce the problem posed by the long-term effects of coronary artery disease, acknowledge the advances made to remedy this situation, and also to underscore the need to continue the search for more and better tools to predict such problems. A fitting illustration of how Esteban touches on these same points with increasing degrees of specificity can be found in the first two paragraphs of the introduction in Document C; here, although I have selected this version as an example, the rhetorical patterning of this stretch of text remained unchanged until the editor of the third journal recommended its removal.

[M1] Despite recent advances in treatment, many people continue to suffer the deleterious effects of atherothrombosis. **[M2, S1a]** Identifying persons at high risk of cardiovascular events would allow for intensified therapy and greater control of cardiovascular risk factors in this population.

[M1] Several biomarkers have been suggested to predict the risk of cardiovascular events (1-3). **[M2, S1a]** However, the results have been controversial, as they add only moderately to the predictive power of clinical data (4-9). **[M2, S1a]** At present, with the exception of lipid levels and troponin in the setting of acute coronary syndromes, no biomarker is used regularly in clinical practice.

In the first paragraph, the author succinctly explains the problem of atherosclerosis, recognizes that progress has been made, stresses that continued work needs to be done, and suggests that a useful way of addressing this issue would be to fine-tune clinicians' ability to detect risk of the ongoing health problems, as doing so would allow for improvements in care. The second paragraph takes the last of these points and goes deeper into the issue. The text cites some of the progress made with biomarkers, though immediately stressing the limitations that have been found with these biomarkers regarding their robustness and extent of current application in clinical work. Both of these Move 1-Move 2 cycles clearly sets the scene by indicating progress, though also pointing out the gap to be filled by the study (Move 2, Step 1A).

Paragraph 3 continues with this issue of the potential of biomarkers, though now the author introduces the novel aspect of his study, proposing to expand the scope of biomarkers beyond those traditionally used to detect coronary artery disease or atherothrombosis. Like the cycles described above, Move 2 (Establishing a niche) foreshadows that the study will search for biomarkers, indicating that, if found, these biomarkers would be helpful for both the general population of patients with coronary artery disease and, especially, those with stable disease (as opposed to primary prevention or cases of acute disease). When I revised the paper (Document B), I found this two-part explanation somewhat unclear and repetitive, particularly as concerns the way in which Esteban attempted to mark the distinction between the general population of patients and those with chronic disease. The following is an excerpt of the paragraph in question to illustrate this point of difficulty (Document A):

[M2, S1B] Patients with coronary artery disease may not only develop cardiovascular complications, but they can also die from other diseases such as malignancies or renal damage. Then, when searching for novel biomarkers in this population, we should consider including also [sic] biomarkers related to other processes and not only to atherothrombosis. **[M2, S1B]** Moreover, in patients with stable coronary artery disease, the situation is further complicated by the absence of accepted prognostic clinical scores as it happens [sic] in primary prevention and acute coronary syndromes (9). Then, searching for biomarkers in this population is specially [sic] relevant.

In the first two sentences, Esteban adds to what is known on the issue (Move 2, Step 1B), after which he argues that biomarkers for these additional pathologies should be investigated. The passage I found less clear begins with the discourse marker “moreover” and continuing to the end of the fragment. My confusion stemmed particularly from the clause introduced by “as it happens” and its relation to the noun phrase “the absence of accepted prognostic clinical scores.” In hindsight, the intended meaning here is something resembling “the absence of accepted prognostic clinical scores like those established in (...)” though confirming this suspicion would have required either an author query or a possible detailed reading of the study cited in (9). After I flagged this sentence for Esteban, he later modified the sentence to the clearer, “Unlike what happens in the setting of ACS and primary prevention, in chronic CAD there are few clinical scales of risk (5), and no one is unanimously accepted in the [sic] clinical practice” (Document E). As a result of this initial confusion, I also found the final sentence to be possibly redundant, and suggested that Esteban consider omitting it. However, he declined to take this suggestion, believing the issue to have been resolved with the aforementioned modification in Document E. These two interventions of mine and Esteban’s reaction to my queries show how I struggled to follow the author’s intended rhetorical strategy due to problems with the text’s surface structure of the text. Though it is likely that a field expert would have easily interpreted the intended propositional meaning, in my experience I have found that errors like these sometimes lead reviewers to request that the manuscript be reviewed by an NES editor, thus prolonging the publication process.

Esteban’s and my efforts in this passage were in vain, however, as the editor requested that the entire paragraph be eliminated. This proposed reduction is important when we take into account that the same sentence which opens the paragraph excerpted above appears nearly verbatim in

the abstract, though the editor did not find reason to delete this from the abstract. Abstracts are characterized by their brevity, which makes Esteban's decision to lead off the abstract this way pertinent to the logical underpinning of the RA. It is therefore feasible that, in an effort to reduce the length of the manuscript, the abstract was taken to be a para-introduction of sorts, and as a result any background information appearing in the abstract was not considered necessary to be repeated. If accurate, this may be a novel use of the abstract in RAs. Whether this hypothesis is well-founded or not, the relegation of background information appears evident.

None of the gatekeepers of the two journals that sent the text for peer review remarked that the text was poorly written; in fact, one reviewer for the second journal found the paper to be well-written and well-referenced. Nonetheless, the methods section was changed in significant ways, though these modifications were in response to content-related issues and had little or nothing to do with the language. Other applied linguists have noted that it is difficult to draw a clean line between reviewer comments on language and criticism on content (Mungra & Webber, 2010). In adapting to the requests of journal reviewers, Esteban made significant changes to certain stretches of text that can best be understood from the perspective of content rather than rhetoric. The following is an example of one of these changes:

Document E

[M4a,b] We included 711 patients with chronic CAD evaluated at the hospitals Fundación Jiménez Díaz, Fuenlabrada, Móstoles and Alcorcón **[M5b]** from January 2007 until February 2011. Date of last follow-up visits was May 2012. The research protocol was approved by the ethics committees of the hospitals. **[M4c]** To be included, patients were required to have a past history of either non-ST elevation ACS or ST-elevation myocardial infarction.

Document G

The research protocol complies with the Declaration of Helsinki [sic] and was approved by the ethics committees of the participant hospitals. All patients signed informed consent documents. **[M4b,c]** The BACS & BAMI (Biomarkers in Acute Coronary Syndrome & Biomarkers in Acute Myocardial Infarction) studies included patients admitted at the hospitals Fundación Jiménez Díaz, Fuenlabrada, Móstoles and Alcorcón in Madrid with either non-ST elevation acute coronary syndrome (NSTEMI) or ST-elevation myocardial infarction (STEMI).

With the exception of the third sentence of the first text above—where the author includes information on ethical concerns—the extract provides the reader with a straightforward description of the data-collection procedure, including information on the sample size, the source of the data, and the criteria for data selection, thereby including all of the constituent elements outlined for this Move 4 by Nwogu (1997). In the second version, however, this move becomes complicated by a reference to “the BACS & BAMl studies,” since there is no explicit indication that these patient data were the ones used in this particular text.

It appears that this and other revisions made by the author came in response to the following criticism from Reviewer 2 in Document F in which the reviewer remarks

The main is [sic] question I have is that the authors always refer to chronic CAD as inclusion parameters but I could not find(perhaps I overlooked?) the exact defination [sic] they used to qualify patients as such. How long ago was a you [sic] past history of (n)STEMI supposed to be, certainly not very long since on page 8 they stat [sic] that “from the last acute coronary event untip [sic] plsma [sic] withdrawal and beginning of follow-up was 7.5 +/- 3 months”. So I would like to have more clear info on the exact def, when was the plasma for the lab analyses done and how long was then [sic] follow up (perhaps a distinction can be made on earlier versus later inclusion after ACS).

This reviewer seems to take issue with the first sentence in fragment taken from Document E above, and the way in which the subsequent version Document G reacts to this reveals a *modus operandi* in which elements are inserted and deleted without impacting the larger rhetorical structure. In the second extract, Esteban moves all references to the size of his sample, the source of his data, and the criteria used to collect these data from the beginning of the methods section to later in the first paragraph, preferring to open with the ethical aspects of his study and then moving on to these apparently more substantial issues. The freedom with which he moves these elements around makes for more heterogeneous prose, although one gets the impression that what is important is for the information to be somewhere in the text, no matter where or how it is woven into the surrounding text.

In other passages, suggestions from the Reviewers in Document F also translate into single-sentence additions, as in the following (Document G): “As there is not previous information on

the panel of biomarkers studied, sample size calculation was not possible,” this in response to the reviewer comment in Document F, which read as follows:

To my knowledge this risk score has never been externally validated. Patients in the Lipid study were recruited in 87 centers in Australia and New Zealand and differed importantly from the Spanish cohort in this study [e.g. LDL cholesterol (...) hypertension (...) and diabetes (...)]. Thus this risk score may be inherently inadequate to predict risk of future events in the Spanish cohort.

Esteban is more confident in his ability to write acceptable methods and results sections than he is with the introduction and discussion. Months after I edited this text for the author, he asked me to edit another article he had written. When I told him that I would take longer than usual due to an unusually long backlog, he told me to only edit the introduction and discussion section. For Esteban, the methods and results sections deal with more routine matters, posing substantially less of a challenge than the other, more dialogic sections.

This attitude is also apparent in the results section, which is written in a somewhat mechanical style. In the paper, results are seldom commented on, leaving analysis of the data for the discussion section. The most significant changes made to the results section were introduced in response to the reviewer and editor comments made by the third and final journal. One of the remarks made by the editor encouraged Esteban to delete three of the seven existing tables and exhorted him not to repeat the data appearing in the tables in the text proper. These remarks echo the observations of Woodford (cited in Swales, 2004, p. 224) suggesting that one of the most common mistakes is to simply repeat the contents of tables in the results. Despite this advice from the editor, little text was trimmed from the section. In fact, because one of the other suggestions made by the editor was to eliminate all subsection headings from the text, some passages had to be lengthened to express what was once made clear in these headings.

The other modifications made to the results section in response to reviewer comments involved matters of a technical nature and did not indicate any lack in Esteban’s ability to write effective RAs in English. Some of these seemed major from my perspective, although Esteban assured me that they were not. In Document F, which is the response from the second journal, Reviewer 1

frames their criticism around what they believe to be a key finding in Esteban's paper. In this finding, Esteban used an existing risk score to build a more complex score with which to perform his study. After the reviewer argued that it was scientifically inappropriate to apply this other score in a Spanish population, Esteban rearranged his analytical approach. It would seem that such a major adaptation would have a wide-ranging impact on the analysis, although the passage that makes reference to this issue appears at the end of the results section and is written in a highly similar fashion in spite of the substantial changes made to the content. To illustrate this point, I will include a brief excerpt of the corresponding sections from the version sent to the first journal (Document C) and the final published version (Document K):

Document C

[M1.1c] The Marschner clinical risk scale (21) obtained a significant predictive value [area under the curve: 0.642 (0.579-0.705); $p < 0.001$] (Figure 2, left). **[M0.3]** Galectin-3, MCP-1, and NT-pro-BNP levels were converted to a value from 1 to 10 according to the decile of their concentration. **[M0.3]** A combined scale was obtained by adding the deciles of the three biomarkers to the Marschner score. **[M1.1c]** Using this scale, the area under the curve increased to 0.744 (0.684-0.805; $p < 0.001$).

Document K

[M0.3] A multimarker score was developed according to the number of biomarkers (MCP-1, galectin-3, and NT-proBNP) whose values were above the median. In this way, a patient could have a multimarker score of 0 to 3. **[M1.1a]** The score value was 0 in 20.0% of patients, 1 in 29.2%, 2 in 31.8%, and 3 in 19.0%. **[M0.3]** When this score was included in the Cox proportional hazards model, **[M1.1a]** each point conferred and [sic] increased probability of developing the primary outcome of 1.832 (95% CI 1.356 to 2.474) with a strong statistical significance ($p < 0.001$; Table 4).

The discussion is another section that changed significantly, especially as a result of the feedback received from the third and final journal. In the early versions of the RA, the discussion opened with a three-paragraph introductory section that preceded the first subsection heading. These opening comments are noteworthy because they are highly similar to the same stretch of text that the editor of the third journal marked for deletion in the handwritten edit appearing in Document H. It therefore seems that Esteban's notions of how to craft the discussion clashed with the standards of the journal gatekeepers, although it must be kept in mind that one of the

primary concerns evident in the editor's remarks (Document I) was to shorten the manuscript's length. As a result, it is not clear that this rhetorical style was considered deficient or whether the opening to the discussion is considered to be less important than others.

Contrasting somewhat with this apparently idiosyncratic or L1-influenced rhetorical style is the conventional style Esteban used in other parts of the discussion. As seen in the results section, the scientific content of this paper morphed as the RA progressed toward publication. In the course of these changes, some variables are regrouped and rearranged within the text. The following passage from milestone version 3 (Document C) and milestone 4 (Document K) is one example (text added in Document K is underlined). What is of interest, however, is the way in which Esteban adapts the rhetorical structure of the paragraph in light of this change.

Document C

[M3] Increasing levels of galectin-3 and NT-pro-BNP joined MCP-1 as predictors of developing this end-point, along with hypertension, increasing age, treatment with proton pump inhibitors and nitrates and lack of statin therapy. **[M5]** While NT-pro-BNP is a well-known marker and predictor of heart failure (...)

Document K

[M4] High galectin-3 and NT-proBNP plasma levels, but not MCP-1 levels, were independent predictors of the end point composed of heart failure and death. **[M6]** This is probably related to the nature of the different biomarkers. **[M5]** Although MCP-1 has its main role at the vascular level, galectin-3 and NT-proBNP are related to myocardial damage. NT-proBNP is a well-known marker and predictor of heart failure (...)

In the second, the revised analysis brings about a change from uniformity to difference, with MCP-1 failing to predict the endpoint. Esteban follows this evidently unexpected result (Move 4) with a two-sentence commentary consisting of an explanation (Move 6) and then a reference to existing research (Move 5), thus rounding out the reporting with evidence from the literature.

Attitudinal aspects

Esteban has a very positive attitude toward the English language in general and also to academic English. He and I met in 2012, and since then have had sporadic contact. Until the informant

interview, we had always spoken in Spanish. However, when we began the interview he asked to have the talk in English, saying “it would make me happy.” He speaks quite fluently and has a remarkable command of syntax and lexis, and the interview was a success.

Esteban had much to say about the process of publishing in English and the lengths that authors must go to to see their work in an English-medium journal. He finds the submission process arbitrary and flawed, and is critical of the guessing game involved in meeting gatekeepers’ expectations. He rationalizes this by remembering that editors and reviewers are humans and therefore have their shortcomings and personal agendas.

When asked why he researches and what he gets out of it, Esteban made it clear that research is a vocation and an extra for his career. He reads and writes mostly outside of his workday as a clinician, and the time he spends doing this additional work has become a source of certain tension in his marriage. In his opinion, producing and publishing research helps him to better interpret the literature he consumes as a clinician: having publishing papers, he is now able to properly contextualize and decipher what is written in the literature rather than taking it at face value.

When asked if he believed he was at a disadvantage because he is not a native speaker of English, he said that this is definitely the case, but that this disadvantage is not necessarily an unfair one. To illustrate this point, he mentioned how easy it is for a native speaker to draft a submission letter and how challenging it is for NNEs academics like him. He believes NNEs know exactly how to phrase these letters in terms of tone, how they position themselves with respect to their reader, etc.

Esteban remarked that one of the primary factors that make it difficult for authors on the academic semi-periphery to publish in English-language journals is not necessarily the language but the fact that they are removed from the professional networks that exist in countries like the United States. He claims that when an editor receives a submission from a Spanish hospital, they

may doubt whether the data are real or falsified, while if an editor has the same suspicion about an American hospital, they can easily find out what kind of work is being done in the hospital.

Implications

Many of Esteban's views about publishing in English-language journals are evidenced in the path taken by this RA and the different inputs that shaped the paper along the way. First, the contributions of the peer reviewers from the second and third journal were highly inconsistent. Some had very little to comment on, while others were meticulous and demanding in their feedback. Comments like the one made by Reviewer 2 of the second journal can be vague and lacking in substance at best, and at worst, may have undertones of suspicion and a hesitance to accept contributions from outside prestigious center contexts. After receiving such feedback, it is no mystery that Esteban should feel disadvantaged—not because of his limited English, but rather because he is so far removed from the networks of information, connection, and power.

Second, Esteban's strategy of writing at greater length than necessary seems to be validated in this TH. The comments and hand-written edits made by the editor of the third journal eliminated parts of the manuscript they believed to be superfluous. In spite of my surprise at the invasiveness of this practice, Esteban was neither dismayed by the way his paper was treated nor unready for this to happen. Based on this textual evidence and the insight Esteban provided during the interview, it is feasible that the author may have drafted these passages with the idea in mind that they may be the first to be pared down.

The attention given to the title of the manuscript by one reviewer and the journal editor is not to be overlooked. Though outside the scope of my study, it would be worthwhile to explore the frequency with which changes are suggested to manuscript titles so that novice authors can familiarize themselves with emerging patterns in particular journals and subgenre, as doing so would make them better equipped to position their papers before peer review.

Text history 2: Manuel

Overview

Manuel, my informant for this TH, is a young but high-achieving basic researcher in nephrology and vascular pathology who is the joint corresponding author of the paper I study here. The other corresponding author, a colleague from another institution, will go by the pseudonym of Tomás. This RA came out of a rich collaborative effort that reaches far beyond the partnership between these two researchers; in fact, the story of the text can be traced back to the early careers of Manuel and Tomás, the networks each of them have consolidated since they first met, and the local, national, and international activities of their discourse community. The connections that coalesced for this RA made for a variety of different contributions to the finished product.

This publication was met with little resistance from journal gatekeepers, and was accepted by the second journal it was submitted to. The authors first sent the text to a highly prestigious journal, which declined to publish the manuscript without sending the text for peer review. As a result, in this TH there will be only one round of feedback from academic literacy brokers. Initially, then, my analysis will aim to describe the textual features and discursive practices that made this RA successful—this within my broader endeavor to describe the writers, processes, and products of expert academic text production in an international context. Fortunately, what the TH lacks in interaction from journal gatekeepers is amply made up for by the wealth of drafts and other documents with which Manuel kindly provided me, including a number of drafts of the point-by-point response letter, which played a prominent role during post-submission revision. The informant interview was held in Spanish on August 20, 2014, lasting nearly two hours. During this time, I gathered a great deal of useful insight on the intricately networked activity in which Manuel and his colleagues engage.

Chronology and texts

Determining this RA's textual lineage was a challenging task, although during the course of the informant interview Manuel and I clarified the series of events that took place throughout the publication process. The drafts I used to construct this TH (see box below) began with a

repurposed version of part of the RA submitted to the first journal; here, the track changes feature enabled for Documents A and B revealed that Manuel and his co-authors made extensive revisions to the text after this initial submission. Unfortunately, however, this was the only evidence that Manuel had saved from this first submission, and as a result I focused my analysis on the writing done for the second.

1. Document A: Draft containing only introduction and methods
2. Document B: Draft containing introduction, methods, and results
3. Document C: First draft containing all sections
4. Document D: Second draft
5. Document E: Third draft
6. Document F: Fourth draft
- 7. Document G: Fifth draft; version sent to me for editing**
- 8. Document H: Version containing my proposed edits**
9. Document I: Sixth draft
10. Document J: Seventh draft
- 11. Document K: Version sent to second journal (eighth draft)**
12. Document L: First draft of point-by-point response
13. Document M: Second draft of point-by-point response
14. Document N: Ninth draft
15. Document O: Tenth draft
16. Document P: Third draft of point-by-point response
17. Document Q: Eleventh draft
18. Document R: Twelfth draft
19. Document S: Fourth draft of point-by-point response
20. Document T: Thirteenth draft
21. Document U: Fourteenth draft
22. Document V: Final version of point-by-point response
23. Document W: Revised manuscript submitted (fifteenth draft)
24. Document X: Journal acceptance letter
- 25. Document Y: Final published version**

Following this first rejection with no peer review, Manuel and the first author of the paper, whom I will call Ramón, revised the paper together alongside Tomás, then had the text edited for language by two different providers of editing services, after which they sent the RA to the rest of their co-authors before finally submitting the text to the second journal, which agreed to publish it following one round of revision. By the time the second journal received the paper (milestone version 3, Document K), the text had gone through ten different drafts or revisions. However, the first draft (Document A) contains only the introduction and methods section, and

the second, Document B, is made up of both of these sections as well as the results. Document C is the first draft containing all sections, and includes margin comments made in Spanish by Manuel and Ramón. The two co-authors used these exchanges to discuss particular passages of the paper, with each suggesting how the text could be improved; the outcome of these negotiations appears in the next version of the text (Document D). Although these were not milestone versions, in my analysis I will focus on some of this back-and-forth between the authors that appeared in these comments because of the rich insights they provide into the writing process and the two researchers' views on the RA genre.

These rounds of revision involving the two core co-authors continued until the first milestone draft, Document G. Document D contains a handful of modifications to the text, most of which are rephrasings with little impact on the overall rhetorical patterning. Some changes are significant, however, especially in the abstract and discussion, where the authors modify claims to novelty, and in a related revision in the discussion, where the authors reinterpret existing literature linked to the aforementioned claims. Like Document C before it, Document D contains a remark by one of the co-authors requesting clarification of a fragment in the results section. The work reflected in Document E is quite similar to what was done in the previous draft, although to a lesser degree. Modifications to the text include a few additions of technical content at the sentence level, revisions to meaning that kept the overall rhetorical shape of the text intact, one comment written by a co-author for the other co-author, and the elimination of a claim to novelty in the discussion. Document F contains rhetorically significant changes to the abstract, and minor modifications to paragraphing and subsection headings. The discussion section in this draft reveals surface-level revision, slight reassessments of the interpretation of the study results, and also two comments between Manuel and Ramón on the accuracy of the draft's reporting of a previous study and the appropriateness of this paraphrasing as support for the two authors' work. With the exception of citation formatting, milestone version 1 (Document G) shows no differences whatsoever with Document F. Following my edit of the RA (milestone version 2, Document H), two subsequent drafts reflect the input of the non-core co-authors, most of whom live and work in other parts of Spain (Documents I and J). Document I contains numerous sentence-level reformulations of language with no change to rhetorical patterning.

Document J differs from the previous draft only in a slight change to front matter, and the third milestone version, Document K, is merely a reformatted version of the previous draft.

After Manuel and Ramón were encouraged to revise and resubmit their text to the journal, they produced an additional seven drafts of the RA proper and five drafts of their point-by-point response. Rather than complete the revised text and then write the point-by-point response, they worked on the two in parallel, incrementally revising the text and developing the response letter as they made these changes.

Evolution of the text and the role of literacy brokers

Manuel recalls that coordinating the study itself was one of the biggest challenges he faced throughout the process. This was because to plan and execute the study, Manuel had to lead a team of researchers from three different institutions in Spain. As the story goes, Manuel first heard about a set of valuable patient samples from his colleague, Tomás. Manuel and Tomás had been classmates during their doctoral studies at a Spanish university outside Madrid. After completing their doctoral studies, both they moved to different cities. Manuel secured a postdoctoral fellowship at the university the IIS-FJD is affiliated with, and, as a part of this fellowship, spent a year in another European city to further his research. Years later, Tomás and Manuel met at an academic conference in Spain, where Tomás told Manuel that Manuel's research group in Madrid may be interested in using a series of samples that had been produced by another research group in Spain, and Manuel seized the opportunity. In this sense, one of the major contributions of Tomás was his role as a conduit between Manuel and the Madrid-based research team at our institution and the researchers who had produced the samples.

The process of writing and revising the text was complex, partly because of the physical distance between the three research centers and also due to the different roles that the co-authors played during the drafting process. The co-authors can be divided into four groups. The first group consists of the two core authors, Manuel and Ramón, who together wrote most of the paper, and Tomás, who also played a part in writing the RA, though to a somewhat lesser extent. Manuel and Ramón collaborated intensively to rework the manuscript after it was rejected by the first

journal. Although not one of the two corresponding authors, Ramón was based in Madrid at the time, which allowed for the two to meet every week and discuss the progress of the paper. Manuel deferred to Ramón's views on certain important issues, such as the decision to send the manuscript to the first journal even though the two knew this to be a long shot. As Manuel explained to me, this was a strategic move on the authors' part, and the two knew that the most realistic positive outcome would be rejection after peer review. Under this plan, the authors would receive valuable feedback from the peer reviewers, which could then be used as input for subsequent revisions of the article. This insight underscores the potentially constructive role of literacy brokers within English-medium international academic publishing but also indicates that Ramón and Manuel sought to benefit from gatekeeper contributions not for immediate publication but rather further along in the maturation process of the RA.

Second, Manuel was assisted by two senior members of his research group. These experienced scientists received a number of drafts of the text, "revising the text, correcting ideas, and changing some of the nuances," correcting some surface-level issues, and enriching the text by suggesting synonyms in order to avoid repetition. Their suggestions led to changes in existing sentences and were behind the addition and deletion of entire sentences or even paragraphs. However, these collaborators did not suggest a more wholesale revision of the text, which suggests that they found the work to be solid from the beginning.

The third and fourth group were located in the other Spanish city where the study was conducted. The third comprises researchers who, after performing the empirical studies used in the text, were consulted to review the manuscript before the text was sent to the second journal. The fourth group had an even lesser role, focusing on technical and formal issues exclusively, though Manuel stressed that they nonetheless were part of the effort.

The authors sent the manuscript for editing by professional literacy brokers on two occasions. According to Manuel's recollection, the first of these interventions was done by a large proofreading and editing company, and the second was done by me. My records show the opposite, although when I mentioned this discrepancy, Manuel said he was unsure which of the

two came first and appeared to see this difference as immaterial. What is certain, however, is that the first revision was made before the third and fourth groups were sent the manuscript. The effect of both such language revisions on the rhetorical makeup of the RA was negligible.

Minimal changes were caused to the text by reviewer comments, and the most intensive reworking took place in the first stage of the text's creation with Manuel and Ramón's joint revision. The first reviewer made only two comments in their report, one of which concerned a missing part of a table and the other suggesting that the authors substitute one term for another. Similarly, the third reviewer makes three remarks: one requesting further experiments, another highlighting an erratum, and a third requesting that the authors comment on an issue related to the small sample size. Unlike these two, the second reviewer made extensive comments which Manuel and Ramón meticulously responded to after performing additional experiments, carefully drafting and rewriting their point-by-point response, and revising the RA proper. Before listing the particular issues found with the paper, the report opens as follows (Document L):

The authors note that there is variability in reports of elevated sCD163 in HIV-1 infected subjects, but there is no discussion of why some studies found elevation and others did not. A number of possible confounders, described below, may each have a small effect, but altogether with the relatively small sample numbers, may bias results toward an increased sCD163 level found in the current study:

The criticism expressed here is highly relevant because it is used as an opening summary of sorts, with particular points numbered in the list that follows. The paragraph opens by mentioning the way the discussion is written, particularly as concerns the text's failure to posit an explanation for the variability seen in the literature. This supposed flaw with the discussion is immediately followed by mention of "possible confounders" which the reviewer expands upon in the subsequent list. It appears that the small sample size looms large in this reviewer report, and this gatekeeper argues that the authors' findings may be more the result of a combined effect between scarce data and the factors to which they make reference here. I will explore this issue further in the following section on the generic characteristics of the text and the circumstances under which it evolved or stayed the same, though I will first list either the verbatim comments

of this reviewer that are relevant to the rhetoric of the paper or summarize the general orientation of each:

1-3: (Need to perform further experiments or statistical analyses)

4: “The marker D-dimer is mentioned in the discussion, but no results are presented to support the argument of a differential form of monocyte/macrophage activation rather than [sic] generalized inflammation.”

5: “Increased activity in the TWEAK/Fn14 signalling pathway has been suggested to be involved in the increased inflammation associated with inflammatory bowel disease. Also, it is widely believed that HIV-1 infection leads to loss of epithelial barrier integrity in the gut-associated lymphoid tissues, leading to increased translocation of bacterial products and increased inflammation. The authors do not propose how HIV-1 infection leads to decreased sTWEAK. Previous studies have correlated TNF-alpha receptor II as a marker correlated with plasma HIV RNA viral load and CD4 decline. If TNF-alpha activity is increased in HIV-1 infection, shouldn't that also correlated with an increased sTWEAK level, rather than decreased?”

6: (Mention of the effect of the small sample size)

7: (Improper format of references)

8: (Need to perform further experiments or statistical analyses)

9: “It's not clear how the authors explain the data for the patients with 'no detectable virus' receiving the ART. Virus related pathways might be not [sic] involved. Are the changes in sTweak and CD163 also observed in healthy controls receiving ART?”

Genre considerations

This RA received no negative feedback on the quality of the writing in English, indicating once again that the author and his network of co-authors and professional language brokers are capable writers and accomplished scientists. The relative ease with which the text passed through the academic gatekeeping system therefore leaves me to ponder whether Manuel's success is a reflection of his ability as a writer or whether the focus of the journal's gatekeepers leans toward scientific content rather than adherence to implicit or explicit norms of academic prose. As was explained in the previous section, this TH is a rich example of collaborative writing, and much of the evidence I gathered helped me to develop a clear picture of the goings-on behind published English-language academic papers. In this particular case, my analysis finds that

the abstract and discussion received the most attention, while the introduction, methods, and results remained largely the same.

The earliest drafts of the paper show how Manuel and Ramón worked through their ideas, even at a time in which the results of their study were still uncertain. The abstract was all but finalized by Document F, meaning that none of the input from fellow co-authors, language brokers like myself, or the academic literacy brokers representing the journal had any substantial impact on how the text was crafted. Prior to this, however, a small number of changes and margin comments appear in Documents D and F, which provide a glimpse of how the authors planned their strategy and negotiated the way this section would be written. One example can be seen in Document D, which introduces a gap move in the “Background” subsection, revealing a certain concern for demonstrating the news value (Berkenkotter & Huckin, 1995, pp. 28-29) of their work. With this rhetorically significant shift, the text, which in Document C closed the subsection with, “Circulating levels of these proteins have been previously associated to [sic] cardiovascular disease,” moved toward greater emphasis of the novelty of their work by finishing Move 1 in the following fashion: “Circulating levels of these proteins have been previously associated to [sic] cardiovascular disease, but no previous studies have fully analyzed their association with HIV” (emphasis is mine, showing the text added).

Evidence of the two authors’ process appears in the results subsection of the abstract as well, when Ramón expresses doubts as to whether a particular result merits mention. The paragraph is written in a highly methodical style that presents the authors’ data by reporting whether levels of a number of parameters in the patients’ blood increased, decreased, or remained the same. In Document D, Ramón flags the penultimate finding, and Manuel responds:

Ramón: I wouldn’t put this in the abstract unless our results are modest.

Manuel: Yes, it isn’t relevant from a clinical point of view. This can be eliminated.

In a post-interview consultation, Manuel confirmed that this exchange took place at a point when all results had been collected and fully analyzed. These remarks show that Ramón had reservations about announcing the finding in the abstract, as he believed that this may have

diminished the value of the abstract in the eyes of the reader. This reflects a certain importance given to the abstract both as a concise summary of the paper as a whole and also as a blueprint to be used when revising the text. When Ramón flags this fragment, he looks forward not only to how the paper will come across from the reader's perspective, but also to how noteworthy the results will appear when the paper is complete.

I was also drawn to another comment made by Ramón in Document D in which he suggests caution when interpreting the results of the study in light of feedback received on the study data during a national conference. The fragment, which was originally included in the conclusion subsection of the abstract, interprets the study findings in broad terms, suggesting that the findings of the study may warrant changes in the clinical treatment given to the type of patients being studied. The text I am referring to is as follows: "Individuals with HCV-HIV co-infection and/or active HIV replication presented an enhanced immune activation which may justify a more careful cardiovascular risk management." Although placed under a heading that suggests certain interpretation of the study results, the paragraph is otherwise lacking in analysis that goes beyond reporting on numerical values, although even so Ramón hesitates. The exchange that ensued went as follows:

Ramón: At the talk in [Spanish city] they were doubtful about drawing from the conclusion from such a small set of patients as ours. It might be better to write that further data are necessary or something to the effect. This seems like an overly bold conclusion.

Manuel: Ok, take it out. As you can see, we say the same thing in the results and discussion. We should put a statement that is more of a summary... I will write one up and send it to you so you can see what you think.

Here, inputs from the discourse community reach back to a face-to-face scientific meeting during which Ramón presented the data that he and Manuel were attempting to publish with this RA. It is not an unusual practice for researchers to give oral presentations on their data and preliminary conclusions in order to receive this very type of feedback from their peers. What is of interest here is the textual evidence included in this TH, underscoring the multitude of input sources that can change the face of an academic text. Also, the fact that this feedback was derived from a

national scientific meeting is not to be overlooked, as it shows the extent to which the national researchers share and perpetuate the values of the international discourse committee.

Between Documents C and F, the abstract received a great deal of attention from the two authors, indicating that the abstract can function as a road map for the writers and for the reader. Rhetorically significant modifications were made mostly between Documents E and F. These included eliminating a finding in the conclusion subsection, which is relevant when we consider the fact that the conclusion was mostly a listing of findings rather than an interpretation of these results. Also, in Document F, the authors reintroduce what was previously a finding without further analysis (Document C), hedging this finding and presenting it as a knowledge claim in F. The change that took place is as follows:

Document C: HIV co-infection and/or active HIV replication enhanced immune activation state.

Document F: HIV co-infection and/or active HIV replication seem to enhance immune activation state in spite of ART.

While the difference between “enhanced immune activation state” and “seem to enhance immune activation state” appears to be minimal, this hedge (Hyland, 2005) marks the shift between unquestioned statement of empirical fact and speculation on the significance of the RA’s findings.

During my move analysis, I noted a clear, almost verbatim similarity between certain parts of the abstract and the discussion. I decided to ask Manuel about this:

Oliver: In my analysis I’ve seen that the abstract and discussion are really similar, aren’t they? Is this something you normally do? In this case, it worked, as the paper was published. Is this something that you consciously...

Manuel: No, when I start with the discussion...I don’t know (laughs). Since the time I was writing my doctoral thesis, I was taught that when you write the discussion you have to say what your main result was and then begin to elaborate on this result. So in the abstract you also have to summarize your primary results, so they are often very similar...at least when I write, they are very similar. So you encompass everything you have found in the discussion. “We found this, that, and that.” So in the following pages

[of the discussion] you have to individually contrast these with the literature. And what [your findings] mean and don't mean. So in the abstract you have to bring it all together and make it all very concrete, and in the discussion I begin writing that way, so logically they are going to be very similar. The results you have are the results you have. You can't change them.

This answer creates the impression that for the author the abstract is not a genre unto itself, but rather a highly concise derivative of the discussion in which the author must succinctly express the primary findings of the study, among other issues. This view, however, is contradicted by the fact that Manuel and Ramón put so much thought and work into the abstract in the early stages of drafting the RA. At another point in the interview, Manuel stated that many researchers in his field of science—himself included—often read “only the conclusion” of an RA in order to get the gist of a publication. It is likely that the abstract has a different level of importance and is put to different uses in other fields of academia, especially more argumentative fields such as the social sciences.

The introduction of the text remained largely the same from the earliest drafts of the text through the published version. None of the reviewer feedback was directed at this section, possibly because in Manuel's discipline writing about research is more a matter of describing sophisticated studies and reporting the equally complex analyses of the resulting data. Indeed, despite the humbling complexity of the concepts discussed in this RA, the paper is surprisingly straightforward in its design.

This simplicity of exposition is most evident in the introduction. This three-paragraph section is predominantly devoted to Move 1 (Establishing a territory), though the first paragraph closes with a Move 2 (Establishing a niche) and paragraph 3 finishes with another Move 2 which is then followed by a very brief Move 3 (Presenting the present work). Briefly, Manuel's study explores the fact that HIV-infected patients are more likely to develop cardiovascular disease. Despite this widely recognized observation in clinical medicine and certain findings that may explain how and why this happens, in the introduction the authors stress that this population of patients requires more sophisticated tools for measuring the risk that HIV-infected patients will have heart disease, and one very promising tool for measuring this specific risk involves using biomarkers. This first

paragraph employs a two-step realization of Move 2 (Establishing a niche) which reads as follows (Document Y):

[M2, S1a] Conventional risk prediction models based on traditional cardiovascular risk factors may underestimate the incidence of atherosclerotic cardiovascular events in patients with HIV infection [7] because they do not consider the specific atherosclerotic processes reported by these patients. **[M2, S2]** Identifying biomarkers for HIV patients at higher cardiovascular risk may be of great interest and could improve cardiovascular risk predictions by traditional stratification scales.

The niche that the text intends to occupy is not entirely novel, as seen from the citation. The first sentence nonetheless highlights the inability of conventional practice to solve the problem at hand, thus making this a clear instance of Move 2, Step 1A (Indicating a gap). In the closing sentence, the authors both raise the issue of biomarkers to remedy this problem and also stress the clinical application and benefits of such biomarkers (Move 2, Step 2: Presenting positive justification). While rhetorically rich, this combination of steps also mirrors the clinical and basic research that the study draws from, creating a balanced opening to the introduction.

The two primary biomarkers included in the study are treated separately in the second and third paragraphs. Both begin with general descriptions of these molecules and then make the case for the link between the biomarker and diseases of the heart. First, these instances of Move 1 mention the ways in which the molecule may play a role in development of cardiovascular disease, citing previous studies that have reached this finding. Next, studies reporting measurements of the biomarker in patients with the disease are mentioned. Finally, studies in which the biomarker has been used to successfully predict development of heart disease are offered. The first of these two paragraphs comes to a close with no progression to a subsequent move. The third and final paragraph, however, concludes with the following Move 2 (Establishing a niche)-Move 3 (Presenting the present work) sequence:

[M2] To our knowledge, there are no studies examining sTWEAK plasma levels in patients with HIV infection. **[M3]** In this study, we analyzed (...) Furthermore, we assessed the influence of antiretroviral treatment on these biomarkers after 48 weeks of follow-up.

This end of the introduction is noteworthy because of the rather simple rhetorical strategy used in this passage and in the RA as a whole: the authors highlight that the molecule had not been studied in HIV-infected patients and then proceed to announce that the study explored this and other biomarkers in these patients and controls, adding in the last paragraph the additional variable of antiretroviral treatment. It would appear from this successful RA that clear development of the main ideas is a welcome trait in introductions, which do not require complex rhetorical patterning but rather are more focused on data.

The methods section underwent the most substantial reworking in the first two drafts (Documents A and B), though these two versions of the text preceded the first draft in which all sections of the paper were included (Document C). By Document C, the section had taken the shape that would hold until the final published document, and overall Manuel did not see cause to revise the section in depth. This consistency is somewhat surprising, however, if we consider that the feedback that most substantially changed the rhetorical patterning of the paper encouraged further experiments to rule out the influence of confounding factors. With one exception, added laboratory work and statistical analysis was addressed in the results and discussion, which indicates that descriptions of what was done in an experiment are not limited to the methods section.

The methods section progresses systematically through all descriptions and minor reports of data such as coefficients of variation, which say more about the reliability of the experimental procedures used than about the actual data that were being sought in the study. Divided into three subsections (i.e., Subjects and design, Laboratory investigation, Statistical methods) each part is devoted a single paragraph containing a high density of technical information. The only remarkable change came about when the authors included mention of statistical analyses that they had carried out in the post-submission stage in accordance with the criticism of Reviewer 2. The comment, in which the reviewer asks whether statistical analyses had been done to eliminate the possibility that the paper's findings had been influenced by confounding factors, led Manuel to include the following passage in the Statistical methods subsection in the revised version accepted for publication (Document Y):

We conducted a multivariate linear regression analysis in order to evaluate the relative impact of different baseline characteristics (age, triglycerides, BMI, antihypertensive treatment and cytomegalovirus IgG positivity) on plasma sTWEAK, sCD163 and sCD163/sTWEAK levels.

This addition covers all of the tests that the reviewer considered to be missing, indicating that the research team had carried out statistical analyses to address these issues and make their findings more robust. What is most intriguing about this addition, however, is not what was done to the text proper but rather the exhaustiveness of Manuel's response in the point-by-point response and the many drafts it took him and Ramón to get there. Indeed, the evolution of the response to this question is remarkable over the five drafts of the response, and the authors drastically changed their response to this feedback. The progress of this point-by-point response letter merits an entirely different study, though here it is worth noting that in replying to the reviewer, Manuel went as far as to draw up a table to illustrate the points he made in the letter, though this table was never included in the RA proper. Given that Manuel writes his reply to gatekeeper feedback either at the same time he modified his text or even before making these modifications, we see the occluded genre of the point-by-point response as a totally separate conversation between the author and the reviewer, complete with its own rhetorical strategies, exposition of fact, and rebuttal of criticism.

Contrary to my expectations, the additional experiments requested by Reviewer 2 had a minimal impact on the results section as well. Apparently with great ease, the authors of the article included mention of the statistical analyses and expanded set of measurements without modifying the rhetorical structure of the results section, thus lending additional validity to Manuel's statements that downplayed the real importance of these post-submission changes to the overall thrust and value of the study. The comparison appearing below, between the version submitted to the second journal (Document K) and the revised version (Document W), illustrates the way in which the rhetorical structure of the results section in this paper absorbed this new information. In this part of the results section, Manuel and his coauthors offer baseline information on the study population and the control group. (All text added in the second of these versions is underlined.)

Document K

At baseline, HIV-infected patients had significantly higher levels of glucose, triglycerides and the LDL/HDL ratio in addition to lower levels of HDL than controls (Table 1). HIV patients at baseline also had elevated concentrations of sVCAM-1, ADMA and hsCRP (Table 1).

Document W

At baseline, HIV-infected patients had significantly higher levels of glucose, triglycerides and the LDL/HDL ratio in addition to lower levels of HDL than controls (Table 1). HIV patients at baseline also had elevated concentrations of sVCAM-1, ADMA, hsCRP, hsCRP, IL-6 and sTNFRII (Table 1). No differences were found for D-dimer. (Table 1).

This rhetorical style and its capacity to incorporate additional laboratory findings or statistical determinations suggests that Manuel's disciplinary community places great stock in raw findings within the results section, which downplays the need for varied and complex rhetoric in the section. Such patterning is likely both easier to adapt to the input of reviewers and also easier to master and use for NNEs researchers.

Part of this adaptability of the results section is due to the highly structured manner in which the section was written. Named subsections guide the reader through the different analytical findings of the experiment. Within each subsection, Manuel and his coauthors show a tendency to use what Williams (1999) refers to as pointers (Move 0.1), directing the reader to either tables or figures containing the numerical data or a visual depiction of their findings. Remarkably, three of the four named subsections open with one of these pointers, indicating that the writing is intricately linked to these elements.

Another rhetorical strategy apparent in this section concerns the use of procedural moves (0.3). In three instances, the authors use procedural moves in what Williams refers to as "covert structure of section role" (Williams, 1999, p. 353), that is, as a means of delimiting sections within the discourse. In particular, Manuel makes reference to what was done when shifting to new issues when there is no subsection break to aid in marking this break in the flow of the pattern. Two consecutive paragraphs begin with "We then" followed by a verb describing these experimental actions, indicating that the rest of the paragraph will offer an account of the results of these methodological steps. In the third example, however, the procedural move is somewhat

more complex; unlike the previous two realizations of this move, this one indicates both *how* the experiment was conducted and *why* these steps were taken. The example I am referring to here appears in the subsection that reports on the analysis of HCV-HIV co-infection and active viral replication, and was not changed at any point along the arc of the text:

After 48 weeks of ART, 23 of the 26 HIV-infected patients had undetectable viral loads with a median CD4 cell count of 435 cells/ml (IQR 300-652). The median viral load in the 3 patients with active HIV replication was 248 copies/ml (IQR 173-759). Because HCV-HIV co-infection and active HIV replication may maintain immune activation, we analyzed the possible influence of these conditions on all the biomarkers analyzed.

The authors justify this experimental procedure and indicate the analysis they used. This Move 0.3 draws the focus away from the results on viral load after 48 weeks of ART to the more specific issue of what the effect of either active virus replication or HCV-HIV coinfection may be on immune activation and, therefore, inflammation and coronary artery disease. In his 1999 study, Williams includes justification as one of the functions of Move 0.3, thus making Manuel's rhetorical patterning similar to the texts analyzed in his corpus. No negative feedback was given on this or any other part of the results section, which once again makes the case that this was a highly effective section.

One salient rhetorical feature of this author's writing is a mechanical, list-like approach to the results section. Manuel writes in a markedly objective fashion when reporting his findings in this section and leaves virtually all evaluation of these findings for the discussion section. All but a few moves in this section fall under Williams's (1999) "statement of finding," which Williams discovered to be a highly common occurrence in biomedical RAs and one that is much less common in other disciplines. Within the large field of biomedical research, basic research disciplines like Manuel's may be even more likely than clinical studies to avoid substantiation of findings and commentary categories (*ibid.*). Indeed, the only move variation is seen in the procedural moves analyzed above, which act as covert metatextual elements. As this apparently dry style of writing seemed to characterize the results section, I decided to ask Manuel about the two opening paragraphs of the results section, the second of which appears above and begins with "HIV patients were older than" (transcribed above). To my question of what impression he

wished to have on the reader in this fragment of text, Manuel detailed his approach to the section, stating the following:

In this case, I like to present result in a very cold manner. “Cold” meaning...you present your result “as is.” You don’t interpret them. You don’t try to give anything beyond them: they go up, they go down, concentration levels are so...but you don’t try to make value judgments about your results. Maybe in some paragraphs, when these are more complex, I may add a sentence at the end that summarizes all the results. But not in this case: it was more a matter of whether values went up or down. In any event, [presence or lack of interpretation] varies based on the article and to some extent on the kind of journal or your mood when you are writing the article. And sometimes you may put an ending sentence, although I normally don’t.

As I had suspected, Manuel consciously reserves commentary and interpretation for other sections, preferring to lay out his results in a transparent, systematic fashion. Indeed, the text and the author interview both support Williams’s finding that many biomedical authors reserve their analyses for the discussion section. What stands out from Manuel’s words, however, is the admission that there is intra-disciplinary variability that stems from the type of article and its degree of complexity and the type of target journal.

The discussion section received the most attention from journal reviewers, and as a result Manuel and his colleagues performed a limited but significant revision to the text during the post-submission phase for the second and final journal. While previous sections of the RA stand out for the relative absence of changes despite the additional data being reported, these issues are magnified in the discussion, leading to the incorporation of Moves ranging from single sentences to entire paragraphs. Few of the reservations expressed by the journal gatekeepers targeted the quality with which the paper had been written. Nevertheless, Manuel demonstrated great resourcefulness in responding to these criticisms.

As I studied the drafts of the RA and the supporting materials, I discovered that Manuel’s practice of working through drafts of the point-by-point response as the source of revisions to the text proper rather than a reflection of these modifications was particularly pronounced in the discussion. Of the three reviewers who evaluated the paper, the second was by far the most exhaustive, requesting additional studies and statistical analyses, justifications of the conclusions

reached in the paper, and calling into question the size of the study sample as a source of robust data. Faced with the challenge of appeasing this gatekeeper, Manuel and Ramón treated the point-by-point response in much the same way as they did the paper itself, proposing responses or brainstorming ways of replying to this reviewer, each author taking turns to either accept or challenge the other's suggested response. The degree to which these responses evolved as the response letter was drafted is substantial; in fact, an early response to the reviewer's call to perform further statistical analyses began as an explanation that the researchers were unable to do so, and ended as an in-depth description of the very analyses they had been asked to perform in the first place. In the five versions of the response letter, some contain internal notes directed either at the other author or to both authors, acting as a "note to self." Examples of these include "develop this further and search for references," commentary on passages ending with "or something to this effect," questions marks expressing doubt, among others. Other notable content in these draft responses include verbatim fragments copied from—or later transferred to—the RA proper, with little additional commentary other than "We have made the following change to the discussion section:" followed by the excerpt in question.

As with previous sections, the discussion is highly structured, reflecting what Dudley-Evans (1994) refers to as the "overarching structure" of the section consisting of an introduction, body, and conclusion. The first paragraphs of the discussion introduce the section by summarizing the experiments that were carried out and the study's main findings. On this issue, Manuel explained to me his penchant for discussion openings that present "raw results," reserving comparison with the existing literature and analysis of findings within the larger research questions for later parts of the discussion. Once all argumentation has been set out in the body of the discussion, the concluding paragraph reiterates these findings in much the same way as they appear in the opening, though pointing less to concrete findings and more toward broader concepts that concern the discourse community. The closing paragraph is as follows:

In conclusion, we describe that sTWEAK plasma concentration is reduced whereas sCD163 and Scd163/sTWEAK ratio are increased in HIV patients compared to healthy subjects. ART diminishes elevation in sCD163 in HIV patients whereas not effect was observed for sTWEAK. Moreover, our study suggests that the treatment with PIs and the presence of HCV-HIV co-infection and/or active HIV replication attenuates beneficial effects of ART on immune activation, therefore facilitating an accelerated atherosclerosis.

During the informant interview Manuel mentioned on a number of occasions that he strives to write papers that are “interesting” for the readers, be they peers or peer editors. He takes great care not to overreach with his conclusions, and the only language in the excerpt above that suggests a logical leap is hedged through the use of the verb “suggests” (underlined).

One rhetorically related criticism made by Reviewer 2 is the one that excerpted in the section on the evolution of the text (see above). This feedback states that the authors had failed to comment on the fact that previous studies had produced conflicting findings on one of the comparisons made by Manuel and his co-authors. My study of this fragment of the discussion lent certain validity to the reviewer, especially because these reports on other researchers’ findings were realized as Move 5 (Reference to previous research). Unlike more conventional rhetorical patterning, Manuel’s argument appeared to stop short of offering an explanation for this lack of consistency, therefore leaving the reader to question whether Manuel’s findings should be construed as supported by the literature or not. Despite this issue noted by the reviewer, this comment was articulated in the reviewer report as a preface of sorts, and therefore the authors did not address the issue. The article was accepted in the end, making it possible that the reviewer chose to overlook this matter. This was not the only instance in which the reviewer failed to follow up on suggestions for the authors: in one of the drafts of the point-by-point response, Manuel and his colleagues make the decision to sidestep a point of criticism made by the reviewer, with the hope that the reviewer would fail to detect this, and that is exactly what happened. This finding suggests that peer reviewers may perform a more cursory evaluation of texts when they are resubmitted, thus making the case that gatekeeper decisions on whether to eventually accept manuscripts are sometimes made in the first round of peer review and are not contingent on successful adherence with feedback by authors.

As Manuel explained to me, part of his paper's merit—and also the source of its complexity both during data collection and the drafting process—is that it represents a combination of clinical and basic research. One of the aspects that struck me about the discussion is the way the authors bring out this aspect of their work. Immediately after the introductory paragraph, the next paragraph offers a brief account of the clinical side of the investigation, closing out the paragraph with a novelty claim that emphasizes the methodological innovation of the study. The first three sentences are devoted to Move 1 (information move), surveying the existing literature for studies that have dealt with the same phenomenon. The fourth and fifth sentences continue as follows:

[M3/M5] In agreement with these studies, our patients show and increased hsCRP, sVCAM-1 and ADMA plasma concentration. **[M1]** In addition to these traditional biomarkers, we have evaluated the sCD163 plasma concentration and for the first time sTWEAK and sCD163/sTWEAK ratio in patients with HIV. (my emphasis)

The hybrid Move 3/5 (Finding/Reference to previous research) situates the study within the tradition of research done in the field, though the next sentence makes the novelty of the new study clear, noting that no other papers have pushed this traditional line of study in this new direction. This Move 1 – Move 1 – Move 1 – Hybrid Move 3/5 – Move 1 makes a strong case for the validity of the research, and I believe that such purposeful writing beyond the sentence level is a reflection of Manuel's desire to produce papers that are "sellable," as he called it.

As with the results section before it, changes made to the discussion in light of reviewer feedback were often seamlessly made, affecting the surrounding discourse little or not at all. One example of this is the ease with which Manuel was able to expand stretches of Move 1 to properly mention previous research related to the additional analyses requested by Reviewer 2. A more compelling case of this—and one that reinforces Manuel's view that downplayed the importance of such additional measurements—is the nearly imperceptible changes to the introductory paragraph. To evidence this flexibility of the rhetorical traits of the discussion, I will excerpt the version first submitted to the journal (Document K) and the final published version (Document Y), underlining the modified text in the latter:

Document K

In the present study, we investigated the association among sTWEAK and sCD163 plasma concentrations, the sCD163/sTWEAK ratio and other traditional inflammatory and endothelial biomarkers (hsCRP, sVCAM-1 and ADMA) with the presence of HIV infection. We also investigated the impact of ART on these biomarkers. We observed that HIV-infected patients had decreased plasma levels of sTWEAK and increased concentrations of sCD163, hsCRP, sVCAM-1 and ADMA compared with healthy subjects. ART decreased the levels of sCD163, sVCAM-1 and hsCRP but did not affect sTWEAK and ADMA concentrations, reflecting a reduction in inflammation and immune activation associated with HIV.

Document Y

In the present study, we investigated the association among sTWEAK and sCD163 plasma concentrations, the sCD163/sTWEAK ratio and other inflammatory, endothelial dysfunction and thrombotic biomarkers (hsCRP, IL-6, sTNFRII, sVCAM-1, ADMA and D-dimer) with the presence of HIV infection. We also investigated the impact of ART on these biomarkers. We observed that HIV-infected patients had decreased plasma levels of sTWEAK and increased concentrations of sCD163, hsCRP, IL-6, sTNFRII, sVCAM-1 and ADMA compared with healthy subjects. ART decreased the levels of sCD163, sVCAM-1, hsCRP, IL-6 and sTNFRII but did not affect sTWEAK and ADMA concentrations, reflecting a reduction in inflammation and immune activation associated with HIV.

The importance of this opening to the discussion is not to be underestimated, as it provides the reader with an overview of the main findings. This means that the rhetoric of the paragraph was able to absorb these modifications, suggesting that they are of lesser importance to the overall study.

The second genre-related modification to the text is seen in the paragraph preceding the conclusion to the discussion. Although little mention was made to the rhetorical characteristics of the paper during the peer-review process, two of the three reviewers questioned the effect of the small sample size used in the study, and these remarks led Manuel to include a penultimate paragraph to the discussion section in which he addresses this issue:

One limitation of this study is the relatively low number of patients. However, the strict inclusion criteria for patients and the design of our study limited us to include more VIH patients. Moreover, it is important to note that in the present work we could not analyze the effect mediated by HCV coinfection and/or persistence of HIV replication on immune activation in an independent manner. However, these data raise an attractive hypothesis to be explored in future studies.

This paragraph was added between milestone versions M and W, and marks the first time Move 8 (Limitations) is included in the article. Here, Manuel demonstrates skillful use of generic characteristics: first, he acknowledges this limitation to the study; second, he follows this admission with two factors that qualify this shortcoming; and last, the paragraph closes with a statement that does, in fact, convey the potential future studies that could be carried out with similar data and methods. Unable to remedy this shortcoming by increasing the sample size, the authors opted to incorporate this reviewer feedback in rhetorically felicitous fashion, phrasing this as a limitation (Move 8).

Attitudinal aspects

Of all the 10 participants in my study, Manuel is the only one with whom I had had no previous contact beforehand; the other nine researchers had either corresponded with me via email or in both through email and in person, though Manuel and I were introduced by another individual while I was searching for volunteers to take part in my thesis. Unlike clinical staff of the IIS-FJD, whose primary focus is care delivery and for whom research is often a complement to their career activity, Manuel and others like him are expected to produce high-impact research in the hope that he and his research group will continue to secure funding from the Spanish Ministry of Science and Innovation. For these Ministry experts, quality is synonymous with impact factor, which leaves Manuel no choice but to publish widely in English-language journals that are increasingly competitive. Indeed, on a couple of occasions during the interview Manuel's response to my inquiries suggested that I was mistaken to think that publishing research in English-medium journals rather than in Spanish was a choice rather than an obligation. As Manuel made clear to me, the only publications that are valuable to him are in English, making it a foregone conclusion that anyone who wishes to find a place within the discourse community will publish in the language.

Although Manuel feels that his NNEs status puts him at a disadvantage relative to other more proficient writers of English, publication in English-medium journals has been the norm throughout his relatively short scientific career. Having come of age at a time when the rewards system in the field of scientific research demands successful communication in English, Manuel has developed a battery of skills and a network of academic peers that have helped him overcome this disadvantage. When describing the effort it takes him to write in English, he assessed his abilities in the following manner:

Well, I don't write as fluently as I do in Spanish, though fortunately I think that, as the structures are very similar, I think I'm more and more capable of producing work that is decent, though not excellent. I would like to have excellent English skills to be able to use turns of phrase, enrich my lexis, construct verb phrases that are much more...grammatical structures that a much better and of higher quality. To improve the language [of my papers]. Not just writing "this goes up and this goes down," but rather, "We believe that our data, which reveal a tendency to..." (...) a little more elegant, more fluent for the reader.

Later, he described his needs and expectations when sending his papers to others to revise them:

Usually when I send a text to someone to scientifically revise it I'm not looking for them to give input on the English but rather the scientific matters themselves. So when looking for English correction I get in touch with people who can help me with [the English].

While Manuel recognizes that excellent prose is more textured and expressive than his, he believes "the structures" of academic English have limited variability, which eases the burden of having to write in his L2. The nature of his field of science is likely more international in scope and data-driven in its epistemology than other disciplines, as the biological phenomena being investigated and the clinical applications being pursued are presumed to be empirically transparent and universally applicable to all patients with HIV/AIDS. Therefore, with numerous research teams all over the world seeking solutions to health problems affecting all humanity, the discourse used by this international field of scholarship is likely well-served by a simpler rhetorical style.

When the need for language assistance does arise, Manuel appears to value input from field specialists over more language-focused support, turning to coauthors and other disciplinary

specialists more often to strengthen the scientific content of his papers than to “go native.” Improvements in style come either from senior co-authors on his team or are requested by professional literacy brokers like me, though in the case of the latter the remit is clearly delimited to linguistic matters, while scientific peers may provide input on both aspects.

It is somewhat difficult to reconcile Manuel’s success in English-medium academic publication and a certain ambivalence he shows toward the English language and its status as the language of his profession. When given the opportunity to hold our interview in English or in Spanish, he chose Spanish, explaining that he finds it much easier to express his ideas in his native language, at least when discussing the topics of my study. During our talk, Manuel appeared more focused on improving his writing than his spoken fluency, though this may be because he believes that it takes longer to achieve oral fluency than to improve one’s writing. He regrets the fact that his postdoctoral fellowship sent him to Paris rather than an Anglophone country, as he believes his English would have benefited substantially if this had been the case.

When questioned about his opinion of peer reviewers, Manuel stated that he has a generally high opinion of them and the review process itself. These scientists, he believes, are critical of work he submits, although not overly adversarial. In spite of Manuel’s favorable opinion of editors, his utterance “The editor didn’t like it” to describe the rejection of the manuscript by the first journal suggests a certain degree of subjectivity and arbitrariness in these decisions. In large part, to publish in the type of journals that Manuel targets, articles must “normally” be of high quality and withstand the critique levied by reviewers as to their novelty and methodology. However, as Manuel puts it, “(...) then there is a series of factors that you don’t control, though I am sure they have an impact: who you are, your experience with the topic of your study, whether the reviewers know you or not.” The author elaborated on this matter later in the interview when I asked him to ponder a hypothetical situation in which the same paper had been written by him, with his same knowledge and qualifications, though as if Manuel lived in the United States, for example. He replied as follows:

Manuel: I don’t know, but this can also be a factor, of course.

Oliver: Is this the first time you've ever thought about this?

Manuel: No. But not because of Spain. For instance, we have a bad image of the Chinese. The Chinese publish a lot and many times they don't...they lie. So I don't think [gatekeepers] doubt the truthfulness of our results. Because Spanish research is very good, very strong. In fact, the times I've gone abroad, far from looking down on me for being Spanish, totally the opposite: people know that we are very knowledgeable, highly trained, and with a great work ethic. I think they have a very good opinion of Spanish research. But I think Spain isn't in the group of first-tier countries like the United States, Germany, or England, and some may look down on you, as if to say "Where are you from?" as a country. But that changes when people know what research team you work in, because we researchers all know which group you come from. Our group is very strong. So if we're talking about researchers who are strong in the [disciplinary] area, they are going to know our group, and they're not going to look over their shoulder at us. If it's someone who doesn't know us at all and we are a strong team, they may look down on you for being Spanish. But normally we don't have any problems with this; in fact, totally the opposite. People have always helped us out...We haven't had that problem of prejudice because we're Spanish. But maybe some other groups, maybe they will have this complex.

Here, Manuel mentions that one possible effect of living and working in the outer circle is that journal gatekeepers may question the truthfulness of the results reported in RAs, and to illustrate this he offers the case of China (a country where he also appears to believe that the researchers falsify data). Though not a top-tier country in the eyes of academia, in some academic circles Spain is a highly respected country that has earned international admiration for its work. One particularly interesting theme arising from this stretch of the interview is the ability for local research groups to consolidate their reputation internationally, thereby mitigating certain inner-outer (Kachru, 1985) hierarchies and underscoring the highly complex and interconnected disciplinary cultures that coexist in international English-medium scientific research.

Implications

More than just a list of names appearing on the first page of the text, Manuel's manuscript is the result of an intricate network of researchers, with five co-authors and one reviewer having the most substantial impact on the final product. The process of drafting and revising the text is the product of academic networks forged in universities, academic conferences, and centers of research, all oriented toward global publication in English. Indeed, one particularly intriguing

finding of this TH is written evidence of audience input received during a national conference held before the paper was drafted. This finding raises the question of how many similar interactions across these genre chains may influence academic texts but leave no written record.

The success of this RA is also partly attributable to aspects of Manuel's discourse community and the rhetorical expectations for these types of text. We have seen in this chapter how Manuel and his collaborators almost effortlessly adapted their paper to include both pre- and post-submission inputs. This adaptability of the text is likely the result of the "internationalization" (Petersen & Shaw, 2002) of the discipline, but also the content-heavy orientation of the field. Within this context of lesser focus on rhetoric and increased attention to data, research groups like the one Manuel belongs to have been able to make inroads in elite international circles, partially nullifying the stigma or the disadvantage that hinders some NNES researchers in other fields. Such open standards that background NES accuracy provide further evidence of increasing ELF rhetorical standards in written academic discourse.

This analysis offers a unique perspective on Manuel's strategies and tendencies as a writer. We have seen a particularly interesting method of responding to peer review, in which the point-by-point response drives changes to the text proper and certain information is reserved for this occluded genre. In another part of the interview, Manuel described how his writing may vary from manuscript to manuscript based on how he feels at a given time; perspectives like this lend an element of credibility to theories that downplay the influence of one's native language on academic writing, raising the possibility that all writers, be they NES or NNES, produce work that is the product of their whole selves and their degree of expertise (Tribble, 2017), and not just their L1.

Text history 3: Susana

Overview

Susana is a senior researcher who has been affiliated with the IIS-FJD for a number of years. She and I have worked together periodically since I joined the institution in 2009; indeed, she was one of the first to ever send me a paper to edit. Unlike other frequent users of the service, Susana has often taken the initiative to meet with me to discuss her papers and my contributions, and during those sessions I have come to know her better than I know most of her peers. Of the ten researchers whose work and experiences are discussed in the present thesis, she is one of only two whose work I have studied both for my master's thesis and the present doctoral thesis. Possibly because of this familiarity, Susana kindly offered me a quite personal account of the circumstances surrounding her experience writing this paper, and to a great extent understanding this TH means understanding the events that surrounded its creation.

For reasons that will be explored in greater depth later in this chapter, this text was written almost entirely by Susana herself years after the data had been collected. Following a series of early publications on the results and after a particularly unfortunate life event, in this text Susana reexamines the findings of her previous work, though without presenting any new data. This lack of novelty made the writing task more challenging, and may have caused this publication to be an uphill climb. In my analysis, I will explain how the demands of the particular discourse community in which Susana attempted to publish this RA initially impeded Susana from publishing the text, partially because her analysis was believed to lack an appropriate intertextual web (Bazerman, 1988, cited in Berkenkotter & Huckin, 1995, pp. 47-48). The informant interview for this TH was held in English on August 28, 2014.

Chronology and texts

Susana published this paper in the third journal she submitted her work to. The first journal rejected the article after sending it for peer review, though for the second journal the editor turned down the submission without having it reviewed. The third and final journal accepted the text after one round of peer review. Susana had saved the rejection letter from the first journal

(Document E) but not the one she received from the second. The feedback from and response to the third journal both appear in Document I.

The submission letters were of particular interest to this TH because Susana sent the RA to the second and third journal without making any changes. Part of my conversation with Susana during the informant interview revolved around why she opted not to rework her paper in light of the reviewer feedback she received from the first journal. Susana's response gave me a much deeper understanding of the motivations and constraints that played a role in this and other decisions.

I received this paper for editing after Susana had drafted it in its entirety. Not all IIS-FJD researchers send me finished work, as many request a revision of a draft of an early version during the pre-submission phase and, in some cases, once again before they submit the text to the first journal or after they have received feedback from peer reviewers. This first text, Document A, is the first milestone version. This is followed by my revision (Document B), the second milestone version. Susana then modified the RA and sent the paper off to the first journal (milestone version 3, Document C). The final milestone version is Document K.

- 1. Document A: Draft sent to me for editing**
- 2. Document B: My edit**
- 3. Document C: Version submitted to Journal 1**
4. Document D: Submission letter sent to Journal 1
5. Document E: Rejection letter from Journal 1
6. Document F: Version sent to Journal 2
7. Document G: Submission letter sent to Journal 2
8. Document H: Submission letter sent to Journal 3
9. Document I: Two-part document: a) interim decision from Journal 3 (accepted pending revision), including reviewer comments; b) letter of acceptance from Journal 3.
10. Document J: Two-part document: a) Resubmission letter to Journal 3, including response to reviewer comments; b) Resubmitted manuscript as sent to Journal 3.
- 11. Document K: Final published version**

Evolution of the text and the role of literacy brokers

Susana wrote this paper mostly herself, and others made limited contributions. Some input was received from Susana's coauthors, from me, and from the peer reviewers of two different journals. Certain coauthors did nothing at all, while others carried out minor administrative tasks such as formatting the reference list. The one exception was a senior clinician from another department, who reviewed the manuscript for content, although Susana downplayed the scope of this coauthor's contribution.

What brought Susana to write the study with such little assistance has much to do with her personal history and the unfortunate death of her research partner years before. This collaborator had conceived of the study but passed away before he could see it through. (For purposes of anonymity, I will provide only cursory details of this back story, although in so doing I must pass over a touching narrative of grit and loyalty.) Of the two researchers, the true expert in the field was the deceased scientist, and Susana was somewhat of a new arrival to the field, having transitioned into the discipline after beginning her career in another area of the biomedical sciences. The passing of Susana's research partner came at a time when a number of the project's findings were still unpublished, and the RA I analyze here is based on these efforts. Years passed before Susana got to writing the text, and by this time many of the researchers with whom she had gathered and analyzed the data had moved on to other institutions. When drafting the paper, Susana included her new junior collaborators to give their careers a boost in spite of their actual contributions. Susana believes this to be an appropriate gesture of appreciation toward those working under her.

In addition to these contributors, I edited the manuscript and submission letter during the pre-submission phase (Document B) and then only the submission letter sent to the third and final journal (Document G). At the time I began recruiting informants for my research, Susana was one of the most frequent users of the editing service. Most of the changes I proposed targeted minor surface-level issues and matters of readability. This absence of rhetorical feedback was unexceptional in the work I have done with Susana, as her writing tends to be clear and well-crafted. During the interview, Susana told me that she prefers to have her work reviewed by a

native speaker because it ensures her that the finished product will be grammatically correct. She described the contributions of editors like me in the following way: “(...) I think I may make mistakes and I don’t like to send my papers without having someone like you or [the Australian postdoctoral student] to read my paper to see these things that may happen, like an “s” or a wrong preposition.” This and other comments Susana made indicated that what she looks for most in an editor is the ability to fine-tune her work on the linguistic surface rather than a more substantive approach.

The peer reviewers of the first and third journal gave Susana feedback on her text, and this input led to some minor changes to the characteristics of the RA. (Some of these were directed at the rhetorical traits of the text, and I shall leave discussion of this for the following section.) The most substantive comments were made by the reviewers of the journal that chose to reject the article, finding flaws in study design, a lack of novelty, insufficient reporting of variables, and an inadequate account of the relevance of the findings, among others. Of these perceived shortcomings, I was particularly drawn to the issue of relevance, since at first glance the reviewer appeared to suggest that a more well-constructed text could have increased the RA’s chances of getting published. The comment I am referring to reads as follows:

Abstract: why is this an important question. The discordance between insulin resistance in puberty and the absence of impact upon NEFA is of interest, but what is the clinical or biological relevance?
--

Confused at how the findings from this RA could be seen to be “of interest” and of unclear “clinical or biological relevance,” I asked Susana to explain this difference. Susana appeared at once to validate the reviewer’s criticism and lament their failure to provide the kind of constructive feedback that could have helped her properly show the relevance of her data. This comment seems to indicate Susana’s expectation that the peer reviewers would act more as benevolent literacy brokers than as adversarial gatekeepers, seeking to increase the quality of her paper rather than reject it.

This issue of relevance came up a number of times in the course of the author interview, and it became a dominant theme of our conversation. As Susana explained, the reviewer criticized

Susana's failure to relate the study's findings to other known aspects of this phenomenon. When I asked her to assess the reviewer's comments on the paper's lack of relevance to the field, she explained the situation as follows:

Susana: The thing is, I was telling you before, the finding was there. What I'm trying to do here is not finding a new [sic] results, just I'm trying to emphasize that it's important. That's why this man or this woman is asking me "Why? Tell me why." I'm claiming [sic] to be published just because I'm saying that this is relevant. I guess here they were demanding more support from me.

Oliver: So do you think that the reviewer is justified in writing this?

Susana: Maybe. What I thought when I read this, I thought that I was missing to [sic] really really explain the clinical relevance. And what I thought when I wrote my paper in that subtle way that you saw is that the reviewer should be helping me because I'm not able to do it because I have my data which is very important for supporting [sic] this. Maybe I was waiting for some idea to help me to emphasize my results.

Oliver: If I can paraphrase this, your expectation was that there was going to be a number of ideas, and you wanted the reviewers to say "OK, I understand what you're trying to say. Why don't you do this to the text.."

Susana: Or why don't you think of this explanation or this idea to support the relevance of your finding?

Here, it is relevant to see how, in her response to my recapitulation toward the end of this excerpt, she draws focus away from the text and toward more abstract comments of explanation and support. For me, the site of the work to be done was in the text, while for Susana situates this activity in the realm of ideas. When examined alongside Susana's other comment on the surface-level improvements she expects from me and other editors, it would appear that she understands the act of writing papers to be transparent reflections of ideas, while my wording of the issue gives more centrality to the text as the locus of academic communication.

Susana wrote this article with the conviction that academic texts are crafted not only by authors but also by the academic gatekeepers who are entrusted with the task of judging their quality. Contributions from disciplinary peers are assessed not as finished products, but rather for their potential to become excellent through change. Susana hoped—or even expected—that the

reviewers would propose an analytical take on her data that would round out the RA. To her chagrin, however, this was not the case. As she put it, reviewer feedback can sometimes help refine a text, though other times it gives authors nothing to work with. When I asked Susana to appraise the reviewer who questioned the relevance of her findings based solely on the seven points included in the reviewer comments, she thought the reviewer was from another field and therefore was unable to visualize the relevance of the paper's findings. She then added that some reviewer feedback suggests that the reviewer makes comments because they are expected to say *something*, no matter how trifling. Indeed, Susana and I were both struck by the comments made by one of the two reviewers from the journal that did in fact accept the text, as in praising the merit of Susana's paper the author invoked the reputation of the funding body appearing in the acknowledgments. To claim that an article must be of high quality because the public agency that funded it is also good is tantamount to saying that all books printed by a given publisher are good not because of their inherent qualities, but rather because the publisher only produces quality work. In light of the apparent lack of rigor of this second reviewer comment, it seems Susana may not be far off the mark in her opinion of the peer-review process. In fact, with other articles she has submitted she has gone as far as to write a letter of complaint to the journal editor calling attention to reviewer comments that contained egregious errors revealing insufficient knowledge of the field. This strategy, which she learned from a Spanish-born colleague of hers who works in the United States and with whom she collaborated while doing postdoctoral work in a North American institution, has previously been successful in cases of unjustified rejection of her manuscripts, securing her a new revision by a different referee.

Susana believes that journals with open peer review embody this spirit of collaboration, placing fewer demands on the way articles like this one are crafted. As a result, the finished product more clearly bears the stamp of the academic literacy brokers who collaborate with the journal. This perceived difference raises interesting questions on the difficulty faced by NNES authors when publishing in traditional versus open-peer-review journals. If Susana's view holds, NNES authors may find greater largesse in these newer formats than in traditional publications, as the former engage with authors' work in an effort to further the knowledge of the discipline.

In addition to having a somewhat critical attitude toward reviewers and the peer-review process, Susana admitted that she dislikes reviewing manuscripts from this particular scientific discipline because of the great responsibility she feels when reviewing others' work. While she is more sure of her expertise in her original field of specialization, she often doubts her ability to appraise manuscripts in this area, often declining requests to review submissions. At one point in the interview she intimated that her reluctance to review papers may have been the reason why this particular paper took many months to go through peer review. In a similar circumstance, another journal to which she submitted a text claimed it was unable to find a peer reviewer for her manuscript, thereby momentarily thwarting her attempt to publish the article. Susana has developed a somewhat adverse relationship with the discourse community, although it is unclear whether these feelings of uncertainty and reluctance impact her work as a researcher and in what ways these issues may manifest.

Genre considerations

The RA underwent very few changes between the first draft and the published version, and of these, fewer still changed the rhetorical characteristics of the text in any meaningful way. Susana's reluctance to rework her paper is noteworthy given the remarks of the reviewers of the first journal, who believed the study's methodological flaws and relationship to other research to be fundamentally lacking. A study of the rhetorical patterning used in the paper shows that the RA uses conventional rhetorical structure, including clear marking of moves and orthodox move cycles, especially in the introduction. As indicated by Susana and the gatekeepers who found the paper to not merit publication, Susana's difficulties stemmed from a certain lack of thoroughness in the data reflected, which was compounded by Susana's inability to prove that her data deserved a second look by the discourse community. Of these two shortcomings, nothing could be done after the fact to remedy the depth with which she and her team had studied the sample. However, Susana did believe that patterns were detectable in her results, though to properly articulate her observations of the empirical data from her research, she needed for the expert reviewers to guide her analysis, suggesting insights from the existing

literature to pull together her argument. Unfortunately, Susana did not receive the assistance she was looking for, and the paper was published mostly as it had first been conceived.

The text uses a structured abstract format consisting of four sections that closely mirror the 4-move framework of Bhatia (1993). Midway into the first subsection, titled “Objective,” Susana uses the explicit lexical item “aim” to signal the objective of her research, and what precedes this information is a brief introduction to the background of the issue and the gap that exists in the knowledge. We can see this progression in the excerpt appearing below, in which I have underlined all of the explicit signals marking the rhetorical buildup to the statement of objective in (5).

Objective: Puberty is associated with (1) decreased insulin sensitivity. Sexual hormone levels have been related with (2) the onset of insulin resistance but (3) their relationship with non-esterified fatty acids (NEFA) remains unexplored (4). The aim of this study was (5) (...)

This opening passage, which is entirely devoted to Move 1, begins with general statements indicating what has been learned (1, 2) before signaling the shift to and presentation of the gap in the research (3, 4, respectively). Once this need has been established, Susana moves to present the study in light of the two previous ideas (5). Rhetorically, Susana includes this very CARS-like style within an IMRD abstract (Lorés-Sanz, 2004), making it an example of a combinatory pattern (Ventola, 1994; cited in Lorés-Sanz, 2004, p. 286).

The abstract remained rhetorically constant across all versions of the RA. This is somewhat surprising when we take into consideration the criticism of Reviewer 2 from the first journal, who in Document E writes, “Abstract: why is this an important question. The discordance between insulin resistance in puberty and the absence of impact upon NEFA is of interest, but what is the clinical or biological relevance?” Such a remark appears to cast doubt on the objective of the abstract analyzed in the previous paragraph, especially as concerns the connection between the background information and the specific gap pointed out in the paper. This difficulty in grounding the study with the insights of previously published papers, however, was a central difficulty that Susana faced, and one in which she hoped that the journal gatekeepers could help her find during

peer review. When she received this feedback, however, Susana decided to resubmit her paper to a different journal, leaving this abstract unchanged until it was finally published.

The introduction of this RA is marked by the presence of two move cycles consisting of Move 1 (Establishing a territory) and Move 2 (Establishing a niche). Each of these cycles is given its own paragraph, and the third and final paragraph is devoted to Move 3 (Presenting the present work). One of the central concerns of the introduction—and of the RA as a whole—is the absence of data on the role of non-esterified fatty acids in the relationship between obesity and insulin resistance in children. While other studies have demonstrated this role in adults, there had been no such research in children, and this was precisely the gap that Susana set out to fill in her paper. Within each of the two Move 1-Move 2 cycles, Susana makes this distinction clear, using differences in available data to carve out the niche for her work. In addition, each of these cycles clearly makes use of statements of increasing degree of specificity (Swales, 2004, pp. 230-232) to bring the focus of the text from broader to more narrow concerns.

To illustrate these points, I wish to analyze the second paragraph of the introduction, especially as this second paragraph also shows how Susana grappled with the issue of rooting her work in the literature even before the aforementioned reviewer questioned the relevance of Susana's study. To properly contextualize this second part, I should first offer a brief account of the cycle that precedes it. In the opening paragraph, Susana establishes that obesity and the onset of insulin resistance are related, after which she enumerates three dynamics by which this happens. She then takes up the third of these phenomena, involving non-esterified fatty acids, and underscores the importance of this mechanism as demonstrated in the literature, highlighting the findings in adults and their relevance to the overall issue. Having presented this knowledge, she then goes on to stress that, as opposed to adults, the role of non-esterified fatty acids has not been elucidated in children and adolescents, supporting this statement with a reference to a previous study by her group in which they found that non-esterified fatty acid levels in children (not adolescents) differed from their obese adult counterparts.

The second paragraph is much like the first in that it uses the contrasting degrees of knowledge on the issue between adults on the one hand and children and adolescents on the other. Here, however, Susana situates her argument more squarely on sexual maturation and the implications this may have on insulin and, therefore, non-esterified fatty acids. As shown in the version sent to the first journal (Document C), Susana revisited this paragraph after I edited the text (Document B), and therefore I will provide both of these versions, underlining the modified text.

Document B

Puberty is associated with decreased insulin sensitivity and increased insulin levels (8-14). The role of sex hormones in insulin sensitivity has been studied, and a relationship between sex hormones and insulin resistance and presence of metabolic syndrome has been established in adults (15-20). Sex steroids have also been associated with insulin and insulin resistance in some studies in children (9, 21) but not in others (22). However, to our knowledge, the relationship between hormones and NEFA levels has not been investigated in a general population of adolescents. Thus, we aimed to determine whether NEFA levels are related to sexual hormones during sexual development.

Document C

Puberty is associated with decreased insulin sensitivity and increased insulin levels (8-12). The role of sex hormones in insulin sensitivity has been studied, and a relationship between sex hormones and insulin resistance has been suggested in adults (13-15). Sex hormone levels have been associated with insulin sensitivity in some studies in children (9, 16) but not in others (11, 17). Besides, to our knowledge, the relationship between hormones and NEFA levels has not been investigated in a general population of adolescents. Thus, we aimed to determine whether NEFA levels are related to sexual hormones during sexual development.

All of the citations were modified, suggesting that Susana re-evaluated the usefulness of these papers to buttress her own study. Also, the way in which she reports these studies changes in parts, eliminating mention of “metabolic syndrome,” hedging “established” to “suggested,” and eschewing “insulin and insulin resistance” in favor of “insulin sensitivity.” Intertextual appropriateness was an area of concern for Susana, which is why she was disappointed by reviewers’ criticism of the way she built her argument on previous research though at the same time aware of the paper’s limitations at this time. Once these changes to the introduction were made in Document C, the section remained unchanged through to publication.

Referred to in this paper as the Experimental section, the methods appear in structured format, containing three sub-sections titled “Study subjects,” “Data collection and study variables,” and “Statistical analysis.” This section did not change whatsoever throughout the text’s trajectory, although in my analysis of the results section I will show how these sub-sections are a near-exact mirroring of the three moves in Nwogu’s (1997) framework (Move 4: Describing Data-Collection Procedure; Move 5: Describing Experimental Procedures; and Move 6: Describing Data-Analysis Procedures), and in this section Susana exhibits a list-like approach when describing the study procedures. An illustrative example of this can be seen in the opening paragraph:

Study subjects: The studied population included two population-based samples comprising 854 (416 boys and 438 girls) 6- to 8-year-olds, and 822 (393 boys and 429 girls) 12- to 16-year olds. Children were participants of a cross-sectional study designed to analyze cardiovascular risk factors in Spanish schoolchildren, and were selected by means of random cluster sampling in schools. Parents were required to provide written consent for their children to participate in the study. All children reported by their parents to be suffering from chronic diseases were excluded. The study protocol complied with Helsinki Declaration guidelines and Spanish legal provisions governing clinical research on humans, and was approved by the Clinical Research Ethics Committee of the Fundación Jiménez Díaz in Madrid.

The way the paragraph progresses is notable, as Susana intersperses ethical issues (e.g., parental consent, adherence to international standards, and ethics-committee approval) throughout. It seems that these regulatory and ethics concerns break the logical flow of the text, although the author uses the issue of parental consent to lead into an account of the exclusion criteria, thereby salvaging the text flow. Legal and ethical matters are not accounted for in Nwogu’s move framework, and it would be worthwhile to investigate how this information can be incorporated into an updated move framework.

The three-paragraph results section comments on the three tables that present the findings of the experiment, and much of the text highlights the most salient patterns appearing in the tables. The first paragraph calls the reader’s attention to results appearing in numerical form in Table 1, the second indicates the relevant findings in Table 2, and Table 3 is analyzed in the third. Reference to the tables appears in the opening of each paragraph, either as a metatextual pointer (Move 0.1) in the first, embedded within a Move stating one of the findings of the study (Move

1.1a, comparison) in the second paragraph, and, in the third, as a parenthetical reference within a Move offering details on the procedures followed (Move 0.3). These run as follows in each version of the RA:

(Paragraph 1): NEFA levels in 6- to 8-year-old and 12- to 16-year-old boys and girls are shown by age in table 1.

(Paragraph 2): Insulin levels and HOMA levels were significantly higher in obese than in non-obese boys and girls (table 2).

(Paragraph 3): Partial correlations, after adjusting by age, between BMI, plasma sex hormones, and insulin resistance-related parameters were evaluated (table 3).

These three paragraph openers all make a clear reference to a table, although doing so in different ways: the first and third introduce the table and its data by focusing on the experimental methods used to produce the data, while the second reports one of the findings that result from the data appearing in the table. One characteristic shared by all three pointer sentences is that none of them refer to the main analytical findings of the paper; this information appears later in each paragraph, as can be observed in the second paragraph, which is excerpted here in its entirety:

Insulin levels and HOMA levels were significantly higher in obese than in non-obese boys and girls (table 2). However, no significant differences in plasma glucose and NEFA levels were found between obese and non-obese children, although girls showed slightly higher NEFA levels (table 2).

I found it quite challenging to perform the move analysis on this section, because on several occasions the difference appeared immaterial, and others there seemed to be a disconnect between the rhetorical intent of the author and its linguistic realization. An example of this can be found in the opening sentence of the third paragraph (excerpted above). Examined out of context, this sentence recounts the procedures used to carry out the study (Move 0.3: Procedural). However, if we examine once again the author's strategy by which she uses the tables to structure the section, making explicit reference to the table to be discussed when beginning each paragraph, it seems that the reference to the experimental procedure is more an example of a nicely varied style of writing than an emphasis on what steps were taken in the

study. In other words, rhetorical moves like this one may act as place-holders for what is stated parenthetically.

The second rhetorically related comment was made by a reviewer from the journal that accepted the manuscript (Document J). In this comment, the reviewer writes:

Either in the section entitled Experimental Study Subjects or at the start of the Results section, the sample should be described more precisely, specifying the number of obese boys/girls in each group; although these data appear in Table 2, their inclusion in the body of the text would make the paper easier to read.

These comments indicate the overlap between the methods section and the results, suggesting that authors can choose where place rhetorical moves that summarize methodological issues. Faced with this choice, Susana opted to modify the results, doing so by including a one-sentence paragraph at the beginning of the results section in which she followed the recommendations of the reviewer precisely, detailing the number of children included in the study sample and breaking this population down by sex, by age group, and by obese or non-obese status. (This was the only rhetorically significant change made to the results throughout the TH.) It is true that this information about the study could reflect Williams's Move 0.3 (Procedural), in which authors of RAs describe how and why they have produced their data, although the manner in which the author includes this information in the text is peculiar in that, like other authors, she chooses to insert this information as a stand-alone move that is in no way linked rhetorically to the surrounding text. The previous section, "Experimental: Study Subjects," does not offer the reader a clear breakdown of all of these parameters, and this suggestion by the reviewer may have been aimed at refreshing the reader's memory as to the population studied before he or she proceeds to read the experimental results. The rhetorical placement of this information, however, suggests again that in texts like this one, reviewers focus more on the information itself than how it is discursively woven into the text.

The discussion section is much like the rest of the RA in that it exhibits conventional rhetorical patterning and the points made are clearly structured, helping the reader to interpret the central

points Susana argues. If, however, the issue of “relevance” was a sticking point in the abstract, this supposed failure to contextualize the study’s findings was even more pronounced in the discussion. Indeed, the same reviewer who believed the importance of the findings to be spurious expressed two similar qualms that targeted the intertextual appropriateness of the discussion. In one of these, they wrote the following (Document E):

The references quoted by authors (23 and 24) have described higher NEFA levels in obese children. However in reference 7 and 21 lower NEFA levels in obese versus normal weight children have been reported and no differences are reported in reference 7 and 25. It becomes confusing and it is difficult to help in drawing any conclusions.

This specific stretch of discourse that the reviewer was referring to is as follows:

[M3] Although obese boys and girls in our study had higher plasma insulin levels and HOMA values than their non-obese peers, no significant differences were found in NEFA levels between them. **[M3]** BMI is positively correlated with insulin and HOMA but not with NEFA. **[M5]** Even though some studies have reported higher NEFA levels in obese children (23, 24), others have described lower NEFA levels in obese versus normal-weight children (7, 21) or no differences between them (7, 25).

This three-sentence-long fragment opens the second paragraph of the discussion section and begins with a two-sided statement of finding (Move 3), stressing the second of these two sentences. The primary argument Susana makes in this paper zeroes in on this disconnect between insulin levels and HOMA values on the one hand, and NEFA levels on the other. As highlighted in a subsequent part of this same paragraph, the known correlations between these indicators in obese adults would seem to suggest that there would indeed be a clear relationship found between these variables, although Susana’s data did not support this. The second sentence (above), containing the key string “is positively correlated,” expressed essentially the same information as the first, although from the perspective of correlation rather than levels (Move 3). When I asked the author to explain her reason for essentially writing the same idea twice, she answered that her intention was to emphasize that this finding is counter-intuitive when one takes into account the results obtained in adults. The third of these sentences compares the findings described in the first two sentences again the literature (Move 5), although rather than

choosing references that support the author's finding, she provides evidence that both coincides and contrasts with her results.

What was the rhetorical intent here? Why did Susana not present her findings more conclusively? I posed this question in more general terms during the informant interview, hoping to confirm or dispel my suspicion that rather than offer the reader solid evidence on the issue dealt with in the paper, Susana here stopped short, stressing throughout the discussion section the need to look deeper into this issue though without venturing an analysis herself. When analyzing the passage of the discussion that the reviewer was referring to, I was struck by the inconclusiveness of the discourse. Susana's response to my question brought into clear view how her individual history and perspective are partly behind this manner of interpreting data:

Susana: Because I don't know. I'm not an expert on all of this, I'm not endocrinologist [sic]. And sometimes when I'm writing something, I try to be very "polite." I'm suggesting that for the reviewer to say me [sic] yes or no or whatever or maybe this is alternative [sic] explanation. I'm chemist [sic], I'm not endocrinologist [sic]. That's my problem here. I'm not even...

Oliver: Can you talk more about that? So you're a chemist and not an endocrinologist. And because you're not an endocrinologist...

Susana: I have been studying a lot, but still I think "maybe...maybe" I'm not right, because it's something that I don't know.

Oliver: So do you find that when you publish, you often write in subtle language?

Susana: Yes.

Oliver: Because of your background?

Susana: Of course. It's different when I'm writing about genetics, because I have been working in genetics all my life, and there I feel more sure about what I'm telling, but here I have to be careful. I think I have to be careful.

After crossing over to a related but different field to work alongside her deceased collaborator, Susana is now reluctant to make firm claims about the conclusions that can be reached based on her work. Lacking confidence in her ability to convince academic gatekeepers of her views, she

chooses to soft-pedal the parts of her papers where others may stake out their place in the ongoing scholarship on the issue. Her hope is for the reviewers to push her papers toward bolder assertions, although that does not always happen; in fact, this TH demonstrates that in an attempt to avoid rejection by making statements that are overly bold, such subtle claims sometimes may lead to rejection.

As mentioned in the previous section, I contributed to this text at two points along its trajectory, though only in the first round did I work with the RA itself. During both the first and second rounds, I was asked to edit the submission letter. Although it is not infrequent for the doctors and researchers to send me these texts for editing along with the articles themselves, what is significant is that for this publication, the second version of the submission letter was changed substantially even though the manuscript itself was sent unchanged to this second journal. Indeed, in the second iteration of the submission letter, Susana adds a paragraph in which she presents the central purpose of her research (i.e., a lack of clarity between the variables studied) and also anticipates the primary conclusions, which were partially inconclusive. I found it peculiar that Susana used the submission letter so strategically, as my notion of the academic-publishing process had me convinced that if a text had been deemed unfit for publication by one journal, this reviewer feedback could provide invaluable input for a revised version. When I asked Susana why she made this decision, she replied:

Susana: The thing is the cover letter you have to make see [sic] the editor that your paper is important, and because here the more [sic] important is the relevance of our findings, I had to make clear that [sic] for the editor. Normally, the reviewers are not going to see the cover letter, but the editor is going to be the one that decided [sic] to put my paper under review or no [sic], so that's why the message was for him.

Oliver: I thought before that a cover letter was kind of a formality.

Susana: No no no. That's why sometimes I'm sending [sic] you my cover letters also. And it depends, I mean, some journals aren't going to do that, but some journals are telling [sic] you "Please explain me why the paper should be published."

This occluded genre is far from trivial for Susana, and she was keenly aware of the need to explain the relevance of her findings to the editor to avoid having her text rejected outright. Although

this particular attempt was unsuccessful, Susana uses the IIS-FJD in-house editing service to make the first impression count. As she states, some journals appear to put greater stock in these texts that are only read by the editor, and this may have led her to make this change.

Attitudinal aspects

Susana has a positive attitude toward English both in the personal and the professional realms, and is one of the few researchers who speaks and writes to me exclusively in English. Following a two-and-a-half-year fellowship in the United States as part of her postdoctoral work many years ago, she has maintained friendships and professional contacts with people in the United States and New Zealand, and these relationships likely reinforce her proficiency in both spoken and written modes of communication. In Spanish-medium journals she has published roughly four or five publications, which pales in comparison to the over fifty texts she has produced in English. This marked preponderance of English-medium texts has led her to find writing about science in her native tongue more difficult than writing in English.

Although Susana has adapted nicely to the pressures imposed by the dominance of English in her field and reports being used to publishing in the language, she feels she is at a disadvantage when competing with peers of hers who live in the inner circle, thus coinciding to a greater or lesser degree with other studies on the views of NNES researchers (D. W. Cho, 2009; Ferguson, 2007; Ferguson et al., 2011; Fernández Polo & Cal Varela, 2009; J. Flowerdew, 1999a; Huang, 2010; Martín-Martín et al., 2014; Pérez-Llantada et al., 2011; Tardy, 2004). When I asked her to assess how much of this disadvantage is attributable to her not being a native speaker of English and what part can be explained by what I referred to as “the scientific environment,” she replied that both are equally important, adding that for someone like her, having the sort of “resources and contacts” that researchers in Anglophone countries enjoy would level the playing field. When I asked if this disadvantage led to a decrease in her total publication output, she said that it did not.

Susana was somewhat surprised when I asked about her experience in publishing in Spanish, thus adding to the notion that for many of the researchers in the IIS-FJD, publishing is tantamount to

publishing in English. To my question of whether she could have published this same study in Spanish, she replied that she could have, but that doing so would have meant sacrificing impact factor, which could jeopardize her chances of continuing to secure research grants to fund her investigations. Susana did add, however, that she collaborates with the researchers working in one of the clinical departments in the hospital, and that because their findings are mainly of interest to physicians and researchers within Spain, she does publish in Spanish when working—albeit occasionally—with these colleagues. This local-international divide in Susana's research publication suggests that the findings of Lillis & Curry (2010)¹ are repeated in biomedical research.

Implications

This TH shows how Susana struggled to honor the legacy of her late colleague by persevering in her attempt to publish all worthwhile findings from their study, no matter how daunting the task. Lacking in field expertise, she articulated her ideas to the best of her ability and hoped to finish the work in the peer-review process. She reached her goal, although only partially: the RA was published, though without the expert contributions she had hoped for.

In this TH, I have focused a great deal on the reviewer reports from the first journal. It has not been my intention to give such a negative slant to this story. However, the unfortunate outcomes in the early part of the publishing process provided valuable insight on the expectations of the gatekeepers of the first journal and led my conversation with Susana into topics that, I believe, bring up some rich themes. First, Susana encountered difficulty when molding her observations according to the RA genre, especially in her use of citations. The reaction of the reviewers from the first journal suggest that claims need to be of an appropriate degree of forcefulness, respecting the culturally bound rhetorical standards found in studies of ERPP (e.g., L. Flowerdew, 2002; Lorés-Sanz, 2011b; Martín-Martín & Burgess, 2004; Moreno, 1998). This view of rhetorical flaws brings me to a second implication: unlike other circumstances in which expert researchers

¹ The authors report that local publications, which often appear in languages other than English, tend to be used for reviews of existing scholarship or applied studies, while international publications, mostly in English, are targeted for contributions seen as new research and whose primary readership is the community of academics.

may struggle to articulate their work in rhetorically appropriate fashion, Susana is very much an expert writer, though in this case her difficulty had to do with her comparative inexperience in this field. Had she been as knowledgeable about the topic as others who have spent their lives researching within the field, this RA would likely have had a different outcome. Lastly, Susana is aware that some peer reviewers act as academic literacy brokers, improving the papers they are sent to evaluate. Although Susana's paper did not get the constructive reading she hoped for, others certainly do.

Text history 4: Carolina

Overview

This RA was written by a clinical neurologist, whom I will refer to by the name of Carolina. Carolina and I have collaborated extensively since I began working at the IIS-FJD in 2009. The text studied here was met with little resistance from academic gatekeepers, who went as far as to commend Carolina on the quality of her writing. Judging from the way the paper is drafted and the feedback received from the journal, it appears that Carolina's success is a mixture of deep knowledge of the writing process, a willingness to draw from different sources to make her writing better, and a keen understanding of the dynamics inherent to the publishing process.

With some noteworthy exceptions, this RA underwent few changes in its rhetorical makeup between the first draft and the final version. Where changes did have to be made, these were minor and entirely welcome additions to the text. Some of the passages that reflect an evolution of the rhetorical makeup bear the mark of academic literacy brokers and, to a lesser extent, my own contributions. However, Carolina's ability to process external contributions effectively is one of the driving forces behind the success of this and other publications she has produced.

The informant interview for this TH was held in Spanish on September 23, 2014, and lasted nearly ninety minutes. In a number of ways, Carolina is a model NNES writer and client. As I will demonstrate in this chapter, part of my task in compiling this TH and analyzing the written documents produced for this publication was to attempt to isolate the qualities that have made her as successful an academic as she is.

Chronology and texts

As shown in the box appearing below, this TH draws from a somewhat low number of drafts because the RA was accepted by the first journal Carolina submitted it to. The first draft studied contains both the original paper I was asked to revise and my proposed edits, and as a result Document A represents both the first and second milestone versions. After Carolina had reviewed my proposed edits and modified the text accordingly, she sent two of her coauthors—

Lola and Andrés—a copy of the text for them to review. This was done simultaneously and not consecutively, and the drafts containing their output are Documents B (Andrés) and C (Lola). The third milestone version, Document D, is the RA as submitted to the journal.

The post-submission phase begins with Document G. Carolina made a number of changes indicated by the reviewers, incorporating these in the draft she sent to me for a second review. To make the editing task easier for me, she underlined all passages that had been changed since my initial edit. The suggestions Carolina accepted appear in Document H, which contains no changes on the rhetorical level. The resubmitted version is Document I, and the only difference with the former is that in Document I Carolina underlined all of the changes she had made to the paper in light of reviewer feedback. Both the submission letter and the cover letter accompanying the point-by-point response appear in this TH; in the case of the latter, I have included both the draft of the text that I revised and the final version as sent to the journal. The published version is included as Document N.

- 1. Document A: Author's first version and my edit (track changes used)**
2. Document B: Revision made by co-author (Andrés)
3. Document C: Revision made by co-author (Lola)
- 4. Document D: Version sent to the first journal**
5. Document E: Cover letter sent to the first journal
6. Document F: Reviewer comments from the first journal
7. Document G: Revised version containing changes in response to reviewer comments.
This version also contains tracked-changes versions of my second edit.
8. Document H: Revised version
9. Document I: Revised version sent for peer review, with those parts of the text
modified as per reviewer comments underlined
10. Document J: Re-submission letter edited by me (track changes)
11. Document K: Re-submission cover letter as sent to the journal
12. Document L: Uncorrected proofs
13. Document M: E-mail sent to the journal regarding the correction of proofs
- 14. Document N: final published version**

Evolution of the text and the role of literacy brokers

Unlike other THs studied here, this RA was written mostly by the participant, and the contributions made by two of the paper's coauthors, Andrés and Lola, did not suggest any refashioning of the paper and focused on the content-related aspects related to their respective fields of specialization. At the time the RA was written, Andrés was affiliated with a pharmaceutical company, while Lola was and continues to be a fellow IIS-FJD employee, and one who over the years has sent me a number of papers she herself has written. For her part, Carolina is one of the most frequent users of my service. Judging from her comments made during the informant interview and the success she has had in publishing in high-impact English-medium journals, having access to in-house editing services has allowed her to compensate for certain weaknesses in her written English and has also taught her to approach writing in English from a different perspective. Additionally, this paper also reveals active and constructive collaboration on the part of the journal reviewers, making the final product an excellent example of how language brokers and academic gatekeepers can shape texts in limited but noticeable ways.

This RA was first sent to me for editing in January of 2013, and the track-changes version reflecting my edit (Document A) contains a number of questions and comments that aimed to improve the clarity and flow of the paper. The higher than average frequency of comments is partly a reflection of the comfort with which I revise Carolina's work and also my awareness that she is quite receptive to my input, even when my remarks go beyond the text's surface. Undoubtedly, Carolina's substantial command of the language makes it much easier for me to grasp the rhetorical structure of her papers; indeed, less skilled writers sometimes send me papers that I struggle to understand in parts, thus limiting my ability to make substantial improvement unless the author is willing to do a lengthy and comprehensive rewrite.

After my initial edit, the author produced two subsequent drafts of the article before sending the text to the first journal, which accepted it for publication following minor changes requested by the reviewers. These two pre-submission drafts contain input from Andrés and Lola. When asked to assess the experience of publishing this article, Carolina told me that publishing this RA easier than others, and during the interview she mentioned a previous paper I had edited for her on a

number of occasions, as journal gatekeepers repeatedly requested revisions for various reasons. In this case, the initial feedback from the reviewers was quite positive, and the article was published after Carolina made a small number of minor changes. Of note, Reviewer 1 began the report by stating that “The manuscript is clear and well-written” (Document F). Praise about the quality of the writing from such a high-impact journal as this one speaks to Carolina’s achievements as a writer, and this is not the first time she has been commended by international English-medium journals. Indeed, during the interview she recalled a prior text that, after sending it to me for editing, was published by a highly prestigious journal; in assessing this previous work, the journal editor commented that while the journal’s policy was to not publish case studies like Carolina’s, the quality of the product was such the journal decided to break with their common practice and publish the text.

When asked whether such impressive results have motivated her to publish more frequently, Carolina replied that experiences like these have actually caused her to publish less. Quality has taken precedence over quantity, and she now aspires to top-level output that can only come from novel and high-quality contributions. Contrary to what many in her field believe, she feels publishing for publishing’s sake is not worthwhile, since lower-impact journals require a similar level of time and effort from researchers. Although in an earlier point in her career she may have felt compelled to put findings to words no matter how much they contributed to the ongoing scholarship, she now administers her time and energy better to optimize her research results. Her comments on bridging the gap between good ideas and good ideas that are publishable are compelling for my study inasmuch as they echo the constraints placed by the RA genre and researchers’ need to articulate their ideas in discursively convincing form.

Carolina reported that the ease with which the RA sailed through the peer-review process was due in part to the contributions of the reviewers, as seen in the following extract from the participant interview:

Oliver: Do you think the experience you had with the journal in terms of reviewers, revision requests, etc. is representative of your experience as an author, or is it more of an anomaly?

Carolina: No, it was a good one. It was good because for this article in particular the reviewers were very good. What I mean by “very good” is that one didn’t raise almost any doubts about it, and the other was constructive, that is, they showed me two or three gaps that my paper may have had when discussing my results so that I could comment on them. So they were good.

(...)

Oliver: This issue of good reviewers, is this representative of your experience?

Carolina: In my case, I’m a reviewer for a couple of journals, and [good reviewers] show you how to do it. They show you how, if the article is worthy of getting published, your job is to try to make it better, make it more complete, suggest some aspect that is really going to make the article improve. And, generally speaking, I have had a good experience with reviewers.

For the author, positive experiences with reviewers like the one described here either provide authors with constructive feedback that enriches the text or, at least, does not focus on the text’s shortcomings. As detailed toward the end of this fragment, her fruitful interactions with “good” reviewers have taught her how to be a good reviewer herself, and, having learned this lesson, she seeks to add value to the texts she referees. For the RA studied here, one of the reviewers suggested a different wording that more clearly expressed the methodological steps that Carolina used. Although this feedback was not directed at the rhetorical thrust of the RA, this contribution is significant in that it revealed the collaborative approach taken by this academic literacy broker.

Another comment made by the same reviewer did in fact lead to a somewhat substantial change in the rhetorical makeup of the text, though not in a way that I would have suspected before embarking on this text analysis. In their comments (Document F), the reviewer asks Carolina whether or not she had performed a certain test as part of the experimental procedure. In response to this feedback, the following version of the text (Document G) indicates that these tests were not performed. And while this addition to Document G is largely unremarkable and focused mostly on content rather than rhetorical shaping, the way this information is manifested in the text reveals a keen understanding of one of the common options available to authors of RA: the Statement of Limitation (discussion section, Move 7 according to the framework of Dudley-Evans (1994)). Although the reviewer does not make explicit reference to what Carolina

referred to as a “gap” in the participant interview, this information was incorporated in the methods, results, and discussion. In the discussion, Carolina adds a new paragraph that begins with “A limitation of this study is that (...)” Such a high-profile addition to the text would be less noteworthy if the previous drafts of the manuscript contained any mention to the limitations of the study, but they do not. This sequence of events suggests a strategy that is closely tied to Carolina’s keen knowledge of the RA genre, by which reviewer feedback can be validated and incorporated in ways that are familiar and acceptable by the discourse community. Also, we see here how changes in methodological aspects of the study have a ripple effect not only on the methods section, but also in the paper’s results and discussion.

This awareness of the options available when writing RAs has partly come about as a result of the work Carolina and I have done together. Carolina was one of the authors who contributed a text for my master’s thesis, in which I analyzed the rhetorical moves used by ten doctors and researchers in the IIS-FJD. During the informant interview for this earlier study, in which I sought to confirm my move assignments against the framework designed by Dudley-Evans (1994), Carolina showed interest in the framework I was using, and asked me to send her a copy so that she could use it when working with novice researchers in her department.

This previous exchange came up once again during the informant interview I conducted for the present study. At one point during our talk, I asked Carolina to comment on how she had revised a particular passage following a comment I had made in Document A. (In my comment, I recommended that she improve a transition for greater clarity.) When asked whether she thought the second of the two texts (Document D) was more readable than the first, she launched into the following commentary about the value of the language brokering I provide:

I often see comments you make and I say to myself, “If I could, I’d call him right now and say ‘What I mean here is this’” so you can understand where I’m going with it. But yes, [your assistance] is a big help for me and is very important. It’s not just about correcting the ‘ons,’ the ‘ats,’ and the ‘ins,’ but about the thrust of the argument. Do you remember when you gave me [the sheet with the Dudley-Evans move framework]? I keep it there as my little cheat sheet. You know, it would have been better if this had been explained to us during our classroom training. It’s true: no one teaches us how to draft a text, no one

teaches us how to write and be published. You pretty much learn by reading others's work. Also, when you're writing in another language, you often do it by pieces, drawing from similar texts in English, which means you don't go all the way through the paper in a way that flows spontaneously from your ideas. (my emphasis)

As this passage illustrates, Carolina is particularly receptive to instruction on different approaches to rhetorical patterning, and mentions the value that researchers like her would derive from explicit training on how RAs are structured at the rhetorical level. She and others like her are often never taught how to go about writing and publishing, and in the absence of formal instruction are left to emulate other published texts in English. Using the literature as a model for one's own writing has its drawbacks, however, and Carolina here stresses that drafting texts with this piecemeal approach often makes it difficult to articulate one's own ideas across an entire piece of writing. While she did not say so explicitly, it is plausible that Carolina's discursively appropriate response to the reviewer comment mentioned above is the result of her willingness to employ strategies that I have mentioned throughout our time working together.

At a number of times throughout the interview, Carolina helped me understand her approach to publishing in English-medium journals and how agents such as language brokers and academic gatekeepers influence texts. As with much of the insights I take up in this doctoral thesis, some of the most compelling information was shared as she elaborated on her responses to my questions. In this regard, the semi-structured interview format was an effective tool for launching conversations on a number of issues central to the processes, agents, and experiences at play when NNES researchers publish internationally. Two of these insights had to do with how she revises her texts after receiving referee comments and also how her writing process has evolved in the years passed since she was a postdoctoral researcher living in the United States.

Unlike some of her peers at the IIS-FJD, Carolina chose to send her text to me for editing on two separate occasions throughout its history: once before sending the paper to her co-authors in the pre-submission phase, and again after revising her work in light of the reviewers' comments. When I asked her why, she sounded almost apologetic, saying that she is careful to highlight the passages that she has changed so that I do not have to revise the entire text again. While I appreciate her consideration for my workload, this comment made it clear to me that authors

sometimes take a “bolt-on” approach when revising their peer-edited texts; in other words, when parts of their manuscripts need to be modified, they are sometimes more inclined to add or delete stretches of text than to reassess the discourse in light of new content or rhetorical refashioning. This *modus operandi* is arguably a result of the piece-by-piece process Carolina referred to in the quote appearing above, and it suggests that NNES authors may be reluctant to revisit texts at a deeper level because they believe they are unable to do so or because the nature of academic discourse in the sciences does not require them to. In any event, my approach to writing and revising is different, and I would be uneasy if I had to limit my revisions to isolated fragments, because doing so would reduce my ability to assess the text as a whole. This manner of working that I have does not go unnoticed by the author, who remarked the following:

Whenever you’ve revised something of mine once, I pretty much don’t need you to go over it again. However, if the reviewers have asked me to to clarify something or discuss an issue in general—causing me to include two or three new paragraphs throughout the text—then I do ask you to edit my paper a second time. And in that case I usually mark the passages that I’ve changed, but you strive for perfection, so you go through everything.

I hesitate to deflect this complement entirely and am grateful to see my efforts appreciated. However, I believe that the author’s surprise may be traced to the fact that the propositional content of the papers I am asked to revise is so complex that a quick once-over would increase the likelihood of mistakes. Also, I possibly take a more discourse-based approach to academic writing, in contrast to the sentence- or paragraph-level focus of some NNES academic writers.

With the exception of the input from Andrés and Lola, Carolina wrote this text mostly by herself. She told me it is common for one of the authors of a paper to do most of the writing, and that this was also her experience in the early stages of her career while doing post-doctoral work in the United States. Working under American researchers, she would send a manuscript she wrote to her superior for revision, much in the same way as she does with me today. She described her boss’s contributions as “finishing touches,” thus downplaying the importance of such revisions for the finished product. Curious to see whether she had acquired her skills as a writer during these earlier exchanges, I wrote her a follow-up email asking her to describe the type of issues

that her American superior focused on. In my question, I specifically mentioned three aspects of revision: surface-level changes such as grammar and spelling, content-related changes having to do with their scientific field, or more discourse-related changes aimed at the way scientific texts are constructed. In her response, Carolina wrote that most of the input focused on surface-level linguistic issues and field-specific matters, and that her boss seldom commented on rhetorical characteristics. This revelation, coupled with her statement that no one teaches researchers like her how to construct academic texts, adds further evidence to the notion that discoursal and generic features are given lower priority in the process of learning about academic writing and in the writing and publishing process itself.

Genre considerations

This paper was well-written from the earliest stages, and the journal reviewers recognized it as such. In addition to its clarity, its rhetorical features are a testament to Carolina's ability to argue her ideas convincingly and also to react to interactions with gatekeepers in ways that both ensured swift publication for the text and improved the RA's scientific quality. The sections that received the most rhetorical attention from the authors and reviewers were the introduction, results, and discussion. Carolina believes that these were entirely warranted, and was thankful to the reviewers for their input. In this analysis, I will attempt to show how methodological concerns raised by one of the reviewers had a significant impact on the evolution of almost all of the paper.

Carolina's abstract remained almost entirely unchanged throughout the RA's trajectory, and no part of it drew criticism from the journal gatekeepers. Indeed, the most substantial modifications were made following my first revision of the paper (Document B), and even these were minor in scope and did not target the rhetorical patterning of the abstract. In fact, when I sent her this text following my edit, I remarked in the margin of the abstract that I found it to be very well-written.

The ease with which Carolina seemed to draft and adapt the abstract of her paper can be traced in part to the standardizing influence of the structured abstract on discourse patterning and a

focus on the paper's propositional content. Of the few alterations made to the abstract in the publication process involves a very slight change to the organization of the text imposed by journal guidelines. Here, I am referring to a change made between the version sent to the first and only journal (Document D) and the published version of the text (Document O). In the two texts, the abstract opens as follows:

Document D

Background: Decreased plasma progranulin levels are a very specific marker for the diagnosis of frontotemporal dementia (FTD) caused by mutations in the progranulin gene (*PGRN*). A frequent neuroimaging pattern in this type of dementia is asymmetric cortical atrophy.

Objective: To screen for *PGRN*-linked FTD in cases with different cortical dementia phenotypes and asymmetric perisylvian atrophy.

Document O

Background and purpose: Decreased plasma progranulin levels are a very specific marker for the diagnosis of frontotemporal lobar degeneration (FTLD) caused by mutations in the progranulin gene (*GRN*). A frequent neuroimaging pattern in this type of dementia is asymmetric cortical atrophy. The aim of this study was to screen for *GRN*-linked FTLD in cases with different cortical dementia phenotypes and asymmetric perisylvian atrophy.

It appears from this subtle change that Carolina had drafted the text with a rhetorical structure that could be easily reworked according to the requirements of target journals. It follows from this that any variations (e.g., structured to unstructured, one structured format to another) are immaterial to the text because the text patterning does not vary much, if at all. This suggests that abstracts in this discipline are comparatively easy to write.

Like the rest of the text, the introduction to the RA contains many standard rhetorical moves. The three paragraphs included in this section mirror the three moves included in Swales's (2004) framework, and within paragraphs Carolina composes a propositionally dense but clearly defined argument drawing from a number of analytical approaches to the disease being studied. Effective use of lexical items "clinical" and "genetic" appear throughout the text, marking shifts between these two analytical approaches to the research question. The section opens with a treatment of what is known about the topic in the first paragraph (Move 1:

Establishing a Territory), though mention of the empirical categories used to study patients with the disease (i.e., family history, clinical presentation, and genetic causes) is limited to general observations.

Devoted to Move 2 (Establishing a Niche), the second paragraph is carefully constructed, providing more specific detail about the particular issue to be explored in the study. As Carolina explained during the interview, expressing the underpinning of the research posed a challenge, as doing so required a careful balance between the conventional wisdom of practitioners and rhetorically defensible evidence from the published literature. The struggle to bridge this gap became evident in this Move, as in this paragraph alone I made five different margin comments (Document B), substantially more than any other stretch in the rest of the RA. A look at the paragraph as sent to me for editing and the remarks I made (numbered in bold and appearing in the bottom of the box below) shows how I struggled to follow the argument she was making:

On clinical settings, some cases with significant frontotemporal features (clinical or on neuroimaging) have additional clinical, familial or genetic (APOE genotype) features that make more likely the diagnosis of other neurodegenerative diseases **(1)**. It is a fact that **(2)** a significant proportion of FTD cases caused by *PGRN* mutations have received a primary diagnosis of dementia of the Alzheimer type (DAT) or Lewy body dementia (LBD) [5,6] **(3)**. An advantage in the diagnosis of this FTD-type **(4)** is that peripheral levels of progranulin are a reliable marker indicating the presence of *PGRN* mutations [7,8] **(5)**. The pathological mechanism of *PGRN* mutations is via haploinsufficiency with a 35% loss of functional protein [9], which is reflected in low levels of serum progranulin even in individuals who are still asymptomatic.

(1) You mention 3 features in this sentence but then in the following sentence you continue to discuss—if I'm not mistaken—only one of these 3. Consider improving this transition or strengthening the connection between the 3 features and what comes next.

(2) This is strong language. I don't know the science, but consider how this statement will be received by reviewers. "It has been demonstrated that" says the same thing but doesn't sound as categorical.

(3) So you're saying that most *PGRN*-linked FTD cases are first diagnosed with DAT or LBD. What does this mean for doctors/researchers like you? Does it mean that the DAT and LBD diagnoses are inaccurate?

(4) Consider replacing this with something like "PGRN-linked FTD"

(5) This sentence is grammatically perfect, but I don't see how it adds to your argument.

One of the more noteworthy rhetorical features of this paragraph has to do with how in the first two sentences Carolina attempts to bring together her observation of the phenomenon based on years of clinical practice and the relevant findings that support the plausibility of her belief. As shown in comments **(1)**, **(3)**, **(4)**, and **(5)**, my failure to grasp the thrust of the first sentence led me to believe that Carolina's argument was something other than a statement on the apparent relation between, on the one side, patients diagnosed with frontotemporal dementia who also present particular features, and, on the other, the existence of other pathologies in these same patients. In spite of this misunderstanding, comment **(2)** illustrates how I was struck by how unmitigated the statement was, and it appears that this lack of modality indicates linguistic transfer from Spanish.

As she often does, Carolina paid close attention to my remarks, and reworked this paragraph. The version sent to the journal (Document D) runs as follows:

In clinical settings, some cases show mixed clinical or neuroimaging features of frontotemporal degeneration and other neurodegenerative diseases. A significant proportion of FTD diagnoses in cases caused by *PGRN* mutations are preceded by a primary diagnosis of dementia of the Alzheimer type (DAT) or Lewy body dementia (LB) [5,6]. Peripheral levels of progranulin are a reliable marker indicating the presence of *PGRN* mutations and can be used to screen for *PGRN*-linked FTD [7,8]. The physiopathological mechanism of *PGRN* mutations is via haploinsufficiency with a 35% loss of functional protein [9], which is reflected in low levels of serum progranulin even in individuals who are still asymptomatic.

Having noted my confusion, here the author eliminates claims of probability of additional diagnoses in the first sentence, opting for a more conservative approach that simply states that these patients sometimes have features that suggest other diseases. In the second sentence, she modulates her assertion, which may have made the reviewers less likely to challenge the statement. In the third sentence, Carolina links the observation concerning peripheral levels of progranulin to its application, apparently in response to my remark on the relevance of this empirical observation.

Appearing in the third paragraph of this section, Move 3 receives about half as much textual space as the previous two moves. In Document A—the version sent to me for editing—Carolina

closes the introduction section with a one-paragraph sentence in which she appears to have a clear rhetorical intent to present the work (Move 3), although in my first edit (Document A) I wrote that the paragraph was hard to follow. In light of these recommendations and the reviewer comments proposing more precise terminology to describe her experiment, this move was lengthened at both at the head and the tail. The modifications made can be seen in the following fragment appearing in three different milestone versions of the text's trajectory: Document A (first draft as sent to me), Document D (draft sent to the journal following my revision), and Document N (final published version):

Document A

[M3, S1] Since asymmetric cortical atrophy is a common feature in FTD caused by mutations in the *PGRN* gene [5], in this study we have screened for *PGRN* mutations in our cohort of cases with well defined [sic] FT phenotypes but [sic] also in a group of cases with asymmetric perisylvian atrophy diagnosed with probable/posible [sic] clinical with dementia of the Alzheimer's type or dementia with Lewy bodies.

Document D

[M3, S1] In this study we have screened for *PGRN* mutations in our cohort of cases with well-defined FTD phenotypes. **[M3, S1]** However, since asymmetric cortical atrophy is a common feature in FTD caused by mutations in the *PGRN* gene [5], we have also studied a group of cases with asymmetric perisylvian atrophy diagnosed with clinical criteria with dementia of the Alzheimer's type or dementia with Lewy bodies **[M3, S2]** with the hypothesis that within these groups there could actually be FTD cases associated with *PGRN* mutations, especially when they had a family history of dementia.

Document N

[M3, S1] In this study *GRN* mutations were sought in our cohort of cases with well-defined FTLD phenotypes. **[M3, S1]** However, as asymmetric cortical atrophy is a common feature in FTLD caused by mutations in the *GRN* gene [12], a group of cases with asymmetric perisylvian atrophy diagnosed with clinical criteria with DAT or LBD was also studied, **[M3, S2]** with the hypothesis that within these groups there could actually be FTLD cases associated with *GRN* mutations, especially when there was a family history of dementia. **[M3, S4]** Our screening for *GRN* mutations was conducted using a two-step approach: first, plasma progranulin levels in all cases and controls were measured; and secondly, the *GRN* gene was sequenced in a subgroup of cases and controls with plasma levels below a certain threshold.

In addition to the issue addressed in the previous section surrounding the impact of my request for clarification, it can be seen in Document D how this revision process led the author to not only break down her ideas into shorter sections, but also to go even further, including explicit mention of the study's hypothesis for the first time. In other words, Carolina reappraised the concepts communicated in the section, choosing to make explicit an idea that had previously been insinuated without appearing in unequivocal language. What is compelling for the purposes of my study is how, without any mention whatsoever of Move 3, Step 2 (Presenting Research Questions or Hypothesis), the author reformulates her argument in a manner that exhibits such keen knowledge of genre as described by Swales.

Having noted the particularly high level of attention that this fragment received between different versions of the RA, I inquired about this passage during the participant interview. Though the intention behind my question was aimed more at this new mention of the study's hypothesis, Carolina remarked that she read my comment about the section being difficult to follow, she decided to break her ideas down into smaller, more intelligible parts. In her view, although the study hypothesis was implicit in previous drafts of the manuscript, the revised version made specific mention of this fact, and as a result the clarity of the writing improved. The move analysis together with the text-based ethnographic approach used reveal that this information on the methods used in the study was a late addition to the manuscript, and that the author benefited from the constructive criticism of the reviewer in rounding out the rhetorical shape of the text. Though all versions of the text—prior to and following intervention by literacy brokers—make alternating reference to the two analytical perspectives throughout the introduction, explicit mention of the two-step approach was included based on the reviewer comments. This elegantly phrased final sentence featuring such explicit language as “Our screening ... was conducted” and the aptly worded “two-step approach” followed by a concise description of the two steps comprising the approach represent a clear example of a rhetorically focused change made to a text written by an NNES author.

Turning now to the methods section, called “Patients and Methods” in this text, matters of the procedures used to analyze data (Move 6) are reserved for the results section, and therefore the

entire Patients and Methods section is composed entirely of Moves 4 and 5. Briefly, the research draws from a large group of dementia patients recruited from both clinical practice at the Fundación Jiménez Díaz and another facility in Spain treating this disease, and one of the primary points of interest in the patient sample is the detail with which the patients had been studied prior to the study reported in this RA. The information presented within this move includes mention of the standards used for diagnosis, the diagnostic techniques used, and the ways in which the patients had been classified based on these parameters. Although the second reviewer raised one issue that led to additions to this section, it concerned offering additional data and did not affect the rhetorical patterning of the section.

The transition from Move 4 to a description of what experimental steps the author and her collaborators took (Move 5b, Recounting the Experimental Process) coincides with a marked increase in the frequency of passive verb forms such as “were taken,” “were kept,” “were analyzed,” etc. While Nwogu (1997), in his framework of rhetorical moves for this section, indicates that both Move 4 (Describing Data-Collection Procedure) and Move 5 (Describing Experimental Procedures) tend to be realized through passive verb forms, the relative absence of the passive voice in Move 4 appears to be derived from the nature of this study, as the patients included in the genetic component of the analysis were assessed because of their pre-existing clinical characteristics and diagnosis and not because of any intervention by Carolina per se. As with Move 4, the second reviewer for the journal raised a question as to methodological steps that Carolina could have taken, writing, “Have the authors tested whether a deletion of GRN was present in the other patients with low GRN level? Please discuss.” In response to this, the revised version of the paper provides a more detailed justification of the criteria they used when determining which patients would be subjected to genomic sequencing, providing bibliographic references that supported this methodological decision.

The results section underwent the most substantial change in the entire RA, especially under the influence of the more active of the two journal gatekeepers (Reviewer 2). The section begins with a one-paragraph general presentation of the values observed in the plasma from patients’ blood, and then the rest of the section presents the results of a number of analyses and observations

deriving from the results of these plasma values. The first part of this more detailed analysis was published with only a slight addition at the end of the paragraph, and its rhetorical moves are as follows:

M0.3 (Procedural), reporting a methodological step performed on a subset of samples with particular characteristics revealed by the procedure performed on all the samples included;

M1.1 (Relation between variables), offering an analysis of the phenotypes of the samples in the aforementioned subset;

M1.1 (Relation between variables), analyzing a more reduced subset determined by the Move 0.3 above, indicating gene mutations found in this subset;

M3.1(c) (Comparison of findings with the literature (different)), in which Carolina remarks that one of the gene mutations was reported in her study for the first time;

M0.1 (Pointer), pointing the reader to a table depicting the clinical characteristics of the patients found to have gene mutations;

M1.1c (Relation between variables), adding additional information on the patients described in the aforementioned table.

The two most salient changes in this section were brought about by comments made by Reviewer 2. Below I provide the excerpt of the passage as originally submitted to the journal (Document D) and the final version reflecting this change (Document N). For the sake of clarity, added text is underlined.

Document D

[M1.1A] Thirteen cases with levels between 73 and 85 ng/ml showed no pathogenic changes in the *PGRN* gene. **[M1.1A]** None of the 55 cases with DAT (...)

Document N

[M1.1A] Thirteen cases with levels between 73 and 85 ng/ml showed no pathogenic changes in the *GRN* gene. **[M2.1A]** Many of these cases revealed single nucleotide polymorphisms in heterozygosis, ruling out the possibility of a complete GRN deletion. **[M0.3]** However, we did not pursue the finding of large exonic deletions **[M3.1A]** because the possibility was very small [19-21]. **[M2.1A]** As we did not find pathogenic changes in any of the cases within this range of plasma progranulin levels (73-85 ng/ml), **[M0.3]** we did not sequence the gene in cases with higher levels.

[M1.1A] None of the 55 cases with DAT (...)

The particular comment that provoked this change is the same one referred to in my analysis of the methods section above (“Have the authors tested whether a deletion of GRN was present in the other patients with low GRN level? Please discuss.”) In this particular section, the reviewer appears to urge Carolina to fill out her justification of the methods used by raising the possibility of a finding that may have been missed by the experiment. In Document N, Carolina substantiates her finding (Move 2.1(a), Other finding) by explaining that the presence of single nucleotide polymorphisms in heterozygosis made complete GRN deletion impossible. She then goes on to justify the fact that no further search for exonic deletions was carried out because previous studies, referencing existing publications to lend validity to this aspect of the study. As she explained during the interview, Carolina reacted favorably to this constructive input from the academic literacy broker, and believed that they increased the quality of her work. Also of interest here is the way in which a single reviewer comment can trigger modifications in more than one section, even when the reviewer does not specify where alterations should be made.

The discussion section of this RA is structured in a way that spells out the primary conclusions of the research in an introductory paragraph, amounting to what Carolina referred to as the “take-home message.” In the section that follows, Carolina takes up both of the two findings appearing in this introductory paragraph and then goes on to develop both this and other points in the rest of the section.

The findings of the study are presented in a way that clearly situates them in reference to the study's hypothesis. The third paragraph of the discussion both reiterates the hypothesis and explains that this hypothesis was disproven by the results of their research. This appears in the following excerpt from Document N, in which I highlight the discursial features that signal the rhetorical patterning used:

[M1] Asymmetric cortical atrophy is present in about 76% of FTLD cases with *GRN* mutations [12,22] and [M1] it was hypothesized that amongst our cases that clinically resembled DAT or LBD but with asymmetric perisylvia atrophy, there would actually be cases associated with *GRN* mutations. [M3] However, we only found *GRN* mutations in patients with welldefined FTLD phenotypes: two with the behavioral variant, one with a progressive supranuclear palsy phenotype, and one with primary progressive aphasia who progressed to a full FTLD syndrome including right corticobasal features.

In the text that follows this fragment disproving the hypothesis (Move 3, Finding), the result is then contextualized with three instances of Move 5 (Reference to previous research) that produced a similar result, thus making Carolina's findings less surprising. Interestingly, Carolina included additional instances of Move 5 in response to reviewer comments that suggested that she include more references to existing literature along these lines.

As explored in the section on the influence of literacy brokers on the RA, Move 8 (Limitation) is a late addition to this TH, first appearing in Document G and in response to certain criticism made by Reviewer 2. Previous versions of the text contain passing references to shortcomings of the study such as the wide range of values obtained, although more direct mention was made after the RA went through peer review. Phrased with the explicit lexeme "limitation," Carolina acknowledges this gap in her analysis, although quickly defends her position using assertions backed up by references to existing studies (Move 5), arguing that this fault in the study is unlikely to have significantly influenced the findings. Again, the patterning of this part of the paper and its deployment during the peer-review process are indicative Carolina's skills as a writer of scientific texts for the Academy.

Attitudinal aspects

The skill with which this author navigates the process of publishing in competitive English-medium journals contrasts somewhat with her assessment of herself as a user of the language, both in general and as a user of the language for research purposes. At the beginning of our interview, when I asked whether she would like to have our talk in Spanish or in English, she did not hesitate to say that she preferred to speak in Spanish. The reason she gave for her decision was the great disparity between her ability to read and write in English on the one hand, and her comfort when listening and speaking. Now that a number of years have passed between her two postdoctoral fellowships in the United States, her spoken English has declined considerably, and she struggles to find ways to maintain her abilities while living in a Spanish-speaking environment.

In Carolina's opinion, she is at a clear disadvantage with respect to her colleagues living in English-speaking countries, and not only because she is a NNES researcher. Experience has taught her that when neurologists like her set out to publish internationally, their physical location has a substantial impact on how their research is received. Truly high-quality studies come out of the kind of robust funding and researcher networks that only exist in the United States. Indeed, these Anglophone-center researchers who receive massive grants from institutions like the National Institutes of Health are compelled to publish in highly prestigious journals that are run by the same cadre of researchers who also benefit from these grants. Thus, fast-tracking studies through high-impact publications is a matter of course, even when their quality lags. According to Carolina's account, location is just one factor conferring access to the topmost echelon of academia, and true privilege is also a question of who is in your circle.

As I listened to Carolina explain the global politics behind academic publishing within her field, I wondered whether she believed that the success enjoyed by scientists living and working in Anglophone contexts was a true reflection of quality. Surely, I thought, if such a bounty of grants, research, and publications was perpetuated by the stakeholders who benefit from the system, these researchers must be unfairly favored by the system. To my surprise, however, she does not

believe this to be the case, and told me that the research ecosystem that favors Anglophone investigators produces work that is objectively superior than the rest.

Carolina herself has reaped the benefits of contact with American scientists thanks to more than three years working in American research centers. Although she still believes herself to be on the outside of the circuit of robust research projects and publications, she feels that her time spent in the United States has helped her secure a position on the periphery of the inner circle, and that she has been able to build on this position thanks to publications like the one studied here. During the interview she mentioned two different invitations she has received from research centers in English-speaking countries; these opportunities came about after she published in prestigious journals, which will likely lead to future collaborations and publications. In her view, none of these opportunities would have come about if she had opted to publish in Spanish-medium journals, as doing so would have drastically limited the reach of the texts.

In spite of the rewards she has earned because of her research output, research comes at a substantial cost in time and effort. Carolina receives no rewards of any kind for her research, and her position at the hospital is heavily focused on clinical activity. When I asked her why she devotes so much to publishing research when doing so does not increase her standing at the hospital or carry a monetary reward, she replied researching helps break the monotony of clinical work, and that if she had to do without research, her job would be much less stimulating.

Implications

The success that Carolina had with this publication is partly due to the positive influence of the journal reviewers, particularly one such reviewer. The feedback given by this gatekeeper alternated between stimulating inquiry and outright collaboration. Gaps in the text were flagged for development, and more basic improvements such as changes to field-specific terminology were suggested. Having undergone such constructive peer review, Carolina has been trained on how to utilize her own knowledge when on the other end of the author-gatekeeper relationship. It remains to be seen whether having such constructive reviewers is a matter of luck or,

alternatively, if Carolina has somehow earned a certain stature among editors and referees, who treat her papers with a similar degree of respect and professionalism.

Carolina writes with skill and meticulousness, and the care with which she goes about writing and talking about writing suggest that she has worked hard to develop a process that is effective and allows her to surmount some of the obstacles imposed by her deficiencies. I would argue that within the system she employs, an openness to views from others is an unmistakable asset. Input from the reviewers was welcomed, and suggestions I made were evaluated and, in some cases, included in her work, even when the scope of what I was suggesting involved more time and reflection.

Especially in the introduction, we have seen how Carolina was able to transfer observations to an RA. Though not without certain difficulty, she subtly introduced notions developed in the clinic by tying them to published texts. This and other passages like it were recognized as generically appropriate methods of asking questions and arriving at answers.

Text history 5: Federico

Overview

This RA was coauthored between three staff members of the IIS-FJD: two from the orthodontics department and another from the dermatology department. Of these, the dermatologist, who will go by the name of Paco, and one of the orthodontists, whom I will refer to as Clara, are senior doctors who completed their medical training and doctoral theses years ago. The main focus of this chapter, however, is the third researcher, Federico. Federico is much younger, and completed his doctorate in 2012 under the supervision of Paco and Clara.

Publishing this work in an English-medium international journal proved to be a trying experience, as four journals declined the article before it was finally accepted for publication by a fifth. Some of the flaws that the academic gatekeepers found with the RA had to do with rhetorical abnormalities, though these were not decisive factors. Federico was eventually rewarded for his perseverance, and did not have to overhaul the study substantially to publish the text; as a result, the first draft is strikingly similar to the published version.

In studying the materials for this TH, I detected certain unconventional move placements, with some Moves employed outside of the sections where they are normally found. These inconsistencies between the rhetorical patterning of RAs described in the Applied Linguistics literature and the RA studied here first became apparent as I was coding the paper, and then Federico confirmed my initial impression during the informant interview. As I had suspected, some remarks made by the journal reviewers found the rhetorical content to be unorthodox, although certain views expressed by these gatekeepers also seemed to challenge commonly accepted knowledge of the RA genre among EAP researchers.

Although Federico assured me that faulty English is rarely grounds for rejection, language quality was criticized by more than one of the reviewers. This negative feedback is especially relevant to the history of this text since I worked on the paper once when it was still an early draft and then, as the text neared final publication, both I and a paid outside editing service revised the English.

As I will describe below, this RA is a regenred part of Federico's doctoral thesis in which the author used professional translation, his own writing and revising, and professional editing.

The informant interview for this TH was held on September 26, 2014. My conversation with Federico, which lasted seventy-three minutes and was held in English, was focused in large part on the process the authors followed. Due to the more transparent nature of the paper's propositional content, the move assignments were less challenging for me, and therefore I did not need to spend much time confirming the communicative purpose in many passages. As a result, much of our discussion centered around why Federico had written the paper like he had. Federico provided me with a wealth of information on his experiences in and perspectives on academic writing for Anglophone audiences, and in the course of our conversation we touched on such topics as the importance of local and transnational relationships, the role played by occluded genres (Swales, 1996) such as the submission letter and the point-by-point response to reviewer feedback, as well as possibly conflicting notions of implicitness and explicitness between NNEs authors and language brokers like myself.

Chronology and texts

The texts that form part of this TH are listed below, with the milestone versions appearing in bold. Unlike other RAs studied in this thesis, I was given a text partially in Spanish and partially in English, and as a result my work was a hybrid between editing their writing in English and translating from Spanish into English.

I was able to access all of the four rejection letters and also the reviewer report from the fifth and final journal. Of particular interest to my study was Document L, containing a) feedback from both reviewers; b) a draft response by Federico and Clara to questions that posed little difficulty for them; and c) written correspondence between Federico and Clara and their dermatologist coauthor, Paco, in which the orthodontists ask Paco for his input on how to best respond to the thornier questions and concerns raised one of the aforementioned reviewers. As for the drafts themselves, the rhetorical makeup of the text underwent little change between the initial drafts and the final version. Despite this, one finding of this TH is that the results section contained a

slightly higher concentration of reworked passages. As a result, I also analyzed the results section of the non-milestone versions in order to best understand the importance of this section and the revision strategies the three authors used.

- 1. Document A: Text to be translated and edited**
- 2. Document B: Version of the text containing my translation and editing**
- 3. Document C: Completed version as sent to the first journal (dermatology)**
4. Document D: Rejection letter from the first journal, with referee comments
5. Document E: Version sent to the second journal (orthodontics). This version reflects the changes made after the text was sent to an external, for-profit editing service to adapt the article to the standards of British English
6. Document F: Rejection letter from the second journal (no referee comments)
7. Document G: Draft of the text following a second revision by me in order to change the spelling back into American English
8. Document H: Rejection letter from the third journal (orthodontics). Rejected by the editor, with no referee comments
9. Document I: Version sent to the fourth journal (orthodontics)
10. Document J: Rejection letter from fourth journal, including referee comments
11. Document K: Version sent to the fifth journal (dermatology)
12. Document L: Revise and resubmit request from fifth journal, including referee comments
- 13. Document M: Final published version**

Evolution of the text and the role of literacy brokers

Although Federico is the first author, all three coauthors participated actively in the writing of the different drafts. Compared to Paco, the allergist, publishing internationally is more of a challenge for Federico and Clara due to their comparative lack of international networks. Most of their publications are in Spanish, and many Spanish-medium publications accept their manuscripts with little negative feedback, suggesting that their specialty has a more local focus. Concerning this particular article, Federico explained that because the study was interdisciplinary—measuring the ability of residents and practitioners from different medical disciplines to correctly diagnose disorders involving the oral mucosa and perioral skin—it was difficult to convince journals of one specialty or the other of the paper's merits. After first attempting to publish the RA in a dermatology journal, the authors then tried unsuccessfully to publish it in three different orthodontics journals, and, after this change of disciplinary target failed, they were finally able to achieve their goal in another dermatology journal.

The research reported in the RA was originally included in Federico's doctoral thesis, which extends 154 pages including front matter and bibliography. He wrote the RA to broadcast the main findings of his thesis. I compared the thesis and the original text sent to me for editing and translation in search of similarities. Some of the findings of this comparison are included as data in this TH. Due to limits of length, however, an exhaustive comparison of these two texts is beyond the scope of my study.

The story of how Paco began collaborating with Federico and Clara, the orthodontists, goes back to a fortuitous encounter at the hospital. Federico, a junior orthodontist at the time the partnership was formed, told me that one day Paco visited the orthodontics department as a patient, and their conversation turned to matters of work and academics. In talking to Paco, Federico discovered that, like him, Paco had trained in the United States, and since then maintained a network of professional contacts among American dermatologists. On another occasion, Federico mentioned to Paco that he would like to earn a doctoral degree, as doing so would improve his status among his peers. Paco urged Federico to go forward with his plan, pledging to act as his co-supervisor alongside Clara. The three then worked together over a number of years, and the RA studied here is the culminating achievement of this alliance. When drafting and revising the text, they met over ten times. Some were face-to-face encounters, while others were held by videoconference, most often when Paco was away attending academic events. During their meetings, the three authors worked directly on the manuscript rather than discussing changes for subsequent revision offline.

Acting as supervisors, Clara and Paco guided Federico as he worked toward completion of his thesis. However, making a publishable English-language article out of the thesis turned out to be a sizable task. To achieve this aim, Federico received invaluable help from Clara and, especially, Paco. As Federico explained, Paco brought name recognition to the RA and also played a substantial part in adapting the paper's "style." Paco's first intervention in the regenerated text came after the combined translation and revision I did of the text adapted from the thesis (Document B) in which I was told to leave field-specific terminology in Spanish and limit my translation and editing to the rest of the text. Paco revised this version, and the result of this

revision was the third milestone version (Document C). Federico extolled Paco's prowess in the following way:

This author, he developed [sic] his career in the States, his English is pretty good, especially writing, so maybe he could [sic] modify some of the sentences in order maybe to make it maybe more I don't know how to explain it, maybe like with best [sic] marketing, you know, like in a nicer way for the reviewers. He has published a lot, he has huge [sic] experience in publishing, and maybe he just wanted to make it to [sic] have like much more punch, for the readers to find it more interesting, so that's why maybe he could change some of these sentences.

Describing the influence that Paco had on the text, Federico sees these modifications as adding to its appeal (i.e., "marketing," "nicer," "punch," "interesting"). This description is relevant inasmuch as it transmits the essence of genre-appropriate writing, albeit in different terms.

What is clear from both the drafts and the interview is that the keen knowledge of Paco, an NNES disciplinary expert, is valued over the contributions I made. While Paco cannot be considered an academic literacy broker within this text because he is a coauthor, he is an expert producer of English-language academic genres like this one. This appears to indicate that, at least in some fields of biomedical scholarship, the skill set offered by NES editors is of lesser value than the expertise of widely published authors; conceivably, either these NNES field experts compensate for gaps in their linguistic proficiency with an acute sense of what academic gatekeepers are looking for or, alternatively, what is valued in academic publishing is less about linguistic correctness and more about effective discourse. In this case, my contribution likely sped up the process of regenerating the Spanish-language thesis, providing the authors with a draft that was mostly in English for them to continue their revision work.

Another strength that Paco brought to the collaborative effort became evident as the RA was just a stone's throw away from publication. As outlined in the list of texts appearing in the second section of the present chapter, Document L contains the request for revision from the fifth and final journal, including comments from two reviewers. One of the two recommends a wholesale revamping of the study, including sweeping changes to its conceptualization and the way it was presented, writing, "In my opinion the study has to be reapraised [sic] and at authors [sic]

discretion [sic], resubmitted [sic] with the adequate changes to material and methods and its evaluation.” However, Clara and Federico were undeterred, and this same document contains a draft response written by the two in which they seem convinced that they were near the finish line. The two orthodontists were unsure of how to manage the reviewer’s comments, and for help with this occluded genre they deferred to Paco. Clara writes, “Let’s see how we can woo this guy” (*A ver como [sic] le entramos a este tipo*). This phrasing is intriguing on two accounts. First, although in a clearly ironic tone, Clara ascribes this blinded reviewer a male gender and suggests that her colleagues and she must “woo” or “court” the reviewer with their reply, hoping that he or she will be amenable to her discursal “advances.” Second, as will be demonstrated in the following section, the authors did not heed this advice, at least not in the way proposed. In this negotiation stage of the peer-review process, the reviewer’s opinion was far from written in stone, and Clara’s remarks for her fellow coauthors intimate that a seductively worded response that acknowledges the reviewer’s feedback may be sufficient, thus eliminating the need to make the changes requested.

In an apparent attempt to leverage Paco’s international recognition as a dermatologist, the RA was first submitted to an international journal in his field. Although the text was sent for peer review, it was rejected for a number of reasons. Below I will excerpt those comments that most clearly point to language and rhetoric (Document D). (For the sake of economy, I will not mark the fragments omitted from my transcription and represented with ellipses; therefore, these extracts are not to be taken to represent a cohesive whole, but rather as fragments taken from their respective reports).

Referee: 1

Abstract:

Very short and incomplete (1)

Methods:

The analysis description is not clear (2) and the sample-size does not seem to link with the plan for analysis.

Results:

- I'm really confused about the results, there is no discussion about where the questionnaire originated (3).

No, there is no limitations section in this manuscript but there are numerous limitations (4).

Referee: 2

Comments to the Author

This manuscript needs to be extensive [sic] revised, mainly in English [sic] language (5).

1. Title: Should be revised

3. Abstract: (...) Results: not satisfactory, the authors should describe the results (6).

3. [sic] Introduction: Check english [sic] (7). Some parts doesn't [sic] have references and author's opinion should only be stated at [sic] discussion section (8).

4. Results: not satisfactory. The authors almost described the results as legends of the tables (9).

6. Discussion: Should be revised and attention to typing mistakes (10).

These two reviewers find fault with a variety of issues in nearly all sections of the article, including the paper's title. They cite missing or misplaced rhetorical moves (1, 3, 4, 6, 8, 9), lack of clarity (2), insufficient reporting of methodological aspects (3), and deficient English or presence of errata (5, 7, 10). Surprisingly, some of the criticism on poor English is worded in poor English, suggesting that these reviewers are themselves NNEs who appear to hold the authors to higher standards than they apply to themselves, at least for these short texts.

Following this disappointment, the text was sent to three consecutive journals within the field of orthodontics and dentistry and was rejected by all three. Both the second and third declined to send the manuscript for peer review. The reason given by the second journal (Document F) was that “It was felt that the study offered only a small advancement in our knowledge over the existing literature,” while the third journal’s rejection letter (Document H) justified their refusal on the grounds that the study was not in the range of topics generally included in the journal and that the authors should consider a journal that was “more aligned with your manuscript- perhaps an orthodontic or oral pathology journal.” Of note, between submission to the first and second journal the authors opted to send the paper to a commercial editing service that belongs to one of the world’s largest publishers of academic journals so that the paper could be revised according to the standards of British English. The resulting text is Document E.

The fourth submission involved another orthodontics journal. Though on this occasion the paper was sent for peer review, the authors were disappointed once again. The rejection letter (Document J) opens with the following remarks to the authors, casting doubt on the appropriateness of the text for an international journal, writing: “In addition to the comments made by the reviewer, it is possible that the results are applicable only to the local area in which the study was conducted. I suggest the authors consider submitting the paper for publication in a journal with a more local focus.” This remark, which echoes the experiences of some NNES authors studied by Lillis & Curry (2010), raises the question of what constitutes international knowledge, and whether a similar study would have had a different publication outcome if it had been performed in the Anglophone center. The single reviewer who evaluated the paper wrote the following (text in bold marked by me to indicate potentially relevant issues):

Reviewer #1 Evaluations:

Recommendation: Reject

Is Question Clearly Identified: Yes

Methods and Research Design: Inadequate

Statistics: Adequate

Figures and Tables: Appropriate as is

Results: New, Original

Conclusions: Based on Results

References: Appropriate

Style: Needs Revision

Interest level: Academics only

Appropriate: No

Overall Scientific Priority: 2

Article tends to state the obvious: Curricular modifications need to be made for improvement in all subgroups. Medical curricula are renown [sic] for how little time is spent with oral pathology.

“Disuse atrophy” applies to memory as well as muscle. The orthodontists could be compared to the general dentists also which is the referral base for the orthodontist.

Article is only as meaningful as the quality of the 2 dimensional photos but they were not shown.

Age specific pathology (teenage and young adult) might have been more meaningful.

Numerous typographical errors noted particularly in the tables.

Of the three issues highlighted, “Methods and research design” appears to index the overall conception of the study and the way in which the authors went about answering their research questions. The question of how this perceived inadequacy could have been remedied leads me to wonder, however, whether the study could have been rhetorically articulated in a more acceptable fashion. For his part, Federico doubts that better writing on the rhetorical level could have helped him at this point in the publication process: when asked whether the way the paper had been written, edited, or translated had caused his paper to be met with such resistance, he assured me that this had not been the case, adding “It’s not the English that failed. It’s the product or ideas or design.” Apart from this matter of study design and the degree to which research needs to be accompanied by effective rhetorical realizations, the category of “Style” appears somewhat vague and may lead NNES authors to wonder what exact improvements are needed. It is also vexing to see how stylistic and “typographical errors” were mentioned by this reviewer despite the fact that the text had been revised both by me and, though at an earlier point in the text’s trajectory, by a paid editing service.

The fifth journal—a dermatology journal this time—agreed to publish Federico’s paper after one round of peer review. As referred to above, Document L contains the reports of two peer reviewers, a draft point-by-point response, and comments made by Federico and Clara in which they ask Paco to weigh in on the most appropriate way to respond to this gatekeeper feedback. Unfortunately, the authors had only saved this draft version of the point-by-point response, although the notes contained in this draft illustrate the contributions of the three coauthors during the post-submission process. The two orthodontists called on Paco to provide expert feedback when Reviewer 1 questioned the description of how the randomized sample had been obtained; when Reviewer 1 suggested that the authors hedge their knowledge claim by being more “circumspect,” as this reviewer believed that the methods used in the study did not warrant as broad a knowledge claim as the one being made in the paper; and also in response to Reviewer 2’s criticism.

In contrast to the more specific issues raised by Reviewer 1, Reviewer 2 refutes some the arguments made in the paper, suggesting that Federico and his coauthors revise the study in depth. The reviewer report begins with “The study performed is intersting [sic], however there are a few problems in its methods that must be tackled before manuscripts’ [sic] resubmission” and closes with “In my opiniorn [sic] the study has to be reapraised [sic] and at authors [sic] descrition, [sic] ressubmitted [sic] with the adequate [sic] changes to material and methods and its evaluation.” In these remarks, the reviewer clearly points to the Materials and methods section, suggesting that the authors revisit the way they had conceived of their study, although not offering any guarantee that these efforts would be rewarded.

As will be explored with greater detail in the following section, the manuscript changed little from the early drafts until publication, even despite the numerous faults found in the text by journal reviewers. One of the changes that was made was initiated by the following comment from a reviewer for the fifth and final journal, about the results section: “As a matter of style, it is not necessary to use a whole sentence to let the reader know about a table. The result can be stated, and ‘(Table N)’ placed at the end of the sentence.” Although I will defer discussion of this rhetorically oriented comment until the following section, what is of interest here is the fact that,

when resubmitting the text to the journal following these comments, Federico and his colleagues made a somewhat cosmetic change to the fragments referred to by the reviewer but did not do all that had been suggested.

During the participant interview, Federico said that he sometimes doubts the validity of referee comments, and a connection may be made between this opinion and the way the authors interpreted the aforementioned criticism. In his opinion, some of the reviewer feedback on this RA was unjustified and may have had more to do with the reviewers' perceived responsibility to critique some aspect of the paper, no matter how dubious the criticism might be. To avoid such negative feedback, Federico tries to "go one step ahead of the reviewer" by anticipating aspects of texts that gatekeepers may find questionable. However, as we see in this TH, these efforts are not always immediately rewarded.

Genre considerations

As I will detail in this section, the RA's rhetorical structure was somewhat unorthodox, and the reviewers expressed reservations as a result. This patterning can be partially traced to the fact that Federico regressed his doctoral thesis to produce this RA and that the thesis was written in Spanish; as a result, sections such as the introduction and discussion have unconventional rhetorical features. In spite of the four rejections he received, Federico made very few changes to the paper's discourse structure, and where changes were made, they appear to be of a lesser degree than that requested by the journal gatekeepers. The sections that underwent the most substantial revision were the abstract and results, although even these alterations were limited. I observed substantial fluidity between different sections of the IMRD structure regarding which types of information belong in which section; this became evident not only in the move-step analyses, but also in my conversation with Federico about writing RAs as well as the feedback provided by journal gatekeepers.

In spite of the peculiar way the text was constructed, Federico's comments about his paper and about quality in English-medium academic publications suggested that he devotes considerable thought to his writing. Throughout our interview, he described my editing service as oriented

toward “style.” For him, English-language papers that have proper style are “reader-friendly,” or easily understandable, and display proper balance between findings and the way findings are presented, contextualized, and analyzed. In his words,

Style is everything in publishing (...) if you just keep writing, giving numbers, stadistics [sic], everything is like so dense that you need to open the window and breathe (...) we believe it’s not going to be published anyway (...) the word is “reader-friendly”.

Federico told me that some RAs are rejected because of stylistic deficiencies, and as a result he puts substantial effort into the way his ideas are presented, attempting to avoid repetitious texts that do little more than present a series of numeric results to the reader.

The primary modifications made to the abstract coincided with changes in journal specialty, that is, the two abstracts submitted to dermatology journals (Documents C and K) were nearly identical, while the two versions of the text that contained an abstract and were sent to publications in dentistry or orthodontics (Documents G and I; Document E contained no abstract) were virtually the same. The difference between these two approaches to the abstract becomes evident if we compare the text sent to the first journal (Document C, dermatology) with the first abstract targeting a publication in the field of orthodontics (Document G), especially as concerns Move 1 (introducing purpose).

Document C

Background: Oral pathology is an interdisciplinary field of unclear boundaries. Only a few studies have been previously published comparing the diagnostic capacity among different specialists taking care of the disorders involving oral mucosa and perioral skin.

Objective: To assess the capacity for diagnosing dermatologic conditions involving the oral mucosa and perioral skin in children and adults among orthodontists, dermatologists and primary-care physicians, the later [sic] including pediatricians and family doctors.

Document G

SUMMARY: The purpose of this study is to compare the capacity of orthodontists, dermatologists, pediatricians and family doctors to diagnose conditions involving the oral mucosa and perioral skin in children and adults.

Unlike Document G, the previous abstract opens with an appeal to the interdisciplinary nature of oral pathology, therefore justifying the fact that the paper was targeting a dermatology journal. Document G is less concerned with convincing the reader of the relation between orthodontists, dermatologists, primary-care physicians, and pediatricians. According to Bhatia (1993), Move 1 is where authors may spell out the goals or objectives of the study, and both versions of the abstract clearly indicate that the study assessed diagnostic capacity (Document C) or, similarly, compared diagnostic capacity (Document G) of different sample groups. Bhatia's descriptive framework also finds that presentation of the intention, thesis, or hypothesis behind the study appears in Move 1, and in the above excerpt only the first develops the issue to be studied and the gap in the existing literature.

Both reviewers for the first journal critiqued the abstract (Document D), although this feedback was not taken up by the authors when they submitted the RA to a different journal within the same specialty (Document K). Reviewer 1 called the abstract "very short and incomplete," and Reviewer 2 noted an unsatisfactory description of the results. In the version critiqued by this reviewer, the results subsection was as follows:

Results: The overall analysis of the test results comparing among [sic] professional groups revealed that dermatologists were clearly better trained than other specialists in oral and perioral pathology, involving children and adults. This advantage over their counterparts in other specialties was seen in both residents and board certified dermatologists. Though by small margin [sic], orthodontists [sic] showed better performance than pediatricians and family doctors.

As presented in this excerpt, Federico describes the results comparatively, leaving out the numerical data behind the assessments on how each professional group fared in the test relative to the other groups studied. Despite this criticism, the abstract in the published version (Document M) takes the same approach, as evidenced here:

Dermatologists were clearly better trained than other specialists in oral and perioral pathology. This advantage over their counterparts in other specialties was seen in both residents and board-certified dermatologists. Though by small [sic] margin, orthodontists showed better performance than pediatricians and family doctors.

As I mentioned briefly in the previous section, one of the reviewers for the first target journal challenged the rhetorical appropriateness of the introduction, writing “Introduction: Check english [sic]. Some parts doesn’t [sic] have references and author’s [sic] opinion should only be stated at [sic] discussion section.” Such criticism lends further credibility to the framework published by Swales (2004), in which he reports that citations are a necessary part of Move 1 (p. 230). When I asked Federico to comment on this, he replied that very little research had been published previously on this topic, thus making it difficult for his collaborators and him to back up their claims. The following stretch of text, which is the opening paragraph in all drafts and milestone versions of the RA, stands out particularly for its missing citations:

Oral pathology is an interdisciplinary field of unclear boundaries. The involvement of the different types of medical and dentistry specialists with separate but related areas of expertise in the diagnosis and treatment of the various processes affecting the perioral skin with its adnexa and oral mucosa is often a controversial subject. These specialists include, on the one hand, dermatologists, maxillofacial surgeons, dentists with special area [sic] of expertise in oral medicine, general dentists, and experts in other areas of dentistry, and on the other hand, primary care physicians, both pediatricians and family doctors.

Here, the reviewer may have expected to encounter a reference at the mention of controversy, as the absence of citations for this statement could appear to be a mere belief held by the authors rather than an idea with a narrative context (Hyland, 2000, p. 21) that is linked to other papers on the matter. Indeed, the other reviewer for this same journal appears to index the same idea when they write, “The manuscript begins and the aim of the study is based on an assertion that is at least biased and inaccurate,” and then refutes Federico’s statement on unclear boundaries. During our talk around this text, Federico called the introduction “personal,” telling me “The introduction is more like a philosophical introduction more than scientific [sic] introduction.” Federico and his coauthors did not modify the introduction to make it more rhetorically conventional in this regard due to the lack of scholarship on this research topic; however, we will see that certain references to the literature were made in the discussion. Despite this unorthodox rhetorical style, the fact that the RA was eventually published suggests differing standards between the two journals.

A move analysis of this introduction reveals primarily two cycles of Move 1–Move 2 (Establishing a territory–Establishing a niche), progressing toward Move 3 (presenting the present work) at the end of the section. Move 3 occupies roughly one-eighth of the entire section, and what precedes it seeks to a) highlight the problem posed by unclear boundaries between different specialties concerning the treatment of oral pathologies and the differing views on which type of health professional is most qualified to detect, diagnose, and treat these conditions; and then b) explore how this lack of clarity coupled with the heterogeneity of dental training both within countries and internationally may be remedied so that orthodontists may aid in diagnosing and treating patients with pathologies of the oral cavity. Within the first of these cycles, Federico establishes the niche that his paper will occupy by arguing that the lack of consensus surrounding which type of health professional should ensure care for these conditions may have a negative impact on the training of dentists. This shift can be seen between the first and second sentence of the second paragraph (final published version, Document M):

[M1] In this regard, Baum,¹ in 1999, stated: “Oral medical (stomatological) disorders are commonly neglected because neither dentists nor physicians take responsibility for them.”
[M2] This lack of clear roles in the management of this pathology may lead to insufficient or even inappropriate training of medical residents in different specialties, owing to the fact that this area of knowledge is considered as falling under the realm of “other specialties.”
[M2] At the same time, another potential risk may arise from the false perception held by some specialists that theirs is the only field sufficiently capable of diagnosing and providing therapy for this type of patient, thereby excluding the possible contributions of experts in other fields rather than seeking the benefits of complementary viewpoints provided by multidisciplinary approaches.

If we take this assertion to be the authors’ establishing of a niche, then the next sentence appears to also follow from the primary notion of unclear boundaries, and can be considered a second instance of Move 2.

The second of these two move cycles (not excerpted here), on the heterogeneous and deficient training of dentists and the need to remedy this with better education, consists of a more layered argument in which the authors a) establish that medical training is not uniform and may be deficient (Move 1) (no references included); then b) recognize the existence of differing opinions as to whether the orientation and status of dental training should be changed (Move 2, Step 1A,

indicating a gap) (references included); after which they c) stress that there is a consensus opinion indicating that dentists may make valuable contributions to medicine beyond their traditional remit (Move 1) (references included); and then d) argue that because orthodontists come into particularly frequent contact with patients, often over a prolonged period of time, orthodontists should be trained to meet the needs of such complex patients (Move 2, Step 1B, addition to what is known).

The methods section contains three sub-headings: Subjects, Materials, and Statistical Analysis. These markers closely match the three moves indicated by Nwogu (1997), namely, Move 4 (Describing Data Collection Procedure), Move 5 (Describing Experimental Procedures), and Move 6 (Describing Data Analysis Procedures). On all of the three occasions in which the RA was sent for peer review, one or more reviewers mentioned flaws in the methods section. The specific comments were:

The methods have serious problems in my opinion (Document F, Reviewer 1)

The analysis description is not clear and the sample-size does not seem to link with the plan for analysis (Document F, Reviewer 1)

Methods and Research Design: Inadequate (Document L, Reviewer 1)

The study performed in intersting [sic], however there are a few problems in its methods that must be tackled before manuscripts' [sic] resubmission (...) In my opiniorn [sic] the study has to be reapriased [sic] and at authors [sic] description [sic], resubmitted [sic] with the adequate changes to material and methods and its evaluation (Document N, Reviewer 2)

Despite these comments' particular focus on the methods section, Federico made very few changes to this section, reserving more comprehensive modifications for the results section. These were instances of addition of text, and both came in response to comments from Reviewer 1 of the fifth and final journal (Document L), in which the reviewer asked the authors to provide more information about how the authors had designed their experiment—specifically, who had chosen the lesions that formed part of a test to demonstrate knowledge of different possible oral pathologies and who had prepared the questions corresponding to such lesions. As for Reviewer

2 (feedback excerpted above), it appears the authors were able to convince this reviewer in their point-by-point response that a wholesale reappraisal of their study design and methods was not warranted, although I was only able to access an incomplete draft version of this response, and the authors did not save the final draft of this document. During the informant interview, Federico remarked that “the design part” of any RA is quite difficult to follow, and that he takes special care when drafting this section so as to make the text more amenable for the reader.

The results section came under criticism from three different reviewers throughout the publication process and changed to a relatively greater extent than other sections, although these changes were mostly superficial. The first such comments were made by the same reviewer mentioned previously, who had complained that the analysis was insufficiently described. The report issued by this gatekeeper has a two-part structure that starts off with a three-sentence assessment of the RA overall, and then divides the second part of the report the following sections: abstract, introduction/background, methods, results. (Here, it is likely that the absence of a section on the discussion in this report is due to an oversight.) In the section devoted to results, the referee writes, “I’m really confused about the results. There is no discussion about where the questionnaire originated. No, there is no limitations section in this manuscript but there are numerous limitations” (Document D). Here, it appears that the referee expects for the results section to contain an account of how the questionnaire used in the study was designed, thus suggesting a certain flexibility or difference of opinion on the content that belongs in the methods section and in the results.

Two reviewers expressed dissatisfaction with the rhetorical style of the results section—one from the first journal (Reviewer 2, Document E) and another from the fifth and final journal (Reviewer 1, Document L). The former remarks that “The authors almost described the results as legends of tables,” while the latter suggests, “As a matter of style, it is not necessary to use a whole sentence to let the reader know about a table. The result can be stated, and ‘(Table N)’ placed at the end of the sentence.” An analysis of the results section does lend some validity to the first of these remarks: in the draft appraised by this reviewer (Document C), the section consists of sixteen sentences, of which six (37%) have the following in sentence-initial position: Table [X] +

VERB to indicate the contents of the table. Also, if we include the sentence “The overall average scores obtained by orthodontists, dermatologists, and pediatricians for the test on lesions in children *appear in Table 4a*” (emphasis mine) as an example of a sentence that points to a table, then the percentage of sentences that follow this pattern jumps to 44%. In terms of the rhetorical moves being employed, this section contains eight pointers to tables (Move 0.1) and eight instances of Move 1.1a (Statement of finding; comparison). The section contains no other moves meant to substantiate findings or comment on them in any way, and all such commentary is reserved for the discussion section. With this high density of pointers and other text describing the results expressed in tables, it appears that the expectation of one or both of the reviewers mentioned here was for the authors to present the results with greater rhetorical variation, possibly providing commentary on the experimental results similar to the types outlined in the framework of Williams (1999).

These comments did bring about changes in Document M, although the rhetorical thrust of these modified passages remains the same. An example of the changes made between the version submitted to the fifth and final journal (Document K) and the published RA (Document M) evidences the superficiality of these changes:

Document M

Tables 5A and 5B show the same data appearing in Tables 4A and 4B, but referred [sic] to oral lesions of adult patients, when the tests were administered to the groups of dermatologists, orthodontists, and family doctors. In this exercise, the dermatologists displayed clear superiority over orthodontists and family doctors ($p < 0.0001$) across all groups and sub-groups. The orthodontists were again more successful than the family doctors in recognizing this type of lesions ($p < 0.05$).

Document O

Comparison of the results from the tests on lesions in children and adults as obtained by dermatologists, pediatricians, family doctors, and orthodontists, respectively, along with their corresponding subgroups are shown in Table 5. In this exercise, the dermatologists displayed clear superiority over orthodontists and family doctors ($P < 0.0001$) across all groups and sub-groups. The orthodontists were again more successful than the family doctors in recognizing this type of lesions ($P < 0.05$).

Following an explicit suggestion to do so (Reviewer 1, Document L), Document M groups the test results concerning lesions in children and lesions in adults into one table. Aside from this, the rhetorical structure of this paragraph is highly similar, which raises the question of why Federico and his coauthors did not opt for a more comprehensive revision. One possible explanation stems from the aim of the study and the possible limitations imposed by the scope of the research question they were asking. Alternatively, the authors may have considered this feedback to be of low priority. Whatever the cause, this pattern of negligible changes did suffice, and the text was eventually accepted for publication. The dynamic seen here lends further credibility to the notion that varied and appropriate use of rhetorical possibilities is sometimes not a priority in biomedical academic publishing, although perceived rhetorical shortcomings may be noted by journal reviewers. This evidence also points to a possible disconnect between what reviewers request from authors when reviewing papers and what they actually will demand that authors do.

Turning now to the discussion section, Federico and his coauthors made almost no changes to this text from the earliest drafts of the RA, possibly due to the comparatively fewer negative comments made by the reviewers about the section; indeed, the only reviewers who took up this part of the manuscript were from the first journal (Document D), suggesting that the paper include mention of study limitations and encouraging the authors to “revise” it, though without further specification beyond “typing mistakes.” The section comprises five different parts corresponding to the main topics argued in the discussion. In the first and fifth, the authors open with a clear introductory move cycle and close the section with an explicitly worded conclusion. This introduction highlights one of the main findings using Move 3 (Finding), and then completes this opening paragraph with a mention of one limitation of the study (Move 8). Specifically, after reporting that orthodontists demonstrated greater capability to recognize a certain pathology than family doctors and pediatricians, the authors write the following:

[M8] Here it must be mentioned that within the field of odontology, truly expert dentists in non-odontological oral pathology are trained specialists in oral medicine, and it is highly likely that, had they been included in this study, these individuals would have scored better than the orthodontists on these tests; however, this study, which was conducted by orthodontists, had the primary aim of comparing the accuracy with which these specialists recognize oral lesions as compared with that of dermatologists and primary-care physicians.

This admission seems to have been written to head off possible criticism during the peer-review process; as mentioned previously, Federico told me “You try to go one step ahead of the reviewer,” and I believe this to be an example. This reference to the limitations was not sufficient for the first reviewer for the first journal (Document D), who claimed the paper had “numerous limitations.”

Immediately after this, the discussion gives a surprisingly lengthy account of two other studies that have taken up the same question as the authors. In seemingly unorthodox rhetorical fashion, each of these two paragraphs presents the study, outlines its main findings, announces the conclusions reached, points out any methodological differences which would preclude comparison between the two. Where these caveats are found, Federico signals any limitations deriving from these differences, offering an account of what steps he took to limit the effect of this aspect of the study’s design. Let us now examine the first of these paragraphs:

[M1] To date, little research has compared the ability of physicians from different specialties to identify correctly [sic] oral lesions with that of dentists. **[M5]** A study carried out in the United Kingdom⁶ compared the capacity to recognize 10 color photographic images of relatively common oral lesions in a group of emergency physicians compared to the accuracy of a group of dentists. The results from the research revealed superior performance in dentists, with 88% correctly diagnosing these conditions whereas only 28% of family doctors got accurate diagnoses. The authors concluded that medical students received insufficient training on this pathology. **[M8]** It must be noted that in the study, the images of the lesions were presented alongside information from medical records. This information was not included in our research, and it may have increased the difficulty of diagnosis in some cases. **[M5]** However, the different groups of professionals who participated in our comparative study were all faced with the same degree of difficulty because all of them had a single color clinical picture as the only tool to establish the diagnosis.

Although this paragraph contains no mention of the RA's primary finding—that the orthodontists performed better on the test than family doctors and pediatricians—, this move cycle appears to focus the reader's attention on the fact that the study carried out in the United Kingdom had similar conclusions. This similarity is particularly relevant in light of the comment made by the assistant editor of the fourth journal (Document L) that suggested Federico's paper would be more suitable for a journal with a more "local focus." Though an exhaustive comparison of the study referenced here by Federico would be required to fully substantiate any analysis of the appropriateness of this criticism, the two articles seem highly similar in their conception and execution, which makes it plausible that "local" studies in the U.K. are published in international English-medium journals, while similar work from the semi-periphery is relegated to a different tier.

Returning to the fragment appearing above, the paragraph opens with an instance of Move 1, which highlights the fact that little research has been carried out on this particular question. The cycle then goes on to describe the methodology, findings, and conclusions of this first study (Move 5: Reference to previous research), suggesting that Federico's findings are consistent with what is known about the topic. This is followed by an instance of Move 8 that does not comment on the authors' own work, but rather remarks on the aspects of the U.K. study that should qualify any comparisons made between the two investigations. By concluding the cycle and the paragraph with a sentence that highlights the comparability of the two studies (Move 5), the authors intend to dispel doubt about the importance of their methodological differences and thereby find support for their findings in the literature. In all, this section appears somewhat unconventional in its move structure, which is possibly the result of the combined impact of regenerating, translation from Spanish, and the effects of the L1 rhetorical culture on the English text.

Following these two paragraphs, the text continues into the third part, where it justifies the methodology they used, especially the images used to perform their study. This part of the discussion opens in particularly explicit fashion: "Another point that should be discussed is the material used for the tests." This contextualization of the way the study was carried out occupies

one-quarter of the entire discussion section, suggesting that the authors used the discussion less as a space for analyzing and contextualizing their findings and more as an opportunity to convince the reader of the merits of their methodological steps. Here and in other parts of the section, Move 1 dominates the discussion, although the nature of the information appearing here suggests that there is rhetorical space to defend a study's design in the discussion, thereby hinting at a certain fluidity within the different parts of the IMRD structure or a possible feature of ELF rhetorical patterning.

Once discussion of methodological issues has concluded, Federico examines more particular findings from his study, using a conventional move cycle that consists of Move 2 (Statement of result) followed by Move 6 (Explanation). The results commented on at the beginning of these move cycles came as somewhat of a surprise, and the text segues into commentary on this unexpected result and later points the reader toward the knowledge claim (Move 7), which will not be made explicit until the final paragraph of the discussion section. The following is an example of one of these move cycles:

[M4] Many pediatricians and dermatologists failed to identify the lesions with an odontological origin in children, and especially gingival fistula, with only two orthodontists providing correct responses. **[M5]** This fact is relevant since these lesions, related to deep caries and dental traumatic injuries, are very prevalent and are accessible during a dimple oral examination.¹³ **[M6]** Additionally, the image appearing in the test offered key clues for a right diagnosis and for the type and origin of the lesion, as the photograph showed the gingival fistula in association with a fractured tooth.

This is the second of two such cycles appearing in the same paragraph, and, though not excerpted here, the paragraph begins with Move 2 (Statement of result), as indicated by the explicit reference to tables: "As seen in Tables 2 and 3, the lesions which caused the highest number of mistakes by the groups assessed—with less than 25% of correct responses—were consistently identified incorrectly by all groups of professionals." Returning now to the text appearing in the box above, Move 4 examines what the authors believe to be a noteworthy finding. The commentary that follows makes use of a reference to another study (Move 5), lending further credibility to the authors' conviction that well-trained professionals should be able to accurately identify these lesions. Following this, Move 6 further argues that these incorrect responses

indicate insufficient training. The move cycle described here is used four times consecutively, and appears to make use of certain rhetorical techniques described by Dudley-Evans (1994). What is noteworthy about this part of the discussion is not how it is written, but rather how deep into the discussion the reader must get in order to read about the study's actual findings and how these relate to previous work.

The divide between explicit and implicit writing was raised during the participant interview when I asked Federico whether he believed his RA contained an explicit hypothesis. He emphatically replied that yes, the article did contain an explicit hypothesis in the discussion section. I was somewhat surprised by this claim for two reasons. In the first place, I am unable to locate any explicit reference to a hypothesis, although I must admit that mine is an outsider's view, as I neither belong to the rhetorical culture nor the discourse community of health-care professionals. Regardless of my ability to decipher the authors' rhetorical intent, Federico stated that at times journal reviewers remark that certain information is missing from his texts. In his words, "(...) sometimes also you really thought that you were not being clear enough because reviewers ask you for things that we really thought were already implicit in our text." From this, it appears that Federico's understanding of explicit and implicit writing differs from my view and the views of the gatekeepers he mentions here. Second, my training both as a writer and as a genre analyst have taught me that the introduction is the most common section for discussion of the hypothesis (Swales, 2004). When asked to explain this preference of his, Federico responded that by including mention of the study's hypothesis in "the conclusion," he makes it easier for the reader to follow the paper. As seen previously in this section, this preference gives greater importance to the discussion section and implies a certain fluidity in move placement across sections of the RA, at least when compared to the frameworks I have used to ground my study.

Attitudinal aspects

Federico is highly motivated to research and publish his findings in English, and reports that he goes to great lengths to sharpen his abilities so that he may continue to participate in the global discourse in his field. This willingness to sacrifice his time and energy to publish in English is partly aided by his positive attitude toward the language in general. During the interview, Federico

mentioned that he had always wanted to earn his doctorate, and that he saw this RA as a way of making his thesis work known to a broader audience. Beyond this particular publication, however, he spoke of research as a means of staying motivated, and that health-care professionals like him who only perform clinical work may someday see their status reduced to that of a technician, while clinicians who perform research earn their place among fellow specialists. For him, working among colleagues like Paco increases his desire to be a productive researcher in his field, although any external pressure, perceived or real, is not as strong as his own internal motivation.

On the topic of publishing in Spanish or in English, Federico told me the he aims to publish in English whenever possible, even despite the disadvantages he faces. The substantial amount of work he must devote to publishing in English is often not rewarded, and he has at times translated some of his rejected manuscripts into Spanish in order to submit them to Spanish-medium journals, though publishing in Spanish is a last resort. Despite the added challenge of drafting, revising, and interacting with academic gatekeepers in English, Federico stressed that publishing in his native Spanish is a difficult task, though considerably less so. As a whole, he spoke of Spanish-language publications as being somewhat second-rate, adding that he only reads English-medium journals when looking to keep abreast of developments in his field.

Federico's admiration of Paco's achievements in English-language international publishing gives us a glimpse at what Federico takes to be the keys to success. In the course of our interview, it became clear that many of Paco's attributes were precisely what Federico lacked at the time. In his view, authors need "contacts" in order to be published, and he has few such colleagues in his field who can advocate for his work to be published in inner-circle journals. Federico's experience has taught him that by getting to know prestigious international researchers, scholars can access valuable advice on how to orient experiments and texts so that they will be accepted by journal editors. Additionally, influential members of the discourse community can introduce novice authors to the very reviewers who will eventually evaluate their texts. Often, these bits of advice are given in informal situations such as social events, and it is safe to say that foreign-based researchers have much less access to these types of encounters.

Federico feels that publishing in English is a substantial challenge for Spanish authors. As he put it during the interview,

I find especially [sic] difficult for Spanish people to publish. OK, as Spanish, Italian, and French, whatever. As [sic] you're not writing in your mother language. For you, although you got somebody helping you translating it's not the same. From the first time you communicate with the journal, you send your email, the way you write your letters, the way you respond to the reviewers, everything. Not all the communication is translated or checked by an English professional. So many times, I guess, these people they see that your letters or your whatever isn't well written, they might lose interest in you.

According to his view, occluded genres such as the submission letter and point-by-point response are a vital part of how written work is received, and NNES academics—especially those from Southern European countries—are at a considerable disadvantage when drafting submission letters, e-mails, and other correspondence. Either due to a lack of time, the unavailability of language-support services, lack of funds to pay for language support, or an unawareness of the importance of these genres within the process, some of the exchanges that NNES authors must have with academic literacy brokers portray authors in an unfavorable light, and may jeopardize their chances of securing publication. As revealed here, English-language publishing demands knowledge of not one genre, but many: from happy-hour chats with journal reviewers to submission letters to e-mail exchanges with individuals who broker texts' entry into the system, deep knowledge of the rhetorical options afforded by the RA is not enough for a NNES author's text to be published.

In one part of the participant interview in which I asked if he considered himself to be at a disadvantage relative to NES authors, he did not hesitate in his reply:

Oliver: So you think you're at a disadvantage?

Federico: Of course. If English was my mother tongue for me it would be much more easy to write. Also, to go to international meetings to know yourself different authors [sic]. To become you know like in the ehm how could you call it, to get into like *en el circuito de autores, digamos*.

Oliver: Like in the circle, the inner circle.

Federico: Yeah. In Spain it's very easy for us to publicate [sic], it's very easy to go to meetings, to present. It's easy because you have more resources.

While the “resources” mentioned here appear to refer to linguistic resources, these and other comments touched on the value of proximity to the people, events, and institutions located in the inner circle. In Paco's case, his lack of geographic proximity has been mediated by sustained contact with key members of his discourse community in the United States, which keeps him well-connected and able to broker relationships between novices like Federico and colleagues in the Anglophone center. Clara, on the other hand, is limited in her ability to network with the Anglophone because her discipline, orthodontics, is not as international as Paco's. The difference between these two figures in this TH suggests a continuum of membership in which one's scientific field plays a role in their ability to reach beyond the local context.

Implications

Some of the shortcomings of this RA were methodological in nature, although the rhetorical makeup of the text was considered to be flawed by more than one reviewer. Having carried out the experiments and published his doctoral thesis based on these data, Federico was limited in his capacity to rhetorically rework his findings into an acceptable RA, even with the expert assistance of Paco. The authors chose to leave the article largely untouched, and where they did make changes, these were either superficial or were limited to additions and deletions requiring no reworking of the meaning beyond the paragraph level. Federico's decision to resubmit as is rather than reorganize his text may have dragged out the process, although there was no guarantee that by refashioning his paper he would have reaped the benefits.

The dynamic between the two orthodontists and the dermatologist presents us with a fine case of academics who successfully bridge the gap between the expanding circle of academic publishing and the inner circle. Federico and Clara followed Paco's lead in a number of ways, from the study design to the drafting of the manuscript to negotiating with reluctant journal reviewers. Considering the way in which these three individuals first came into contact and how beneficial the partnership was for Federico here, it is likely that much brokering like this occurs in large research institutions like the IIS-FJD.

The opinions of the assistant editor suggesting that the article was more appropriate for a “local” audience raise questions about fairness and the politics of access. Seeing how studies like his have been published in international journals while his is considered to be inappropriate for the inner circle has likely led Federico to feel that he is treated unfairly. This disadvantaged position, however, appears to be compounded by the fact that Federico consumes exclusively English-language scientific literature, as in doing so he and many others like him may be contributing to publication dynamics which grant international status to Anglophone-center research while asserting that scholarship outside of certain English-speaking countries is most fit for local practitioners.

Text history 6: Miguel

Overview

This RA was written by a senior nephrologist at the IIS-FJD, whom I will call Miguel, and was met with a high degree of resistance from the academic journals to which it was submitted. Indeed, one of the two reviewers for the third and definitive journal remarked that the writing was rhetorically deficient in the discussion section. Despite this feedback, Miguel's uptake of the recommendations made by this same reviewer went beyond the specific suggestions made, and as a result altered the rhetorical shape of the text in meaningful ways.

Miguel's experience in publishing this RA is representative of other attempts at English-medium international publication throughout his career. On the one hand, he engaged extensively with fellow NNES coauthors as well as with other literacy brokers, and this chapter will illustrate the different types of knowledge that these collaborators contributed. Also, Miguel originally drafted the RA partly in Spanish and partly in English, making this a particularly rich example of writing across languages and across rhetorical cultures, while also providing a glimpse of how NNES academics utilize different resources in order to compensate for their perceived shortcomings. Unlike his previous experiences, however, Miguel was assisted by an expert NNES colleague from his department, whose comprehensive overhaul of the paper brought it more in line with conventional rhetorical norms and who also provided Miguel with key assistance when responding to journal reviewers.

The informant interview for this TH was held in Spanish on April 16, 2015 and was shorter than others, lasting 41 minutes. During the interview, which was held in the early morning hours in order to accommodate Miguel's very busy schedule, Miguel summarized the four-year effort that went into the publication and spoke at some length about the ways in which different individuals impacted the text as well as his opinions on the merits of pre-submission peer feedback. Several of his comments also described the great challenge that he faces when attempting to broadcast his findings in written scientific English.

Chronology and texts

This TH is remarkable for the particularly high number of drafts that Miguel and his coauthors produced. When I met with Miguel in June of 2013 to gather these versions of the RA and other relevant materials, he handed me over sixty versions of the text that were in no clear chronological order, thus making my first task one of ordering the drafts in the most accurate way possible. To do this, I first relied on the file names they had been given. The information contained in these file names included journal names or abbreviations thereof, the initials of his coauthors, and, in some cases, dates. After grouping the drafts by journal name, I then examined the coauthors' initials and the dates. In case of repeated file names or discrepancy between the information in these names, I resorted to the "last modified" date feature available in the document's metadata.

Before performing my textual analysis, I asked Miguel to confirm a number of issues concerning the major events that took place in publishing the RA. Likely due to high demands placed on his clinical, teaching, and research output, however, he struggled to remember such important aspects as how many journals he had submitted the text to before it was finally published, and was unable to verify that the chronology I had come up with was accurate. As a result, the margin for error in the collating process is wider here than in other THs. I attribute this difficulty to my failure to convey to authors like Miguel how important it would be for me to trace the sequence of their drafts, although I also believe that had I been more insistent on strict compliance with these requirements, some of my informants may have been more reluctant to take part. Despite this unexpected difficulty, my study of the drafts revealed just one discrepancy in the abstract, therefore leading me to believe that this procedural issue did not have a substantial impact on my analysis.

To the extent that I was able to determine through my examination of draft file names, this RA was accepted after being rejected by two journals. Nineteen of the drafts Miguel gave me were written in a combination of Spanish and English. For the purposes of my study, I included only the last of these texts written partially in Spanish in order to analyze the effect of the transition between the two languages on the rhetorical characteristics of the texts. This TH contained four

milestone drafts: the version sent to me for pre-submission revision, the changes I proposed, the draft as submitted to the first journal, and the final published text. (Two key documents that I was unable to access are the rejection letters from the first and second journals, as Miguel had misplaced these.) The box below lists all the texts used for the text-focused component of the study, with the milestone versions appearing in bold.

1. Document A: Version of the RA in Spanish and English
2. Document B: First draft of the text fully in English
3. Document C: Second draft (pre-submission)
4. **Document D: Track-changes version containing my revision**
5. Document E: Third draft (pre-submission)
6. Document F: Fourth draft (pre-submission)
7. Document G: Fifth draft (pre-submission)
8. Document H: Sixth draft (pre-submission)
9. Document I: Seventh draft (pre-submission)
10. Document J: Eighth draft (pre-submission)
11. **Document K: Ninth draft (version sent to first journal)**
12. Document L: Submission letter sent to first journal
13. Document M: Tenth draft
14. Document N: Eleventh draft
15. Document O: Submission letter sent to second journal
16. Document P: Twelfth draft (version sent to second journal)
17. Document Q: Thirteenth draft (version sent to third journal)
18. Document R: Response from third journal
19. Document S: Fourteenth draft
20. Document T: Excerpt of Document R, containing the reviewer responses from the third journal
21. Document U: Fifteenth draft
22. Document V: Sixteenth draft
23. Document W: Version resubmitted to third journal
24. **Document X: Final published version**

Evolution of the text and the role of literacy brokers

As with other THs, early on in the informant interview I asked Miguel to summarize the process of drafting, revising, and publishing the text. In posing the question, I took great care to help him understand the distinction I am making in this study between the ideas transmitted in an RA (the content) and the writing-up of those ideas (the text).

Oliver: Can you give me a summary of sorts of what you remember about this work, what the publishing process was like? Not the study itself, but rather the process of publishing it, in terms of the people who contributed to the text, difficulties you had. I mean, how do you remember this text, this publication?

Miguel: It was laborious, because we sent it to two, three journals and the truth is I had to put a lot of work in to get a good result [out of our work] because in the end, saying the same thing, but presenting it or saying it one way or another...some reviewers found it good, and others didn't, and it was hard for us to get it published.

Here, Miguel hints at two issues affecting NNES authors as they strive to publish their manuscripts: journals' standards of manuscript quality on the one hand and the arbitrariness of their decisions on the other. Publishing this RA was such an arduous task because there was no unanimous opinion in reviewer feedback, causing Miguel and his colleagues confusion on what they needed to change about their paper. What is surprising about this assessment is the fact that he believed that his core message articulated in the paper stayed the same, which raises the question as to what he considers to be "the message" and how this perception of his may contrast with the opinion of journal gatekeepers, who likely assess manuscript quality as a more relationship between data and the generically appropriate articulation of data.

This RA reflects the work of number of collaborators, both named authors and others. The rhetorical makeup of the text changed substantially, although Miguel assured me that the core ideas of his study were present from the earliest drafts through to the published version, making this an interesting study into the way a text can be shaped rhetorically to meet the demands of the discourse community. Four types of individuals contributed to the RA: collaborators orienting primarily toward content, who mostly wrote their contributions in Spanish; myself, the in-house editor, who oriented entirely toward language; a colleague of Miguel's and fellow employee of the IIS-FJD, whose extensive experience in publishing in English and great expertise in nephrology were applied to both toward content and rhetoric before and after submission; and, lastly, the journal reviewers, some of whom made suggestions which were incorporated in the final product.

Miguel professed his strong belief in collaborative work, and this text is no exception. In his words:

(...) I conceive of research as something that, like everything in life, needs more than one person to be involved in. What I mean is, I don't feel like I am the holder of the truth, and I like for colleagues whom I trust to give me their input on what I am doing. Although the general idea was mine and I developed the study, it is true that before I subject my work to the filter of revisers and, especially, the people who will be reading the article, I am interested in knowing the opinion of people with whom I surround myself and trust.

In addition to the great stock he places in collaborative research, Miguel's statement underscores his sensitivity not only to reviewers and editors as gatekeepers, but also the readership of the journal. Researching and writing with fellow nephrologists in mind further demonstrates Miguel's commitment to science beyond a mere drive to notch as many publications as possible in journals with the highest impact factor.

During the early parts of the text's trajectory, Miguel solicited input from four of his colleagues, all of whom oriented more toward content than on the way the text was written. Evidence of this communication appears in margin comments in Spanish, and especially in the earlier versions of RA. Some of these comments suggest text to be translated into English and inserted into the article, while others remark on how the RA could be improved without proposing specific formulations of these changes. An example of the former can be found in the discussion section of Document C, that is, the draft that preceded my edit. The collaborator—whose identity I was unable to discern—writes a comment in Spanish that is later translated and inserted into the subsequent draft of the paper. The comment is as follows: "Few [hemodialysis] units measure serum calcium at the beginning and after the dialysis session." Employing this same strategy, in the sixth draft (between milestone version two and three), a collaborator highlights an entire paragraph of the discussion and writes in the margin:

Careful with this! Postdialysis increase of proteins sequesters Ca^{++} and to compensate [sic] and so that Ca^{++} remains stable (since it is controlled by PTH) total calcium is increased (this is why reduced calcium cannot be evidenced with a 2.5 bath when evaluating total calcium). When 48 hours pass and proteins have normalized, Ca^{++} remains low (although not as low as in post-dialysis) and total calcium also decreases (through not significantly).

Despite the agrammaticality and scientific shorthand used here, it is of interest to see how Miguel's coauthor chooses to call his attention to this content-related aspect of the paper. Though Miguel's willingness to sound out other researchers may have caused the number of drafts to increase substantially, this statement by Miguel is indicative of his humility and deference not only to the judgment of the journal gatekeepers, but even more so to the greater scientific community.

One possible cause of the negative feedback which will be explored in the next section of this chapter is the fact that much of this early work was written in Spanish. This approach was the result of Miguel's own practices and those of his collaborators who played a role in the early stages of the process. This code-switching may have caused the text to be rhetorically articulated according to different conventions, and it suggests that Miguel feels more comfortable with academic writing in Spanish than he does in English. When I asked him whether he made a habit of drafting his papers in Spanish, he replied that he writes primarily in English, resorting to Spanish when he feels that he cannot get his ideas across in English.

One collaborator had an overwhelmingly positive impact on Miguel's paper, suggesting modifications to both content and rhetoric, and also advising Miguel on how to reply to reviewer comments in his point-by-point response. This colleague, Óscar, is a fellow employee of Miguel's department and also a native Spanish speaker. He was included as a coauthor in the later stages of the publication process, and his initials appear in Documents S, U, V, and W; considering that Document T is not a draft but rather an excerpt from the reviewer responses included in Document R, Óscar contributed to all drafts of the text in the post-submission process. Miguel described Óscar's contribution as a "critical appraisal" of the writing and the development of the argument, calling him a highly skilled writer of academic texts in English. As Miguel and I were

discussing the issue of notoriety within the discourse community and the potential for name value to guarantee publication of an author's manuscripts, Miguel stated that Óscar may be one of a "handful" of Spanish nephrologists with such clout; indeed, Óscar currently serves as editor-in-chief of an international nephrology journal. As for his opinion on what had made Óscar into such a successful academic in international journals, Miguel mentioned Óscar's fellowship in the United States but also remarked that he is extraordinarily well-read within the field and resourceful when navigating the ins and outs of the peer-review process. Part of this resourcefulness manifests in Óscar's deep understanding of how to frame responses with the occluded genre of the point-by-point response, which indicates that expertise as an international academic includes keen knowledge of not only how to research and how to write, but also how to approach certain situations that arise within the publishing process. Since the publication of this article in 2014, Óscar's advice on how to publish in English-medium journals has caused Miguel's "problem" with publishing in English to go away, and others in their department have also benefited from Óscar's sound advice in similar fashion. From this it can be concluded that Óscar's insider knowledge is highly valuable to less skilled authors like Miguel, and especially when such expert field knowledge is coupled with a keen ability to overhaul NNES writers' texts on the rhetorical level.

As noted in the list of texts used to construct this TH, I also collaborated in this RA, and Document D contains my proposed edits, made either directly in the body of the text or in margin comments, where I remarked on various aspects of the RA though without intervening directly. Examining this version, I see little that makes the text stand out from other authors' papers I review at the IIS-FJD; although the RA contained a number of passages that I deemed to require substantial reworking for the sake of clarity, many texts I work with have comparable room for improvement. Likewise, most of my suggested changes to the manuscript were recommendations for clarification due to errors in syntax, requests to improve transitions between ideas, style-based remarks in which I advised the author not to write in one-sentence paragraphs, and also some advice on adapting the text to the target journal's instructions for authors.

In an unexpected turn of events, after I returned the paper to Miguel he asked if I would like to be included as one of the article's co-authors. Unaware that I did not meet the requirements to be included as such (Albert & Wager, 2004), I accepted his gracious offer. However, Miguel was advised against this by a collaborator who was sent the RA for his contributions. In explaining this initial gesture, Miguel argued that if statisticians' contributions are considered to merit coauthorship, then he believed that language editors deserved similar treatment.

Genre considerations

Despite Miguel's comment indicating that the essence of the RA did not change throughout the publication process, the rhetorical structuring of the text underwent a substantial reworking. This transformation is particularly evident in the results section and, to a somewhat lesser extent, the discussion. The introduction was partly redone, while the methods and abstract remained primarily unchanged from the first draft included here until the RA was published. A number of factors appear to be behind these changes, including remnants of the Spanish rhetorical style that survived the process of translation into English, efforts to sharpen the RA's focus and situate the experiment in relation to other works appearing in the literature, and the recommendations of one journal reviewer. In spite of the gaps in the materials made available to me by Miguel, this TH, more than others in this study, shows how rhetorically oriented changes were made and brings into focus the influence that Óscar and the aforementioned academic literacy broker had on the article.

Miguel uses code-switching as a means of overcoming certain limitations in his ability to write in English. In the previous section, I noted that the first nineteen drafts he gave me were mostly in Spanish, and that only the nineteenth was included in this study. Because the first English version of the text was a translation of the Spanish and did not seem to account for rhetorical differences in academic texts between English and Spanish, I inquired about this practice, seeking to pinpoint his views on these differences. He replied that he has always worked in this fashion but has never stopped to consider why and has never considered changing. When I asked him whether he believed that texts written in Spanish are constructed differently from those written in English,

he replied that yes, English academic texts are completely different from those in Spanish. He went on:

The problem is that when you make English sound Spanish, it comes out terribly. The turns of phrase and expressions you use [in the two languages] to convey a given concept are totally different, so you have to get used to [the English way], and that's not easy to do. So for me it's totally different to express an idea in Spanish...you use more words, while in English you may have a turn of phrase that gets your intended meaning across quickly, right?

From this, we can infer that his writing in English tends to have a markedly Spanish feel, manifesting as longer sentences and fewer expressions or turns of phrase. It appears, however, that Miguel believes these differences to be articulated at the sentence or paragraph level rather than at the level of discourse, although, as we will see later in this section, some substantial modifications to the text made to bring the RA more in line with a conventional move structure took place at the level of rhetoric.

Although the abstract received little attention in the early revisions of the RA, this section was reworked slightly by Miguel's colleague, Óscar, whose interventions are first seen in the text in Document S. Prior to Óscar's revision and thereafter, the abstract mostly adhered to the framework of Bhatia (1993, p. 78), with each of the section's four paragraphs realizing the four Moves: introducing purpose, describing methodology, summarizing results, and presenting conclusions (*ibid.*). Óscar's work made the abstract more rhetorically conventional overall. Two changes brought about this transformation: on the one hand, a more refined Move 1 (introducing purpose) making more explicit reference to the problem and limiting the contextualization of the problem, and on the other, moving aspects of methodology to Move 2 (describing methodology). This change is made evident by comparing Document P and Document S. Although there is one intervening draft between the two (Document Q), it is in unstructured form, and there is substantially greater similarity between Documents P and S than between these two and Document Q, which leads me to believe that Document S was revised based on Document P rather than Document Q. The two passages I am referring to are as follows:

Document P

Background: [M1] Dialysis patients have a high incidence of vascular calcification, a process in which bone and mineral metabolites such as calcium play an important role. [M1] Two types of exogenous calcium intake occur in hemodialysis (HD) patients: oral and venous delivery during HD sessions due to dialysate calcium. [M1] Most HD recommendations suggest a [sic] neutral calcium balance as possible because a negative balance causes a temporary rise in parathormone (iPTH). [M1] The objective of this study is to determine the effect on bone and mineral metabolism parameters of two different calcium concentrations: [M2] one with 3 mEq/L and the other one with 2.5 mEq/L according to patient's [sic] serum calcium levels before HD.

Document S

Background: [M1] Vascular calcification is highly prevalent in hemodialysis patients and is associated with adverse patient outcomes. [M1] A positive calcium balance may result from oral or dialysate calcium and may contribute to vascular calcification. [M1] Clinical guidelines recommend a neutral calcium balance, since a negative balance drives parathormone (iPTH) expression. [M1] We have now explored the impact of dialysate calcium on bone and mineral metabolism parameters according to predialysis serum calcium levels.

The second of these two texts opens with a broader focus, highlighting the basic problem (vascular calcification) in a way that resembles what Swales (2004) describes as establishing a territory, albeit for the introduction section. This more general point of entry emphasizes the clinical relevance of the phenomenon and then establishes the causes of this problem, thereby setting up a logical sequence that leads up to the statement of purpose at the end of Move 1. (While it is true that this statement of purpose may merit a separate move, Bhatia (1993) groups such statements under Move 1.) In its previous form, this section omits the fact that vascular calcification is harmful for patients, apparently under the assumption that the reader will infer this. Compared to the revised version, Document P anticipates the methods sub-section of the abstract, revealing the two different calcium concentrations used in the study; on the other hand, in Document S the author gives more of a summary, leaving a more detailed description for subsequent sections of the text.

The aforementioned changes made to Document S to highlight the problem posed by vascular calcification are scaled back somewhat in the final published version, where the section on

background information is shorter. Document V—one of the drafts worked on by Óscar—rewords the opening to the abstract, and the final draft (Document X) reads as follows:

[M1] A positive calcium balance may contribute to vascular calcification, while a negative balance increases iPTH. **[M2]** We explored the impact of different dialysate calcium concentrations on bone and mineral metabolism parameters according to pre-dialysis serum calcium levels.

Here, the issue being investigated is no longer situated as broadly as in Document S, and the text is written in a way that once again assumes that the reader will be able to infer certain information, such as a) the fact that vascular calcification and increased iPTH are deleterious to patients' health; b) dialysate calcium concentrations impact calcium balance; c) bone and mineral metabolism parameters are indicators of calcium balance; and d) pre-dialysis serum calcium levels are a factor in the extent to which dialysate calcium will influence calcium balance. Length limits for this section may have been the cause of this change, although Miguel was unable to confirm this.

The introduction section was moderately revised toward the end of the TH, narrowing its scope and foregrounding its place within the ongoing scholarship on the topic. No notable changes can be seen between milestone versions D and K, although a substantive revision does happen between versions W and X by reducing its length. Interestingly, one of the reviewers of the third journal (Document R) criticized the lengthiness of the discussion and not the introduction, writing “the discussion is too long and repetitive,” although Miguel trimmed back this section as well. In large part, the fragments that were pared down in the introduction informed the reader of what is known about the recommended calcium intake for hemodialysis patients and how other studies in the literature have established these optimum levels for patients with different pathologies. When I asked Miguel to comment on this reduction in the introduction and what he believed the abridged introduction may have lost, he gave the following reply:

Miguel: Well, you see, [the revised version] says the same thing but more concisely. It loses concepts that I believe are of interest. The thing is, it's true that when writing a scientific text—and, especially, for a journal—you can't write at too much length. So you lose depth but you make the text quicker to read, which is the aim.

Oliver: So you say that you lose interesting things. Like what?

Miguel: Concepts like, for instance, [in the earlier version] it provides a good description of how kidney patients are classified and gives data that are omitted in the later version in order to make the text as concise as possible, taking for granted that the reader knows certain things that may have merited explanation. It's true, however, that the same references are [in both drafts], so anyone can go to the works cited to see what we were explaining.

In his view, the need for brevity came at the cost of thoroughness, as the previous drafts of the text gave a more complete overview of patients with renal pathologies, how they are treated, and why they receive such treatment. It therefore seems that the author tends toward a more extensive use of Move 1 according to the framework of Swales (2004), providing related yet non-essential information on the topic before proceeding to establish a niche in Move 2. Though not specifically mentioned in this excerpt from the interview, Miguel used similar language when describing the differences between academic writing in Spanish and English, suggesting that this difficulty may have been caused by differences in academic writing culture, with Miguel's different style meeting resistance from this reviewer. Of interest, however, are the numerous syntactic errors in this reviewer's comments, suggesting that this gatekeeper is a fellow NNES academic, though one who in this case imposed certain NES rhetorical norms on the text.

Contrasting with this reduction in background information, in later drafts Miguel develops more fully the gap in the scholarship, thereby increasing the visibility of Swales's Move 2, Step 1A within the paper. The evolution of this move between milestone versions K and X is as follows:

Document K

[M2, S1A] At present, there is much debate surrounding dialysis with low calcium concentrations. Both the KDOQI¹³ as well as the KDIGO¹¹ guidelines recommend using low calcium concentrations in the dialysate. It is unclear whether dialysis sessions in which adequate calcium balance and homeostasis are maintained result in a negative balance equivalent to the calcium absorbed from the patient, which will nearly or fully equal the urinary calcium excretion. Most HD recommendations are as neutral as possible, because (...)

Document X

[M2, S1A] There is an ongoing debate surrounding the ideal dialysate [calcium] concentrations. KDOQI guidelines recommend a default dialysate [calcium] 2.5 mEq/l with potential use of higher concentrations in certain patients, while more recent KDIGO guidelines recommend a dialysate [calcium] 2.5–3.0 mEq/l [6, 13]. **[M2, S1A]** However, KDIGO guidelines have been criticized for not taking into account the calcium concentrations gradient across the dialysis membrane, which depends on baseline predialysis calcium levels [11]. KDIGO and Spanish Society of Nephrology guidelines recommend a neutral calcium balance during hemodialysis [6, 14], because (...)

Without prompting by reviewers and no material evidence to support this change, Document X revisits the two sets of guidelines referenced in these two extracts, finding discrepancy where there was once consistency. In both drafts, the opening sentence notes the existence of debate, hinting that the text would go on to highlight this gap (Move 2, Step 1A). As the second sentence commences, however, we immediately note the divergence in the rhetorical focus of the two: in Document K, the adjective “both” indicates consensus between the two guidelines, which prolongs the reader’s expectation to learn about the divide in opinion, with concrete evidence of a gap in the knowledge not appearing until the following sentence, although the text does not argue for or against either of the two guidelines. In contrast, Document X contains a different assessment of the two guidelines, highlighting what now appears to be opposing views and noting the criticism leveled at the KDIGO guidelines due to a perceived oversight. In addition, the latter of the two drafts introduces a third professional body that has issued recommendations on calcium balance during hemodialysis, situating this third association, the Spanish Society of Nephrology, on the side of the KDIGO guidelines and in opposition to the arguments in the KDOQI guidelines. In light of this change made in the text, we can see how Miguel, with the assistance

of Óscar, adjusts the positioning of the text in relation to the literature through the use of Move 2.

The methods section received the least attention from Miguel, his co-authors, or other individuals who influenced the text, and none of its rhetorical characteristics were challenged or modified between the earliest drafts and the final published product, which is likely the result of the straightforward nature of this section within medicine and other hard sciences. Reviewer 1 from the third and final journal makes a number of comments requesting additional information to be included in the methods section, summing up these recommendations by writing, “Therefore more information is needed about treatment data and results.” The stretches of text devoted to what Nwogu (1997) calls Move 5b (Recounting the experimental process) are markedly chronological, possibly owing to the fact that, in the words of the author, this study is a “prospective longitudinal intervention study.” These moves are realized through temporal adverbials indicating the sequence followed during the experiment.

Unlike the account of the experimental process mentioned above, the moves that identify the main research apparatus are very list-like, as in the following extract from the published version (Document X):

All patients received four-hour conventional HD using high-flux dialyzers, with Polynephron dialyzers 1.7–1.9m² (Elisio 190 H®. Nipro Corp., Osaka, Japan) with an ultrafiltration rate of 74–76 ml/h/mmHg, and with high-flux helixone (FX 80 H®. Fresenius Medical Care AG, Bad Homburg [sic], Germany), with an ultrafiltration rate of 59 ml/h/mmHg, unchanged throughout the duration of the study. Conventional diffusive dialysis was used exclusively without diafiltration with a bath concentration of bicarbonate 35 mmol/L and an average ultrafiltration 3.1± liters/week. The bath composition, while displaying calcium variation, was common, with potassium 1.5 mmol/L, magnesium 0.5 mmol/L, and Na 140 mmol/L.

As seen in the text underlined by me, the primary focus of this paragraph, which appeared in the sub-section called “Study Design,” was the instruments used to administer hemodialysis, and there appears to be few sequential or other elements that give the paragraph its unity.

Turning now to the results section of this RA, we come to the section of the paper that received the first major overhaul of its rhetorical characteristics. After submitting the RA to the third journal, Miguel's reaction to the remarks made by both reviewers of the third and final journal (Document R) brought about a substantial change in the discourse patterning of the paper, though particularly as concerns two comments made by this reviewer, which read as follows:

1.- Title: Considering the results I would recommend [sic] to include [sic] the word "Individualization" in the title [sic]

(...)

3.- Study design: Treatment data were recorded, especially those influencing mineral metabolism: calcidiol, calcitriol or paricalcitol, calcimimetics, and phosphate binders (calcium dose was recorded in the case of calcium-based binders{ Where is recorded? [sic]). There is no information about the eventual influence of these treatments and the results obtained. In other words for example : were results different [sic] in patients under calcimimetics? Therefore more information is needed about treatment data and results.

Although the version submitted to the journal already stressed the importance of individualization in the abstract and the concluding remarks of the discussion, the reviewer advises further underscoring this recommendation. This preoccupation with the title is of interest both because it was proposed by a reviewer and also because it seems to include article titles as an essential element of the paper, and one that is subject to change within the peer-review process. Miguel, however, went even further, changing not only the previous title from "Importance of basal serum calcium in the optimization of dialysate calcium concentration" to "Individualization of dialysate calcium concentration according to baseline predialysis serum calcium," but also making repeated reference to this in the results section. Indeed, the first such reference, appearing in the third paragraph of the results section, was eventually articulated in a combined Move that alludes to the experimental steps taken in the study. This change and the rhetorical realization I am referring to here can be seen by comparing the version originally submitted to the journal (Document Q) and the final published version (Document X):

Document Q

Finally, [M0.1] table II shows [M0.3] the area under Receiver Operating Characteristics (ROC) curves for baseline total calcium serum levels prior to dialysis, after categorization of post-dialysis change of total calcium, ionic calcium and iPTH circulating levels into increase and/or decrease. [M0.3] Using a dialysate calcium concentration of 2.5 mEq/L, [M1.1] the cutoff for baseline total serum calcium with maximal sensitivity and specificity that defines the turning point from which there is a calcium depletion (blood ionic calcium) after dialysis was set at 8.75 mg/dL (p:0.002).

Document X

[M0.3] We next searched for baseline predialysis total serum calcium cut-off levels that might allow the individualization of dialysate calcium, based on the premise that a positive calcium balance might be reflected by an increase in calcium levels or a decrease in iPTH immediately postdialysis as compared to baseline predialysis values and, vice versa, a negative calcium balance may acutely decrease calcium and increase PTH. [M0.3] The area under the curve (AUC) in ROC curves was used for this purpose. [M0.3] Post-dialysis versus pre-dialysis changes of total calcium, ionized calcium, and iPTH levels were categorized based on whether they increased or decreased (table 2).

Though not excerpted here, the revised version transfers all mention of the results of these statistical analyses to the following two paragraphs of the section, devoting one paragraph each to the data corresponding to calcium concentrations of 2.5 mEq/l and to concentrations of 3 mEq/l. The earlier version begins with a combined Move consisting of a pointer (Move 0.1 according to the Williams (1999) analytical framework) that describes the contents of the table and also Move 0.3 (procedural), indicating the analyses that yielded these data. This earlier text then goes on to present a statement of the finding (Move 1.1) that is the cutoff yielded in the experiment. The revised version, on the other hand, leads with this cutoff, phrasing the cutoff as Move 0.3 (procedural). As seen here, this Move justifies the use of the procedure for establishing cutoff points as a means of achieving treatment individualization and presenting the rationale behind the use of cutoff points for this purpose. Similarly explicit reference to “individualization” is made twice more before the results section comes to a close; in one of these cases, it is the criticism of Reviewer 2 that sparks revision and, as in the example above, the authors modify their paper in a way that draws attention both to what was done as well as the results of these experimental steps. Together, this pattern demonstrates a concerted effort on the part of Miguel

and his coauthors to emphasize the individualization made possible by the study's findings, doing so even in parts of the RA not specifically mentioned by the reviewers.

Two other noteworthy changes to the text came in response to reviewer comments that appeared to address methodological issues but were nonetheless dealt with in the results section. Reviewer 1 of the third and final journal mentions the lack of reference to the influence of existing treatments on patient response to different levels of calcium dialysate (see Comment 3 above) and, in another comment (not excerpted here), suggests providing additional detail on the different patient by classifying patients in the sample by cuartiles or tercile. Again, Miguel resolves this issue in the results rather than the methods, adding the following two sentences: "Nine patients were on cinacalcet and 13 on paricalcitol. Subgroup analysis disclosed that the overall behavior was similar to the full cohort (not shown)." Inclusion of this text in the results section is noteworthy inasmuch as it evidences the rhetorical space afforded by the results section for authors to fashion their arguments and appease journal gatekeepers's concerns about apparently procedural issues.

The discussion received direct criticism from one of the journal gatekeepers, who remarks in Document R that it is "too long and repetitive" and also recommends that Miguel discuss his findings in relation to certain works not previously cited in the paper. Curious to learn Miguel's reaction to the issue of excessive length, I asked him whether he thought this was a fair assessment, to which he replied with the following:

Well, if the reviewer says so... It's true that Anglo-Saxon texts tend to be very, very, very brief and very concise. It's true that we were trying to support our argument by making reference to the existing literature, making our case against [what has been written about this topic] from one side and from the other...which is a good thing in that it gives the concept more firmer rooting, though this reviewer felt it was repetitive, so we changed it and that was that.

Here, he begins his reply by insinuating in a somewhat ironic tone that reviewer comments are to be taken as valid because their status grants them this prerogative. Miguel then goes on to recognize that the manuscript he had sent to the journal may have been abnormally long for an English-medium publication, thus attributing this difficulty to the influence of the rhetorical

tradition of his L1. However, by stating that the excess text was intended to “support the paper’s arguments by citing the existing literature and then contradicting it from one end and then another,” he echoes either Dudley-Evans’s Move 1 (introduction), in which authors often refer to previous research that is believed to be pertinent to the present study, or Move 5 (Reference to previous research), an opportunity for scholars to compare their research to what has been done previously in the field either to support their own findings or to compare their claims and explanations to existing ones.

It is true that when the RA was submitted to the third journal (Document Q), there were numerous instances in which concepts included in the introduction were once again taken up in this final section; in some parts, this information was related to the study proper but was somewhat obliquely related to the experiment itself, while in others background information was provided with seemingly excessive length. Also, the discussion section in this version opens with an introductory paragraph that is essentially a recapitulation of the findings expressed in the results section, and, breaking with conventional genre structure, did not tie these findings in with any analysis of these findings. These aspects of the discussion may have led Reviewer 1 to react negatively to the RA’s discussion section.

The published version of the article more clearly uses the results of the study to argue the main claims made by Miguel and his coauthors; as of the opening paragraph, we are reminded of the findings as evidence of the need to individualize hemodialysis treatment. A comparison between the opening paragraph of Document Q and Document X illustrates the evolution that led to this:

Document Q

The most interesting results of this study are fourfold. First, there is a significant increase in total serum calcium immediately after dialysis with a dialysate calcium concentration of 3 mEq/L, while no changes occur with a dialysate calcium concentrations [sic] of 2.5 mEq/L. Second, a concomitant significant decrease occurs in PTH levels immediately after dialysis with a dialysate calcium of 3 mEq/L, and a significant increase with a concentration of 2.5 mEq/L. Third, these postdialysis changes in serum calcium and PTH levels return to their baseline after 48 hours. Fourth in patients with pre-HD serum calcium below 8.75 mg/dl the dialysate calcium concentration recommended is 2.5 mEq/L.

Document X

[M3] The key finding of this study is that baseline pre-dialysis serum calcium predicts the post-dialysis serum calcium and PTH response to different concentrations of dialysate calcium. **[M7]** This allows the definition of specific cut-off points of pre-dialysis serum calcium that discriminate patients whose calcium balance will likely be positive or negative according to the dialysate [calcium] levels, thus providing a tool for the individualization of dialysate [calcium] levels. **[M3]** Thus, 2.5 mEq/l dialysate [calcium] should be used in patients with baseline predialysis serum calcium <8.75 mg/dl, and probably in those with <9.15 mg/dl in order to prevent a positive calcium balance. **[M3]** By contrast, 3.0 mEq/l dialysate [calcium] is more likely to result in an approximately neutral calcium balance (change in ionized calcium induced by dialysis within a $\pm 10\%$ range) in most patients with pre-dialysis calcium >9.15 mg/dl. These choices minimize the changes in ionized calcium and PTH in the course of the dialysis session. **[M7]** However, this guidance should be adapted to individual patient needs regarding calcium balance.

The second extract highlights the major findings of the study and offers the author's interpretation of these findings in terms of the purposes of the study. Leaving behind the previous formulation of the introduction within what Dudley-Evans refers to as the overarching three-part framework (1994, p. 224), the text now selects just one finding as key and develops that finding in a way that leads up to the claim regarding the appropriateness of individualization, realizing this in two cycles. The argument begins with an objective recapitulation of a finding (Move 3) and then, based on this finding, argues that, as in the study being presented, baseline values be used to arrive at an assessment of the appropriate treatment of dialysis patients which, as has been argued in the results section, should be an objective of effective dialysis treatment (Move 7, claim). The introductory paragraph then initiates a second move cycle, detailing the specific data produced in the study (Move 3, finding) that support the claim of the appropriateness of this approach, ending with a similar but related statement about the

appropriateness of cut-off points (Move 7, claim) but also cautioning that individualization is necessary and that numeric indications like these cannot be used blindly (Move 7, claim).

In addition to further emphasizing the issue of individualization through such statements as “Thus, individualization of dialysate [calcium] is mandatory” and references to the need to “adapt calcium balance to individual patient needs,” Document X includes three acknowledgments of the limitations of the study (Move 8). This move was absent from previous versions of the text, and are used to acknowledge reviewer criticism concerning the authors’ failure to include certain experimental parameters in their experiment and the inability of the study’s findings to shed light on the issue concerning which of the variables tested is the most appropriate indicator to be used in hemodialysis patients.

Attitudinal aspects

A senior nephrologist with over twenty years of experience in the field and a devoted teacher and researcher, Miguel has an overall positive attitude toward English as a whole and believes the language to be fundamentally important to his profession. Publishing in English poses a substantial challenge for him, however, although his difficulties have recently been reduced substantially thanks to Óscar’s assistance. Indeed, this RA marked one of the first such collaborations between Miguel and Óscar, and from Miguel’s assessment of this relationship I understood that he now feels substantially more confident when writing academic texts in English.

When asked about the decision of whether to publish this text in Spanish or English, Miguel replied that he chose to write the article in English to increase its potential readership. He targets international journals in English for papers that he considers to make the most important contribution to the field. However, he has published extensively in Spanish, and one of journals he publishes his Spanish RAs in is a bilingual journal, providing translation into English at no cost to the authors. For this reason, he considers English to be an essential skill for his field of work, and he has little fear of presenting his research in English during high-level academic conferences and meetings.

Throughout the interview, Miguel made frequent reference to the difference between spoken and written academic English, highlighting the difficulties he experiences with written communication in English, including such issues as the comparative succinctness of English compared to his native Spanish and his incomplete knowledge of certain turns of phrase that are particularly useful when writing about research. Manuscripts of his have sometimes been rejected by English-medium journals because of perceived deficiencies in the way the texts were written, and he believes he is not fully aware of what the precise expectations of journals are. Miguel feels he is at a disadvantage relative to colleagues of his who live and work in Anglophone countries, and that this disadvantage is mostly because English is not his native language and also because of the great competition among researchers wishing to publish in English. For this reason, publishing in English requires a substantial effort from him, and he must shoulder this burden by devoting even more of his free time to research projects and to writing them up in English.

Aside from language-related factors, Miguel remarked that international journals published in English are generally reluctant to accept texts submitted by foreign research groups. When I inquired why this might be, he cited linguistic challenges as the primary cause. Despite this, Miguel also remarked that the quality of the manuscripts chosen for publication by some English-language journals is sometimes uneven, owing at least partially to the fact that the fate of a given text is often influenced by the choice of the reviewer, and that knowing the reviewer personally greatly increases the chances of acceptance.

In spite of the great effort that he must devote to publish internationally, Miguel has a strong vocation as a researcher, and he believes that a physician must strive to develop new ideas and teach in order to fully meet the expectations of the profession. Although the institution we both work for does have expectations that Miguel will have continued scientific output, his belief in the importance of research is independent of such external expectations. As analyzed at some length in this chapter, the support he now receives from Óscar has made the task of English-medium publication considerably more manageable, making it likely that he will continue to publish in English in the future.

Implications

Certain issues that have arisen in this TH make it particularly instructive for this study. First, Miguel's use of code-switching as a strategy to overcome his limitations in written academic English have both advantages and disadvantages. On the one hand, writing in Spanish allows him to correspond with his many collaborators in a way that helps the authors develop ideas in the early stages of a manuscript's development without limitations of language. Given that the text went through many different drafts and incorporated a great many contributions, had this business been carried out in English, some collaborators may have been more reluctant to issue their opinions because of their limited ability to do so in English. On the other hand, use of Spanish to write and to write about the RA may have hindered the text's progress toward publication, as Miguel indicated he had to "take the Spanish out" of the text on the sentence and the rhetorical level in order to move the article toward publication.

When remodeling the text, a number of the changes made are of particular interest from a rhetorical perspective. One concerns the great adaptability of the results section to accommodate a variety of rhetorical strategies. Using the results section, Miguel foreshadowed a central claim of the paper and argued for the validity of that claim, doing so in a way that framed these ideas within descriptions of procedure. In the results section, we also saw how the authors made small adjustments to satisfy the journal gatekeepers. This pattern suggests that the results section is highly sensitive to change in the post-submission stage and that matters often concerned with the methods or discussion section may be successfully articulated in the section.

Two individuals had a major impact on this RA—one being a coauthor and the other a journal gatekeeper. Miguel's colleague, Óscar, had a tremendously positive influence on this RA. Working on a level beyond lexicogrammar and other more superficial concerns, Óscar reformulated Moves, adding or removing ideas to best structure the paper's argument. This type of intervention is the product of both deep field expertise and an awareness of the RA genre; though not impossible, such revising would be an ambitious undertaking for a language broker and would certainly require great trust on the part of the author and prolonged engagement in order to capture the intended meaning and fashion an effective argument. Also, the skills that Óscar

brought to bear on the paper were especially crucial in adjusting the title, results, and discussion to give greater prominence to the issue of individualization. As for the influence of Reviewer 1, this gatekeeper also had a largely positive impact on the RA, although this was only possible because Miguel embraced the reviewer's suggestions so wholeheartedly. The evidence from this TH indicates that Miguel could have fulfilled his obligations to this reviewer by simply agreeing to modify the title, although what actually happened was a more sweeping revision of the text to foreground this key characteristic of the paper.

Text history 7: Juan and Rafael

Overview

This TH is of particular interest to my thesis because it involves not just one informant, but rather two researchers who publish together frequently, both of whom have used my services extensively. The informant interview for this TH was held in Spanish on May 14, 2015. Over the course of our 85-minute conversation, they voiced many of the same views on publishing in English, with one often elaborating on or seconding the opinions of the other. In opening up the interview to an additional author of this RA—written by a senior clinical allergist, Juan, and a biochemist/molecular biologist, Rafael—our discussion focused the personal accounts of these two researchers as members of their discourse community.

After two allergy journals declined to publish the manuscript without sending the text for peer review, the authors rethought their strategy, sending their work to a journal from a different but related field of the health sciences. This decision paid off, as the journal accepted their contribution. Along its path toward final publication, the paper changed substantially, though many of these modifications are attributable to the fact that the version sent to the first journal was a short communication, while when the paper was sent to the second and third journal, it was in RA form. The feedback received from journal gatekeepers was mostly centered on content, although some of the qualms that the reviewers had can be linked to key issues of NNES publishing and genre studies such as authorial presence and claims to novelty. Notwithstanding the added effort required to change from a short communication to a full paper, Juan and Rafael felt this text was comparatively easy to publish, especially because in this case the reviewers did not insist that additional experiments be performed.

Chronology and texts

According to my original plan, this TH was to study one of the many RAs I have been sent for review by Juan. However, despite the explanations I gave him and his fellow informants before beginning my research, when the time came for me to begin collecting the drafts from one of Juan's RAs, he told me he does not customarily save copies of drafts and correspondence from

his publications. Fortunately, Juan informed me that one of his close collaborators and co-author of numerous papers, Rafael, does keep very meticulous records of the Microsoft Word files created throughout the drafting process, so I sought Rafael's assistance in gathering these materials. Rafael, a researcher with a doctoral degree in biochemistry and molecular biology who is currently employed by the Immunology Laboratory of the IIS-FJD, offered me access to a wealth of materials with which to reconstruct the text's trajectory. However, of the two RAs authored by Juan that Rafael spoke to me of, the one that contained the most detailed information was one in which Juan had played a somewhat lesser role, and the most substantial contributions had been made by Rafael. Faced with the choice between faithfulness to my original research plan and a more nuanced look at how an RA fares in the publication process, I opted for the latter and asked Rafael to kindly take part in the informant interview. This turn of events made me realize that I had failed to adequately explain the requirements of my study guide to Juan. Future research using a similar approach should ensure greater care is taken to avoid such situations.

In all, Rafael gave me some 53 documents, including manuscript drafts, submission letters, extensive correspondence between collaborators, reviewer feedback, and even correspondence with the final journal's copyeditor. Since my thesis is concerned only with texts drafted in RA structure—that is, having an introduction, methods, results, and discussion—and given that the article submitted to the first journal was in short communication format and did not meet the requirements for inclusion in my study, I attempted to maintain my focus on the RA without renouncing any relevant observations from the text in its previous form. In particular, I was eager to explore how the changes to the paper may provide insight on the distinctive characteristics of the RA. Therefore, as will be apparent in the following sections, part of the interview itself and post-interview correspondence were devoted to fleshing out this issue of the different subgenres that researchers can choose for their publications.

In spite of Rafael's extensive archiving, establishing the chronology of the 53 texts he provided me with was an arduous task. On a number of occasions in which the document file names did not provide adequate information on the order in which the texts had been produced, I had to resort to the document metadata to determine when a particular draft was saved, and even so

there still remains some margin for error. However, I am confident that any possible inaccuracies in this assignation process had little impact on the conclusions reached in my analysis.

As with other THs appearing in this thesis, the move analyses that drove the text-analytical component on my study were rooted in a number of milestone versions that helped me to gain a rough idea of how the text had changed over time. Deciding which texts would be considered milestone drafts required that I make certain adaptations to my study design, however. The change from short communication to RA format would have complicated any attempt at analysis across the entire trajectory of the paper; at the same time, omitting all drafts in short communication form would have caused this study to lose part of its empirical faithfulness. Therefore, as depicted in the box below, I have expanded the number of milestone versions from four to seven, adding the three drafts (Documents A, B, and D) in short communication form to the usual series of milestone texts (the milestone versions appear in bold).

- 1. Document A: First draft of the text (short communication format)**
2. Document B: My edit of the first draft of the text (short communication format)
3. Document C: Submission letter sent to Journal 1
- 4. Document D: Version sent to Journal 1 (short communication format)**
5. Document E: Rejection letter from Journal 1
- 6. Document F: First draft of text (in IMRD format)**
7. Document G: Email correspondence between coauthors
8. Document H: Second draft of text
9. Document I: Third draft of text
10. Document J: Fourth draft of text
11. Document K: Fifth draft of text
12. Document L: Sixth draft of text
13. Document M: Email correspondence between coauthors
14. Document N: Email correspondence between coauthors
15. Document O: Submission letter sent to Journal 2
16. Document P: Version sent to Journal 2
17. Document Q: Rejection letter from Journal 2
18. Document R: Eighth draft of text
19. Document S: Submission letter sent to Journal 3
20. Document T: Version sent to Journal 3
21. Document U: Response from Journal 3 (request for minor revision)
22. Document V: Tenth draft of text
23. Document W: Eleventh draft of text
24. Document X: Twelfth draft of text
- 25. Document Y: My edit of the twelfth draft of text**
26. Document Z: Thirteenth draft of text
27. Document AA: Correspondence between coauthors
28. Document BB: Draft of response to reviewers (with my revision of the response)
29. Document CC: Submission letter for revised draft sent to Journal 3
30. Document DD: First revision of text as sent to Journal 3
31. Document EE: Response from Journal 3 (request for second revision)
32. Document FF: Fifteenth draft of text
33. Document GG: Sixteenth draft of text
34. Document HH: Submission letter for second revision sent to Journal 3
35. Document II: Seventeenth draft of text
36. Document JJ: Revised version of text as sent to Journal 3
- 37. Document KK: Final published version of text**

Evolution of the text and the role of literacy brokers

As mentioned in the previous section, Juan played a more minor role in writing this text than in other RAs he has asked me to review, and Rafael wrote the majority of the manuscript himself. Building on the idea of the text's first author—a researcher from a different research center in the region of Madrid—Rafael set about drafting the text based on clinical experiments performed by this external co-author. For his part, Juan took on a more advisory role, critically reading drafts sent to him by Rafael for aspects more strictly related to his area of expertise, making handwritten comments on hard copies of these drafts and then discussing these issues with Rafael in person. Following these face-to-face encounters in the hospital, Rafael would incorporate any modifications in subsequent drafts, though most often there was no written record of these interactions. When asked why he preferred to comment on co-authored manuscripts on paper, Juan answered that he prefers for his views be taken as proposals for debate rather than orders. And in response to my question of whether he ever regrets not keeping more detailed records like Rafael does, he replied: “Always.”

This article was rejected by two allergy journals before it was eventually accepted for publication by a third journal, focused on dermatology. Two rounds of revision were required by the journal. Citing issues with the content conveyed in the manuscript, the two allergy journals declined to send the manuscript for peer review. The first rejection letter was as follows (Document E):

Unfortunately, your manuscript entitled [title redacted] is not acceptable for publication in [journal name redacted].

According to the editorial policy, [journal name] consider manuscripts dealing with allergens only if they describe novel IgE-binding proteins or if they present relevant progress with respect to the state of the art knowledge about a given allergen. LTP is a well known allergen and therefore your work, although technically sound, cannot be accepted for publication in [journal name].

In Document Q, the second journal expressed its refusal to publish the manuscript in the following way:

Your paper was considered during our initial screening procedure. In the context of competing manuscripts, the editor regrets to inform you that the manuscript is not acceptable for publication in [journal name redacted].

The editor felt that allergic reactions to melon were relatively uncommon not least as evidenced by the small number of patients in this study and that an allergen characterisation study did not reach priority for publication in [journal name].

In summary, the first cited a lack of novelty or “relevant progress,” while the editor of the second journal believed the findings to be unrepresentative due in large part to the small sample size.

Here, it is worth noting that before submitting the text to the second journal, Juan and Rafael reworked the text from a short communication to the longer RA format. (Short papers have a reduced length and receive faster decisions from editorial boards (Hyland, 2000), although information on common rhetorical patterning and internal structuring is scarce.) To make this change, they divided the manuscript into the four-section IMRD format, lengthening the text considerably in the process. Though I shall leave a more in-depth analysis of how they transformed their original paper into an RA—or, as Juan and Rafael referred to it, a “full paper”—for the next section of this chapter, a number of remarks made by the informants are pertinent to my analysis here of the evolution of the text and the agents who had a hand in how the paper’s characteristics came to be. First, the authors pointed out to me during the interview that the first journal was the most desirable of the three and that both Juan and Rafael had previously published short communications in the journal. Second, the relatively small number of patients included in their study dissuaded them from writing up their findings in a lengthier format, as they believed the first journal would surely have deemed their submission to be of insufficient merit for a full paper because of this limitation. And last, both the second and third journals to which the text was sent do not publish scientific findings as short communications, and therefore the authors had to decide between expanding the text to make an RA out of a short communication or rethink their study in some more fundamental way. Convinced of the value of their findings but pressed to publish frequently and in journals of sufficient stature, they chose to rework their text into an RA. Though they would have preferred to secure publication in the first journal, Juan and Rafael assured me this was a minor compromise.

Another factor that had an impact on the development of this RA is the authors' decision to submit their RA to a dermatology journal after their first two unsuccessful attempts. When I asked Juan and Rafael to explain this decision, they answered:

Rafael: There are certain studies that you can publish in a number of different specialties. In this case our choice of journals was obviously...you always try to publish in the top allergy journal, not because anyone tells you to but because of the demands of the field. Whenever you fail to publish at a certain level of journal prestige within your area, you don't go any lower. You know, these journals are part of a ranking system according to their impact factor. So whenever you go below a particular impact factor you don't keep going down; instead, you try to publish your paper in another area that has an impact factor that is higher than or at the same level so you can, say, maintain the standards that the institution asks us to. So right now they are asking us to publish in the first decile or quartile. You know what that is, right?

Oliver: Yes.

Rafael: So what I mean is, if I am trying to publish in the first decile or the first quartile in allergy and I get rejected, then if I have to keep going to lower-impact journals, then I'm in the second quartile. And I'm not interested in that. So what do I do? I go to another specialty. In this case, because this paper fit in dermatology, I sent it to this journal.

Having previously adapted their paper to the RA format, this shift to a target journal outside of their field of specialization was the second compromise made by Juan and Rafael in their effort to publish this paper. Adapting their work for a different subgenre in the first case and then a new audience in the second did not involve a major overhaul to the paper itself, however; indeed, even the submission letters remained much the same. The letter sent to the second journal (Document O) is unchanged from the letter sent to the first journal (Document C), and then the only difference in the submission letter sent to the allergy journal (Document S) is the addition of two sentences at the beginning of the text, as shown below (the text added in Document S is underlined):

The relevance of contact allergy to plant-related food has recently emerged. Oral allergy syndrome is one of the most characteristic symptoms of fruit allergy, but it also causes systemic reactions. Recently, the number of patients reporting contact allergy with melon peel has increased strikingly. The present study marks the first time that melon-peel LTP has been shown to be both a major allergen and responsible for contact urticaria patients [sic] sensitized to this protein. Sometimes, allergic reactions attributed to melon pulp ingestion are really caused by contact to the peel of this fruit while eating it. This kind of patients could tolerate melon pulp ingestion if they are not sensitized to this part of the fruit, but only to the peel. This finding has an important clinical relevance as it may improve the diagnosis, biomarkers of allergy diseases or future immunotherapy treatment for these patients.

Following two rounds of revision, this dermatology journal agreed to publish the text. In suggesting that the journal accept the paper, Reviewer 1 (Document U) writes, somewhat perfunctorily,

GENERAL COMMENTS:

Although not particularly surprising in its findings (particularly in view of the previous report by the same group (Ref 9) the study is properly performed, methods are adequate, and conclusions are supported by the produced data.

Echoing the assessment of the editor of the first journal, Reviewer 1 remarks that this RA tends toward findings that are known by the community and adds little. Though not proposing any specific changes to the claims or the general ideas in the paper, this reviewer nonetheless seems to relegate this RA to a category of article in which researchers confirm the work of others, thereby denying its novelty. These two views contrast with the critique made by the editor of the second journal, who dismissed the findings as “uncommon” on account of the small sample size. Taken together, it appears that the gatekeeper of the second journal interpreted the manuscript in a very different way than the first and the third. This apparent arbitrariness in feedback may have caused Juan and Rafael to question the quality with which their RA was evaluated, thus reinforcing a number of negative opinions on the peer-review process that the two voiced during the interview.

I revised the manuscript on two occasions: once before the text was sent to the first journal as a short communication (Document B) and, as appears in Document Y, again in the version sent to the third and final journal. In the first of these edits, my work focused primarily on word- and sentence-level aspects as well as certain matters of consistency of style. The second of these rounds of editing had a similar focus, and included a number of comments about fragments that appeared to be unclear. Aside from the article proper, Juan also sent me a draft response to the reviewer comments received from the third journal, and this revision of mine appears as Document BB. (This request to edit the point-by-point letter brings to mind an anecdote Juan told me a number of months after the informant interview. As he tells it, many years ago, he was chided by an academic journal for having responded to reviewer comments in a way that they found to be inappropriate, although Juan had no intention of phrasing his remarks in an offensive manner. He has not forgotten this unfortunate experience, and often asks for assistance when wording his response to gatekeepers to keep this from happening again.)

Juan always sends his English-language manuscripts for editing before submitting them to journals, and Rafael said that he often does, though not always. This particular publication was somewhat peculiar in this regard, however, as in one of the e-mails exchanged between Rafael and another co-author, the co-author mentioned that the text was to be sent to a language broker other than myself. When I asked Rafael why they had made this decision, he replied that to his recollection, at this particular moment I had a substantial backlog of work and could not review their manuscript as promptly as they needed. Despite the contributions of not one, but two editors, however, the third journal nonetheless criticized the quality of English, citing “[difficulty] to read in some parts” (Document U), requesting that the authors have the article “revised by a native English speaker.” Juan and Rafael replied diplomatically, writing, “Following your suggestions, the manuscript has been revised by a native English speaker who specializes in medical translations.” Juan and Rafael both cast doubt on these types of remarks by journal gatekeepers, adding that reviewer reports sometimes contain glaring errors, which suggests that these reviewers themselves are NNES members of the discourse community. As they put it, gatekeepers sometimes criticize the English of their papers merely because they are not from Anglophone countries, doing this only to “cover their backs” as overseers of the language quality

of manuscripts. If Juan and Rafael are correct in their assessment, NNES reviewers subject colleagues to the same discriminatory treatment they most likely have received from other peer reviewers, some of whom may also be from non-English L1 backgrounds. Rather than defend others like them, they seem to internalize and perpetuate the stigma of inferiority attached to NNES academic writers. Indeed, NNES reviewers may actually be more demanding of English language quality than their NES counterparts.

This was one of several comments made by Juan and Raphael in which they questioned the rigor of the peer-review process; in fact, the two sometimes have doubts on not only the ability of referees to judge linguistic quality, but also their expertise in the scientific content. One of the anecdotes that came up during the interview went as follows:

Rafael: Your hands are tied. Whatever the reviewer says, goes. Keep in mind that a you can't contradict a journal or reviewers. You can, however, get around [their points of criticism], but you have to do what they say.

Juan: In fact I once met a researcher who published widely and had a lot of experience and he used to tell me, "write back and say that you've had the English revised, but send him the paper as is"...

Rafael: As is.

Juan (laughing): And then they didn't say anything else about it (*laughs*).

Rafael: It's true.

In commenting on this passage, I will postpone certain remarks until the next section. What is pertinent to the matter being discussed here, however, is the degree to which these remarks suggest that these comments are often apocryphal. Indeed, in the first few minutes of the participant interview in which I asked the co-authors to summarize the experience they had in drafting and securing publication of this RA, their highly critical opinion of the peer-review process became immediately apparent when they described the process using such adjectives as "nonsensical" and "tedious" and spoke of having to withstand the "stupidity" of certain reviewers and their "erroneous" interpretation of manuscripts owing either to their failure to even read them or their overall ignorance of the concepts being discussed.

Some of the recommendations made by the reviewers of the third journal were taken up by the co-authors of the paper, though not all of them. As referenced above, Document U contains the response from this journal, in which the authors are encouraged to make minor revisions to the text and re-submit the manuscript for consideration. Here, the same reviewer who found fault in the writing of the text (Reviewer 1) also offers a number of sentence-level corrections to the text, and the authors incorporated these modifications without comment. One fragment of this reviewer's comments reads as follows:

Discussion. The first sentence is confusing in its present form. Maybe (...) is better.

Lines 226-229. Which study are the authors referring to? In the present one I wasn't able to find any cross-inhibition experiment between peach and melon LTP.

Although the issues referenced in these comments do not concern the text's rhetorical makeup, we see here how the reviewer shapes the text rather than simply critiquing it, thereby taking on a more active role in its creation. An even greater contribution is made by Reviewer 2, who suggests that the title of the work be changed, stating: "The title should be more specific mentioning [sic] melon allergy (rather than food allergy) as it is the main subject of the study."

While the authors promptly agreed to this slight change in the title and the minor sentence-level edits proposed, they did not agree with the other two suggestions made by the second reviewer. These remarks by the reviewer were phrased in a way that is difficult to interpret, suggesting that this reviewer is also a NNES academic:

With regard to the study design, I have two comments which may be considered in the discussion section as potential limitations of the study. First, the study would have been more comprehensive if an oral challenge with purified melon pulp was [sic] performed to confirm that the oral symptoms experienced by the patients are strictly due to LPT, especially that [sic] some patients had profilin sensitization. Another study limitation is that the contact challenge test with the melon peel extract was performed in 1/3 of patients only in which [sic] it reproduced the symptoms hence providing a correlation between history, serology/SPT, and clinical allergy in each individual case tested.

In their response to this feedback (Document CC), the authors refute the validity of the first point by citing another published study that justifies the diagnostic criteria used and challenging the

reviewer's claim that LPT is "involved in the elicitation of oral allergy symptoms to melon pulp, so far." In a follow-up conversation I held with Juan to confirm my understanding of what the reviewer was asking, he explained to me that this comment indicated that the reviewer had misread certain passages in the text in which the authors expressly stated that the aim of their work did not encompass oral allergy and that, while such an inclusion would have been possible, it would have called for a much broader investigation. When responding to the second of these supposed study limitations, they explain in the point-by-point response that, working with a limited sample size and a small amount of purified protein, their paper contained a well-referenced justification of the fact that contact challenge was only performed in one-third of the patients included in their sample. Adding a very brief reference to this fact in the revised study, Juan and Rafael were able to convince this reviewer of the procedural and rhetorical soundness of their study.

Genre considerations

As mentioned at some length in previous sections of this chapter, this TH is unique in that it traces the textual reorganization done by Juan and Rafael both from the subgenre of the short communication to the RA and then from an allergy journal to a dermatology journal, thus revealing how the fate of these researchers' work was conditioned by both the research outlets afforded by the genres and the decisions of gatekeepers. The present section of this chapter on the rhetorical traits of the texts will demonstrate the substantial influence that the genre had on rhetorical patterning, as well as the negligible impact of the change to a different speciality. Substantial rhetorical changes were made to the discussion and abstract that can be traced to feedback by post-submission academic literacy brokers. One of the primary challenges that Juan and Rafael faced with this text involved how to both mention their previous work on the topic while arguing convincingly that this paper merited a separate publication. This struggle can be seen in both the pre-submission and post-submission phase. Another compelling feature of this TH is the way in which it captures Juan and Rafael's strategies for publishing RAs and short communications. Evidence presented here will be used to make the case that the short communication privileged descriptions of results over aspects related to methods, and that in

expanding their previous paper to the RA genre, they did so by adding details on methods and also background information, especially in the abstract, introduction, and discussion. This move across the genre network (Swales, 2004) bears similarity to the phenomenon of regenring (English, 2011), in which texts are repurposed within a different genre or subgenre.

As I began my conversation with Juan and Rafael I was intent on learning about their views on the differences between a short communication and a full paper. I struggled, however, to move our discussion past issues such as differences in word length or depth of description. In general, their answers indicated that an RA is an expansion of a short communication or, conversely, that a short paper is a condensed RA. The language used by Juan and Rafael in the following fragment illustrates this:

Oliver: And what procedure do you follow when adapting a text...a short communication to a full paper?

Rafael: Expand.

Juan: Develop the sections of the paper. The two are practically the same, only an RA is more developed.

Rafael: Add figures.

(...)

Juan: When you get down to it, it's almost more difficult to write a short communication.

Rafael: Yes, because you have to abbreviate a lot. The introduction makes you abbreviate a lot.

Juan: Shorten and summarize everything a lot.

Oliver: So the primary idea is a full paper, which is then abbreviated to make the text into a short communication.

Here, Juan appears to indicate that the two types of text are virtually the same, although a short communication is an abridged version of the a full paper, or RA. And although the language they used here may have been influenced by the way in which I posed the question—that is, because

of an assumption that a full paper is derived from a short communication—it can be interpreted from this exchange that their use of verbs such as “expand,” “develop,” “abbreviate,” “shorten,” and “summarize” indicates that their default notion of academic writing within their discourse community is the full paper rather than the short communication. Indeed, I made the last statement (transcribed above) to synthesize what I gathered to be their opinion, and they did not challenge my summary. However, the post-interview correspondence I held with Juan and Rafael left me somewhat unconvinced that we had fully understood each other, especially in the following email response I received from Rafael: “A short communication, rather than a full paper in condensed form, I think of it as being an article containing preliminary results but with great relevance. In the case at hand, we had a small cohort of patients, which is why we made the choice [to write the paper in short communication form].” The authors did allow that the RA format is their most common choice of submission, and Rafael’s comments above suggest that they would have opted for an RA if they had had a sufficiently large patient sample.

The text’s abstract underwent a substantial degree of change that mirrors in large part the changes made to the body of the text to make it an RA. These modifications can be summarized as a) separating methodological content from the results of the paper so that the abstract no longer flows from experimental steps directly into the results observed from these steps; b) the addition of background information; and c) a highly significant repositioning of the study that was articulated in the abstract and discussion.

As mentioned in the section “Evolution of the text and the role of literacy brokers,” one of the reviewers of the final journal worded their report in a way that comes across as less than enthusiastic, stressing that the study provided little novelty and could best be characterized as a confirmation of the authors’ previous work. In the second round of revision for this journal—for which I was unable to obtain documented evidence—Juan and Rafael revised the concluding remarks in both the abstract and discussion to reflect the reviewer’s assessment. In its earlier form (Document II, or the penultimate draft), the final paragraph of the abstract begins as follows: “This study demonstrates for the first time that melon peel LTP has been shown to be both a major allergen and responsible for contact allergy.” This, however, was changed to “This

study confirmed our previous findings that melon-peel LTP is a major allergen and is responsible for contact allergy.”

Once the paper was reworked as an RA, the rhetorical structure of the introduction remained virtually untouched by any of the agents who took part in the process. One of the substantive themes that emerged in peer review concerned the issue of novelty given the overlap between this RA and a previous paper written by the same group of authors (and cited in this work). Aside from this issue, which only translated into a slight modification of the abstract, the fact that the changes first made to the text when the paper was refashioned as an RA for an allergy journal were left untouched when the RA was sent to the dermatology journal indicates that the constraints of the genre held substantial sway over the shape of the text, and also that the differences between the two disciplines and their disciplinary communities were negligible. Both authors confirmed this during a post-interview discussion.

When articulated in the short communication subgenre (Document D and previous drafts), the introduction offers a highly concise account of background issues. Then, as the authors shifted to the IMRD structure, they added to this rhetorical skeleton considerably. A comparison of the introductory content contained in Document D and the version sent to me for editing once in RA form (Document X) illustrates this. (By way of contextualization, at this part of the introduction, the authors have already established the background of the issue (Move 1, establishing a territory) and are here establishing a niche by indicating a gap (Move 2, step 1A).

Document D

Recently, we have seen a large increase of patients reporting contact allergy to melon peel. In light of this increased prevalence, the present study was carried out in order to investigate whether one particular main melon-peel allergen is responsible for these symptoms.

Document Y

Allergic reactions after eating melon (*Cucumis melo*) are frequent. Oral allergy syndrome by [sic] ingestion of melon-pulp is the main manifestation and systemic reactions have scarcely ever been reported³⁻⁵. Contact urticaria has been described as a typical clinical manifestation of peach peel allergy and it has been reported that it might be associated with sensitization to LTP (...). Up to now, several allergens have been identified in melon-pulp: (...). Recently, our group was the first to describe the involvement of two new proteins (a lipid transfer protein (LTP) and a thaumatin-like protein (TLP)) from melon peel in a case of contact urticaria and angioedema to this fruit¹¹. The family of LTPs is widely distributed in plant-related food. The first LTP described was found in peach¹². It is a low molecular weight protein (around 8-9 kDa) that caused severe symptoms not associated with hay fever. This protein was designated as Pru p 3 and has a high cross-reactivity to LTPs from other fruits which belong to the botanical Rosaceae family. However, LTPs have been described in botanically unrelated vegetable foods and the homology between them is very heterogeneous¹³.

The present study was carried out in order to investigate whether one particular major melon peel allergen turned out to be responsible for these symptoms.

In the example presented here, what was once a brief observation of the increasing prevalence of this allergy now arrives at this same point by starting from a broader perspective, that is, an explanation of the more common manifestation of melon allergy (i.e., to eating the fruit). The authors then mention allergens in the peel of other types of fruit that have been known to cause contact allergy, thus centering the issue further by limiting discussion to the peel. Next, the discourse returns to melon, offering an overview of the allergens known to be present in melon, which sets up the sentence from the original draft on contact allergy to the peel of melon.

The rhetorical structure of the introduction has a marked presence of Move 1 (Establishing a territory), and the transition to Move 2 (Establishing a niche) is somewhat difficult to perceive. Following a self-mention in which Juan and Rafael cite a recent study of theirs in which they described “involvement of two new melon-peel proteins” in a single case of contact urticaria to

the fruit, the authors make the transition to Move 2 in the following paragraph, after which they close the introduction by presenting their study (Move 3). To illustrate this, I include the relevant excerpt from the final published version (Document KK).

(...) **[M1]** Recently, our group was the first to describe the involvement of two new melon-peel proteins (one LTP and one thaumatin-like protein) in a case of contact urticaria and angio-oedema to this fruit.¹¹

The family of LTPs is widely distributed in plant-related food. The first LTP to be described was found in peach.¹² It is a low-molecular-weight protein ($\approx 8\text{-}9$ kDa) that caused severe symptoms not associated with hay fever. This protein was designated as Pru p 3 and has a high degree of cross-reactivity to LTPs from other fruits that belong to the Rosaceae family. However, **[M2, S1B]** LTPs have been described in botanically unrelated vegetable foods, and there is little homology between them.¹³

[M3] The present study was carried out in order to investigate whether one particular major melon-peel allergen is responsible for these symptoms.

After mentioning their previous work in which they demonstrated the involvement of two allergens in a single case of contact allergy to melon peel, the next paragraph links this protein, LTP, to allergic reactions to foods containing the protein, and, they suggest, “botanically unrelated vegetable foods.” Building on the argument made previously, the niche established here is one that seeks to add to what is known (Step 1B) that is, by suggesting, in the final sentence of the paragraph, that similar severe reactions may be caused by LTPs. Having established this rhetorical sequence, the final paragraph then presents the work (Move 3), hypothesizing that melon-peel allergens may have this effect.

The self-mention is prominent in the rhetorical structure of the introduction, and the previously mentioned comment made by Reviewer 1 (Document U), which challenged the novelty of the study given the authors’ previous publication, appears to cast doubt on the claim to novelty being made by Juan and Rafael. As pointed out by the reviewer, if one case of an allergic reaction to a particular food had previously been demonstrated, this new experiment provides broader evidence of the same finding.

Though the order of sections in IMRD RAs would indicate that my next object of analysis should be the methods section, my observations from this paper call for an exception here. Particularly,

one paragraph contained in the earliest draft of the text appears almost entirely unchanged in the final published version of the text. The placement of this stretch of text, the fact that it did not spark criticism from journal gatekeepers, and the way in which the authors handled this information in the methods section sheds light on the strategy used by Juan and Rafael and raises a number of questions on the relative importance of the different sections of IMRD-formatted papers. The paragraph in question, which appears in the first half of Document A—which, it should be recalled, contains no sections—is included below, followed by the same text as it appeared in published form.

Document A

To search for possible allergens, a melon peel extract was prepared by homogenization in phosphate-buffered saline, dialyzation, and lyophilization. Potential allergenic components of melon peel extract were detected by IgE immunodetection after SDS-PAGE separation. An IgE-binding band about ≈ 7 -8 kDa was identified in all of the patients (Figure 1). This band from the melon peel extract was manually excised from the gel, digested with trypsin, and analyzed using MALDI-TOF and LC-ESI-IT (LC-MS/MS) following the methodology of Pastor et al⁷. MS/MS was performed and a sequence of internal peptides was obtained: SLNQAATTADRR for and 8 kDa protein, identified as the same melon peel LTP we had described before.

Document KK

Sodium dodecyl sulfate-polyacrylamide gel electrophoresis and immunoblotting

The potential allergenic components of melon-peel extract were detected by IgE immunoblotting after SDS-page separation. An IgE-binding band with a molecular mass of ≈ 7 -8 kDa was detected with all the patients' sera (Fig. 1). This band from the melon-peel extract was manually excised from the gel, digested with trypsin, and analysed using MS/MS. MS/MS was performed and a sequence of internal peptides was obtained, SLNQAATTADRR for an 8-kDa protein, identified as the same melon-peel LTP we described previously.

I will argue below that the methodological details contained in both of these excerpts were developed in the results section of the IMRD paper. What is pertinent to the present analysis is the fact that the fragment from Document A is the only reference to these methodological details contained in the paper and also the fact that the authors placed this language in the results section of the RA rather than the methods. This suggests that short communications are more

focused on results than methods, allowing authors to allude to methodological issues without providing greater detail. Another implication of the development of this text is that the results section was the core idea that Juan and Rafael wished to convey, and one that served both the short communication and RA genres. Overall, the results section was not changed almost at all once the paper was reworked as an RA, suggesting that it was sufficiently clear and well-written for the journal gatekeepers.

Once drafted as an RA, the methods section—here called “Materials and methods”—remained virtually intact. As I argued in the foregoing analysis of the results section, some of the key methodological concepts were articulated in a way that was appropriate for either a short communication or for inclusion in the results section. Once in IMRD form, however, Juan and Rafael expanded on these concepts. To illustrate this, I will use the opening sentence from the same passage from Document A excerpted above, showing how this was treated in the RA methods section.

Document A

To search for possible allergens, a melon peel extract was prepared by homogenization in phosphate-buffered saline, dialyzation, and lyophilization.

Document KK

Preparation of the melon-peel extract

Melon peel was cut into small pieces and homogenized in phosphate-buffered saline (PBS, 100 mmol L⁻¹ phosphate buffer 100 mmol L⁻¹ NaCl; pH 7-5) at 30% (w/v); the proteins were extracted by agitating with magnetic stirring for 3 h at room temperature. The sample was centrifuged at 10 000 g for 30 min and then dialysed against distilled water at 8 °C using dialysis tubing. Protein extracts were lyophilized and stored at 4 °C. The total protein content was determined using the Coomassie Plus (Bradford) Assay (Pierce, Rockford, IL, U.S.A.).

Both of these passages can convincingly be coded as Move 5 (Describing data-collection procedure) under the framework of Nwogu (1997), though the second goes into substantially greater detail on the particular steps taken, giving fuller treatment to constituent elements such as a) identification of the main research apparatus and b) recounting the experimental process. Furthermore, a comparison of the two passages brings to mind comments made by both Juan

and Rafael that described the process as one of “expanding,” “developing,” and “adding.” As the RA was filled out in part by adding detail to the methods section, it can be argued that the methods section is of lesser importance in the process of rhetorically shaping this RA.

The Materials and methods section is divided into seven subsections, each one focused on a particular procedural step. Other than the first subsection, titled “Patients,” the other six subsections contain elements described in Nwogu’s Move 5, in which researchers describe experimental procedures. None of the moves in this Materials and methods section make reference to the statistical methods used to interpret their data (Move 6), although this absence is consistent with the observations made by Nwogu in that the study is largely qualitative rather than quantitative. In the numerous instances of Move 5, most make combined reference to both Constituent Element A (Identification of Main Research Apparatus) and Constituent Element B (Recounting the experimental process). Some moves can be attributed to Nwogu’s Element C (Indicating criteria for success); these are primarily statements on the steps taken by the research team to ensure the validity of the data they used in their experiments, and in this regard may be considered quality-control measures. An example of one of these statements is underlined in the following excerpt taken from Document F:

SDS-PAGE

SDS-PAGE was carried out using the Hoefer SE 600 electrophoresis system (GE Healthcare). Polyacrylamide concentrations of 14% (w/v) and 5% (w/v) were used for separating and stacking gels, respectively. 20 µg of protein extract protein [sic] were applied per lane. To ensure proper protein separation and visualization, the gels were stained with PageBlue Protein Staining Solution (Fermentas International, Inc., Canada) or used for immunoblotting as described below.

In opening the last sentence with the “to- infinitive” clause functioning as an adverbial subordinate clause of purpose acting as an adjunct in which the infinitive verbs is “ensure,” the reader understands that the staining referred to in the main clause is performed to guarantee the success of this component of the experiment, thus making this an example of Element C.

Unlike the other sections, the discussion was reworked in significant ways, and especially as a result of the academic brokering done by the reviewers of the third and final journal. This feedback prompted Juan and Rafael to modify one of the knowledge claims of the paper, which in turn brought them to rephrase one aspect of their findings as a limitation and also recommend further research into the issue. Independently of this reviewer feedback, the revisions made before the time the RA was sent to the second journal also provide evidence of the authors' awareness of rhetorical convention in the discussion section.

Both as a short communication and as an RA, the text makes sparing reference to the actual findings. Indeed, much of the rhetorically relevant changes made as the paper was brought into IMRD format involved expanding on background information, as can be seen in the following comparison between the version sent to the journal as a short communication and the second draft produced in IMRD form (Document H):

Document D

LTPs are very highly conserved and have a molecular weight of around 9 kDa. They are usually involved in systemic allergic reactions due to vegetables, and the homology between LTP sequences from botanically unrelated vegetable foods varies from 35% to 95% (8).

Document H

LTPs are very highly conserved proteins and have a molecular weight of around 8-9 kDa with high stability to thermal processing and to pepsin digestion. These properties makes [sic] them potent food allergens and explain that typically [sic] induce systemic symptoms after ingestion. They are polypeptides widely found in the vegetable kingdom and are implicated in the formation and the defense against pathogens (PR-14). These proteins have been identified in Rosaceaes, other vegetables [sic] foods and in pollens. The high rate of sensitization to vegetables in the Mediterranean area can be explained by sensitization to panallergens (profilin and LTP) and the important degree of cross-reactivity between each of these proteins [citation]. Homology between LTP sequences from botanically unrelated vegetable foods varies from 35% to 95% [8]. For example, Tri a 14, the LTP from wheat, that [sic] has been described as the major allergen associated with baker's asthma caused by wheat flour sensitization, has shown poor cross-reactivity with its peach homolog in some patients [16].

Studying the events surrounding this change, I came across a margin note made by one of the external coauthors in Document J which provided part of this additional background information. This information and more was included in Document H and remained unchanged in the published text, and can be most accurately classified as Move 1. Juan and Rafael's extensive use of Move 1 (information move) both in the short communication and in the expansion of the discussion left me curious to know why explicit reference to their findings was left until the second half of the section, choosing rather to fill the section with a summary of related but different scholarship on the issue. As an example, I asked them why the second paragraph of the section in IMRD format does not discuss their actual findings, and Rafael replied that his strategy when writing discussion sections is to put results in context. Allowing that the second paragraph of the section was rather broad, he insisted that the text took up the study's findings in more direct fashion as it progressed.

One passage that was given a substantial amount of attention comes toward the end of the discussion section, where the authors relate their current and past work. As of the moment when the text was adapted as an RA, the authors attempt to simultaneously link and mark the distinction between the two studies, both of which investigate the same phenomenon. In the following excerpt taken from Document H, I underline the addition to this paragraph to illustrate how the text developed:

The first description of an LTP in melon peel was recently performed by our group [6]. The present study has demonstrated the clinical relevance of this LTP in a large group of patients with contact allergy to melon peel. In one of our previous work about melon allergy, the absence or scarce presence of this protein in melon pulp was pointed out [6]; this observation is consistent with the fact that the majority of patients who are only sensitized to melon peel tolerate melon pulp ingestion. Also, in this study the absence of cross-reactivity between LTP from melon and peach was confirmed by immunoblotting inhibition assays using the patient's serum pre-incubated with peach extract. In addition, immunoblotting assay using anti-Pru p 3 in melon peel extract did not recognize any bands.

Having previously described the allergen, the authors argue that their findings illustrate how it can affect patients. The added text (underlined) provides an account of the additional measures

taken by the authors to strengthen the empirical evidence, which serves to mark the distinction between this experiment performed in 14 cases and their previous report of a single case.

Despite these efforts, however, this passage drew criticism from Reviewer 1 in the first round of peer review. This reviewer made two comments on this passage, stating that a) the authors had not performed the experiments necessary to demonstrate the absence of cross-reactivity between LTP from melon and peach; and b) the forcefulness of Juan and Rafael's claim that peach melon LTP cross-reactivity does not occur could not be concluded from their data. Following this feedback, by Document X the second half of the paragraph was reworked as follows:

(...) was pointed out. LTP is a panallergen showing frequent cross reactivity among LTP from different sources, mainly rosaceae fruit and nuts. Nonetheless, this cross reactivity is variable among different sources and so Ole e 7 or Par j 1 are not included in this group²⁶. In our experience we were un able [sic] to proved [sic] cross reactivity between peel LTP from peach and melon, although other studies should confirm this matter.

What was before a general statement on the relationship between patients with melon-peel allergy and their lack of symptoms when eating melon (Document H, above) is walked back to a less specific statement on the allergen and its ability to show cross-reactivity in different food sources. Also, their previous claim on cross-reactivity (Move 7) is now an admission to the contrary (Move 4, unexpected outcome) followed by a recommendation for future research (Move 9). Though stemming from content-related criticism from the reviewer, here the authors phrase their revision in a way that is rhetorically conventional, expressing this information as a realization of Move 4 and Move 9.

In Document I, the authors include what Dudley-Evans (1994) refers to as an introduction to the discussion, which is part of an “overarching three-part framework” (p. 224) to this section. Here, Juan and Rafael allude to the knowledge claim (Move 7) made in the paper, previewing their major conclusions. In its earliest appearance in the text, it reads, “Results of this report provide evidence that melon-peel LTP was a major melon allergen for patients allergic to melon peel, but also, melon-peel LTP was a relevant allergen able to elicit contact urticaria.” Although this phrasing was revised and edited to adapt to the genre, this “nutshell” version of their findings

remains in all subsequent versions, introducing this central aspect of the discussion and the study as a whole.

Returning now to the supposed limitations of the study that I addressed in the previous section of this chapter, the fact that one of the reviewers for the third journal suggested that limitations be mentioned in the paper, together with the fact that the authors were reluctant to highlight their study's shortcomings, led me to ask Juan and Rafael if they customarily included such rhetorical moves in their manuscripts. Rafael first replied that the limitations are implicit in the discussion and do not require more direct mention. Juan, on the other hand, made the following statement: "When I see [that my work has limitations] I like to put them so they don't call me on them, and this lets me put greater emphasis on the results that I did achieve." This statement hints at a particular use of rhetorical moves to head off criticism about particular aspects of research that are less than ideal, thereby limiting the number of faults that can be remarked upon by gatekeepers. In doing this, it appears that Juan deftly positions his academic texts in a particular way so that his audience can focus on the contributions made at the same time that they recognize the rhetorical code used when admitting shortcomings.

The most significant impact of peer review occurred in the second round of revisions for the third and final publication. Although Rafael did not save this reviewer feedback, the reviewers requested that the authors rephrase the concluding comments of the discussion and abstract to reflect that the study did not present new findings but rather a confirmation of the authors' previous work. In earlier drafts, the final paragraph of the discussion began with the following sentence: "In conclusion, this is the first time it has been demonstrated that melon peel LTP is responsible for contact urticaria in patients sensitized to this protein." The revised sentence reads, "In conclusion, this study confirmed our previous findings that melon-peel LTP is both a major allergen and is responsible for contact urticaria in patients sensitized to this protein." Rafael stressed, however, that this change in the knowledge claim was of minor importance to him or his work.

Attitudinal aspects

Both Juan and Rafael have published extensively in English and recognize that the rewards system is set up in such a way that publishing articles like this one in Spanish would have hindered their chances of receiving continued funding. More and more, their grant applications and reviews of their scientific output prioritize aspects such as number of texts published, how well these articles are positioned (i.e., impact factor), collaborations with other research groups, patents, transferability, and overall visibility. With the recent cuts in funding for research in Spain, they have not found it more difficult to find bodies willing to pay for their experiments, although they have observed that these grants are too meager for them to pay for collaborators, thus increasing the burden placed on them. Securing funding from the European Union is becoming more and more important. Rafael believes, however, that EU funding is quite difficult to receive because of what he referred to as “closed lobbies” of researchers who are unwilling to relinquish their access to these funds.

Because their peers only read indexed journals and because virtually all indexed journals are English-medium publications, submitting texts to Spanish-language journals would dash their hopes of having their work read by others in the academy. This view echoes previous findings on the factors that lead NNES scholars to publish in English rather than their L1 (Lorés-Sanz et al., 2014; Martín-Martín et al., 2014). The two admitted that writing academic texts in English is more difficult than in their native tongue. When writing in English, Juan and Rafael feel they have to accept the impossibility of effectively communicating in English what they could say in Spanish, although this handicap is somewhat lessened by the fact that they can have their texts revised by a language broker like myself.

In spite of Juan’s admittedly negative view toward English as a whole, both he and Rafael derive a sense of satisfaction from their work. For Juan, investigations like the one described here allow him the opportunity to identify problems in his day-to-day clinical practice and then seek answers to these problems. This personal reward drives him to devote part of his free time on nights and weekends to research, as the one day per week he is allowed by the institution is insufficient for

him to have the type of scientific output he feels necessary. Rafael, on the other hand, does not have clinical activity to perform, and is able to carry out his research during the work day.

When our conversation moved to issues focused more on their overall experiences when writing in English for the Academy, Juan and Rafael both indicated that papers submitted from non-Anglophone countries are often met with resistance from editors and reviewers, with these small groups of inner-circle academics showing certain reluctance to accept contributions from the outside. Contrasting with Rafael's earlier assessment that the publication process is somewhat "nonsensical," they both believe that they understand the expectations of journals concerning the type of texts that merit publication, although they also stated that by knowing the editor personally, NNES authors' chances may be bolstered considerably. For his part, Juan believes that he has earned certain recognition within the international community of researchers in his field because of the work he has published; however, he admits that he does not participate much in the social side of these informal research networks and is unsure of how much his name recognition influences the actual process. Rafael feels he is becoming more and more recognized within international research circles, citing the increasing frequency with which journals ask that he review papers and the increasing number of times that, while attending international conferences, fellow attendees say they are familiar with his work. When asked whether the experience of publishing this paper would have been different if they had been from an inner-circle country, they both stated that it would have been easier, though this difference would be less attributable to linguistic factors than to more frequent contact with journal editors. Indeed, in responding to my question as to whether any of their manuscripts had ever been rejected outright because of perceived deficiencies in the quality of the writing, they both said that this has never happened to them, adding that they doubt this is the case for any of their peers.

Implications

This TH has shown how the pressure to publish frequently in high-impact journals led Juan and Rafael to draft a paper that was both highly similar their previous study, though with certain remarkable differences from a linguistic point of view. The limitations imposed by their small patient sample led them to attempt a non-RA publication in a highly prestigious journal, although

they were rebuffed in this attempt, as more than one reviewer saw little cause for a second publication, at least in the way it was being presented. Determined to achieve their goal, the authors compromised first by turning to the more common RA genre and then, once this proved unsuccessful, to a different field of specialization. As their paper neared acceptance, they then had to renounce their claim to novelty, accepting to publish their findings as a confirmation rather than a more significant and original contribution to the field.

Several comments made by Juan and Rafael challenge the credibility of journal feedback. In particular, the two authors voiced their disbelief at the failure of one reviewer to grasp one of the basic elements of their argument. Their response to this reviewer reveals, however, that certain feedback can be refuted if the authors can provide sufficient counter-argument. The expertise with which they adapt to resistance is derived from their deep knowledge of the publication process and also a battery of rhetorical tools that reflect many of the conventional rhetorical moves I cite in this doctoral thesis.

Text history 8: Jesús

Overview

The eighth TH examines an RA written by a senior hematologist, who will go by the name of Jesús. The paper is based on his doctoral thesis, which he wrote in Spanish and successfully defended in 2012. Throughout this chapter, we will see how adapting a dissertation into an English-language RA came with certain challenges, some of which are attributable to differences in language and others to the constraints of the RA genre. The informant interview for this TH took place on June 12, 2015 and was held in Spanish. Like other informants who kindly gave of their time for my own research, the author's decision to hold our conversation in Spanish was motivated more by a need for brevity than any reluctance to speaking in English. During our sixty-minute conversation, my questions about the text itself and my inquiries into his background as a scholar and producer of academic texts in English made it clear that his perseverance in the face of resistance is a major part of his success. Working hard to surmount what he believes to be certain inner-circle cultural mores that favor researchers working in Anglophone contexts—particularly the United States—the author showed great enthusiasm for his work and articulated clear ideas of the barriers that researchers like him face. In addition to the insights gained from the text and from Jesús's perspective on this RA and the enterprise of publishing in English in general, my own experience in investigating the article and its authors paints a complex picture of the agents and processes involved.

Chronology and texts

As with other THs analyzed in this doctoral thesis, my analysis was centered on a number of milestone texts produced throughout the drafting, editing, and peer-review processes. The table below lists all of the documents used to construct the TH, indicating these textual milestones in bold. Document C is the first draft I edited, while Document L marks the end of the pre-submission drafting and revision process. The final published version, Document P, is the third of these milestone versions.

The path that this RA took toward publication is marked by two unique features, both of which will be discussed in different sections of the present chapter. First, Jesús originally prepared this paper for submission to a particular journal; however, when his two senior coauthors advised him against it, claiming that the paper stood little chance of being accepted, Jesús revised the text so it could be sent to a publication with a lower impact factor. This second journal accepted Jesús's work following one round of reviewer feedback. Second, the ancestry of this RA reaches even further back than this, as the published text is a translated and regenred version of part of his doctoral thesis.

1. Document A: Draft translated into English following input from senior coauthors (Jesús's thesis supervisors)
2. Document B: Draft adapted to the submission requirements of a particular journal (text not eventually sent to this journal)
- 3. Document C: Draft sent to me for editing (first round)**
- 4. Document D: Version containing my first round of editing**
5. Document E: Draft including the edits accepted for inclusion into the text (track changes)
6. Document F: Subsequent draft (no track changes)
7. Document G: Subsequent draft of the text
8. Document H: Subsequent draft of the text
9. Document I: Version containing my second round of editing
10. Document J: Draft including the edits accepted for inclusion into the text
11. Document K: Subsequent draft of the text
- 12. Document L: Version of the text as sent to the first journal**
13. Document M: Reviewer responses and draft of point-by-point response
14. Document N: Point-by-point response to reviewer feedback
15. Document O: Revised draft sent to first journal
- 16. Document P: Final published version**

Evolution of the text and the role of literacy brokers

A number of actors contributed to this text. As Jesús explained, the story goes back to the early part of his career as a practicing hematologist, when he came to realize that much of the scholarship on the diseases he was treating drew heavily from molecular biology. Finding much of this literature to be beyond his grasp despite being a trained specialist, he decided to enroll in a master's degree in molecular biology. Once he earned his degree, he then embarked on a doctoral thesis, and his two thesis supervisors, both of whom were his instructors in his master's

degree, appear as coauthors of the RA studied here. Interestingly, Jesús's supervisors tried to dissuade him from attempting to publish this particular paper altogether as they found it to be too "basic." Rather than abandon the idea of publishing this text, however, Jesús reconsidered his first target journal, opting to submit the paper to a publication with a lower impact factor. Going forward with his plan, Jesús sought his supervisors' input on the paper, sending it to them after it was first translated into English (Document A). Jesús described their influence on the text to be more oriented toward content, though he also told me that they gave him insight on how to "sell" or "pitch" the article, which suggests that some of this advice had to do with how to express the study data in a rhetorically effective manner. Early on in the interview, Jesús and I had the following exchange in which I attempted to clarify the different contributions within the text's history:

Oliver: From [Document A] through to the published version, were there a lot of changes made to the text, either because of your collaborators' input or feedback from the reviewers and the editor?

Jesús: I first sent it to my thesis supervisors, who are [names redacted] (...) they were familiar with the work in Spanish, and I sent them the version in English...they made comments about where I could make improvements to this version, which had been summarized and shortened from the original work that they were familiar with in Spanish.

Oliver: Are you referring to content or language right now?

Jesús: To content...only content.

Some six minutes later, however, he says the following:

Jesús: Then the ones who revised the text were the two people I mentioned before, who were my thesis supervisors ...

Oliver: ...who focused on the content...

Jesús: ...on the content and on how I should pitch the results. The rest of the researchers only contributed to technical matters, and not in drafting the paper.

It appears from these two excerpts that according to this researcher's perspective, pitching the text can be subsumed—at least part of the time—under the category of RA content, although

Jesús's thesis supervisors sometimes gave advice on how to take results from an experiment or series of experiments and articulate those results in a way that satisfies the expectations of the journal gatekeepers, thus reflecting the kind of rhetorical "machining" referred to by Swales (1996).

Another researcher, Belén, contributed to the text, although her role was limited to "technical" aspects including translation of the original work in collaboration with Jesús and adapting the text to the requirements of the target journals. Belén acted as the conduit between the author and me, although I learned of this limited role she played due to a surprising series of events.

At the time of my study, Belén was a junior researcher in the hematology department who frequently sent me papers to edit. Though these articles did not contain any of the authors' names, I did not find this to be out of the ordinary, as there are no institutionally-mandated standards regulating who can send me work or the type or shape of these texts. I therefore proceeded as usual under the misconceived notion that it was she who was reading my proposed edits and comments. As I had been in such frequent contact with Belén, I decided to ask her to participate in my research, which she graciously accepted, and we agreed that this particular RA—which in reality had actually been written by Jesús—would be a fitting example of *her* writing. Although I had explained the requirements of my study to her both verbally and in writing, Belén did not express any reservations during the informant-recruitment phase. When I contacted her to schedule the informant interview, however, she seemed doubtful that she would be able to answer all of my questions about the rhetorical patterning of the paper and why the text had been written the way it had. Perplexed by her answer, I inquired as to why she felt so unsure, to which she responded that the paper had actually been written by another member of her department, and that she had only carried out certain administrative tasks, one of which was to liaise with me during the in-house editing process. This late-stage realization forced me to make an unexpected shift in my study population, and fortunately Jesús agreed to take part. The false assumptions of mine that became apparent in this series of events, however, raise the question of how often this happens in my daily work. Also, this misunderstanding indicates that more in-depth information and education are needed on the remit of the agents participating in

the writing and editing process, as doing so would sharpen mutual understanding between author-clients, collaborators within the discourse community, and language professionals called on to assist in the process.

I edited the RA on two occasions. The first of these edits is seen in Document D, which was the second draft to appear in English, and had been adapted to the manuscript requirements of the journal to which it was originally intended to be sent. After my original edit, however, Jesús decided not to submit the text to this journal, but rather to another, and therefore between my first edit in Document D and my second edit in Document I, the text was revised another three times (as these are not milestone versions, I only consulted these drafts when looking to pinpoint rhetorically important changes across the RA's trajectory). In the course of talk about who had contributed to the writing process and what they had done, Jesús made some rather telling commentary on how he views the editing service I provide. In his account, he sent the text to me for "spelling and grammar correction," which indicates that he was originally either uninterested in or unaware of the deeper types of editing that I usually provide. In addition to the numerous direct interventions made in Document D, the text also contains 101 margin comments geared toward a range of topics including comments on possible plagiarism, queries on what the author's intended meaning in a particular passage was, explanations of my edits so as to avoid introducing conceptual errors into the text, comments on the rhetorical features of English RAs, and ways in which the paper's rhetorical structure could be improved. This view expressed by Jesús may stem from confidence in his abilities to write effectively, skepticism of input from outsiders to his discourse community, or an unawareness of the strong link between content and form in writing (J. Flowerdew, 2000, p. 145).

During the peer-review process, which Jesús considers to have been more demanding than the norm in his experience—and even inordinately so given the relatively low impact factor of the journal—the RA was reviewed by two of the journal's referees. The reviewer reports drafted by these reviewers appear alongside Jesús's point-by-point response letter in Document N. The first reviewer articulated their questions, concerns, and criticisms in six points, establishing no hierarchy for these remarks. The second reviewer, on the other hand, expressed three "major

concerns” and fourteen “minor concerns.” The issues brought up in the reviewer reports and the way Jesús so adroitly fielded these requests both in his point-by-point response letter and in changes made in response to this feedback offer compelling evidence of the essential role played by these occluded genres and the ways in which authors may use the different parts of the peer-review process to position their work effectively. I will leave more extensive commentary on the rhetorical shaping that took place for the following subsection of the present chapter, though the themes arising from these interactions have implications for both aspects of my research.

The first minor concern of the second reviewer targets the surface-level quality of the paper’s English, and reads, “Some sentences were difficult to understand due to poor syntax”—this despite the fact that I had edited the manuscript twice before it was submitted to the journal. During our conversation, Jesús seemed somewhat dismayed by this comment, and during the time his paper was under consideration, he responded to this remark by ensuring the reviewer that the manuscript had been revised by a native speaker “again” in order to remedy this. It appears from my records, however, that while Jesús believed that I had intervened once before and once after the article was submitted, I had actually revised the text twice before the first submission, my first edit taking place before the text was reworked for submission to the second journal. In any event, below is an excerpt of the exchange Jesús and I had. These statements made clear the degree to which he believes language, location, and power come together in his field:

Jesús: Aside from technical aspects, they often make a comment that says that...I think it’s because we’re not native English speakers, and even when the manuscript has been revised by you or another editor...they always add a comment that says, “This text must be revised by someone who speaks English or a native English speaker.” I’ve always gotten comments like that.

Oliver: So do you look upon these kinds of comments with doubt, then?

Jesús: I think non-native authors have a much harder time publishing. High-impact factor journals are always in English and the top ones are from Anglo-Saxon countries...it’s not just that they are native speakers of English, it’s that they are from the U.K. or the United States. And whenever they see a Latin name, I think they make it harder for us to publish in those journals.

Oliver: So would this be a sort of resistance on the part of these journals?

Jesús: In my opinion, I believe that they think the research we do is of a lesser quality. Maybe because they don't know us. The Americans know how all of the research centers in the United States function, the British in the United Kingdom (...) in the end we all know each other, though the Americans don't know who we Spanish [researchers] are. So they don't know us personally as physicians or as researchers, so I think they are more exhaustive with us and demand more of us.

Oliver: Okay, okay.

Jesús: Because this has happened to me with other translators who have translated other papers of mine. They always make that comment, and I think, "But it's already been revised!"

While this treatment he feels he and other "Latin" scientists like him are given has not discouraged him from attempting to publish his work in English-medium international journals, the pattern of apparently unnecessary requests for language revision has led Jesús to believe that there is pervasive doubt on the part of American journal gatekeepers as to the quality of research produced in Spain. In his view, American hematologists who are recognized members of the scientific community feel confident about the quality of the work done within their own country, but view contributions from other countries with skepticism. Lacking other means through which to channel their reservations about research quality, these gatekeepers target NNEs authors' English. If these claims made by Jesús are true, these American gatekeepers are exercising linguisticism (Phillipson, 1992). As for Jesús's complaint of requests for repeated revision of texts by NES editors, the disadvantage these hurdles constitute and the unfairness of this practice are evident given the cost of language support, especially in times of shrinking research budgets.

In this TH, my comparison of Jesús's point-by-point response and the actual modifications Jesús alluded to in these replies to the reviewers reveals a surprising dynamic. On two occasions, this informant appeared to report that he had heeded the reviewers' advice and made certain changes to the RA proper, though these changes were not precisely as they had been described in the letter. The first reviewer recommended making a clearer distinction between the two subtypes of patients used in the study. In his letter, Jesús writes "It has been clarified in the

abstract (page: 3, lines: 4,5 and 7) and also in the text (page: 7, lines: 18, 21, 25; page: 8, lines: 4, 7 and 8).” However, the revised draft sent along with this reviewer response shows that only a few words had been modified, and it would appear that no real distinction between the two groups is made. In the following excerpts I compare the passage on page 7 of the article that Jesús cites as evidence of this change. To illustrate what I believe to be negligible differences, I will include both the version originally submitted to the journal (Document L) and the resubmitted text (Document O).

Document L:

An analysis of the data from the expression profiles revealed a large molecular heterogeneity. Clustering was performed using the SOTA method [6], and 2 molecular subtypes were identified. By analyzing the expression profile and comparing both molecular subtypes, we found differential expression, with FDR <0.05 in 1092 genes, 418 of which were overexpressed in subtype A and 674 overexpressed in subtype B.

Document O:

An analysis of the data from the expression profiles revealed a large molecular heterogeneity. Clustering was performed using the SOTA method [6], and 2 different molecular subgroups were identified. By analyzing the expression profile and comparing both molecular subtypes, we found differential expression, with FDR <0.05 in 1092 genes, 418 of which were overexpressed in one group and 674 overexpressed in the other subgroup.

It seems hard to imagine how the language underlined in O clarifies the subtypes, as the revised text does little more than change “subtype” to the synonyms “group” and “subgroup.”

This same dynamic is seen again with the other reviewer, who recommends a rather ambitious rewrite of the text. Again, in his point-by-point letter, Jesús states that he has incorporated these into his text even though the actual changes tell a different story. The first item of feedback I am referring to here, which reads, “In the abstract, results and discussion, the authors over-interpret pathways that did not reach statistical significance ($p < 0.05$, but $FDR > 0.25$) between the two subtypes,” appears to dissuade the author from focusing on this aspect of his experiment, since it did not reach statistical significance. Jesús replied to this feedback with three bullet points— one excerpting the minor changes made to the text, the second stating that this issue had been

“clarified,” and in the third reporting that the text had been rewritten in order not to overinterpret results. However, the revised draft of the text that accompanied this point-by-point letter show little to no change, which is surprising given the fact that the text was eventually given this reviewer’s blessing. Replicating this dynamic once again, the second comment by this same reviewer takes issue with the experimental evidence that the informant used when arguing the significance of the paper. This remark was phrased as follows (Document M):

It is already recognized that CLL is dependent on BCR signaling. This has translated into therapeutic strategies using highly active kinase inhibitors targeting this pathway. Therefore, enrichment of BCR-related genes in tumor compared to normal samples (which was not statistically significant in this data set and based on 2 genes, BTK and BLNK) at best might validate previous work, but does not reveal new pathogenic mechanisms or targets as suggested by the authors.

The reviewer urges Jesús and his coauthors to reconsider the strength of their claim, reducing the take-home point from one of a discovery (“new pathogenic mechanisms or targets”) to a validation of what is already known. Despite the clear rhetorical effect that downgrading a central claim is likely to have, in his response to the reviewer feedback, Jesús includes an excerpt from the revised text, prefacing the excerpt by writing “To clarify this concept we have modified the text being [sic] as follows.” The change referred to is an addition to a paragraph of the discussion section which appears not to alter the extent of the claim whatsoever. Below is the paragraph in question, with the added text underlined:¹

¹ Here it is worth noting that this change occurs not between Documents L and O as suggested by the chronology established for this TH, but rather between Documents K and L, with draft L thought to have been the draft sent to the journal for consideration and before peer review.

In this study we have identified a group of genes with differential expression between CLL and normal B lymphocytes from lymph nodes. These genes are directly involved in tumor cell biology in CLL, conferring proliferative advantage or inhibiting apoptosis. If we analyze the differences between a normal lymph node and CLL, there is only one molecular pathway with different gene enrichment between the 2 groups: the molecular pathway of BCR, which is overexpressed in CLL samples. According to previous studies on BCR, molecular heterogeneity in CLL is determined by signaling through BCR, [1, 13]. Tumor lymphocytes of high-risk patients have a molecular profile formed by BCR-linked genes [1]. These results are consistent with the interpretation that the stimulation and growth signals from BCR allow cell proliferation and prevention of apoptosis, and therefore favor the survival of tumor lymphocytes in CLL [25]. The permanently active stimulation of the BCR in tumor lymphocytes may be one of the pathogenic disease pathways, so the development of therapies directed against BCR-mediated signaling is of interest in CLL. Different oral agents targeting BCR-associated kinases, including spleen tyrosine kinase (Syk), Bruton's tyrosine kinase (BTK), and phosphatidylinositol 3-kinases (PI3K) have been explored in early-phase clinical trials in patients with CLL. The compounds that are currently investigated in patients with CLL include ibrutinib –inhibitor of BTK, fostamatinib-inhibitor of Syk and idelalisib (BS-1101) –a specific isoform of the PI3K inhibitor [26,27].

What is telling about this author-reviewer interplay is how this supposedly major concern was resolved with a modification of a much lesser order. In spite of the apparent thrust of the reviewer feedback that called on the author to rethink the way he was presenting his study, Jesús passes this test by acknowledging two studies while neither addressing the primary issue posed by the reviewer nor challenging this criticism, thereby suggesting that this was either an oversight on the part of the reviewer or that there is an unspoken rule among the discourse community by which certain criticism can be acknowledged in point-by-point letters but not actually addressed in the text. In addition to these two possible explanations, a third hypothesis may be that the author interpreted this point of criticism to be a veiled request to include one or both of the citations appearing at the end of this fragment. In any event, this example of brokering appears to challenge the rigor of the peer-review process or at least hint that this reviewer's comments did not need to be taken at face value.

As I will show in the following section, Jesús refashioned parts of his paper to emphasize the aspects of the text that the reviewers believed would make a suitable contribution to the journal and also incorporated the flaws found by the gatekeepers as limitations to the study. Both of

these strategies reflect deep knowledge of the genre and of the leeway afforded to authors within the peer review process.

Genre considerations

As I will show in this section, the rhetorical makeup of this RA changed in significant ways while a primary knowledge claim remained the same throughout. Jesús's drafts show how he searched for the most appropriate way to present his findings and position his paper, molding his results in a way that satisfied his peer reviewers. Doing this required him to give substantial prominence to other researchers' work, casting his study as a validation of research done by his peers and as a promising method to be used in future experiments. The most intensive rhetorical work done in this TH was focused on the abstract, introduction, and discussion, while the methods section was hardly given any rhetorically significant revision.

The lineage of this text added to the challenge of publishing this RA. Reformulating the findings from his doctoral thesis, Jesús had to simultaneously summarize part of the original study within the genre structure of the RA and also translate the paper into English. In our talk about these difficulties, Jesús explained to me that while he was pursuing his doctorate, he made the conscious decision to write a monographic thesis rather than the more common thesis by publication; in his view, doing so allowed him to address a single issue and see it all the way through. Once he completed his doctoral studies, however, this choice meant that in order to publish part of these findings he had to do additional work to adapt his study accordingly. Though happy with the result of his thesis, Jesús expressed a certain degree of regret at having written the text originally in Spanish because of these difficulties, although the requirements of his university left him no choice in the matter. In related commentary during the informant interview, Jesús told me that he usually drafts his texts directly in English.

On a number of occasions during the informant interview, Jesús demonstrated great awareness of academic discourse conventions and an ability to articulate the strategies and processes he has developed over the years when writing for publication. Several of the comments he made during our talk hinted at a certain sensitivity toward the genre of RAs and how academic papers

are different between Spanish and English. When he is able to draft papers directly in English, as is most often the case, he proceeds with the understanding that all RAs have a set of common characteristics in terms of text length and what he referred to on two occasions as how the language is “organized.” He described his approach to translating his study and adapting it for this RA in the following way:

When translating I always use published articles as support to see how they organize the language, examining these texts to see how they can serve as an example for what I am doing...not copying these other texts, because with the programs that journals have, they know whether or not you have copied a passage, and I think papers need to be under a certain percentage of similarity to other published works, so we know how this works. I read similar texts and see how they organize the language so that I can write in a similar way, but not exactly the same.

This excerpt from the interview is of interest because of the informant’s use of *organizar el lenguaje* (organize language)² as this description could refer to sentence-level organization or to a more discursively sensitive rendering of ideas beyond the sentence.

During my post-interview correspondence with Jesús, he kindly forwarded me a digital file with his doctoral thesis so I could understand the type of changes he made when writing this RA. This, however, is a difficult comparison to make. The thesis spans ninety-five pages in total including front matter. When I asked Jesús to orient me to the part the RA is derived from, he directed me to the results section. Twenty-two pages in length, this section contains sixteen results statements, each articulated as a noun phrase describing the contents of the subsection. The result that the RA is based on is the third of these, “Molecular heterogeneity in lymph-node samples from patients with CLL.” A brief comparison of the thesis with the final published version revealed that much of this subsection from his thesis remained intact, with entire paragraphs translated nearly verbatim and appearing in the methods and results sections of the published RA. The same, however, was not the case with the other sections of the RA, and this information was either culled from various parts of the previous work and from Jesús’s knowledge of the

² My translation

topic. Commenting on the changes he made, Jesús told me that the only section that required substantial development was the discussion, and in large part the changes were made to update the bibliography, adding references published after he completed the thesis.

Turning now to the different sections of the paper, my study of the abstract made it clear that this part of the final product did not appear until Document F, which gives the impression that the author may understand the abstract to be independent and ancillary to the RA proper. Taking a look at the milestone version in Document K—the draft as sent to the first journal—the statement of purpose is surprisingly brief. Here, I am referring to the first sentence and to the way in which the abstract very quickly moves past Bhatia’s (1993) Move 1 (introducing purpose) and Move 2 (describing methodology), devoting the bulk of the abstract to Move 3 (summarizing results) and then briefly offering conclusions (Move 4, presenting conclusions). The opening reads as follows:

[M1] The different clinical and biological validated [sic] prognostic markers do not adequately explain the pathogenesis of CLL. [M2] Biopsy samples of lymph nodes from 38 patients with CLL were analyzed. [M3] We found differential expression in 1092 genes, 418 overexpressed in subtype A and 674 in subtype B.

Despite this apparent glossing over Move 1 of the abstract in the second milestone version, the abstract becomes even more focused on results by milestone version N (first draft of the text post-review), when Jesús removes Move 1 entirely in response to the feedback received from reviewer 2 which questioned the appropriateness of “over-interpreting” findings that did not reach statistical significance. While it is true that the word limit set for the abstract factored in to Jesús’s decision to omit rhetorical moves, we also see how the abstract increasingly emphasizes results at the expense of rhetorical convention, with reporting of the study outcomes and inclusion of reviewer feedback taking precedence over rhetorical strategies that set the scene for the study.

The introduction to the text also underwent substantial revision as it progressed toward publication. This reworking shows a gradual, though incomplete, move away from a more disquisition-like approach that may be more appropriate for longer genres such as a doctoral

thesis. Similar to the rhetorical condensing seen in the abstract, much of the reworking done in the introduction eliminates instances of Move 1 (establishing a territory). As a result, moves once employed to establish territory and segue into uses of Move 2, Step 1A (indicating a gap) or Move 2, Step 1B (addition to what is known) are reduced in length. Such modifications to the paper's move structure can be seen in the following excerpts taken from the draft first sent to me for editing (milestone version 1, Document C) and the same section as sent to the journal for peer review (milestone version 2, Document K):

Document C

Chronic lymphocytic leukemia (CLL) is a heterogeneous disease with a clinical presentation ranging from indolent to advanced-stage disease. Therapeutic intervention is scarcely required in patients with indolent disease, whereas chemotherapy treatment is frequently required in patients with advanced stage disease. However, CLL is generally considered as an incurable disease and, consequently, the development of new therapeutic strategies is a key goal in this malignancy.

Several clinical and biological prognostic factors have been identified, such as the Rai and Binet clinical staging systems specific [sic] cytogenetic alterations, mutational status of immunoglobulin (Ig), and the expression level of CD38, ZAP-70, and LPL. The implication of these biological predictor factors with the molecular pathogenesis of the CLL is still under investigation, and an eventual therapeutic application is not yet developed.

Molecular Heterogeneity of Chronic Lymphocytic Leukemia

The different clinical and biological prognostic markers that have been validated to date do not adequately explain the pathogenesis of CLL, and the implication of such prognostic markers in the development of the disease is still under investigation.

Document K

Chronic lymphocytic leukemia (CLL) is a heterogeneous disease, with clinical symptoms ranging from cases of advanced disease, which usually require chemotherapy, to indolent disease, where therapeutic intervention is scarcely necessary. The different clinical and biological prognostic markers that have been validated to date do not adequately explain the pathogenesis of CLL, and the implication of such prognostic markers in the disease is still under investigation. Identifying new molecular-level variables by analyzing gene-expression profiles through massive analysis techniques may contribute to a better understanding of the pathogenesis of the disease, the development of more precise biological prognostic factors, treatment based on specific risk, and the identification of new therapeutic targets.

In the excerpts above, I have underlined the main linguistic realizations indicating that the author is employing what Swales (2004) refers to as Move 2 (establishing a niche). In both versions, Jesús makes reference to ongoing investigations along these lines, implying that his research fits within this evolving body of knowledge. More significant to the rhetorical intent, however, is the very clear language used to point out the gap in the research, emphasizing that the markers used to date cannot adequately explain the pathogenesis of the disease, which is why this rhetorical move in both drafts can most accurately be considered Move 2, Step 1A (indicating a gap). The reworking done between these drafts illustrates how Jesús shortens background information while maintaining the same rhetorical framework of the section.

The methods section of the RA underwent comparatively little change and flows somewhat mechanically, providing information on a number of the study's characteristics. The first part of this section, which has no sub-section heading, offers details on compliance with regulatory issues or scientific standards such as informed consent, ethics-committee approval, and adherence to quality protocols, while the two other paragraphs within this sub-section summarize the characteristics of the patients included in the sample according to a number of clinical and biological parameters (i.e., prognostic markers). One point of interest regarding this first section has to do with the way in which Jesús, between milestone versions C and L—that is, the draft first sent to me for editing and the version submitted to the journal—groups together all transparency-related data in the second of these versions, while in the previous ones these matters were more integrated in rhetorical moves per se. This rearranging can be seen in following excerpts between the two drafts:

Document C

All patients signed informed-consent documents allowing the use of tissue for research purposes, and the study was approved the Ethics Committee in Clinical Research of the [name of hospital redacted]. Biopsy samples of lymph nodes from 38 patients with diagnosis of CLL were analyzed. Patients were diagnosed according to the most recently revised (2008) classification of tumors of hematopoietic and lymphoid tissues of the WHO. Samples were obtained from centers ([names of hospitals]). The samples included in this series were collected between 2000 and 2007. The mean age at diagnosis was 67 years (39-87). Of the group of 38 patients, 24 were men and 14 were women, with a ratio of 1.7:1. The clinical characteristics of the patients included in the study are listed in Table 1.

Document K

All patients signed informed-consent documents allowing the use of tissue for research purposes, and the study was approved by the Clinical Research Ethics Committee of the [name of hospital]. The biological samples and clinical data of patients included in the study were provided by the [name of research institution] Biobank. The samples and clinical data were collected, processed, and stored according to quality protocols, ensuring the safety and confidentiality of the donors' data. Material and data transfer was approved by external ethical and scientific committees.

As seen here, Document K eliminates such information as the number of samples, the diagnoses assigned to these samples, the criteria used to arrive at the diagnoses, the years in which the samples were gathered, mean patient age, patient sex, and the indication of where this information is presented in table form.

Jesús makes particularly frequent use of Move 5 (describing experimental procedures), likely because the aim of this paper was to establish the validity of the analysis he used in his doctoral thesis. Within Move 5, Steps A (identification of main research apparatus) and B (recounting experimental process) figure prominently and in a way that combines these two steps, as in the following passage excerpted from milestone version 1 (Document C):

RNA extraction and microarray hybridization for gene-expression profiling texts

RNA extraction from samples of lymph nodes and peripheral blood was performed using the RNeasy kit (Qiagen, Hilden, Germany) following the manufacturer's instruction. The RNA obtained was quantified by Nanodrop 1000 (Thermo Scientific, Wilmington, DE, USA) and visualized in agarose gel. Extraction of RNA from lymph node samples was conducted on fresh [sic]. From peripheral blood samples, RNA extraction was performed after subsequent thawing to cryopreservation [sic] in liquid nitrogen using 10% DMSO as cryoprotectant [sic] cell.

Despite the surface-level errors seen in the passage, the two steps of Move 5 are so closely entwined here that it is challenging to assign these sentences to just one step. Taking the first sentence after the subsection heading as an example, the experimental procedure (extraction of RNA) and the apparatus used to do this appear equally prominent in the sentence. The same can be said of sentences 2 and 4, indicating that the author makes a habit of realizing these moves in tandem. When I asked him to tell me what he considered to be more central to the message of each passage—e.g., *extraction* vs. *RNeasy kit*, *quantification* vs. *Nanodrop 1000*, and the fact the the RNA was visualized vs. the means used to carry this out—he told me that they were equally important. This strategy employed by Jesús thus challenges the usefulness of separating these rhetorical aims into two separate moves within biomedical academic discourse rather than grouping them into one.

The results section also places substantial emphasis on the procedural details of the study, doing so in a way that links instances of Move 0.3 (procedural move) with Move 1.1a (comparison between study subjects). In investigating the phenomenon of chronic lymphocytic leukemia, Jesús searched for differences in activity (known as “differential expression”) among genes derived from cells of patients with the disease. To do this, he had to undertake a number of experimental steps, yielding data which in turn were subjected to further experimental procedures, and so forth. When writing about his experiment, Jesús frequently pairs these two rhetorical moves. The beginning of the first paragraph of the results section from the first milestone version (Document C) serves as a good example of this pattern:

[M0.3] An analysis of the data from the expression profiles **[M1.1A]** revealed a large molecular heterogeneity. **[M0.3]** If clustering is performed using the UPGMA method **[M1.1A]** not clearly differentiated groups [sic] are obtained. **[M0.3]** However, when the SOTA clustering method was used (12), **[M1.1A]** we identified 2 molecular subtypes in the samples tested (Figure 1). **[M0.3]** By analyzing the expression profile and comparing both molecular subtypes, **[M1.1A]** we found differential expression, with FDR <0.05 in 1092 genes, 418 of which were overexpressed in subtype A and 674 overexpressed in subtype B.

The first sentence summarizes the finding to follow. In the following two sentences, Jesús describes two different types of clustering analysis he used to determine whether a pattern could be seen in these expression profiles. While the first of these two methods failed (i.e., UPGMA), the second did reveal differences, called “subtypes” here. These subtypes then progress into the next phase in which the aforementioned subtypes are examined against one another, with this procedural move (0.3) yielding a false discovery rate (FDR), that is, an indication of the extent to which the relationship purported as being shown in the comparison may actually be untrue. This same pairing of instances of Move 0.3 and one of the different steps included in Move 1 continues for the entire first paragraph, outlining the researcher’s logic and steps followed.

Evaluative language is largely absent from the results section, and, with the exception of a relatively high presence of Move 0.3 (procedural), most all moves can be coded as Move 1 according to the framework of Williams (1999). While Williams includes in his scheme what he refers to as “substantiation of finding” and “non-validation of finding” moves (2.1 and 2.2 and their respective sub-moves), it appears that Jesús’s RA does not indicate this type of hierarchy between the different findings whereby one or more findings are central and others serve to support or at least not contradict those classified as Move 1.1. In fact, if we examine the findings in terms of the explicit objective appearing at the closing of the introduction, that is, “[to] identify specific molecular genes or pathways to which design [sic] a molecular target strategy of treatment,” Jesús states that while his analyses indicated 4 such pathways in one subgroup and 1 in the other, the statistical analyses he carried out to test the mathematical validity of these observations revealed that they did not reach statistical significance. For this reason, the author appears hesitant to evaluate his findings in this section, and only uses Move 3.0 (explanation of finding) on one occasion, when he writes in carefully hedged fashion, “This subtype appears to

be characterized by greater dependence of signaling by cytokines and activation of the NFkB pathway, while subtype B seems to depend more on cell cycle proteins such as cyclin-dependent kinases (CDKs).”

The rhetorically relevant changes made to the results section mostly appeared between the first and second milestone versions (Documents C and L, i.e., the draft sent for editing and the draft as first submitted to the journal) (Jesús made these revisions independently, as input from his thesis supervisors were incorporated in Document A). In one such modification, he eliminates one move in which the first of two “clustering methods” failed to reveal differences between molecular subtypes, though does not do the same to the second such method, in which the clustering process did reveal differences. In other passages, mention of results that did not reach statistical significance are shortened so that they no longer make reference to the findings whose empirical validity is challenged by this lack of statistical difference, preferring to transfer mention of these findings to table form or include them in the supplementary information section outside of the text proper. Jesús alluded to this strategic revision in one of his comments made during the informant interview, telling me that he was aware of the limitations of his study but chose not to show them, and that this awareness was important in securing publication for this RA.

Most likely because of the failure of the study to reach statistical significance in some key aspects, the discussion section relies heavily on Move 1 (information move) and Move 5 (reference to previous research), thus backgrounding mention of the author’s findings and foregrounding influential existing studies. Despite the shortcomings of the data, however, my study of this section across the text’s trajectory reveals how Jesús positioned and then repositioned his argument, changing the rhetorical makeup of the section considerably from early drafts to the final published version. The move cycles used in the discussion often include instances of Move 1 as the head and then proceed on to Move 3 (statement of finding) and then Move 5 (reference to previous research) and, once again, Move 3. A particularly good example of this rhetorical structuring comes in the first paragraph of the section, where Jesús grounds the opening in the overall problem that the disease represents (i.e., wide clinical variability) and efforts that have been made to understand patterns in the disease (disease staging) and then adapt treatment

strategies to these patterns. However, due to the particularities of the patient sample used in his study, the RA's findings only partially supported this existing empirical evidence. We can see how this rhetorical structure is articulated in the first milestone version (Document C):

[M1] CLL is a disease with a wide clinical variability. Many efforts have been made to classify patients into risk groups and thus allow individuals with increased probability of disease progression or shorter survival to be identified. Traditionally, we have used the clinical stages of Rai (13) or Binet (14) to determine disease staging, allowing us to maintain an expectant attitude, or if was [sic] in advanced stages, which determine [sic] the need for starting a specific treatment. Logically, patients in advanced stages have a worse prognosis than those in earlier stages, as shown by survival curves (15). **[M3]** Considering that in the series used for survival analysis there were no patient samples at Rai stage 0 (lymph node sampling), only Binet stage (A vs BC) has been able to be validated as a prognostic marker in terms of treatment-free survival and Rai stage (I vs II-IV) as a prognostic marker for overall survival. **[M3-M5]** As in previous publications, the early stages of the disease have a higher treatment-free and overall survival rate than the rate seen in more advanced stages. **[M3]** In our series, none has been validated as a prognostic marker of disease progression.

In the first three sentences, Jesús provides increasingly more specific detail about the disease characteristics and efforts made to help patients with the disease. Specifically, because the disease is heterogeneous, scientists have sought to classify the disease, because doing so benefits treatment effectiveness; these efforts have yielded two staging systems to determine which patients are most likely to have a poor prognosis. In the sentence beginning with “considering,” the author shifts to Move 3 (finding), making his first illusion to his own results when he states that his study did not include patients in a particular stage of one staging system, thereby limiting the capacity of his data to validate all of these stages with his results. In the final sentence of this paragraph, we see a hybrid Move 3-Move 5 (reference to previous research-finding), as he begins the sentence with the dependent clause “As in previous publications,” thereby contextualizing the finding that follows within other research whose findings are consistent with his. Despite the efforts shown here to demonstrate validation of a system of disease staging, this entire section, in which the author builds up this first mention of his results in the discussion, is eliminated altogether between the first and second milestone versions, thus shifting the orientation of the claims in the section.

Later in the discussion, Jesús exhibits an idiosyncratic rhetorical style in which he expands upon apparently minor findings using lengthy stretches of background information regarding the genes that showed increased expression in his experiment. The passage I am referring to here is as follows:

[M1] Other authors have identified a molecular signature related to survival [1, 12]. Although not an objective of this work, [M3] we were unable to find an association between molecular subtypes and time of treatment or overall survival. Genes with increased expression in one of the molecular subtypes (subtype A) include IL-1A, STAT4, and TCL1, all involved in cell survival and inhibition of apoptosis. [M5] IL-1A has been identified as a susceptibility gene for developing CLL in different studies [13-14]. STAT4 is part of the (...)

This passage leads with Move 1 (background), which Jesús uses to set up reference to the finding (Move 3) expressed in the next sentence. Following this, the author launches into a seemingly unconventional string of moves made up of Move 5 realizations, occupying roughly 15–20% of the entire length of the discussion section with this rhetorical strategy. The admission that this finding was not an objective of this work together with the lengthy discussion of other scholars' findings concerning these genes suggest that this is something of a rhetorical detour in this RA. Neither of the two reviewers saw it fit to comment on this rhetorical patterning, however, which indicates that Jesús successfully utilized this strategy to root his work in the existing literature.

One of the key findings of the study is its failure to link the molecular subtypes detected in the patient samples and existing prognostic markers or clinical outcome. The author acknowledges this unexpected finding as a Move 4 (unexpected finding) but then goes on to defend a more limited interpretation of the relevance of his results, highlighting the methodological validity of his approach and focusing on the one molecular pathway found to be overexpressed in both of the two subgroups detected in his study (Move 7, knowledge claim). This move realization in the final published version of the text reads as follows:

[M4] Although we obtained two molecular subtypes, one where the genes and molecular pathways of apoptosis inhibition are predominant, and another where the genes and molecular pathways involved in cell proliferation predominate, neither of these molecular subtypes have a proven impact as a prognostic factor, because there are no demonstrated differences in treatment-free survival, progression, or global free survival between the groups. **[M7]** However, despite the lack of prognostic implication, these findings open the door to the selection of subject-specific molecular targets, allowing the design of individualized therapeutic strategies for each patient.

Despite all the other changes the RA underwent across 12 revisions from multiple sources of input, this move combination remained unchanged, making it one of the paper's most central arguments. Although his findings were mitigated by the lack of correlation with progression-free survival, Jesús successfully fashioned the article around the idea that efforts to develop therapy approaches to chronic lymphocytic leukemia should take a similar approach. As mentioned in the previous section of this chapter, this RA is focused on methods rather than novel findings, and the reviewers who evaluated the paper recognized its value as such and encouraged Jesús to further emphasize the way this study fits into the literature.

The reviewers critiqued the study from a number of angles, and in response Jesús tended to incorporate these flaws as limitations to his study (Move 8). In their report, reviewer 1 writes the following: "Which is the plausible explanation to [sic] a very low rate of CD38+ cases in a series with majority of cases of non-mutated and ZAP-70+ CLL?" In his point-by-point response, Jesús begins his reply by writing, "The explanation may be related to the technique," followed by commentary intended to explain how this was the case, and then indicating to the reviewer that "this limitation" had been mentioned in the text. In this way, Jesús uses limitations as a means of acknowledging the input of the journal gatekeepers even when the reviewers' comments did not explicitly suggest such a reaction. Reviewer 2, on the other hand, was more direct, and wrote the following: "Page 13, line 21: expand on limitations as discussed above," to which Jesús responds: "Limitations of the study have been expanded (page 14, lines: 4-12)." In highlighting not only the type of rhetorical move required but also indicating the precise part of the manuscript where this information should be included, the reviewer encourages the author to review what appears to be all of his or her comments made to the author and refer to these within the discussion. These suggestions appear problematic given the fact that this same reviewer made twelve previous

comments, thereby leaving the author to guess which of these issues should be posed as limitations. In responding to this feedback, Jesús chose to focus this revised commentary on the small number of patients included in the study, although of the twelve preceding comments, the only one that bears a relation to the limited data set reads:

Therefore, enrichment of BCR-related genes in tumor compared to normal samples (which was not statistically significant in this data set and based on 2 genes, BTK and BLNK) at best might validate previous work, but does not reveal new pathogenic mechanisms or targets as suggested by the authors.

In including this mention of the paper's limitations, Jesús does so in a way that echoes the comments made by Reviewer 1, which also highlight that scarce new data are contributed by the paper, asserting that the experiment's value is that it involved samples of lymph nodes obtained from patients diagnosed with chronic lymphocytic leukemia.

One final theme revealed by the TH involves the way in which Jesús responded to both reviewers' calls to include certain bibliographic references. Reviewer 1 provides specific papers for the author to include, while Reviewer 2 included as a "major concern" the following: "Discussion should be written in the context of previous work showing upregulation of NFkB and cell proliferation-related genes, particularly within the CLL lymph node environment." In response to this last remark, Jesús writes, "We have rewritten the discussion and mentioned the previous work of Mittal AK Mol Med 2014 Jul 15;20:290-201, regarding the role of NFkB upregulation and its relation within the CLL lymph node microenvironment." This dynamic shows how the author negotiated with the reviewers, who may be two of the very authors Jesús was asked to cite in his work. Rather than focus on the claims made in the discussion or the aspects of Jesús's data, these reviewers appear to be more concerned with the recognition Jesús gave to his peers.

Attitudinal aspects

Throughout our interview, Jesús evinced great vocation for his job and a desire to produce outstanding work that is recognized internationally in journals like this one. When I asked him whether this same RA could have been published in Spanish, he told me that it could have,

although doing so would have been inconceivable for him because it would have meant substantially limiting the number and kind of readers accessing his work. In his view, when an RA is published in Spanish, only the abstract has a chance of being read by his international peers because it is in English, although in another part of the interview he also expressed doubt as to the frequency with which Anglophone-center scientists read papers written by authors they have not seen during scientific conferences. This notion that publication in international journals is not enough to gain a foothold in the community of hematologists came as a surprise to me, and suggests that non-center researchers like Jesús must succeed both in the oral and written media to compete with their American and British counterparts. In explaining why he prefers to devote the effort to publishing in English, he mentioned not only the recognition this could earn him by other researchers who may, if impressed by his work, offer him collaboration opportunities in their investigations, but also the exposure that international publishing gives him for pharmaceutical companies who may be interested in recruiting researchers to collaborate in their clinical trials. These firms are by and large from Anglophone countries and have substantial funds for clinical studies, and by publishing in English-medium international journals, Jesús is situating himself as an expert in the diseases he has worked so assiduously to specialize in.

Thanks to his practice of reading the literature produced in his field in English, he has reached a point in which he feels English is not a major handicap per se, and that publishing papers like this one in Spanish is not substantially easier. When asked whether he believed he knew what journals' expectations are as to the papers they agree to publish, he did not hesitate in replying that he did. When considering a particular journal to send his manuscripts to, he carefully studies the type of articles published recently by the journal so that he knows beforehand that the editor will not reject his work outright, claiming that the submitted paper does not match the type of work they customarily publish. Another key factor when surveying potential target journals is who has published in that journal before, as this awareness of which of his peers have had their findings appear in a given journal provides an indication as to the type of study the journal tends to publish.

The quality of his writing has never caused an article of his to be rejected by an academic journal, and the author doubts that quality research is ever turned down because of perceived deficiencies in the language. While recognizing his point of view, I question whether the journal gatekeepers would have been able to see the quality in his work if Jesús had been less skilled at presenting his research in the genre of the RA. As shown in the section on the rhetorical evolution of the text, great effort was expended to portray the results of the study in a way that resonates with what is accepted by the discourse community. The degree of abstraction used to fashion a text of these characteristics surely requires a high level of linguistic knowledge, which comes at great expense in time, effort, and money. Indeed, the overlap between surface-level accuracy and knowledge of the genre is difficult to draw, and part of the evidence we see in this TH demonstrates how success in academic publishing is a matter of representing data in a way that resonates with reviewers, each with interests of their own and whose particular assessment of the text may differ accordingly. Indeed, during the informant interview Jesús mentioned more than once that reviewers tend to find fault with the quality of the English, although he also volunteered that this criticism often comes from peers of his who are not native English speakers themselves and who, despite their expertise in their scientific field, are not without their own limitations in the language.

On an overlapping topic, when I asked Jesús whether he believes himself to be at a disadvantage compared to NES colleagues when publishing in English, he said he does not, arguing that quality science is always recognized. He did concede, however, that NNES researchers must endure lengthier revision processes and put forth a greater effort to draft papers, thereby suggesting that at least in terms of time and effort spent, the playing field is not an equal one. To my hypothetical question challenging him to envision how the process would have been different had he published this from an inner-circle research institution, he ventured to say that the manuscript would have been accepted outright because the editor and reviewers would have immediately situated him within a research group that was familiar to them, especially if the group had been from the United States. In his view, the issue faced is not about sub-par English or the quality of research produced in the IIS-FJD, but rather the general unfamiliarity with what kind of research output he and his colleagues produce. Fortunately, however, he believes this

tide is changing somewhat as certain Spanish institutions gain recognition from Anglophone-center authorities, especially within the field of clinical trials.

Implications

This chapter is a testament to the drive Jesús possesses and the adroitness with which he manages lack of encouragement from within his support network and some degree of resistance from the greater academic community. Translating and transforming part of his doctoral thesis required a mixture of persistence, resourcefulness, and collaboration from pre-submission collaborators. In this example, Jesús navigated the peer-review system, finding the right balance between deference to review comments and pragmatism when replying to reviewer comments. This approach appears to come from a wealth of knowledge of the mores of the academic publishing community, and this know-how contrasts with the relative inexperience of others who have recently earned their doctoral degree.

With regard to the contribution I made to the RA, Jesús's remarks on my role and the initial confusion as to who had written the article raise doubts as to whether efforts to revise beyond the sentence level may be in vain. The fact that in the early stages of my analysis of this TH I believed that the person I had been liaising with was actually a go-between who assisted the author rather than the author herself hints that at least some of my comments may have fallen on deaf ears, not least because I write these remarks with a concrete, individual reader in mind. Additionally, Jesús's description of the role of English language professionals in scientific publishing indicate that I may have devoted more time and effort to the text than I was being asked to. In light of this feedback, more precise, author-by-author understandings of the desired scope of editing may be beneficial for all.

The rhetorical makeup of the paper evolved in a way that appears to see certain rhetorical moves as expendable when concerns of content arise. I have raised doubts, however, as to the actual intent behind some of the content-related concerns of the journal gatekeepers, especially given their seemingly disproportionate preoccupation with the inclusion of specific citations and their failure to follow through with the requests they made for changes to the text. Indeed, this

chapter has shown a certain disconnect between the author's response to reviewer feedback and the actual modifications made in the text. A careful analysis of the passages the author claims to have modified reveals only slight adaptations, hinting at a certain gray area between what is said and what is meant during peer review.

Text history 9: Laura

Overview

Written by a doctoral student from the Genetics Department of the IIS-FJD, this is the only TH in this thesis that failed to be published during the study period, and at the time of writing remains on hold as the author, a novice writer of academic texts in English whom I will call Laura, devotes herself to other career projects. In the written and spoken communications she and I had about the text, Laura provided me with useful background information, helping me understand the difficulties she faced. I am very thankful to her for opening up to me about this disappointment, especially since her experience provides such a clear counterpoint to some of the other THs I have constructed. In the sections that follow, I will analyze the numerous texts that Laura produced in this, her first attempt at publishing an RA. As I hope will become clear in my description, my analysis of the texts and the events that surrounded this would-be publication will be both atypical within my study but no less instructive of the pitfalls that may beset inexperienced NNES researchers.

The informant interview for this TH took place on June 23, 2015 and lasted 58 minutes. During this time, Laura explained that she decided to hold the interview in Spanish for the sake of clarity, although she does have a positive attitude toward the language and will continue to work in English in the future. During the course of the interview, she provided me with a rather detailed account of the writing process that she and the two senior coauthors—who will go by the pseudonyms Beatriz and Patricia—followed, and a number of these remarks segued into some rich commentary on the challenge that the writing process poses for her. In a way, this TH tells the story of an opportunity that came with a number of conditions, and these constraints were unmanageable on this occasion.

Chronology and texts

The documents that make up this TH are anomalous for a number of reasons. First, after Laura's manuscript was rejected by the journal to which she initially submitted her RA, she has neither revised the text nor submitted it to another publication. As a result, there is no milestone version

with response to feedback from journal gatekeepers and no subsequent attempt to publish the paper elsewhere. Second, Laura and her collaborators did not send me the text for revision before submitting it to the target journal, which means my analysis does not take into account the perspective of the language broker at any time. While it is true that pre-submission editing by me was not a prerequisite for inclusion in this study, all other THs do feature this step in the process. The reasons Laura gave for not requesting my assistance with the text have much to do with the time pressure she was working under, and this aspect will be factored in as I situate the trajectory of the text within this and other circumstances.

The absence of these two sources of input shifts the focus of the TH to the earlier stages of publication process, and as a result I took additional methodological steps to describe the textual process and product. Although in the email instructions to the volunteer informants I specified that I would be studying their drafts, a number of the versions Laura gave to me contain changes to only one section of the RA rather than a revision of the text as a whole, the reason being that Laura focused on one section of the RA at a time. Therefore, while this TH is lacking in interactions between the authors and journal gatekeepers, using these versions one can trace the steps that Laura took when writing. In light of this substantial difference, I have added to my usual focus on milestone versions a more detailed analysis of the nature of the changes made in these drafts. A table depicting the sections of the RA that Laura revised appears after the list of documents that make up this TH, which I hope will provide a better contextualization of how this text came to be. This closer look at Laura's rough drafts is the third unique trait of this TH.

The following is a list of materials that make up this TH. The milestone documents appear in bold:

- 1. Document A: First draft of the text (written by participant herself, no revision by senior co-authors)**
2. Document B: Second draft of the text
3. Document C: Third draft of the text
4. Document D: Fourth draft of the text
5. Document E: Fifth draft of the text (with contributions of Senior Co-author 1)
6. Document F: Sixth draft of the text
7. Document G: Seventh draft of the text
8. Document H: Revision of Materials and methods and results
9. Document I: Revision of the introduction
10. Document J: Revision of the introduction, Material and methods, and results
11. Document K: Revision of the introduction, Material and methods, and results
12. Document L: Revision of the introduction, Material and methods, and results
13. Document M: Revision of the introduction, Material and methods, and results
14. Document N: Revision of the introduction, Material and methods, and results (with contributions of Senior Co-author 2)
15. Document O: Eighth draft of the text (with contributions of Senior Co-author 1)
16. Document P: Ninth draft of the text (with changes to discussion made in collaboration with Senior Co-author 1)
17. Document Q: Submission letter to the first and only journal
- 18. Document R: Version sent to the journal**
19. Document S: Rejection letter from the journal with reviewer comments

At the time it was submitted (Document R), this RA had a rhetorical structure that was highly unconventional, and the way the text was written drew criticism during peer review. What is more, if, in its final state, the text did not contain the rhetorical traits expected by the discourse community, the previous drafts were even more different from what an RA usually looks like. For reasons that will be explained below, these can be most accurately understood as rough drafts.

Due to the nature of the drafts, a move-step analysis would have been an arduous task, as the text began as a report of two cases and then evolved into an RA describing 11 cases. Along the way, the text's content changed radically, making an evaluation of the rhetorical makeup of the paper a highly challenging enterprise, if in not an altogether unwieldy one.

This "roughness" is apparent in many of the texts themselves. Some contain notes that Laura made to herself, while others have incomplete sections or highlighted passages. Aside from this textual evidence, Laura explained to me that she would first produce a revised version of a previous draft, meet with her supervisor-coauthors, and, during these meetings, her supervisors

gave Laura verbal indications of the changes that needed to be made. Unfortunately, no written documents were saved from most of these meetings.

Faced with the challenge of telling the story of this RA, I first performed a rhetorical study of the milestone versions of the text in order to ensure methodological consistency with the other THs. Observing the relative scarcity of data that could be derived from only two milestone versions—i.e., the first draft (Document A) and the version sent to the journal (Document R)—I then examined the drafts produced in between, coding the texts not for rhetorical moves like in other THs, but rather indicating where Laura had modified the text and at what point.

The first draft (Document A) contains all sections (abstract, introduction, methods, results, and discussion) and was used as a baseline with which I tracked the shifting focus of Laura’s attention during the presubmission process; as a result, this column is not included in this table. Where I detected changes to the text, these changes were marked as “superficial” when they involved changes on the word level or relatively short rephrasings that left the notional meaning intact, and “substantial” when they indicated additions or deletions of longer stretches of text, reshuffling of paragraphs within or across sections, major changes to content, additions of metatextual elements such as sub-section headings, and other such modifications. The table below presents the findings of these studies. All lettered columns refer to the documents listed above; this list does not include the submission letter (Document Q) or the rejection letter from the journal (Document S), but rather focuses on drafts for the RA proper. Superficial changes are shaded in light gray and substantial modifications, in black.

	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	R
Abstract																
Introduction																
Methods																
Results																
Discussion																

In the following two sections on the agents who contributed to the text and how the text was first created and then evolved rhetorically, I will comment on some of the more salient observations I made of Laura's process of learning to write an RA. Where relevant, I will refer back to the moments along the text's path when she revised certain sections and the depth of these changes.

Evolution of the text and the role of literacy brokers

In 2012, Laura enrolled in a doctoral degree program in biology, and over the next four years worked toward completion of her thesis, which she succeeded in doing in November, 2016. Like many in the field of the sciences, she initially opted to submit a publication-based thesis consisting of three different published articles, and this paper was to be one of them. In April of 2014, her research group in the Genetics Department of the IIS-FJD received an invitation to submit an article for a specially themed issue of a scholarly journal outside of what is strictly her field of specialization, and she seized the opportunity to submit this particular text for consideration, hoping to include it in her dissertation. The invitation was originally extended to colleagues in the IIS-FJD Gynecology Department, inviting them to join a two-center collaborative effort between geneticists and gynecologists. As originally planned, the text would be coauthored between the researchers from the two institutions, and, as is customary, the IIS-FJD gynecologists offered the opportunity to their colleagues in the genetics department, catching Laura at a time when she needed to capitalize on such an opportunity. When I asked Laura if scientific texts frequently come about through extensive intra- and intercenter networks as in this case, she told me this was nothing out of the ordinary and that oftentimes researchers look for ways to include their colleagues in the work that they do, knowing that these symbiotic relationships are often beneficial for all concerned. So, together with Beatriz and Patricia, she worked on the article for the next four months, eventually submitting the article in September of the same year.

Laura described the overall experience as a difficult one. As she put it

Oliver: How would you summarize the process of drafting and trying to publish this text?

(...)

Laura: It was difficult (*laughs*). A team of gynecologists invited us to publish this paper in a journal. There was a deadline, and it was a race against the clock, even though I had a paper in mind [before receiving the invitation]. It wasn't exactly like this one, but it was about a similar topic and I wanted to publish it, but since we got invited to publish in this one obstetrics journal, we had to position it towards that field because [our coauthors] were participating, and we had to add more cases to make it more robust. So we had a basis [on which to draft our paper] but then it got a lot more difficult to adapt [our work] to accommodate the other people who were going to be coauthors. And we had a deadline that, if I remember correctly, was less than a month. So it was all very tight...to get our results and then to draft the paper, add cases...I had to put all the results together as quickly as possible so I could start writing the paper as soon as possible, and that's where things got complicated, adding more cases and then having to write it up, so...

Oliver: So it was hard for you to bring it all together into one paper?

Laura: Yes, of course, let's say...give it the right focus and have it all make sense with all the cases I had to add...to create a text that made sense.

In her explanation of the experience, Laura seemed to report a certain sense of discomfort with the task of adapting her research group's work to the demands of the type of text required for the specially themed issue and for the audience that would be reading the work. From this passage, one gets a sense that the arrival of this invitation to publish in the specially themed issue was something of a mixed blessing, and caught Laura rather unprepared for the task with which she had been charged. Accepting the challenge, she expanded the number of cases studied from two to eleven, thus altering the shape of the text considerably from beginning to end. This expansion of the text's content had a substantial impact on the materials used to make up this TH and on Laura's experience in creating the text.

Working under a rather ambitious deadline, Laura drafted the text alongside Beatriz and Patricia, both of whom work in her same department. In the early stage of the drafting process, Laura would submit rough drafts of the paper to Beatriz and Patricia for their feedback. These texts mostly reflected progress on single sections of the RA, and Beatriz and Patricia would return the drafts to Laura with typed or handwritten notes instructing Laura on how to modify the text. As the deadline approached, the three co-authors eschewed this note-taking process and began modifying the text directly in order to save time. Much of the feedback obtained during these

conferences concerned additions that were to be made to the text, and the participant color-coded some of these changes so that her two supervisors could easily locate them. Two of these annotations include “*THE ULTRASOUND FINDINGS HAVE TO BE INCLUDED HERE!*” (Document M, methods) or, in the list of co-authors in Document M, “*THE OBGYNs NEED TO BE INCLUDED*”.

Unlike the other texts included in my doctoral thesis, Laura did not send me the manuscript for review at any time because of the difficulty she encountered in completing the manuscript and the delays that these problems caused for her and her co-authors. I do, however, recall at least one occasion around the time when they were working on the article when Beatriz—with whom I have frequent face-to-face contact—asked me how long I would take to revise the text; when I answered that I would need a few weeks, she told me that they could not wait so long and decided to submit the article without revision by a language specialist. (This race against the clock did not cease for the rest of the process; in fact, Document Q, the submission letter to the journal, contains a request for the text to be considered despite the fact that it was being sent two days after the deadline.) During the informant interview, I asked Laura whether she believed that the fact that it had not been revised by me or someone like me had had a significant impact on the text’s fate, she answered that her paper would likely have been improved by a language broker, although she doubts that such a contribution would have been able to remedy the deeper flaws in its content. (Of note, her response to my question about whether the study could have been published in Spanish was quite similar, stating that even in her native language the fundamental problems with the study being reported would not have been resolved.)

One month after submitting her paper, however, Laura received a rejection letter from the journal, which stated that “although the topic is of relevance and interest, I regret to say that we will not be able to consider your work further as we do not feel the work contributes sufficient new insights to those already published in the literature.” This rejection was based on the assessment of a single reviewer, who provided Laura with harsh criticism on many fronts. The following passage is a transcription of these comments in their entirety. For the sake of clarity, I have underlined the language the reviewer used to phrase their criticism, numbering these phrasings for analysis.

This is a paper that claims to describe (1A) the non-invasive prenatal diagnosis of 11 cases of skeletal dysplasias. However (1B), in four they have no pregnancy outcomes (2) and in two they performed NIPD for sex selection in an X-linked condition and as the fetuses were female they were not at risk (3). It is poorly written (4), poorly referenced (including quoting presentations given at meetings where no abstracts were published) (5) and relatively uninformed (6). It gives the impression of a series of case reports strung together to try to make a paper (7). These authors have published cases of NIPD previously and I wonder how many of these cases have been reported before (8). Furthermore, the authors used invasive testing to confirm their results. This significantly limits the value of non-invasive testing (9)

Fetal sex determination to direct invasive testing in these circumstances is well described (10) and cannot be claimed (11) as diagnosis of a skeletal dysplasia. Further, relatively few skeletal dysplasias are x-linked (12)

Six cases were achondroplasia, of which outcome is not known (13) in three.

Lines 10-20 I would dispute the statement (14) that the diagnosis of skeletal dysplasias is usually done after 22 weeks. Increasing [sic] serious dysplasias are diagnosed earlier in pregnancy, and in good hands in the late first trimester. Most dysplasias, with the exception of achondroplasia, are detected at the time of the routine fetal anomaly scan.

These authors consider that the 'personalised' approach is the best approach to take for NIPD of skeletal dysplasias. In clinical practice this would be labour intensive, extremely costly and extremely impractical (15) in a busy service laboratory. A sequencing approach using panels to cover multiple mutations would preferable [sic] and has been reported (16). In addition, as this group develops a new assay for each case, presumably they will continue to offer invasive testing for confirmation of the results.

With regard to the methodology, artificial spiking for validation is flawed (17) in that adult DNA is longer than [sic] fetal and so may give misleading results.

The grammar needs attention (18) if this paper is accepted for publication.

In summary, unfortunately I do not think that this paper adds significantly to the literature and may in fact mislead readers as NIPD for single gene disorders should not require invasive confirmation as CPM and other factors seen in aneuploidy testing do not apply

Here, "the grammar" (18) and writing (4) seem to be the least grave of the concerns highlighted by the reviewer. More broadly, the reviewer believes that the paper evidences a lack of awareness of the literature (5, 10, 13, 16), a lackluster degree of thoroughness in reporting (2),

and even a sub-par knowledge of the information that guides clinical practice (14, 15). Some of these comments are based on his or her notions of common practice, which may also be informed by an awareness of the literature (14, 15), while in others these remarks more overtly index previous publications (3, 9, 11, 12, 16, 17).

The comments open in scathing fashion with a summary of the topic (Mungra & Webber, 2010) (1A, B) that prefaces a wholesale discrediting of the RA rather than attempting to check mutual comprehension. At no point does the reviewer make much of an attempt to praise Laura and her colleagues for their efforts, and goes as far as to make a veiled accusation of scientific misconduct, “wondering” whether some of the cases included in the paper have been included in other published works of theirs but are not cited in the paper (8). In the reviewer’s opinion, the text can scarcely be called a “paper” and bears a striking resemblance to a series of case reports stitched together to look like something else (7).

After receiving this carefully worded but caustic feedback, the author changed her plans, abandoning the idea of producing a publication thesis, eventually defending her thesis in monographic format. Curious to learn whether this series of events had had any impact of her decision and whether a different outcome may have motivated her to stay the course and present the remaining two articles required for completion of an article-based thesis, I wrote to the informant a number of weeks after we had held our interview. She assured me that this had not been the case, and that even if this particular RA had been accepted, her need to complete her doctoral thesis within a particular time frame would have made the publication-based format unfeasible for her due to the little time she had to complete her PhD and the lengthiness of the peer-review process. Also, after receiving this rejection, she has put this article on hold and is currently devoting her attention to other career pursuits. In the future she hopes to publish articles on other, more up-to-date laboratory techniques than the one described here.

Genre considerations

By studying these texts, I was able to understand how Laura approached the challenge of drafting this RA. My observations of the parts that she changed over time and the depth of these changes

suggest certain hesitation at the beginning of the process and more hurried effort toward the end. In general, Laura tended to incorporate data in raw form in the early stages, articulating these concepts in more rhetorically appropriate language toward the end, mostly with the assistance of Beatriz and Patricia. As will also become evident below, Laura's writing relies heavily on background information at the expense of authorial presence.

The abstract changed considerably under the influence of Beatriz and Patricia and also in response to the requirements of the target journal. Document A contains the following three-paragraph abstract:

[M1] The use of existing cell free fetal DNA circulating [sic] (ccffDNA) in maternal blood could allow performing [sic] prenatal diagnosis of fetal conditions in a safe way. The study of the paternal mutation in maternal plasma either in recessive or dominant disorders (paternally inherited) and the fetal sex determination for X-linked disorders, [sic] may avoid the need for conventional prenatal diagnosis (PD), which entails a risk of fetal loss.

[M2] In this work we report the Non Invasive Prenatal Diagnosis (NIPD) in two consecutive pregnancies in a couple at risk of having a fetus affected with Metaphyseal Condrodysplasia, Mc Kusick type (CHH) [sic] one case of Ellis Van Creveld Syndrome (EVC) [sic] both diseases with an autosomal recessive pattern. Six NIPD cases of Achondroplasia (ACH) (autosomal dominant disorder) and two consecutive pregnancies of hypophosphatemic rickets (XLH) [sic] an X-linked dominant disorder paternally inherited diagnosed [sic] by fetal sex determination.

[M3] The status of the fetus for the paternal mutation has been correctly characterized in all cases studied as well as the fetal sex determination in the X-linked disorder case. NIPD results were concordant with conventional PD.

Each of the three paragraphs employs one of the first three Moves of the Nwogu (1997) structure in order, that is, i) introducing purpose, ii) describing methodology, and iii) summarizing results. The first of these provides the reader with background information on the technique that Laura used in the study, focusing on the development of cell-free fetal DNA and its potential for safe prenatal diagnosis. The second sentence of the first paragraph provides greater detail on how the procedure is performed (i.e., studying paternal mutations in maternal plasma) and why this method is safer (i.e., avoiding conventional diagnostic methods, which introduce a risk of fetal loss). Using the modal verbs "could" and "may," Laura expresses that the aim of the paper is to confirm this possibility, thus making this an example of a conventional Move 1. The second

paragraph is entirely devoted to the methodology of the study, and is therefore coded as Move 2 (describing methodology). Non-invasive prenatal diagnosis is an application of cell-free fetal DNA, and here Laura explains that the study excerpted in the abstract will exemplify the potential referred to in Move 1. The third paragraph offers a summary of the results (Move 3), confirming that the procedure was successful in all of the cases studied. In this fashion, the unconfirmed information taken up in Move 1 is now taken to be confirmed. The abstract does not contain Move 4 (presenting conclusions), although because of the nature of the purpose (i.e., the possible benefits of the technique), this may have been unnecessary.

As stated previously in this chapter, the paper was overhauled substantially from the early drafts until the time it was submitted to the first journal, and it is to be expected that the paper would be modified accordingly. Despite the impact of changes in content, the later drafts reflect a more precise knowledge of the purpose of each of the abstract's sub-sections, as evidenced in the following change seen in Move 2 of the final version (Document R):

Document A

In this work we report the [sic] Non Invasive Prenatal Diagnosis (NIPD) in two consecutive pregnancies in a couple at risk of having a fetus affected with Metaphyseal Condrodysplasia, Mc Kusick type (CHH) one case of Ellis Van Creveld Syndrome (EVC) both diseases with an autosomal recessive pattern. Six NIPD cases of Achondroplasia (ACH) (autosomal dominant disorder) and two consecutive pregnancies of hypophosphatemic rickets (XLH) and X-linked dominant disorder paternally inherited diagnosed by fetal sex determination.

Document R

Methods: Non Invasive Prenatal Diagnosis in a total of 11 pregnancies at risk of four different skeletal dysplasias because of family history or ultrasound findings is shown: 1) an X-linked Hypophosphatemic rickets (2 pregnancies); 2) recessive Cartilage Hair Hypoplasia (2 pregnancies), Ellis Van Creveld Syndrome (1 pregnancy) and 3) “*de novo*” Achondroplasia (6 pregnancies).

Circulating cell free fetal DNA (ccffDNA) was extracted from maternal plasma in the first trimester of gestation for cases referred for family history and in the third trimester of gestation in cases with ultrasound findings.

Real Time PCR (RT-PCR) and/or minisequencing were used for the analysis of ccffDNA.

In addition to the clarity lacking on the sentence level in Document A and despite the fact that Document A is not in structured form, Document R more clearly summarizes the methods used

in the study, providing brief but informative details on the sample included (e.g., size, disease, method of detection) and the procedures followed. In fact, Document A does not offer any concrete indication of what steps were actually followed beyond “non-invasive prenatal diagnosis.” This weakness in the abstract was later remedied in the later versions, seemingly due to the influence that Beatriz and Patricia had on the text.

The introduction shows a markedly high presence of Move 1 (establishing a territory), but also employs Moves 2 (establishing a niche) and 3 (presenting the present work), albeit less prominently. Starting in the earliest drafts, the text also contains what I will refer to as five easily identifiable sections consisting of one or more paragraphs. The overall thrust of these sections can be summarized as follows:

1. General description of hereditary bone diseases, including their clinical manifestations, prevalence, and inheritance patterns (3 patterns), and concluding with a metatextual device announcing that the paper will present cases from each of the three different pattern-based groups and then describe the treatment options available for each one **(M1)**.
2. General description of one disease per pattern, devoting one paragraph to each **(M1)**.
3. Discussion of prenatal diagnosis in general and the risks of conventional prenatal diagnostic techniques, followed by a description of the advent of the non-invasive prenatal diagnostic techniques which do not carry this risk **(M1)**.
The section concludes with a mention of how additional knowledge is required in order for these techniques to gain more widespread use, which reads as follows: “However, NIPD for monogenic disorders is still under development and is not yet so widely available in routine clinical practice” **(M2, S1B)**.
4. Discussion of the promise and challenges surrounding the use of NIPD as a result of a number of circumstances **(M2, S1B)**.
5. Descriptive presentation of the work **(M3, S1)**.

As can be observed in the table appearing above, Laura substantially revised the introduction in the drafts corresponding to Documents C, G, I, and O, and in doing so tempered the predominance of Move 1 somewhat. Indeed, Document R features a substantially shortened introduction section that is centered less on the diseases in general and more on the diagnosis of such diseases. A look at the third of these sections—where the author reflects on the contribution NIPD has made to efforts to screen for the diseases discussed in the article—

illustrates the way this part of the text is narrowed in focus so that it more succinctly and directly can arrive at one of the primary take-home points of the paper, that is, that these diseases require a “personalized” or “individualized” approach. (In the fragment below taken from Document A, I have underlined the information that is also included in Document R; in other words, all of the concepts not underlined in Document A were eliminated by Document R.)

Document A

[M1] For those couples at risk of transmitting a monogenic disorder, Prenatal Diagnosis (PD) is available from the first trimester of gestation. **[M1]** However, PD requires the use of invasive obstetric techniques for fetal sample collection like Chorionic Villus Sampling (CVS) of Amniotic Fluid (AF) both involving a risk of miscarriage (0.5-1%).

[M1] In 1997 the presence of cell free fetal DNA circulating [sic] (ccffDNA) in maternal blood was described for the first time ⁽²⁾ opening a new Era in fetal medicine, the Era of Non Invasive Prenatal Diagnosis (NIPD). **[M1]** NIPD is based on the analysis of ccffDNA for genetic diagnosis of the fetus without the need of [sic] invasive obstetric procedures and thus preventing pregnant women from the miscarriage risk associated to [sic] conventional PD. **[M1]** It has been in the last decade when this field has experienced an exponential boost ⁽³⁻⁷⁾. **[M1]** At present, some NIPD tests are already available in the clinical practice such as fetal sex assessment (for pregnancies at risk of X-linked disorders, like X-linked hypophosphatemia), RhD determination (for pregnancies at risk of RhD alloimmunization) and the Non Invasive Prenatal Testing (NIPT) for the most common aneuploidies ⁽⁸⁾.

[M2] However, NIPD for monogenic disorders is still under development and is not yet so widely available in routine clinical practice.

[M1] Some of the peculiarities of the NIPD are that ccffDNA represents only 9-20% of total DNA in maternal plasma ⁽⁹⁾, and nowadays these fetal sequences cannot be isolated from the maternal ones. Hence, initially most of the studies have been restricted to the diagnosis of paternally inherited defects that are not present in maternal [sic] genome, so to ascertain its fetal origin. **[M1]** Nevertheless, the use of new technologies, especially Next Generation Sequencing (NGS), is changing the panorama and now the study of fetal maternally inherited NDA tracts in maternal blood is also possible ⁽¹⁰⁾. **[M1]** However, NGS is at present an expensive technology and not always available in diagnostic labs. **[M1]** Even more, this technology requires the analysis of several samples at the same time to be cost-effective.

[M2] In contrast, PD of monogenic diseases usually requires an individualized approach for the analysis of the specific familial genetic defect.

Document R

[M1] Since the detection of circulating cell free fetal DNA (ccffDNA) in maternal blood ¹, the possibility of Non-Invasive Prenatal Diagnosis (NIPD) has been widely explored. **[M1]** Cell free fetal DNA can be detected in the maternal blood from the 4th week of gestation ², represents around 10% of the circulating DNA ³, and it is rapidly cleared from the maternal circulation ⁴. This means that 'a safe' prenatal diagnosis can be perform [sic] early in pregnancy. **[M1]** NIPD of skeletal dysplasias in the clinical practice is limited to screening pannels [sic] for the most common mutations linked to ACH or thanatophoric dysplasias ^{5,6}. **[M2]** However, cases with a family history of skeletal dysplasia with a known defect require a personalized study of the familial defect/s.

Here, much of the background information that was once included in the first draft is treated more succinctly, allowing the author to get across much of the same degree of detail with greater clarity. As compared to Document A, where Move 2 appears to be buried in information that contributes little to the rhetorical progression of their argument, in Document R Laura more effectively makes the case for the benefits of NIPD while giving greater prominence to Move 2, that is, that the developments in NIPD cannot be applied in blanket fashion because of the distinct characteristics that some diseases present. These modifications illustrate how Laura and her supervisors reworked this Move 1-Move 2 sequence without altering the underlying ideas, bringing the paper more in line with the type of rhetorical structure described by Swales (2004).

Laura and her coauthors made other modifications to the introduction that concerned how to treat background information and, more specifically, how to bridge the gap between field-specific genetics knowledge and the expertise of the discipline of gynecology. This effort is seen in the way the authors reworked the text along its trajectory and is intimated in Laura's comment (excerpted above) in which she remarks on the difficulty of striking this balance. First included in Document I—which contained a substantial rewrite of the introduction and no other sections—the passage below illustrates how the authors altered their text to do this:

Skeletal dysplasias are a large heterogeneous group of disorders with a prevalence of 3 per 10,000 births and 20 per 10,000 stillbirths.

Prenatally, suspicion of a fetal skeletal dysplasia is done [sic] on the basis of sonographic detection of long-bone shortening. This diagnosis is mostly done in the third trimester of gestation, as shortening starts from the 22-24th week of gestation, after time of routine scan. But even in the third trimester misdiagnosis is common. For a definite diagnosis, molecular genetics investigations are needed together with radiology exams and, in case of birth, with other postnatal investigations.

This stretch of text, which remained untouched for the rest of the RA's trajectory, can most accurately be described as Move 1 (Information move), and displays a certain preoccupation with situating the role of genetic diagnosis within clinical practice. While there is no change in the Move or Move cycle per se, the revision is noteworthy inasmuch as it exemplifies rhetorical use of scientific literature of differing degrees of specificity to fashion an RA for a diverse readership.

In analyzing the methods section, it must be kept in mind that Laura originally planned this research to investigate only two pregnancies in the same couple, and therefore this is the number of cases included in the first two drafts (Documents A and B). As of Document C, the number of cases was expanded to eleven, which altered the shape of the text in many ways. In the first draft of the RA (Document A), Laura begins with Move 4, Step A (indicating source of data), describing the patients whose blood was used in the procedure. The rest of the text proceeds in largely sequential fashion and consists of a series of Move 5, Step B (describing experimental procedures, recounting experimental process) or Step B (identification of main research apparatus) realizations. The difference between these two steps is slight, with some placing more focus on the actions taken, while others giving greater prominence to the materials used.

The first substantial change to the methods section took place in Document C. In expanding the number of cases, Laura reshuffled certain information between paragraphs, although in large part she wrote the section based on the same rhetorical template appearing in the two previous drafts, changing only things like disease names, values, and other information of no rhetorical importance. In Document H, however, she added an introductory paragraph to the section to describe procedural aspects that were common to all of the eleven cases. The paragraph is as follows:

In all cases studied twenty milliliters of material blood were [sic] taken in EDTA tubes at different weeks, in the first trimester of gestation, except for achondroplasia cases. Sample processing for maternal plasma collection and DNA collection has been also [sic] carried out in the same way for all cases as previously described (3).

Although the move structure described by Nwogu (1997) does not include mention of an introduction like this one, Laura here aims to lessen some of the repetitiveness by describing the study in overall terms. This excerpt can most accurately be described as Move 5, Step B (describing experimental procedures, recounting experimental process).

Even more so than other sections, the paper's methods received intensive work in the final drafts, especially as of Document L. The most relevant of these changes takes place when the authors broaden the information included in the section's introductory paragraph to indicate that the

diseases investigated fall under three different categories according to their pattern of inheritance. This addition, however, was removed in Document O.

Like the methods, the results section is written in a rather perfunctory manner. During the participant interview, Laura revealed that writing this section of RAs is quite tedious for her, as it requires extensive research on the names of the genes being studied and other details relevant to what the study outcomes were. Because of the nature of the tests performed, several of the results reported in the article in turn created or eliminated the need for further testing, and these additional tests were expressed in instances of Move 0.3 (Procedural). The following is an example taken from Document R:

Fetal sex determination revealed a female fetus in the first pregnancy and a male fetus in the second one. Hence, multiplex PCR for *SRY/RMRP* genes was only considered in the second pregnancy.

Also due to the study's aim to show the circumstances under which non-invasive prenatal diagnosis can often replace the traditional invasive procedures used, this section contains a number of instances of Move 2.1 (Substantiation move) in which the results of the non-invasive procedures are compared to those of invasive procedures after both are performed. An example of this that appears in both milestone versions of the text is as follows (this excerpted from Document R):

In the first pregnancy the g.236A>G mutation was detected in maternal plasma. Conventional PD by chorionic villus sampling at 12 weeks of gestation confirmed the NIPD results and revealed that the fetus was also carrier [sic] of the maternal mutation, and therefore affected.

As indicated in the table above, the first substantial change was made in Document C with the addition of nine cases to the study sample. Similar to the methods section, this change in content was not reflected in a reworking of the rhetorical characteristics of the section, and most of the revisions involve surface-level changes and additions of certain elements of content that did not affect the rhetorical structure of the text. In Document H, the author began to include tables and references to tables in the text, marking a change of some significance within the text. As with

the methods section, the most significant changes to the results began with Document L, and this section underwent two more substantial modifications and one superficial change before the RA was submitted to the journal. Document L reflects two additions offering greater detail, although this detail is more closely related to methods than to results in the strict sense of the word. This change, which took place between Documents K and L, is shown below:

Document K

The analysis of the *EVC2* gene using artificial mixtures of paternal DNA in maternal DNA revealed that the detection limit for the paternal mutation and *SRY* gene was 5%.

The analysis of the *EVC2* gene in the two plasma samples revealed the absence of the paternal mutation in both samples and the presence of the *SRY* gene.

Document L

The analysis of the *EVC2* gene using artificial mixtures of paternal DNA in maternal DNA revealed that the detection limit for the paternal mutation and *SRY* gene was 5%. After this step both maternal plasma samples collected at 9 and 12 weeks of gestation were studied by minisequencing and these studies revealed the absence of the paternal mutation and the presence of the *SRY* gene. By this way we confirmed the presence of a fetal origin in the samples studied.

Leaving behind an undeveloped style that used one-sentence paragraphs to indicate steps taken in the study, in Document L Laura more clearly marks the progression of the experiment steps, provides further information on when these actions were taken and what procedures were used, and also indicates the conclusions derived from the resulting data.

The discussion is by far the most challenging section for Laura to write, as she revealed to me during the interview. I had not originally planned to ask this question, though the issue came up while Laura was explaining to me that the section abbreviations (e.g., “int,” “mat,” “met”) appearing in the document file names stood for the section that had been revised in that particular draft. As the word *discusion* [sic] was not included until Document P—or the last version drafted before the article was sent to the journal—she remarked the following:

Laura: It's true that with the discussion I sometime take time to try and...if I'm pressed for time and can't get to the discussion...because it's the part that I have the most difficulty writing...

Oliver: So the discussion is the most difficult.

Laura: Yes. It's the most difficult, especially to bring all the concepts together so that they make sense. It's what I...the part that's most difficult for me to write is the discussion. So because I know that sometimes I spend more time on it, I get to the discussion and say "Ok, let's take a look at this [other] part and then I'll take a closer look at the discussion at a later time."

Once again, she feels less confident when stringing ideas together and creating a text that flows logically, although rather than devoting more time and effort to this task, she postpones work on the section, which may have added to her difficulty in the end.

Throughout much of the writing process Laura maintained the discussion she had originally drafted, which contained a somewhat peculiar sub-section called "Future Perspectives." In this section, Laura makes no direct allusion to the study's results, but rather comments on further uses of the procedures explained in the study and how the application of these techniques may evolve in the future. This sub-section, which in my experience is uncommon in biomedical RAs, was eliminated in later drafts of the text, possibly because the information conveyed in it lay outside the scope of the paper or could more appropriately be summarized and used as a simple Recommendation Move as indicated by Dudley-Evans (1994).

Similar to the patterns we have seen in both the introduction and methods, Laura also devotes a great deal of the discussion to background information (Move 1, Information Move). In the final draft (Document R), this section, which consists of 13 paragraphs, contains no explicit reference to the author's own work until the fifth paragraph. And when she does begin to mention her work, she does so in unconventional fashion, and the way this information is conveyed may have led the reviewer to challenge this claim. The passage from Document R to which I am referring is as follows:

Taking into consideration the low percentage of ccffDNA in the sample (9-20%)³, an optimized NIPD protocol must show a high sensitivity comprised [sic] between 2-10%. As a group working in clinical practice, we know that the standardized protocols should be adapted to the specific cases, depending on the molecular basis of the disease and for this reason, we consider of [sic] great importance a personalized NIPD approach to every single pregnancy as it [sic] happens in conventional prenatal diagnosis.

Rather than follow rhetorical convention by presenting a result (Move 2) or finding (Move 3) and then either commenting on such a result by indicating it to be expected or unexpected (Move 4) or comparing it to previously published studies (Move 5), Laura makes this statement on the need for a personalized approach by using a reference to the literature and by putting forth the “knowledge” of her department. This rhetorical strategy appears to have backfired on Laura, as seen in the following comment made by the reviewer:

These authors consider that the ‘personalised’ approach is the best approach to take for NIPD of skeletal dysplasias. In clinical practice this would be labour intensive, extremely costly and extremely impractical (15) in a busy service laboratory.

Knowing the negative outcome of the submission, we are left to wonder whether the claim could have been successfully argued if the discussion’s rhetoric had been shaped differently. Specifically, if Laura and her coauthors had designed their study to prove or disprove the argument by which each case is taken to merit a personalized approach, or if they had molded their argument in a way that made this assertion less prominent, would the reviewer have had such an easy time of criticizing the paper’s merits on so many fronts?

Despite the alleged flaws in the points she makes in the discussion and her great reliance on background information, Laura does make use of certain common move cycles, thus suggesting that much of her difficulty was related to issues other than writing per se, and that, if provided with a new opportunity, she may rise to the challenge. The section contains one cycle consisting of Move 1 (Information) – Move 3 (Finding) and another made up of Move 1 – Move 3 – Move 7 (knowledge claim). In addition, changes made between Documents P and R evidence a more rhetorically orthodox style, as seen in the following passage:

Document P

In the monogenic disorders field, the introduction of a [sic] high [sic] sensitive and precise technology has allowed the design of panels for some disorders caused by a prevalent mutation like occurs [sic] in diseases like achondroplasya, thanathophoric dysplasia or cystic fibrosis (XXXX, personal communication).

Document R

In the monogenic disorders fields, the introduction of a [sic] high [sic] sensitive and precise technology has allowed the design of diagnostic panels that allow the study of different mutations at the same time. They usually have the most prevalent mutation together with outnumbered mutations. This has been the case for achondroplasia, thanatophoric dysplasia or cystic fibrosis ¹⁹. **[M8]** The application of such a panel may have helped in the diagnosis of the fetuses where the common mutation was not found.

Here, the same basic information is reformulated between these two drafts, with the second of these passages explicitly indicating the limitations of the study (Move 8). Though it is likely that a knowledgeable reader may have arrived at this same conclusion through inference, it is also true that members of the participant's discourse community may be more welcoming to such admissions and would find language like "may have helped" to be more familiar in this type of text.

Attitudinal aspects

This researcher has a positive attitude toward English and embraces the opportunities she will have to use the language in her career in the near future, although when she and I spoke she was less sanguine about her career prospects as a researcher. The remarks gathered in this TH found Laura in the final stages of writing her doctoral dissertation, and she was once again under certain time pressure to conclude. In the fall of 2015, she moved to another city in Spain to begin a postdoctoral position, where she would participate in meetings with other European colleagues. Although she enjoys researching more now that she has become more comfortable with it, she believes she is up against fierce competition. Laura believes that the need to write papers like this one in English puts her at a disadvantage relative to others, although she believes this disadvantage to be linguistic and not related to other factors.

Implications

In this TH, we see how an inexperienced researcher struggled to adapt to the demands inherent to a publication thesis, and the story of this text can be considered something of a “crash course” in both academic writing and the world of English-language academic publishing. Faced with the need to be the first author of two articles and a co-author of a third, Laura compromised on her publication plans and agreed to adapt her work to the opportunities afforded by her network, both directly (i.e., her thesis advisers) and indirectly (i.e., the researchers who often collaborate with her advisers). Unable to persevere under her circumstances, she opted to shelve this paper.

Laura seems to have an inchoate knowledge of how to design studies, draft a text of these characteristics, and adapt to differing demands of audience. Rhetorically, her writing here lacks in authorial presence, as evidenced by overuse of background information and unconventional claims of expertise. Her idiosyncratic approach to rhetorical structuring was tempered throughout the text’s arc, though not entirely, as some of the rhetorical moves appeared to be clearly out of place.

With all the paper’s shortcomings, however, the reviewer comments seem unnecessarily harsh and border on outright unprofessionalism. Little or nothing in the reviewer’s blunt assessment gives hope to the authors or allow them to save face, and suggest a lack of benevolence toward the authors and little concern for the betterment of science. On examination of the named authors and their academic degrees, it seems this reviewer could have easily identified Laura as a novice academic writer and spared her the scathing remarks without changing the final editorial decision. From my outsider perspective, the treatment Laura received during peer review was overly critical and appears unnecessary, and may have been the result of the personal or professional agenda of the reviewer.

Text history 10: Ernesto

Overview

This tenth TH was authored by a team of researchers who belong to the IIS-FJD departments of pediatrics, pediatric endocrinology, endocrinology and nutrition, and the lipid metabolism laboratory. The informant and corresponding author, whom I will call Ernesto, is a senior member of the pediatrics department and a highly experienced researcher within the field of metabolism and endocrinology. This text is the second publication to come out of the doctoral thesis of the first author, Silvia, a junior researcher who completed part of her medical training under the tutelage of Ernesto in the years leading up to the publication traced here. Her doctoral thesis, which was defended in 2014, was supervised by both Ernesto and another of the paper's senior coauthors.

Ernesto described the post-submission review process as quite challenging. This difficulty was not extraordinary in his experience, however, as he reported that in recent years it has become substantially more difficult to publish in English. Few substantial changes were made to the article from the beginning to the end, though this and other decisions that will be discussed in this TH were made consciously and stem from Ernesto's great pragmatism and awareness of the constraints he faces both as a researcher and as a clinician and university professor. While not compulsory, both the decision to publish this work in an English-medium international journal and the choice to attempt publication in high-impact journals were part of a strategy that Ernesto was readily able to articulate, revealing a deep understanding of the opportunities available to NNEs researchers like himself and the limits they face in this endeavor.

One such limit that came to light as I traced the history of the text was not related to the authors' ability to write clearly and in a rhetorically felicitous manner, but rather had to do with the what were perceived as methodological shortcomings. According to Ernesto's assessment, the fact that the investigation was undertaken as part of a course of doctoral study required the authors to make certain compromises on the breadth of their analysis, and any attempt to broaden the number of variables studied would have made the study unwieldy for a doctoral thesis. In

addition to criticizing the methodological robustness of the study, gatekeepers found fault with the authors' attempts to cite their previous work, raising interesting questions about membership and hierarchy within the discourse community. Limitations imposed by time, resources, and perceived outsidersness were some of the themes that arose in the 42-minute informant interview, which took place in the early morning hours on July 7, 2015 and was held in Spanish.

Chronology and texts

One major absence is the first round of reviewer feedback received from the third and definitive journal. When I requested the materials related to the text, I learned that these had been misplaced and could not be recovered. Had I been able to access this document, I would have obtained a more complete understanding of the issues that the reviewers focused on in their initial comments. In spite of this, however, certain comments made in the second round of reviewer feedback alluded to the recommendations made in the first round and to ways in which the authors had responded to these comments, thereby limiting the impact of this gap. The following list details the different documents made available to me, with the milestone versions appearing in bold.

- 1. Document A: First draft sent to me for revision**
- 2. Document B: My edit of the draft**
- 3. Document C: Version sent to the first journal**
4. Document D: Rejection letter from the first journal
5. Document E: Version as sent to second journal
6. Document F: Rejection letter from the second journal (following peer review)
7. Document G: Letter sent to editor appealing the rejection; subsequent denial to reconsider the decision
8. Document H: Draft of point-by-point response to second round of reviewer feedback from third journal (first round of reviewer feedback missing)
9. Document I: Definitive point-by-point response to reviewer feedback
10. Document J: Draft revision of text sent to third journal (following second round of reviewer feedback)
11. Document K: Revised version sent to third journal
12. Document L: Letter of acceptance from third journal
- 13. Document M: Final published version**

Evolution of the text and the role of literacy brokers

Finding a journal that was willing to publish this RA was an arduous task for Ernesto and his coauthors, although Ernesto was not surprised by such resistance. Indeed, Ernesto stated during the informant interview that fewer and fewer RAs are accepted by top journals nowadays, with some of the most sought-after publications accepting between five and ten percent of all submissions received. This degree of difficulty was compounded by factors that can be attributed to the study's scope and the methodology used. On the one hand, the relatively small size of the FJD hospital limited the researchers' ability to recruit a broad sample of patients, thus decreasing the robustness of the data. An additional hindrance had to do with the time frame in which Silvia—currently a staff physician, who at the time the data were being collected and also when the RA was published, was in her residency—had to complete the study; indeed, rather than devoting the five to six years it customarily takes to compile and analyze the data, Silvia had to limit this process to two or three.

Silvia, the first author, was responsible for most of the drafting of the article. Ernesto's main role was to supervise Silvia in this process, and as the senior member of the research team he was chosen as the corresponding author. Aside from Ernesto, two other senior researchers provided input and appear as named coauthors: one of these individuals was Silvia's other thesis supervisor, and the other, one of the other informants appearing in a different chapter of my study. Of these last two, however, Silvia's thesis supervisor provided input only on content, while the other coauthor did shape the text in some ways. Taking this into account, it can be said that this paper was aided by input from senior collaborators, with one, Ernesto, playing a more substantial role than the other.

Ernesto's primary contribution was in assisting Silvia in drafting the study in generically appropriate fashion. Interestingly, Ernesto made it clear to me that the skill set that Silvia lacked at the time was not precisely about language aptitude or the ability to write in English, but rather had more to do with writing English-language RAs. During the interview, he called Silvia's proficiency with the language "extraordinary," and stressed that she wrote the text in English despite the fact that the conceptual rooting of the study was in Spanish. Though Ernesto spoke

admiringly of Silvia's grasp of the language, he referred to her need to master "medical style." This notion of rhetorical competence and genre-appropriate writing for the disciplines has been raised previously within the discipline of EAP, suggesting that the challenges that novice NES and NNES academics face when acquiring academic competencies have a great deal in common (Hyland, 2016; Swales, 2004; Tribble, 2017).

The authors sent the RA to me for revision before submitting it to the first journal, and my contribution can be seen in milestone version B. Most of my contributions came in the form of direct interventions in the text rather than remarks made in margin comments. While in my experience these in-text interventions usually target word- or sentence-level issues and aim to clarify meaning, on occasion these proposed modifications have certain ramifications for the rhetorical makeup of the article, bringing certain aspects into sharper relief, making explicit what was once more implicit, and other suggestions that arise as I revise authors' drafts. As will be explained in some detail in the following section on the rhetorical evolution that the text underwent, a number of my interventions left their mark on the text's rhetorical style, as they were accepted by the authors in their entirety.

When asked to evaluate how the RA evolved from beginning to end, Ernesto told me that, save for certain suggestions made by journal reviewers for the third and final publication, the similarities between the earlier and later drafts far outweigh the differences. Even before he made this admission, however, I was eager to inquire about why the first two rejections did not motivate him to rework the text in the hopes of improving its chances of being accepted by the next journal. He replied that he tends not to make substantial revisions to his work unless the reviewer feedback appears to indicate that the journal is likely to accept a revised and resubmitted manuscript. Under this premise, Ernesto sends his work first to high-impact-factor journals and then, if unsuccessful, resorts to other publications with an increasingly lower impact factor. He believes that rejection from academic journals situated in the first quartile of his specialty is often no indication of whether his papers are good, as most often these journals turn 90 to 95 percent of all submissions away, often doing so without sending the work for peer review. Additionally, attempts to rework papers based on comments made by previous

gatekeepers may be in vain, and in some cases subsequent reviewers criticize the very features that had been changed in response to feedback from previous journals. Lacking time to research, Ernesto seeks to maximize the return on the time he invests, hedging his bets when he feels that there is more to lose than there is to gain. The RA of his that I analyze here exemplifies this dynamic: after rejection by the first journal with scant feedback from the editor, the second journal's reviewers found the paper's flaws to be too substantial to merit publication, and following these first two unsuccessful attempts Ernesto and his collaborators did find a publication that appeared interested in their work.

Rejecting the paper without sending it for peer review, the editor of the first journal explained his decision in the following way (Document D): "We regret to inform you that your manuscript does not meet our current needs. Mainly, it does not focus enough on peptides (even though insulin is there)." When I asked Ernesto to comment on this, he said that he was not surprised at this decision, adding at another point of the interview that he is a reviewer for the journal, and in his experience reading published work and reviewing submissions from his peers, he has come to hold the editor in very high regard, calling him "brilliant." Though the rejection came as something of a disappointment to Ernesto, Silvia, and their fellow coauthors, he took heart in the editor's words and believes that they were sincere.

Two points of interest came to light as we discussed this first submission. First, Ernesto remarked that while the content of the RA was clearly outside of the focus of the journal, he believed that if he had either changed the title of the paper to bring it more in line with the journal's scope or if one of the concepts treated in the text (i.e., insulin resistance or "factors affecting insulin or other peptides") had been foregrounded, the outcome may have been different. What is striking about Ernesto's assessment from the vantage point of genre studies is the extent to which it reflects what Berkenkotter and Huckin (1995) refer to as "the highly contingent and tentative epistemological status of the scientist's knowledge claim" (p. 49). Rather than a transparent account of what researchers like Ernesto and Silvia observed, the RA genre is a conversation with peer reviewers, editors, and the discourse community as a whole. As ideas are refracted through this ecosystem, what was once seen as a secondary aspect of research may be reformulated in

such a way as to meet the needs of journal gatekeepers and the community of scientists. Views like Ernesto's challenge narrow conceptions that understand the text and the title to be transparent, inflexible representations of the findings of the study, and hinting that scientific findings can be cast in different ways to appeal to different target journals.

As mentioned previously, the RA was sent to the second journal unchanged after it was rejected by the first. This time, the text was sent for peer review, although the reviewers found flaws with both the study design and the reporting of the study, and these faults led them to suggest that the text be denied publication. I was able to access only the second and third reviewers' comments for this publication, and both of these individuals found the sample to be inadequately described, and therefore the validity of the RA's conclusions were considered to be somewhat shaky. While Reviewer 2 believed that the article failed to report any findings of interest, Reviewer 3 recognized this lack of novelty in the findings but conceded that the text did add new information, especially as regards "the controversial relationship between insulin resistance and vitamin D." This difference in criteria appears to indicate that some journal gatekeepers demand new findings for them to be granted publication, while for others, new descriptions of previously recognized problems in research make for valid contributions.

Ernesto attributed the methodological shortcomings singled out by the reviewers to the limitations of time and size that Silvia faced. At the time the study was being conducted, Silvia was both a resident and doctoral student, and as such could only perform research with the time and resources she had available. Ernesto did take issue with some of the particular remarks, however, as he believed them to be inaccurate. Believing as he does that submissions sent to three reviewers rather than two are likely to have received one positive and one negative review from the first two—leading the editor to request a third review in order to break the tie—Ernesto appealed the editor's decision to reject the manuscript, citing inaccuracies in the criticism received from Reviewer 2 (the reviewer who found nothing new in Ernesto's work). The central issues raised in the letter of appeal appear in the following excerpt:

If you can read [sic] the manuscript you will see that the values of insulin resistance (HOMA index) were not expressed in absolute values because they were influenced by sex and pubertal stage. Thus, they were expressed in Z-score (see subject [sic] and methods and discussion) according to Tanner stage and sex of our population. It is the great advantage of our research regarding [sic] previous reports. That was the reason to [sic] analyze all pubertal obese children together instead of different Tanner stages, because de [sic] insulin values were standardized in Z-score (SDS).

Here, Ernesto urges the editor to allow him and his coauthors the opportunity to revise the text, claiming that the criticism levied by the Reviewer 2 citing a failure to indicate the subjects' puberty stage was inaccurate, and going as far as to highlight the richness of the approach used to do so when compared to other studies in the literature. However, this was to no avail, as Ernesto later received the response not from the editor him- or herself but rather from what appears to be the editor's assistant, stating:

I have forwarded your email to the Editor-in-chief, [name redacted], who asked me to let you know that he doesn't reconsider his previous decision, as according to the scores assigned by both reviewers and the Editor the manuscript does not achieve enough priority to be published in [journal name redacted].

In addition to the methods of the study, another fault found by both Reviewer 2 and Reviewer 3 had to do with the way in which the RA reported on the cohort and study design used. In the version of the text sent to this, the second journal, the authors begin the Material and methods section with a very short subsection titled "Design." This section consists of a one-sentence paragraph that reads, "A cross-sectional study of the prevalence of vitamin D deficiency was performed in the Pediatrics Department of Fundación Jiménez Díaz between January 2011 and January 2013 [4]." In the pre-submission stage, this description had been rewritten a number of times, suggesting that the authors may have felt they were on unsteady footing. Reporting on the manuscript, Reviewer 2 writes, "Details about the cohort and study design should be included in the manuscript," and Reviewer 3 concurs, going further into why this was believed to be inappropriate:

The section of Study design needs more detailed description. Reference can be made without details only if you assume the readers should be familiar to [sic] the contents referred. Here you really cannot assume that all readers are familiar to [sic] one of your previous publications.

Although uttered by only one reviewer, these remarks index shared assumptions of the discourse community, and the view taken here clearly situates the authors as outsiders—or, at best, newcomers—to the disciplinary community. In particular, the reviewer at once recognizes the practice of self-citation as a highly formalized and standardized code that can replace more lengthy descriptions of experimental methods (Hyland, 1999) and rebukes Ernesto for improper portrayal of himself and his colleagues as members of the disciplinary community (Hyland, 2001). Ernesto told me he was surprised by this criticism, and believed the comments to be motivated by the fact that the work cited had been published in a different journal and also because the text they were referencing is in Spanish. On this point of referencing a text in Spanish, Ernesto found it questionable for a paper to be given this outsider status because it is in Spanish, especially as the text was indexed and translated into English by the journal itself. Curious to locate this text they were citing, I found that the journal had only begun translating authors' work into English as of the June 2014 issue, though the article appeared just before this change, in April 2014. Regardless of whether the reviewers could have read the text in English if they had wanted to, no mention was made of the article only being available in Spanish, and it appears that comments like these imply a certain hierarchy of voices and privilege within academic publication. If we consider this treatment received by Ernesto and Silvia in light of the abnormal syntax used by the reviewer, it is conceivable that the reviewer who disparages this rhetorical move is also an NNES researcher, though one who enforces a strict interpretation of the inner-outer divide.

One issue that is particularly telling about this exchange is the way in which good writing, membership and exclusion, the dominance of English as the language of scientific publication, and the genres of scientific research come together. Although the reviewer targets a particular passage in the methods section, it would be inaccurate to claim that this is an issue of the authors' ability to write in English or to state that the methods section was particularly challenging for

Ernesto and his colleagues. Rather, this gatekeeper situates these Spanish researchers on the periphery or semi-periphery, denying them certain ground the authors had laid claim to by citing themselves.

Though the gatekeepers representing the third and final journal found some of the same methodological faults as the reviewers from the previous publication, they did finally accept the text after what appears to be one round of revision. After their work had been criticized for what the reviewers took to be an insufficient set of parameters on the study subjects, Ernesto and his collaborators included explicit mention of these shortcomings in Document J, which is a draft of the revised article eventually sent to the third journal, and this modification is maintained in its final, published form. The change appears to mark an attempt by the authors to preclude criticism on issues stemming from the somewhat limited scope of the study explained above. Nonetheless, one of the reviewers remarked on this perceived lack nonetheless, writing the following:

You state pubertal subjects as being in stage II-V, it may be better to split the groups further into mid puberty and completed puberty as it is unclear from the data whether there are [sic] any difference in the stages of puberty between the groups which may account for the differences.

In the authors' point-by-point letter, they again recognize this matter to be a weakness in their study design, and respond with some resignation when they write:

We also agree that it would be better to split the groups according to the pubertal stages instead of pubertal and prepubertal group. Unfortunately, we did not assess parathyroid hormone (PTH), alkaline phosphatase, calcium and phosphorous as we had already included in the discussion as a limitation of our work.

Highlighting the fact that this limitation was already mentioned in the text may have been a way to put an end to further exchanges with the journal gatekeepers on this issue. This strategic use of feedback from previous experiences of peer review and the framing of this information as a limitation to the study show how, if persistent, authors can use prior failures to their advantage and successfully publish their work.

In addition to the RA's handicaps mentioned in this chapter, the story of this text reveals how Ernesto was stymied in his attempts to leverage his own stature and knowledge. Ernesto is at the same time a bona fide member of his disciplinary community—reviewing submissions for a prestigious journal—but also one who was criticized for making assumptions of his own membership by citing his work in a passing manner. In the part of our talk that focused on his experience and the implications of the aforementioned criticism, Ernesto told me that he was first given the opportunity to review submissions after having done his doctoral work under a fellow Spaniard who is one of the journal's editors, and for the last several years he has received requests to evaluate manuscripts related to the sub-field that he researched during his doctoral thesis. When acting as evaluator rather than evaluated, he tries to be exhaustive but also constructive, although he remarked that he cannot help submitting authors when the methodology of their papers is so faulty that they make the text unsalvageable.

During this part of our talk about his attempts to find recourse in the editor and what he believed to be an unfair assessment of the RA, Ernesto's use of the term "lobby" to reference groups granted privileged status by reviewers and editors became the focal point of a lengthy discussion on the hierarchy among academic authors within his field and the possible ways in which this pecking order has come to be. In his view, the output of some research groups is accepted virtually sight-unseen by the target journals of their choice, while those who are not a part of this select few must endure multiple rejections and, if they are fortunate enough to find a publication that is willing in principle to consider their work, often do so only after more than one round of revisions. Either granted or earned or both, the favor enjoyed by these "strong research groups" limits the time and effort required for them to achieve their aim, thus making them more able to undertake new research, thereby contributing to what appears to be a vicious cycle that reinforces existing relations of power.

In describing this dynamic, Ernesto stated that Italians are one of these privileged groups for whom acceptance of manuscripts is a matter of course. This is so because Italian-American researchers working in the field are in the position to host Italian researchers who travel to the United States as visiting researchers. Once there, these European investigators form ties with

North American research teams, and on their return to Italy, they leverage their connections with Italian-American scientists to fast-track their publications in prestigious journals, and the cycle perpetuates itself across generations. Unlike their Italian colleagues, when given the chance, Spanish researchers who participate in these same fellowships are unlikely to return to Spain, thus preventing a bidirectional flow and precluding the establishment of the same relations that Italian research groups enjoy. We did not go further into what leads Spanish researchers to emigrate rather than return to their home country, although I have a strong suspicion that this assessment of Ernesto's is part of a broader commentary on what he believes to be lack of opportunity for Spanish researchers in their home country.

Motives aside, these remarks made during the informant interview indicate that Ernesto believes he and his colleagues are certainly not members of the inner circle of their disciplinary committee, although what is most revealing is his conviction that certain countries have come to occupy positions within the upper echelon of scientific research within their field, surpassing in prestige a number of research groups made up of native-born Americans. It follows from this that the notion of "inner-circle" academic researchers described by certain applied linguists has become less a matter of language, at least within this field of biomedical research.

Genre considerations

Certain significant modifications were made to the rhetorical makeup of this RA as a result of the influence of the academic literacy brokers and me, the in-house editor. In response to the contributions of both, the text became somewhat more rhetorically orthodox. On the other hand, the discursively relevant changes introduced in response to the input of the academic literacy brokers representing the third journal were centered around the methodological shortcomings of the study and the inappropriate use of self-mentions discussed in the preceding section of this chapter. Prior to this feedback, the authors' writing exhibited many of the traits found in the descriptive frameworks which I have used to guide my analysis, including the inclusion of an explicit hypothesis and use of an internal structure within the discussion section. The more idiosyncratic features of the text can be seen in such aspects as the lengthiness of background information appearing in both the introduction and discussion.

The abstract of the text is included in all documents that form part of this TH. It is possible—and, I would argue from experience, likely—that the authors produced a number of drafts that preceded the first version included here, thus increasing the likelihood that at some point along the process of writing the RA there was no abstract included. This, however, does not take away from the fact that the version sent to me represents the most complete product prior to pre-submission editing, which indicates that the authors use their institution’s editing service after they have created the best possible text they can and not as a means of cleaning up the language of the paper before it is sent to other coauthors for their contributions or input. Not all IIS-FJD researchers use the editing service in this way, however; indeed, I often receive early drafts for revision before they are sent to external coauthors, and in some cases I believe that this early-stage revision is the only one that I do. If my suspicion is correct, then this would indicate that these NNEs authors understand my editing services to target surface-level issues rather than deeper, more rhetorical concerns. This practice is likely seen outside of my institutional context, and even among Spanish researchers from different academic fields, and may be attributable to a number of factors, including what I believe to be a widely-held view of writing as a purely formal matter of proper grammar, lexis, and other surface-level issues; varying degrees of availability of language-support services; and the time constraints these NNEs researchers face.

The abstract began with a four-part structure consisting of the sub-sections background, patients and methods, results, and conclusions, although the published version expands this number to five and changes these sections to background, objectives, methods, results, and conclusion. This rearranging was done in adherence of the requirements of the different target journals, though the rhetorical characteristics of the abstract remained virtually identical from start to finish. In general, both of these journal-mandated formats are aligned with the four-move framework published by Bhatia (1993), and Ernesto and his coauthors did not veer from this. A comparison of the first milestone version (Document A), i.e., the draft sent to me for revision, and the published version (Document M), demonstrates the extent to which the “background” and “objectives” of the text mirror Bhatia’s Move 1 (introducing purpose):

Document A

Background: Puberty can affect vitamin D levels. The goal of this study is to analyze the relation between vitamin D deficiency and puberty in obese Spanish children and its possible interrelation with the degree of insulin-resistance [sic].

Document M

Background: Puberty can affect vitamin D levels.

Objectives: The goal of this study was to analyze the relation between vitamin D deficiency and puberty in obese Spanish children, along with the possible interrelation between vitamin D status and degree of insulin resistance.

Both documents are somewhat striking for the brevity of the background information, summing up the issue to be discussed in the RA in just a few words. What is also of interest here is the ease with which the authors split the section in Document A into two, requiring no rearrangement of the language. The clear rhetorical marking provided by the subheadings of structured abstracts appears to guide authors in writing these sections. Also, this example of the rhetorical evolution of the text raises the possibility that authors draft their abstracts in such a way that they can be easily adapted to different configurations of the structured abstract, hinting at a “boilerplate” rhetorical makeup of this section that is easily adapted to different textual arrangements.

The authors wrote the introduction section in a way that gave certain prominence to background information, devoting most of the space granted for this section to a description of the link between obesity, adolescence, vitamin D levels, and other related matters and to the ways in which other researchers have shed light on these issues. Though lengthy, this background information is articulated in a way that reflects what Swales (2004) describes as topic generalizations of increasing specificity (pp. 230-232). The authors make skilled use of this resource to develop the argument from broad concerns down to the narrow topic they intend to zero in on in their paper. The text fragment below is a transcription of the two last paragraphs of this section as they appear in Document A. I have chosen this particular stretch of text because of the way in which it exemplifies the use of these increasingly specific points as the authors make their way toward their own research and also because it highlights the way in which the authors mark the shift between Moves 1 (establishing territory), 2 (establishing a niche), and 3 (presenting the present work).

[M1] To date, vitamin D deficiency pathogenesis has been attributed to various factors, including adiposity level, race, season of the year, geographical area, level of sun exposition [sic] and dietary intake⁵. **[M1]** The fact that vitamin D deficiency is more frequent in adolescents than in prepubescent children^{5,6} **[M2, S1A]** suggests that puberty may be a factor for vitamin D deficiency. **[M2, S1A]** However, there is [sic] very little data regarding the influence of puberty in vitamin D status of obese children and adolescent [sic] or of its possible correlation to [sic] insulin-resistance⁶ [sic].

[M3, S2] For these reasons, we hypothesized that obese pubertal children would show a higher degree of vitamin D deficiency than prepubertal children. **[M3, S1]** We decided to analyze the relationship between vitamin D deficiency and puberty in obese Spanish children, as well as its possible influence on insulin-resistance [sic].

Given that the first two moves contained in this excerpt mention the factors that have been found to cause vitamin D deficiency in general (Move 1) and because the part of the sentence located before the citation focuses the analysis on how this phenomenon affects adolescents more acutely than prepubescent children (i.e., Move 1, with more specific mention of the issue as pertains to adolescents and prepubescents), it can be concluded that the rest of the paragraph outlines the particular gap that the authors intend to focus on in their study (Move 2). This gap is articulated clearly through the discourse marker “however” and the explicit language “there is very little data.” As seen in the second paragraph above, the section ends with a short but very clear indication of the study’s hypothesis (Move 3, Step 2: presenting the present work, presenting research questions or hypothesis) as well as an instance of Move 3, Step 1 (announcing present research descriptively). Although Moves 2 and 3 are given less extensive treatment than Move 1, Ernesto and his coauthors here show that rhetorical moves can nonetheless be expressed succinctly.

Concerning the authors’ indication of the gap in the scholarship, one of my proposed revisions suggested a more explicit realization of this move, as can be seen in the two excerpts below, which were taken from the version sent to me and the one submitted to the first journal, respectively. (For the sake of clarity, I have underlined the fragments to which I am referring in this paragraph.)

Document A

The fact that vitamin D deficiency is more frequent in adolescents than in prepubescent children^{5,6} suggests that puberty may be a factor for vitamin D deficiency. However, there is [sic] very little data regarding the influence of puberty in vitamin D status of obese children and adolescent [sic] or of its possible correlation to [sic] insulin-resistance⁶ [sic].

Document C

The fact that vitamin D deficiency is more frequent in adolescents than in prepubertal children [5,6] suggests that puberty may be involved in the deficiency. However, there is [sic] very little data in the literature regarding the influence of puberty on the vitamin D status of obese children and adolescents, and the possible correlation between vitamin D status and insulin resistance requires further clarification [6].

In Document A, the gap is rather subtly suggested with the language “very little data,” and this statement of scarcity is also applied to the noun phrase “its possible correlation to insulin-resistance.” If we base the analysis of this sentence on lexicogrammatical aspects, the referent implicit in “its” is unclear, as it may refer to either “*the influence* (of puberty on vitamin D status)” or “vitamin D status” alone; and while the difference between the two is minor, I chose to make the referent explicit, thus signaling to the authors my interpretation of the text. In doing this, I introduced more specific mention of the gap in the literature, and the authors accepted this proposed change without further modification. This same dynamic can also be seen in the final sentence of the introduction (transcribed in the previous paragraph): noting that the paragraph commenced with a mention of the hypothesis of the study, I inferred that the sentence beginning with “We decided to analyze the relationship” followed logically from the opening sentence, and therefore proposed the phrase “To test this hypothesis,” which was also incorporated into the text without further consultation. These two examples illustrate the ways in which my proposed revisions may foreground or make more explicit certain ideas that are more implicit in the text, possibly making rhetorical shifts more evident to the reader. However, as we have seen in these two examples, my interventions sometimes spell out my understanding of the text so that authors can become aware of the possible interpretations of their writing, and should these interpretations be inaccurate, they may make the appropriate adjustments. In either case, my contributions included above reflect rhetorically “standard” move realizations, and in this case these realizations brought the text more in line with my view of discursively appropriate writing,

thus representing editing that goes beyond the surface level of the text and constituting a deeper engagement in language brokering.

With one exception, the methods section of this RA remained unchanged in its rhetorical makeup throughout the drafting and revision process. (Aside from this exception, the only parts of the study that were added, removed, or changed were content-related issues not germane to my analysis.) In addition, none of the edits I proposed concerned the rhetorical structure of the paper, and in large part the move-step analysis I performed revealed an almost complete parallel between the section's sub-headings and the moves described by Nwogu (1997). Instances of Move 4 (describing procedure for data collection) appeared in the sub-section "Design, Study subjects," Move 5 (describing experimental procedures) in "Study variables, biochemical determination," while Move 6 (describing data-analysis procedures) was the move used in the sub-section "Statistical analysis."

The matter of the self-mention that has been analyzed in the preceding subsection of the present chapter is most sharply evident in this section. My study of this TH made it clear that this allusion to the authors' previous work with little additional development was something of a late inclusion in the article, and following the negative feedback received from the reviewers representing the second journal, the authors reconsidered this decision. The changes made to the methods section in this regard can be seen in the following fragment:

Document A*Design*

Cross-sectional study of the prevalence of vitamin D deficiency performed at the Pediatrics Service of Fundación Jiménez-Díaz between January 2011 and January 2013⁴.

Document C*Study Design*

A cross-sectional study of the prevalence of vitamin D deficiency was performed in the Pediatrics Department of Fundación Jiménez Díaz between January 2011 and January 2013. More details about sample size calculation and study design are explained in a recently published report of our group (4).

Document E*2.1 Design*

A cross-sectional study of the prevalence of vitamin D deficiency was performed in the Pediatrics Department of Fundación Jiménez Díaz between January 2011 and January 2013 (4).

Document J*Study Design*

A cross-sectional study of the prevalence of vitamin D deficiency was performed in the Pediatrics Department of Fundación Jiménez Díaz between January 2011 and January 2013.

Sample size calculation

Sample size was calculated with Ene [sic] 2.0 program. A minimum of 96 obese children and adolescents was required for an estimated 50% prevalence of vitamin D deficiency among obese children (7,8), with a 10% accuracy (ω) and a 95 % confidence interval as we previously reported (4).

Interestingly, the more expansive reference made to the previous study is included in Document C (i.e., the draft submitted to the first journal) but is then removed in Document E, which was the text sent to the second journal. As mentioned previously, this drew criticism from the journal gatekeepers (Document F), leading Ernesto and his colleagues to include a section in Document J titled “Sample size calculation,” in which they offer some of the information included in this previous text. In light of the criticism sparked by this passage, it is revealing to see how the authors appear to have had doubts about how to rhetorically cast their methods even before they received this feedback. The formulations they used in Documents A and E are mostly identical. In E, on the other hand, they appear to have doubts about referencing their work

through a non-integral citation, choosing rather to make specific mention of their methodology through an integral structure. Although this more explicit citation strategy may not have helped them avoid criticism from the journal gatekeepers, the evolution of this passage does hint that they were aware of the need to phrase this self-citation wisely.

The results section was modified only through inclusion of a sub-section in response to the feedback of the second journal's reviewers, who found that the RA would benefit from a control group comprising lean subjects and also that the analysis of the subjects should take into account the subjects' pubertal stage. Of interest, the inclusion of these sections had no effect on the rhetorical construction of the surrounding text. As in other sections, my proposed edits were accepted almost in their entirety, although none of these concerned rhetorically significant attributes and rather were directed at surface-level issues.

The authors here favor a pattern in which procedural issues (Move 0.3 according to the framework of Williams) appear as the leading move in a cycle, followed by one or more moves outlining a finding expressed either as a comparison between variables (Move 1.1A) or a statistical relation between variables (Move 1.1C). The section's opening illustrates this pattern well; below is a transcription of the first draft sent to me for revision (Document A):

[M0.3] Data was collected from 120 obese children between the ages of 6 and 17.

[M0.1] The clinical and biochemical data of both pubertal and prepubertal obese children is showed [sic] in table 1.

[M1.1A] Vitamin D deficiency rate was 75% in the pubertal group and 46% in the prepubertal group. **[M1.1C]** Furthermore, vitamin D levels were statistically significantly lower in pubertal obese subjects compared with prepubertal ones (Table 1).

The four sentences excerpted above each correspond to a different rhetorical move, and the sequence is as follows: Move 0.3 (procedural), Move 0.1 (pointer), Move 1.1a (comparison between variables), and Move 1.1c (relation between variables). From a rhetorical standpoint, the author's use of what Williams (1999) describes as a pointer (Move 0.1) is noteworthy, as this move directs the reader to the table included in the text so as to better understand the measurements referred to in the previous sentence.

Appearing after this excerpted passage, the third and last paragraph of this section is a particularly representative sample of the rhetorical style used throughout the results, as it combines a comparison between groups and an evaluation of the relationship between variables under study within a single move cycle. Another point of interest is seen in the degree to which these short paragraphs so closely match up with the rhetorical moves to which I have assigned them; indeed, it appears that Ernesto and his collaborators use breaks in paragraphs to block off their ideas. Though in my revisions I often urge authors to refrain from writing in one-sentence paragraphs, I did not mention this in the indications I made in Document B. Nonetheless, these three paragraphs appear as one in the final published version, suggesting that the journal proofreaders considered this to be more appropriate and also lending validity to my assessment.

The discussion section of this text is clearly structured according to the three-part internal framework found by Dudley-Evans (1994) in his corpus, marking clear boundaries between the introduction, body, and conclusion of this section of the paper. The introductory sub-section opens with what I coded as an information move (Move 1) that emphasizes the novelty of the work with respect to other studies while providing an overview of the procedures followed. The passage to which I am referring appears as follows in the final, published version:

[M1] To the best of our knowledge, no previous studies conducted in different seasons of the year have analyzed the influence of puberty on vitamin D levels in obese Spanish children, and the possible connection between puberty and insulin resistance. **[M3]** In this study, we observed a higher prevalence of vitamin D deficiency in obese pubertal children than in obese prepubertal children.

With this opening sequence of moves, the authors appear to anticipate an evaluation of their work, foregrounding the place within the ongoing scholarship that their study should occupy and also stressing the importance of the variables they examined in their study. This focus on methods may have been a deliberate attempt to divert attention from the somewhat modest findings of their study, as the two reviewers from the second journal found little novelty in their findings. The second move excerpted above presents the finding that a higher incidence of vitamin D deficiency was found among the obese pubertal children included in their study than in the obese prepubertal group. However, in the text's introduction, the authors state the

following: “The fact that vitamin D deficiency is more frequent in adolescents than in prepubertal children (5, 6) suggests that puberty may be involved in the deficiency.” Taking into account the proximity of these two moves in the introductory sub-section of the discussion, it can be argued that the authors wagered that the novelty of their methodology would be sufficient to outweigh the shortcomings in their findings. If this is true, other RAs like this one may frame their arguments in the same way, possibly creating a dichotomy between studies that rely most on findings and studies that showcase novel methodological approaches.

After this introductory section that follows a Move 1–Move 3 pattern, the authors open the “body” sub-section of the discussion with a lengthy treatment of the background and justification of their study. This part of the discussion examines the problem of vitamin D deficiency using a wider lens, arguing that abnormally low levels of this vitamin are a problem in adolescence as a whole, though without narrowing the issue further to obese adolescents and pre-adolescents. Once this problem has been established, the authors go on to stress the problem that this issue poses in obese pubertal and prepubertal children, then going further to highlight the increased risk of vitamin D deficiency during the winter months.

Though not included in the explicit hypothesis appearing in the introduction of the RA (described above), the discussion does address the somewhat surprising results found concerning the relationship between vitamin D deficiency, puberty, and insulin resistance in the obese prepubertal and pubertal children who participated in the experiment. The authors phrase this unexpected outcome (Move 4 according to the Dudley-Evans framework) in terms that appear to minimize the importance of these findings, as seen in the following excerpt appearing in Document A:

This increased risk of vitamin D deficiency during puberty in obese children doesn’t seem to be related to differences in indirect adiposity parameters, which include IMC SDS and the waist SDS. Additionally, the difference between pubertal and prepubertal obese children is measurable during most seasons, although it is not as evident in spring, probably due to a smaller sample size.

While the asence of a link between vitamin D deficiency, puberty, obesity, and indirect adiposity parameters is hedged by the modal construction “doesn’t seem,” the lack of a difference between pubertal and prepubertal obese children during spring is paired with an explanation that points to the sample size as a possible explanation for this surprising result.

In apparently idiosyncratic rhetorical style, the text features a paragraph that discusses a research question not explicitly addressed by the experiment itself. The paragraph, which was not commented on by the journal gatekeepers at any point and remained part of the text throughout its arc, reads as follows:

The reasons why vitamin D deficiency is higher in obese children during puberty are unknown. Our study is descriptive, and as such, it is not meant to provide an etiology for this deficiency. However, like other authors (6), we suspect that the lower levels of vitamin D observed in obese pubertal children are linked to decreased sun exposure resulting from a sedentary lifestyle. We also suspect that vitamin D intake in both obese and non-obese adolescents is lower than in prepubertal children (24). In conclusion, we believe that the combination of limited sun exposure, lower daily intake of milk and other vitamin D-rich foods, coupled with the increased demand of vitamin D due to growth and bone remodeling, explains the increased vitamin D deficiency observed in obese adolescents.

Here, the admission that the very issue being dealt with in this paragraph lies outside of the scope of the RA is noteworthy, as it anticipates that the analysis offered here is only tangentially linked to the paper’s actual findings. This stretch of text struck me because of this admission and due to the fact that it is so difficult to categorize under the Dudley-Evans framework, which has a specific move (Move 6) on findings that are unexpected or differ from existing scholarship, but does not include a category for authors to venture conclusions that would require a different study design altogether.

Despite what may be considered to be less common rhetorical decisions in the discussion—i.e., foregrounding methods in lieu of results, lengthy treatment of background information, and analysis of issues not addressed by the study per se—the reviewers did not find much fault at all with the way the discussion was written, and the only substantial rhetorical change that this section underwent came in the form of expanded mention of the study’s limitations discussed in the preceding section of this thesis. This inclusion of reviewer feedback by articulating the

gatekeepers' points of criticism as limitations to the study can be most clearly seen by comparing the paragraph on limitations in the first version included in this TH (Document A) and the final one (Document M); this added information is underlined in the second text excerpted below.

Document A

Our study presents several limitations, such as its transversal nature, the absence of a nutritional survey, and the absence of data about exercise, sun exposition [sic], or [sic] body composition.

Document M

Our study has several limitations, such as its cross-sectional nature, the absence of a nutritional survey, and the absence of data about exercise, sun exposure, and body composition. In addition, we did not assess parathyroid hormone (PTH), alkaline phosphatase, and calcium and phosphorus levels. Finally, due to the reduced sample size, we could not compare vitamin D levels among the different Tanner stages in the four seasons of the year and, for that reason, we compared prepubertal and pubertal obese children during the entire year.

The expanded version of this paragraph incorporates two of the points that the reviewer feedback centered around. Both issues were raised by the reviewers of the second and third journals, leading the authors to recognize these points of improvement as limitations to their study. The authors head off potential criticism from reviewers by making this admission in rhetorically conventional fashion, thus incorporating the input from other academic literacy brokers.

Attitudinal aspects

As with other informant interviews, the final part of my talk with Ernesto was devoted to his experiences with research and publishing research internationally in the English language. He believes that Spanish researchers like him face great challenges with Anglophone journals, although these obstacles are becoming less and less an issue of language. He finds that scientists from his country receive substantially less help from existing networks in the United States, although this position as outsiders is even more pronounced with journals based in the United Kingdom. In his view, submissions from researchers from Italy, Greece, and Israel are more well-received by North American journals because of the inroads that researchers from these

countries have made within the disciplinary culture. In Ernesto's view, researcher migration to the United States in past generations has placed these scientists in a position to benefit scholars visiting from the country of their ancestors. Such a relation is predicated, however, on the enduring loyalty of these emigrants to the citizens of their country of origin—even at the expense, it would seem, of other opportunities.

Although Ernesto does feel he is at a disadvantage when compared to Italian, Israeli, and Greek scientists within his field, he believes he is more disadvantaged when competing with these fellow European scientists than he is with researchers who live and work in the Anglophone center. In giving this assessment, he stated that his deficiencies with English were certainly a hindrance fifteen years ago, though in the ensuing period the increased availability of NES translators and editors coupled with an improvement in his writing have nearly eliminated this problem. Sub-par English has “never” been given as a reason for his manuscripts to be rejected, and he feels that comments that target the language of his articles are sometimes made, although often these remarks appear when reviewers are at a loss for more substantial points of critique; indeed, Ernesto has come to question the legitimacy of certain comments of this nature when the text has been previously reviewed by an NES individual. Despite the diminishing challenge of English within his experience in publishing, the added time required for pre- and post-submission editing prolongs the process and lowers his group's potential research output, although his overall opinion of English both within his disciplinary community and in the greater world is highly favorable. What is most surprising about Ernesto's views is the fact that, while he believes he is aware of the expectations of target journals as concerns high-quality manuscripts, he believes Spanish researchers like him are at more of a disadvantage when compared to well-connected Italian researchers than to scientists who live and work in Anglophone countries. Taken together, this notion suggests that, at least for him, the politics of international academic publishing have all but eliminated the language barrier, though at the same have erected certain transnational networks that are at least as formidable for what Ernesto refers to as “emergent” groups of researchers. This opinion partially reflects the changing views of culture as described by Connor (2004): by this account, writers' L1, physical location, and networks have become uncoupled (Baker, 2013). Unlike the small culture vision based on non-essentialist conceptions

of human social activity, however, Ernesto's experiences are framed as reified small culture, in which this new configuration bears many of the same characteristics of previous notions of culture in which L1 and geographical location were conflated.

As our conversation turned toward his motivation to research, he described research as a fundamental part of his profession, although he lamented the impact of time constraints on his research output. In addition to clinical and research endeavors, he is devoted to his teaching work with the university, and he is a firm believer in the need for clinicians employed in teaching hospitals to be involved in clinical practice, research, and teaching. While the path toward top-quality international publications is not easy for any researcher, he feels that pediatricians like him face a particularly stiff challenge because of the need to secure funding, design studies, recruit patients, collect data, draft and submit texts, and then navigate the waters of the peer-review process. Rejection is often disheartening, although after failing he always finds the will to try again and encourages younger generations of pediatricians to do the same. Strictly speaking, he is not under any obligation to research, though he cannot conceive of his medical career without it. This vocation has helped him persevere despite the recent budget cuts affecting research in Spain, the difficulties that extracurricular research time poses for work-life balance, and the increasing demands that target journals place on texts for publication. In the last two or three years, his group of researchers has managed these challenges quite well, and have sent more and more articles to English-medium publications, and it seemed to me that this trend will continue.

Implications

Based on the informant's input on the enterprise of publishing in English in general and also our talk around the text analyzed here, a number of themes can be seen in this TH. From a textual point of view, we have seen how regenred studies like this one may have certain limitations in their methodology, placing them at an initial disadvantage in the peer-review process. However, a number of Ernesto's remarks and the evolution of the text proper show how these limitations may be mitigated provided the methodological underpinning of the study is of a minimum level of quality. The strategies that can be used to strengthen the chances of a text being published

include repositioning of the title, foregrounding certain aspects to bring them in line with the expectations of the target journal, and placing greater focus on methods when the study's findings are less than novel. Changes like these may help other RAs get published sooner, and lend validity to the notion of the RA as a highly malleable type of text.

In this TH we have seen the willingness with which the authors accepted my proposed changes to the manuscript, even when these revisions carried certain rhetorical weight. Following what I believe to be my own rhetorically orthodox style from my Anglophone perspective, I made suggestions that highlighted certain shifts between moves and steps, while in other places I recommended that they foreground ideas that I believed to be more central to their intended meaning. Ernesto and his colleagues incorporated these proposed changes on nearly every occasion, thereby hinting at a particularly fluid and mutually respectful author-editor relationship.

A number of author strategies come to the forefront in this TH, and in this case these strategies appeared to carry significantly more weight than formal accuracy in English. Ernesto demonstrated a highly developed ability to interpret the feedback from target journals, and reacted accordingly to maximize his chances of securing publication. He seized on windows of opportunity to a greater or lesser degree, calling upon resources such as the letter of appeal. Though not all of these bore fruit, they are indicative of a measure of negotiability in academic text publishing that may not be evident at first glance.

Lastly, the issue of Ernesto's sense of outsidership is central to the story of this publication in that it paints a nuanced picture of disadvantage. More subtle than a mere language issue, we see textual evidence of journal reviewers calling into question his use of self-citation as a means of asserting his and his coauthors' place within the discourse community. Couched in terms such as "reporting," the reviewers of the second journal challenged the authors' notoriety within the discourse community while overlooking certain rhetorically unconventional characteristics of the text. In Ernesto's view, publishing in English is substantially more influenced nowadays by

transnational allegiances and disciplinary prestige, although he and his colleagues have yet to secure a foothold in these circles of highly prolific researchers.

Discussion

After studying a total of one hundred eighty-three documents and having conducted nearly twelve hours of interviews, the findings of my study paint a varied picture of the agents, processes, and dynamics behind these ten RAs. As can be seen in the table below, my informants are a diverse group of researchers at different stages of their careers, and their writing targets different disciplinary audiences. In this discussion I will attempt to synthesize the primary findings of my research. These findings and other issues that emerged from my research are organized by subheading for presentation and analysis.

	Informant pseudonym(s)	Others named in TH	Discipline	Career stage	Clinical / Basic / Mixed
TH1	Esteban		Cardiology	Senior	Clinical
TH2	Manuel	Ramón Tomás	Vascular pathology	Senior	Basic
TH3	Susana		Lipid metabolism	Senior	Basic
TH4	Carolina	Andrés Lola	Neurology	Senior	Clinical
TH5	Federico	Paco Clara	Orthodontics	Junior	Clinical
TH6	Miguel	Óscar	Nephrology	Senior	Clinical
TH7	Juan		Allergy	Senior	Clinical
	Rafael		Immunology	Junior	Basic
TH8	Jesús	Belén	Hematology	Senior	Mixed
TH9	Laura	Beatriz Patricia	Clinical genetics	Junior	Clinical
TH10	Ernesto	Silvia	Pediatrics	Senior	Clinical

1.1 Degree of success

The texts experienced varying degrees of success. The most well-received contribution was the text in **TH4**, which was praised for being “clear and well-written” by the first and only journal to which the author submitted the paper, while the experience of the researcher in **TH9** was clearly the most difficult, and as a result of this and other issues the paper has been put on hold for the time being. As can be seen in the table below, only three of the papers were accepted by the first or second journal, and all others that were eventually published were successful after three or more attempts.

	<i>Published?</i>	<i>No. of journals</i>
<i>TH1</i>	Y	3
<i>TH2</i>	Y	2
<i>TH3</i>	Y	3
<i>TH4</i>	Y	1
<i>TH5</i>	Y	5
<i>TH6</i>	Y	3
<i>TH7</i>	Y	3
<i>TH8</i>	Y	2
<i>TH9</i>	N	n/a
<i>TH10</i>	Y	3

1.2 Rhetorical revision in pre- and post-submission stages

Peer reviewers frequently find fault with the language use and style of NNES-authored submissions (Belcher, 2007; Lillis & Curry, 2015; Mur-Dueñas, 2012, 2013), constituting one of the linguistic disadvantages of NNES scholars when writing for international publication. In

addition to explicit reviewer feedback, another indication of the rhetorical challenges faced by the authors is the extent of revision work carried out in particular sections of the IMRD structure between the early drafts and the finished product. By observing the attention that authors paid to the rhetorical structure of these sections both as they were preparing their manuscripts and in response to feedback from gatekeepers, a fuller understanding of the difficulties that these sections posed can come into focus. An analysis of the sections that underwent the most substantial change in rhetorical structure relative to the other sections of the same paper can be seen in the table appearing below. While a more comprehensive and structured heuristic (e.g., Gosden, 1996; Lillis & Curry, 2010) would be necessary to study the extent to which each section changed relative to all the texts included in my corpus, this analysis provides a rudimentary understanding of the hurdles involved. Excluding **TH9** from the analysis due to the extent of the modifications caused by the shift from case report to RA, we can see how the discussion and results sections were the two most likely sections to be revised, while the methods and abstract tended to remain mostly the same or be subjected to light revision.

	TH1	TH2	TH3	TH4	TH5	TH6	TH7	TH8	TH9	TH10
<i>Abstract</i>	L	M	L	L	M	L	M	S	S**	L
<i>Introduction</i>	S*	L	M	M	L	M	L	S	S**	L
<i>Methods</i>	M**	L	L	L	L	L	L	L	S**	M
<i>Results</i>	L	L	M	M	M	S	L	M	S**	M
<i>Discussion</i>	S	M	L	M	L	S	S	S	S**	M

Degree of rhetorical modification by section. L=light modification; M=moderate modification; S=substantial modification. *=influence of length limits and journal editor intervention; **=influence of substantial changes to content.

Rhetorical shortcomings were the subject of criticism in **TH3**, **TH5**, **TH6**, **TH8**, **TH9**, and **TH10**. This grouping, however, merits qualification. The personal and professional circumstances of the author in **TH3** make a convincing case for treating the unfavorable feedback she received as more an issue of content than possible rhetorical transfer in her writing. Similarly, the RAs in **TH9** and, to a lesser extent, **TH5**, also make it difficult to reach broad conclusions on rhetorical weakness. In the former, the sole reviewer offered such sweeping criticism that it is difficult to draw a clean line between supposedly poor knowledge of scientific concepts and lackluster writing. As for the latter, the paper was also criticized as having a number of flaws, and many of the comments

made seem to indicate that the study would have been more well-received in an applied, local journal published in Spanish.

Three THs (**TH1**, **TH2**, **TH4**) revealed that the rhetoric of the RA was either a minor impediment or no impediment at all. The authors of both **TH1** and **TH4** were commended for the quality of their papers, and in **TH2** only one reviewer remark took aim at the rhetorical structure of the paper, though not forcefully so. These authors' success in navigating the peer-review process indicates the degree to which they have become experts within their respective communities (Englander, 2006) and can be used to provide new descriptions of successful processes (Gosden, 1996) and products. Examples like these make it necessary for a broader focus of inquiry into possible sources of disadvantage faced by NNES researchers beyond L2 capabilities (Ferguson, 2007; Ferguson et al., 2011; Lillis & Curry, 2006b; Swales, 2004; Uzuner, 2008), and lend credibility to analyses that locate the keys to success in the expertise of writers (Ferguson, 2007; Tribble, 2017; Wood, 2001).

I will now review the primary trends detected in instances of rhetorically significant pre- and post-submission revision and gatekeeper criticism. As in the THs appearing earlier in this thesis, my analysis of these moments in which the RAs were crafted and negotiated with the journal representatives will be contextualized within the section of the canonical IMRD structure in which this rhetorical work took place. In synthesizing the events of the THs in this fashion, I hope to contribute to the evolving knowledge of the RA structure, its rhetorical patterning, and the issues faced by NNES authors when writing them.

1.2.1 Discussion and introduction sections

The discussion section was one of the more common sites of negative reviewer feedback, and it drew three types of criticism. First, the informants were criticized for failing to mention study limitations in **TH5** and **TH7**. It has been argued that members of the discourse community may expect to see some commentary on the limitations of the study or, more precisely, a statement on the extent to which the results can be extrapolated (Swales & Feak, 2012), and in these cases the authors did not adhere to this standard. Second, the issue of intertextuality and citation arose

in **TH3**, as the gatekeepers found the author to have insufficiently woven her empirical impressions within a framework of existing studies (Hyland, 2000). Third, in **TH6**, one reviewer found the discussion to be too long and repetitive, hinting at rhetorical transfer from Spanish (Lorés-Sanz, 2011b). NNEs academic writers from Spanish-speaking countries and others have been known to struggle with the linearity, absence of digressions, and brevity that characterize the English rhetorical tradition (Golebiowski, 1998; Pérez-Llantada, 2010; Pérez-Llantada et al., 2011; cf. Taylor & Tingguang, 1991), and it appears that this was the case here as well.

Together with the introduction, the discussion section is widely believed to be the most important section of the IMRD structure (Berkenkotter & Huckin, 1995; Hyland, 2000), most likely because the two are the most dialogic (Uzuner, 2008) and are the sites in which authors elaborate arguments (Lorés-Sanz, 2011b). Additionally, the discussion is the textual space where biomedical researchers tend to point out the significance of their contributions to the ongoing scholarship, often through explicit authorial presence indexed by the use of personal pronouns (Carcu, 2009; L.-J. Li & Ge, 2009). Such complexity may cause the discussion to be one of the most problematic sections for NNEs authors (J. Flowerdew, 1999a; Martín-Martín et al., 2014; Moreno et al., 2012; Mur-Dueñas et al., 2014; Pérez-Llantada et al., 2011; St. John, 1987; Uzuner, 2008). This is precisely the experience of the informant in **TH1**, who only seeks language support for the introduction and discussion, and by the writer in **TH9**, who drafts the discussion last due to her lack of confidence in her own ability to write effective closing sections. Unfortunately for these two, the discussion may not only be the most difficult and important, but also the most commonly read; indeed, the informant in **TH2** reported that scientists like him tend to read study conclusions first and then decide whether the rest of the paper is worth reading. In addition to these author viewpoints, the table appearing above shows how my study supports other work on revision (Gosden, 1995), finding that some of the most intensive revision of rhetorical structure took place here.

As for the introduction, the way the informants in **TH3** and **TH5** drafted the section was also met with certain resistance by the journal gatekeepers. However, these were the only two instances of rhetorically related criticism directed at this section, and the authors included in my sample

tended to perform either light or moderate revision here (see table above). The feedback in **TH3** stressed the paper's failure to argue convincingly for the relevance of the research results; though no indication was given as to whether this should have been done in the introduction, discussion, or both, it seems the reviewer was suggesting a more appropriate intertextual framework (Hyland, 2000). In **TH5**, the reviewers noted that the section was also lacking in references, and believed it inappropriate that the contained the author's opinion. On this issue, Hyland (ibid.) has found that "rational argument is a social matter governed by disciplinary norms and oriented to achieving an intersubjective consensus through persuasive means. Notions of what counts as convincing argument, appropriate theory, sound methodology, impressive logic and compelling evidence are community-specific" (p. 8). It therefore appears that what the informant referred to as the "philosophy" he articulated in the introduction would have been more welcome if he had scaffolded his beliefs with previous scholarship by his peers.

1.2.2 Methods and results sections

The methods section was the site of a relatively high concentration of specific gatekeeper commentary in the post-submission phase and rhetorically related revision both before and after submission, especially in **TH3**, **TH5**, and **TH10**. This degree of attention indicates that this is a highly important part of the IMRD structure for some (Peacock, 2011). Methods sections are highly formulaic (St. John, 1987; Uzuner, 2008), reporting the procedural bases of the investigation, thereby making this section more technical and supposedly transparent. However, the three aforementioned THs were considered to have an insufficient degree of what Swales (2004) refers to as "elaboration" (pp. 219-220), and the reviewers indicated that further description of the sample (**TH3**), the procedures (**TH10**), and the analytical approach (**TH5**) should have been included. This scrutiny of procedural matter brings to mind the remarks made in **TH2** and **TH8**, in which the informants suggested that submissions from certain countries (e.g., China) have become suspect for alleged falsification of data. Similarly biased opinions against the science culture of certain regions have been reported (J. Flowerdew, 2000; Gosden, 1992). However, it has been found that Move 6 (describing data analysis procedures) has become obligatory in medical RAs, and the space devoted to this move has increased over time, presumably as a means of persuading the audience of the dependability, accuracy, and aptness

of the findings and to ward off potential criticism stemming from doubts regarding the results (L.-J. Li & Ge, 2009).

Quantitative research tends to be reported in prose that is highly patterned and less varied, and as a result NNEs may feel more comfortable writing in the hard sciences than in other fields which demand more qualitative research (S. Cho, 2004; J. Flowerdew, 1999a). This characteristic is most pronounced in the methods and results sections, which have been found to have certain overlap in the propositional content expressed in them (Swales & Feak, 2012). Despite the lesser rhetorical burden of these sections, NNE authors tend to devote a great deal of revision and rhetorical manipulation in the results (Gosden, 1995), evidenced here by the high number of THs showing a moderate level of rhetorically related revision across all drafts (5/9). The results section attracted a comparatively lower degree of attention during peer review, and none of my informants told me that they find the results to be a challenge for them as writers. The results section, however, was the subject of critique in **TH3**, with one reviewer urging the author to more fully describe the sample either in this section or in the methods. A reviewer in **TH5** also remarked on the way this section was written, noting that the section resembled legends of tables.

1.2.3 *The abstract*

Several scholars of EAP have found that the abstract is a separate genre (Gosden, 1993; Hyland, 2000; Lorés-Sanz, 2004; Samraj, 2005) meriting study independently from the RA which it (often) promotes. All of the RAs studied in my sample contained an abstract, though in many cases the abstract was not written until the later stages of the process. Though none of the informants indicated that the abstract was particularly difficult for them to write, specific gatekeeper criticism was leveled at the abstract in **TH3** for having failed to demonstrate the importance of the research question, and gatekeepers evaluating **TH5** believed the abstract to be short and incomplete and containing an unsatisfactory description of the results. These perceived shortcomings ran throughout the reviewer feedback and were not limited to the abstract.

Apart from explicit, rhetorically-linked criticism, the abstract was subjected to significant modifications in **TH2**, **TH5**, **TH7**, and **TH8**. Contrary to findings on Spanish-authored abstracts in

the social sciences (Martín-Martín, 2003), the abstract in **TH8** increasingly foregrounded the results of the paper as it neared publication. **TH7** illustrates how a shift from the subgenre of the short communication to the RA structure led the authors to rework their abstract, while the case of **TH5**—in which the authors first opted to submit their work to a journal with a more international scope and, once this was unsuccessful, modified the abstract before sending their paper to a journal with a less international readership—further supports previous studies that have found that the rhetorical structure of the abstract mirrors the generic predilections of the discourse community (Samraj, 2002a, 2002b, 2005). Challenging the widely held belief that abstracts tend to emulate the rhetorical patterning of the introduction (Martín-Martín, 2003; Samraj, 2005), the informant in **TH2** bases his abstract on the discussion, which he believes is the section of the IMRD to which readers pay most attention.

In the following section I will highlight some of the practices that I observed as I studied all the authors in my sample, their texts, and the way their texts were created and received by their respective discourse communities. My aim in doing so is to document and reflect on best practices as well as analyze approaches that may have hindered these NNES authors.

1.3 Practices evidenced

1.3.1 Key decisions and publication strategies

For most of the informants, choosing English as the language in which to write their RA was a foregone conclusion. Indeed, the informant in **TH3** seemed surprised that I should even ask the question at all, thus confirming the dominance of English as the language of scientific communication, (Ferguson, 2007; Uzuner, 2008), especially in the biomedical sciences (Curry & Lillis, 2014; Gea-Valor et al., 2014; Martín-Martín et al., 2014). When asked what would have been the difference if the paper had been written in Spanish, the authors noted a lower impact factor (**TH3**) and, similarly, a decrease in readership (**TH7**). The informant in **TH3** does sometimes publish in Spanish, though mostly when she collaborates with another department of the IIS-FJD whose research output has a more local focus, targeting the applied community of clinicians (Lillis & Curry, 2010). In what appears to be a dynamic that perpetuates the dominance of English, the informant studied in **TH6** chooses to write in English and publish internationally when he believes

his contribution to science to be especially significant. This same system of prioritizing publications has been found elsewhere (Swales, 2004).

The RAs differ in the number of coauthors who played an active role in drafting and revising the manuscripts. The informants in **TH1**, **TH3**, **TH4**, and **TH8** wrote most of their manuscripts themselves. To a much greater extent than in the humanities, academic articles published in the sciences most frequently include a number of named authors, though their degree of involvement in the actual drafting of these manuscripts often varies (Lafuente-Millán, 2010; Pérez-Llantada et al., 2011). The RAs included in my corpus show that the work of writing papers may be distributed according to a number of configurations, such as tandems of senior researchers (**TH7**), novices who write their papers alongside one or more expert academics (**TH5**, **TH9**, **TH10**), and multi-level collaborations like the ones described in **TH2** and **TH6**.

Another practice revealed by these ten THs is the dynamic of regenring, or reformulation of scientific concepts under new generic structures (English, 2011). Though the concept of regenring originated from studies in educational contexts (ibid.), a parallel can be made with some of the evidence from my study. The informants in **TH5** and **TH8** discussed the challenge of repurposing their doctoral theses within the generic structure of the RA, while the researcher in **TH9** had to rework her research from what was to be a case report and make it into a publishable RA. Evidencing a separate but related pattern, the circumstances that led the authors in **TH7** and **TH8** to reconfigure their arguments and the processes they followed bear a resemblance to the concept of genre sets or genre systems as different outlets for academic knowledge that academics can resort to in response to certain social constraints (Hyland, 2002).

The RAs analyzed in **TH5**, **TH6**, and **TH8** provide insight into the presence of code-switching and translanguaging (Canagarajah, 2011; Creese & Blackledge, 2010) when NNES academics publish in English. The author in **TH5** and his collaborators sought to make the most expeditious use of my editing services when they asked me to translate into English all but the most field-specific terminology, after which their highly expert collaborator translated these terms into English. This behavior mirrors the strategy used by other NNES scholars when writing in English (Lillis & Curry,

2006b), although hybrid strategies like this one have been found to be the least desirable for authors (Martín-Martín et al., 2014). The informant in **TH6** resorts to his L1 when he feels that his limited capabilities with written English would require him to modify the complexity of his language if he chose to write entirely in English. Other Spanish scholars have reported similar writing behavior (Pérez-Llantada et al., 2011). Such purposeful use of code-mixing and code-switching has been described as one of the pragmatic strategies employed by ELF speakers (Jenkins, 2011).

1.3.2 Engagement with the discourse community and perseverance

Throughout my study, several informants exhibited an awareness of how their work could be received during peer review, thereby displaying similar behavior to the authors studied by Gosden (1995). Rather than conceiving of writing and submission to journals as an isolated event that triggers either acceptance or rejection, authors like those studied in **TH1** and **TH5** write defensively, going beyond what they believe to be the necessary degree of depth in their writing so that excess description or other language can be pared back at the request of journal gatekeepers. In other THs, an informant used the point-by-point response as a tool to guide the revision process (**TH2**) and a different researcher told me he closely examines reviewer reports for signs suggesting that a substantial review of a text may bear fruit and convince them to publish the manuscript (**TH10**). These practices show how the informants understand writing for research publication to be a conversation with designated representatives of the discourse community.

Academic genres such as the RA are both recurring rhetorical artifacts that reflect the social relationships and the communicative purposes that drive language use and constitute these same relationships (Swales, 1990). Their tendency to recur in patterns, however, both limits the way writers can use the genre and also opens up windows for them to achieve their personal goals (Hyland, 2000). The authors that make up my sample of informants harnessed this potential in a number of ways. First, some made skilled use of rhetorical machining (Swales, 1990) in the revised versions of their texts, opting to wait for reviewer feedback before commenting on the limitations of their studies and then incorporating this feedback as limitations in the discussion

section (**TH2, TH4, TH7, TH8, TH10**). Additionally, in **TH5** and **TH10**—both of which Swales (2004) would classify as “methods” papers more than “results” papers—this aspect of the study was foregrounded throughout the trajectory of the text, thus replicating successful practices documented in the EAP literature (Berkenkotter & Huckin, 1995). In another example of acute sensitivity to the response of the “immediate audience” (Gosden, 1992), the informant in **TH8** increasingly foregrounded what he perceived to be the most valuable contributions of his study, backgrounding those that were less successful.

Second, the RA in **TH6** shows how the author made the gap (Move 2) more and more prominent as the text made its way through peer review and neared publication, demonstrating how academic brokering can change not only the structure of texts but rather how knowledge is presented (Baker, 2013). Third, the informants studied in **TH4** and **TH6** seemed to reflect on and incorporate reviewer feedback in a way that highlighted these new additions across the entire text, and not only in the sections referred to by the reviewers, which suggests a “ripple” effect of such feedback and demonstrates these authors’ awareness of the interplay between the four sections of IMRD texts within a “coherent narrative of inductive discovery” (Berkenkotter & Huckin, 1995, pp. 46-47).

My study provides evidence of the complex dynamic between submitting authors and journal gatekeepers and ways in which authors may flout referee control during peer review. Echoing the delicate balance between obedience and resistance to reviewer criticism (Belcher, 2007; Gosden, 2001), the informant and his coauthors in **TH5** provide a vivid description of this negotiation with reviewers, referring to it as an act of “wooing.” Similarly, in post-submission review, the informant in **TH2** opted to overlook one of the issues noted by a referee. In a less flagrant example of resisting reviewer intrusiveness, the informants in **TH8** and **TH5** seemed to exaggerate the real modifications made to their text in their point-by-point reply. All three of these strategies were successful, contrasting with previous analysis (Gosden, 2001), and calling into question the true importance of the remarks made by the reviewers.

Albeit unsuccessfully, the author in **TH10** appealed the rejection of his manuscript by one of the journals, and this same strategy was mentioned in the informant interviews for **TH3** and **TH8**, which suggests a pattern. The three individuals recognized that these requests for reconsideration most often fail, although the author in **TH8** recalled one particular instance in which he prevailed.

Similar instances of perseverance came up in a number of THs. Resisting the demoralizing effect of four rejections, the author in **TH5** succeeded in publishing his manuscript on the fifth attempt, even without a substantial overhaul of his paper. When the reviewers emphasized the limitations of the paper studied in **TH10** despite the fact that these methodological shortcomings had already been mentioned in the text, the informant and his collaborators showed resolution and confidence in their writing, recognizing this flaw in their point-by-point response and declining to make any additional modifications. With similar resolve, the informant in **TH8** shrugged off the discouragement of his mentors after they advised him against attempting to publish his findings; fortunately, the author stayed the course and was rewarded for his efforts. For their part, the authors in **TH7** shifted academic genres following an initial letdown, refashioning what was originally a short communication into an RA. Other studies (Belcher, 2007; Curry & Lillis, 2014; Uzuner, 2008) have also found persistence to be a key factor for success in NNES academic publishing in international journals, and these THs lend further validity to this finding.

1.4 Text-external issues impacting degree of success

1.4.1 Physical distance and manifestations of geographical separation

Mostly in the form of comments made during informant interviews, the THs constructed in this thesis suggest that certain text-external issues may have impacted the degree of success that these NNES authors had. As expressed in **TH1**, **TH8**, and **TH4**, NNES authors' limitations due to their NNES status is compounded by a certain protectionism, causing Spanish researchers to be stymied in their efforts to publish in high-impact journals. The author in **TH4** reports a self-perpetuating cycle in which North American scientists who are awarded enormous research grants are then given preferential treatment by inner-circle gatekeepers, who will at some point in the future need to publish their own research and will rely on similar treatment from those

scholars whose work they judged at an earlier time. This suggests that the rewards system is structured in such a way that one of the primary mechanisms of gauging research output—that is, the RA—is less accessible for non-center researchers, compounding the already substantial disparity in funding opportunities between North America and Spain.

In addition to this institutional isolation, the informants interviewed in **TH1**, **TH5**, **TH6**, **TH7**, and **TH8** also feel a personal isolation that comes with living outside of the United States and other Anglophone countries. The informant in **TH1** perceives that, when evaluating a submitted manuscript, journal gatekeepers often query members of their professional-social network, informally investigating whether the research reported in the manuscript coincides with the community's knowledge of which lines of research are being pursued in the institution. As explained to me by the author, some journal gatekeepers may be suspicious of off-network (Belcher, 2007) scholars when editors and reviewers cannot make such inquiries. Touching on this same topic, others in my study (**TH5**, **TH7**) remarked that while there is certain resistance to newcomers from the outside, meeting editors or reviewers in person can partially remedy this situation, though the physical distance and lack of funding may make these encounters unlikely. According to this logic, it also seems feasible that face-to-face contact with journal gatekeepers has a substantial impact on authors' reactions when they receive rejection letters from journals, making it unlikely that the researcher in **TH10** would have taken the bad news from the first journal as a genuine message of encouragement had he not known the editor personally and held him in such high esteem. Lack of contact with scientific and discursive practice is one of the non-linguistic resources that have been found to be detrimental to NNES researcher efforts (Ferguson, 2007; Hyland, 2016; Uzuner, 2008).

1.4.2 Internationalization, international network, and “inner-outer” NNES academics

Despite the added difficulty imposed by geographical distance, some of the THs studied here outline certain mitigating factors that can bolster NNES academics' chances when competing in a globalized knowledge industry. In many cases, these factors emerged from descriptions given by informants and their portrayals of the non-informants who contributed to their RAs.

The informant in **TH2** owes his accomplishments both to his own merit and the opportunities afforded by his powerful national research group. Groups like his and the one that gave the author in **TH9** the opportunity to publish in collaboration with another department can play a decisive role in NNES researchers' careers, opening opportunities for local scholars to network with scholars from abroad and publish internationally, even despite possibly limited proficiency in English (Lillis & Curry, 2006b, 2010). The anecdotal evidence offered by the informant in **TH10** further underscores the role played by networks, suggesting that in his particular field, ethnic allegiances among Italian and Italian-American researchers have realigned across national borders, bestowing on members of their particular group certain privileges not even enjoyed by "native-born" residents of the United States.

Four THs provide evidence of NNES researchers who have succeeded in blurring the lines between what Kachru (1985) referred to as inner, outer, and expanding circles of scientific research. One is the informant in **TH4**, who has earned an international reputation in spite of the limited funding she enjoys, her lack of a strong research group, and what she perceives to be certain limitations in her English. Another is the informant in **TH2**, who has notched numerous publications within his field at a relatively early stage in his career. In addition to these two informants, the senior dermatologist (mentioned in **TH5**) and the senior coauthor in **TH6** both seem to have overcome limitations imposed by geography and language, as both played key roles in their respective THs. Based on what I learned about these individuals, this status of what I will refer to as "inner-outer" researcher is the result of a number of interacting factors.

One attribute of these inner-outer academics is the presence of active international networks. We observed how the collaborator who supported the informants in **TH5** leveraged his clout to the fullest, sending the manuscript first to a dermatology journal. This individual, as well as the collaborator who contributed to the RA in **TH6** and the informant in **TH4**, have all enjoyed extended stays in the United States and since then have expanded their networks both there and elsewhere. Either as a result of time abroad and continued contact with the Anglophone center, all three were commended as expert writers either by my informants or by the journals to which they submitted their manuscripts. Indeed, the informant in **TH5** describes the value that his

collaborator brought to the RA as “marketing,” thus echoing the description used by Harwood (2005) and closely resembling the “selling the research” described by one of the informants studied in Pérez-Llantada et al (2011, p. 24). For his part, the informant in **TH6** attributes the success that his prestigious colleague has had to his great command of the scientific literature and the skill with which he is able to navigate—and help others navigate—the peer-review process. In all three of these cases, the degree to which they have penetrated the international circle of academics is paralleled by their writing skill, suggesting that success as an NNES researcher is a combination of a command of academic genres and familiarity with the communities that use them to attain their goals.

Another factor that likely contributes to the success that these individuals reached here—and, also, the success that other informants may have with other publications—is the international reach of the discipline. As indicated in other studies (Lorés-Sanz et al., 2014; Mur-Dueñas et al., 2014), certain disciplines are more global in scope than others, and as a result the members of these discourse communities tend to publish more frequently in English. The difference between the two senior coauthors in **TH5** is particularly illustrative of this divide: while the orthodontist publishes frequently and with ease in national Spanish-language journals of orthodontics, the dermatologist is even more prolific, boasting numerous international publications in English. This difference is reminiscent of the local-international split observed by Lillis & Curry (2010), with local disciplines such as orthodontics articulated in languages other than English, while more “universal” disciplines like dermatology are seen as having importance around the world and are thus published in English.

1.4.3 Influence of literacy brokers

Language brokering is a resource that may help NNES researchers overcome their difficulties when writing their academic papers in English. Several studies have shown that users are often satisfied with the services rendered by professional language brokers (Lillis & Curry, 2010; Pérez-Llantada et al., 2011; Ventola & Mauranten, 1991), although in other cases the author-clients may be disappointed by the broker’s lack of field expertise (Fernández Polo & Cal Varela, 2009; Gea-Valor et al., 2014; Lillis & Curry, 2010). **TH1**, **TH4**, and **TH10** are examples of beneficial

relationships that I, the in-house editor, have been able to forge with the informants. Despite her high standing within the scientific community, the informant in **TH4** is convinced that my contributions benefit her work, and was able to elaborate on the benefits that my input has both for her own texts and her efforts to teach novice researchers working under her. Similarly, the informant in **TH10** showed an inclination to incorporate my proposed edits that aimed to make the text more rhetorically conventional, thus displaying deeper improvements to texts.

Though not the only aspect that these brokers tend to focus on, the two examples cited in the previous paragraph illustrate how NES language professionals may help the texts produced by NNES academics adhere to gatekeepers' standards of language appropriateness and the rhetorical, discursual conventions of English (Benfield & Feak, 2006; Burrough-Boenisch, 2003; Mur-Dueñas, 2013; Shashok, 2001). However, there is often confusion or lack of agreement on what aspects require improvement (Bisaillon, 2007; Burgess & Lillis, 2013; Harwood et al., 2009), and authors have been found to be more willing to accept surface-level changes proposed by brokers than those targeting deeper issues in the writing (St. John, 1987), likely due to a mismatch in the field expertise of the NNES author and the NES language broker (Mur-Dueñas et al., 2014; Willey & Tanimoto, 2012), but possibly resulting from other more personal factors. The informants in **TH2** and **TH8** did not expect me to revise their papers beyond the word, phrase, and sentence level; thus, had we clearly established my remit for these projects I could have made more efficient use of my time. In other THs studied here, speed was clearly a more pressing issue than the precision or depth of my revision (e.g., **TH7**, **TH9**). Though it is widely accepted that quality science carries more weight in the peer-review process than well-crafted writing (Gosden, 1992; Pérez-Llantada et al., 2011), papers with substandard English may be rejected if they lack traits that would otherwise make them stand out from the rest or attract gatekeeper criticism for other reasons.

Some of the journal editors and reviewers acted as academic literacy brokers (Lillis & Curry, 2006a), shaping the texts in important ways, while others took on a more adversarial role. The unsolicited feedback received in **TH1** caused substantial overhaul to the text, to such an extent that the final product was much the work of the editor-in-chief. Such input from the editor is not

uncommon (Gosden, 1992; Lillis & Curry, 2010), though the degree of this particular intervention may be extreme. The informant did not chafe at this treatment, indicating that the stance of concerned interventionist (Lillis & Curry, 2015) was most welcome. I learned of other positive experiences from the author-informants in **TH2** and **TH4**, who believe that reviewers and editors often improve submitted texts. A particularly interesting example of this arose in **TH2**; the informant, who first submitted his paper to one of the most prestigious journals in the field, did so not because he thought that he could secure publication, but rather with the hope that even when rejected, his paper would benefit downstream from the feedback of peer reviewers, as has been shown to happen (Casnici, Grimaldo, Gilbert, Dondio, & Squazzoni, 2017). A similar position was taken by the informant in **TH3**, although the informant appeared to rely too heavily on the goodwill of these gatekeepers (Gosden, 1992).

Not all of these informants had such positive experiences, however. **TH9** features a case of particularly fierce criticism from the sole reviewer, who found most everything in the informant's work to be unworthy of publication. The failure of this reviewer to allow the informant to save face exemplifies the disconnect between the politeness that characterizes published academic work and the treatment given to some authors in peer review (Kourilova 1998). In this and other THs, informants received negative remarks on the English of their papers without any specification as to what improvements needed to be made, as sometimes happens to NNES researchers (Mur-Dueñas, 2012). Additionally, we have seen in a number of THs that some of the reviewer comments that criticized "the English" also contained substantial errors in spelling in syntax, which suggests that the reviewers themselves are NNES individuals. More and more journal reviewers are L2 users of English (Ferguson, 2007), though some may be holding their peers to standards that they themselves do not—or cannot—comply with.

1.4.4 Importance of occluded genres

Throughout this discussion I have made occasional reference to the importance of occluded genres. My analysis has focused on the strategic use of the point-by-point response to resist gatekeeper control, the key role played by senior collaborators in advising their collaborators on how to correspond with editors and reviewers outside of the RA proper, and I have also

mentioned instances in which referee reports triggered appeals by the informants. In addition to this textual evidence, the informants in **TH1** and **TH5** specifically mentioned the covering letters and point-by-point replies as areas of weakness for them. Despite the availability of in-house language support in our institution, the informant in **TH5** insinuated that language specialists may not be sufficiently equipped to help NNES authors in the multiple circumstances in which they must communicate with international journals. Occluded genres have been found to be one of the most vexing aspects of the publication process (Martín-Martín et al., 2014), and in response some EAP experts have incorporated a wide array of academic genres in their research and training materials (Cargill & O'Connor, 2006).

Not all of the authors feel hobbled by occluded genres, however. Following an initial rejection, the informant in **TH3** used the submission letter to reposition her RA before the journal gatekeepers without making any changes to the text proper, thus suggesting that these occluded genres are an integral part of the way a text is received. In **TH2** we have also seen how authors can use correspondence with reviewers as an opportunity to convince the discourse community of the value of a text; the informant and his coauthors used the point-by-point response to brainstorm their revisions to be made during peer review, producing multiple drafts of the text in which they planned substantial changes to the RA proper. In this regard, the point-by-point response marked a totally independent conversation with the reviewer and editor.

1.4.5 Written ELF and learning to write

The willingness of some journals to publish the informants' texts despite the apparent rhetorical transfer from their L1 suggests a certain openness to the contributions of these NNES authors (J. Flowerdew, 2001; Swales, 2004). The primary sign of hybridity between the authors' L1 background and the rhetorical conventions of English academic writing included a tendency toward implicitness (Bellés-Fortuño & Querol-Julián, 2010; Golebiowski, 1998; Valero-Garcés, 1996). One example of this is the tendency of one of the informants in **TH7** to implicitly rather than explicitly signal the study limitations. Additionally, the case of **TH5** supports the findings of other IR studies carried out in Spain (Mur-Dueñas, 2010; Sheldon, 2011) in that the informant argued that the hypothesis of his study was not only implicit in his paper but also that it was

located in the discussion rather than the introduction, thus marking another possible feature of written ELF.

In large part, the journals appeared to prioritize the propositional content expressed in the RAs, even at the expense of rhetorical orthodoxy. This emphasis on what was being said was particularly evident during post-submission revision. It has been previously shown that Spanish biomedical researchers tend to revise their work in a way that is more piecemeal than recursive (St. John, 1987), and I found this to be largely true. Two particularly illustrative examples of this were seen in **TH2** and **TH4**. During the post-submission phase, the informants in **TH2** seemed to tack on and cut out propositional content with great ease, thus creating an image of a rhetorical structure that is highly malleable. The author in **TH4** did much the same, adding a stand-alone move to her paper that was not accompanied by any other modifications to the surrounding discourse. It would appear, then, that what the author in **TH2** referred to as the similarity of “structure” that is pervasive in scientific papers in his field has made RA publishing more manageable for him and others, which may serve to reduce the sense of linguistic disadvantage felt by some NNES authors.

The issue of how these authors have learned to write effectively came up in a few of the informant interviews. The informants in **TH4** and **TH8** pointed out that they had learned to write through years of reading other academic papers, which allows them to incorporate phrases and other expressions from model texts (Gosden, 1992; Wood, 2001). The author studied in **TH4** talked of her time as a postdoctoral fellow in the United States, where she coauthored papers alongside her mentor. Interestingly, the contributions that her mentor made to her texts did not focus on discursal or rhetorical issues, thereby supporting theories of genre acquisition in the local milieu (Berkenkotter & Huckin, 1995), with “local” being understood to represent either one’s physical or metaphorical, community-based surroundings. Of all my informants, only one (**TH2**) mentioned explicit instruction in academic writing; a similar absence of overt training has been found in other contexts (Fernández Polo & Cal Varela, 2009; Gledhill, 2000).

1.4.6 Writer attitudes and the influence of the English language in NNES academic writing

Attempts to determine the attitudes of NNES authors toward the dominance of English as the lingua franca of science are complex (Ferguson et al., 2011), and my study has been no exception. The opinions given by my informants reflected an interplay between their overall view of English, self-assessments of disadvantage when writing in English, and their experiences with the peer-review process. Of all the informants I interviewed, the pair I talked to as part of **TH7** expressed the most straightforward negative views; they believe they are disadvantaged as NNESs, they have mildly negative opinions of the language as a whole, and were overtly critical of the peer-review process. Others have also been found to have such beliefs (St. John, 1987). The informants in **TH1**, **TH3**, **TH6**, and **TH10** have positive views on the language, though in light of the difficulties they have faced, have developed an attitude of pragmatic resignation as regards their need to publish in English (Pérez-Llantada et al., 2011); this pragmatism has motivated them to search for methods of overcome the hurdles they face by seeking the help of language brokers (**TH1**, **TH3**, **TH10**) and partnering with senior colleagues, who in return become named coauthors (**TH6**). Others, such as the authors in **TH2** and **TH4** are more ambivalent, stating that they feel disadvantaged by the way the peer-review system works, though do not believe this to be unfair. Similar attitudes have been documented among Spanish academics (Ferguson et al., 2011; Martín-Martín et al., 2014). The scholar in **TH8** does not believe he is disadvantaged by the widespread use of English in science, and nearly one-third of the sample surveyed by Flowerdew (J. Flowerdew, 1999b) held the same view.

Having explored the rhetorical difficulties these authors encountered in their papers and the extent to which other related factors may have shaped their experiences, the opinion expressed by the informant in **TH10** appears to sum up the role played by language in NNES international publishing in international English-medium journals: English, though still a factor in the additional burden faced by these scholars, is becoming less and less of a handicap. The fact that nine of ten RAs were published in international journals is a testament to the success that NNES authors are having when they attempt to publish their findings in English (Benfield & Feak, 2006; Burroughs-Boenisch, 2003; Ferguson, 2007; Ferguson et al., 2011; Iverson, 2002; Kourilova, 1998; Tribble, 2017). As Ferguson (2007) astutely points out, a narrow focus on the textual products obscures

the dynamics inherent to the processes these academics may endure. The L2 status of authors like my informants still poses an additional burden (Ferguson et al., 2011; Hanauer & Englander, 2011; Huang, 2010; Tardy, 2004; Uzuner, 2008), though other factors may be equally or more important, such as the degree of expertise of the author in manipulating the rhetorical and discursal resources inherent to their discourse community (D. W. Cho, 2009; Ferguson, 2007; J. Flowerdew, 1999b, 2000; Tribble, 2017), perseverance (Belcher, 2007; Curry & Lillis, 2004; Uzuner, 2008), physical and social distance from the Anglophone center (Ferguson, 2007; Hyland, 2016; Lillis & Curry, 2006b, 2010; Uzuner, 2008), and the degree of internationalization of the author's discipline (Lorés-Sanz et al., 2014; Mur-Dueñas et al., 2014).

Conclusions and limitations

The consolidation of English as the lingua franca of science that has taken place over the last several decades has expanded the possibilities for participation in research at the same time that it has erected barriers—real or perceived—limiting access by scientists from other languages, other cultures, and other sociocultural circumstances. As the opportunities for NNES researchers to broadcast their findings in their L1 have dwindled, these researchers now must compete in English for a place in the conversation of science, where they are pitted against NNES and NES peers. Expertise, evidence, and persuasion are the tools they have to do this.

The ability to argue the merit of scientific research is closely bound up with the complexities of language and the ways language enables us to do things in the world. To communicate effectively and achieve their pragmatic goals, writers, readers, speakers, and listeners call upon a wide range of skills, one key element being their familiarity with research genres in English and the discourse communities that they serve. Whether NNES academics are sufficiently equipped a priori to do so or, alternatively, face additional challenges stemming from their L1 background, is a problematic issue, and one for which there is no simple answer.

The aim of the present doctoral thesis has been neither to demonstrate linguistic disadvantage nor discount this as a possibility. Rather, this study has endeavored to offer a situated perspective of NNES writing to explore the many text-internal and text-external forces that may influence the overall phenomenon and the way these variables interact to determine what gets published and what does not. Fine-grained knowledge of the contributions these academics put forth to their international discourse communities, the processes and circumstances that gave rise to these contributions, and the mechanisms used to judge and reshape this academic work is necessary because it adds to the empirical basis on the issue. Such knowledge can then be used to inform the approaches taken by the variety of stakeholders involved.

1.1 Revisiting research questions and evaluating study hypotheses

My approach has been guided by three main research questions. First, I wished to chronicle the creation of ten different RAs in order to obtain a fuller understanding of the authors, their goals

in publishing the text, the people they either involved by choice as collaborators or who became involved as gatekeepers, and the way these individuals worked together or against each other. My second objective was to study the written products as they took shape and at a number of writing endpoints common to each process (here termed “milestone versions”), one of the most important being the moment at which the text was first submitted to a journal. This analysis allowed me to characterize the rhetorical patterning they contained and, using some of the more influential descriptions appearing in the EAP literature, determine the extent to which this mirrored or diverged from the patterns commonly used in scientific discourse. Third, I sought to describe the way these NNES authors’ texts were received, and with this information arrive at a more nuanced understanding of the deficiencies they were believed to have.

The hypothesis with which I undertook this study posited that, on the whole, the THs would evidence a marked concentration on propositional meaning at the expense of deeper, discoursal issues. I further postulated that this failure to write and revise according to a more conventionally English rhetorical structure would provoke language-related criticism, therefore hampering the authors in their endeavor to publish.

Part of the evidence I have analyzed confirms the hypothesis that the writing of these NNES authors displays a predominant preoccupation with propositional meaning, and that either this concern for content or their limitations as writers makes their texts markedly foreign in their rhetorical traits. Especially in the revisions made by the authors and their coauthors, but also in the way they drafted these ten RAs, some informants prioritized the presence of content over the way these concepts could be used to further their argument. In general, their scientific expertise was able to come through more clearly in the results and methods sections, although they often had to rework these sections to convince editors and reviewers of the procedural soundness of their studies. The volunteers I studied were more likely to stumble in the more argumentative sections of the discussion and introduction, where they themselves reported greater difficulty, and this self-assessment was validated by my move-step analyses and measurements of the degree of rhetorical change in these sections.

Despite the valid points made by critics of “big culture” approaches to writing arguing that these perspectives overlook the complexities and continuous state of flux that characterize contemporary society, the previous experience of these informants and others like them cannot be ignored as factors here. Only one of the authors reported having received explicit training on how to write in English, and the instruction he received appears to have been limited to general and abstract notions on the shape of certain sections of the canonical IMRD structure, with little to no indication on how to put this into practice. This absence of overt training likely reflects both the failure of the hard sciences as a whole to recognize the importance of effective writing and also the chronic difficulties that plague the Spanish education system in terms of English training and, possibly, the lesser attention given to both L1 and L2 academic writing instruction in Spain. If learning to write well is a substantial challenge even in one’s L1, these three factors make it an even steeper uphill climb for any Spanish scientist to write in generically conventional English.

Though parts of these RAs contain markedly unconventional rhetoric, the fact that nine of the ten texts were eventually published in international journals is evidence that this type of writing was successful in the end, often without any major overhaul of the way the paper was structured. Therefore, scientists who receive reviewer feedback proposing comprehensive changes to the paper or the research itself do have the option of submitting their work mostly unchanged to another journal, as this perseverance will most likely be rewarded and require substantially less time and effort than restructuring the paper or redesigning the study in a more fundamental way.

While I did not test for this in my study, there is clearly a difference between the satisfaction and rewards that come from papers published by the first journal and those published after four or five attempts. In light of my findings, I agree with other researchers (Ferguson, 2007; Politzer-Ahles, Holliday, Girolamo, Spsychalska, & Berkson, 2016) who caution against limiting analysis of NNES writing to a mere issue of whether academics publish their papers or not, as the degree of work, time, and resources required is often hidden by bibliometric measurements. This drain on material and nonmaterial resources certainly had a net negative impact on the researchers who endured prolonged publication processes, and efforts should be devoted to ameliorating this situation.

Unlike the majority of the informants I studied, a few demonstrated that they have mastered the rhetoric of the RA to such an extent that this aspect does not constitute a limitation for them during peer review. Commended at times by prestigious Anglophone-center journals for the quality of their writing, they know how to design and execute their studies and can effectively frame their arguments within the constraints of the RA, therefore making my hypothesis on the rhetorically marked character too coarse and in need of nuance.

A key theme of my study emerged from the unexpected but equally compelling observation that success in academic writing is not always rewarded to authors whose papers most closely adhere to NES models, and that sometimes the “best” writers—and, possibly, the most knowledgeable scientists—are not recognized as such during peer review. Though I am not in the position to assess the scientific merit of the texts in my corpus, my research does enable me to advance certain claims about biomedical writing in the context of the genre. By taking highly successful exemplars such as those studied here as models of expertise, we can isolate the factors that make them so successful and train others who wish to bring their research output to similar levels. In the next section I will explore some practices that could be used to channel expertise into non-Anglophone settings and transmit this knowledge from experts to more novice scholars.

A fundamental attribute of this concept of expert research writing is the ability of scientists to understand the social system within which they must maneuver, the weaknesses inherent to their circumstances, and the ways in which their research activities and their research writing can maximize their chances for success. As expertise should be measured from the vantage point of the audience (Gosden, 1996) rather than that of the analyst, the THs studied here provide examples of success (Englander, 2006) and expert writing (Tribble, 2017). Expert writing exhibits a series of traits that come together to increase—or, at times, decrease—the chances of a text seeing publication. Within this framework, the limitations of an NNES author can be validated and also placed in context alongside other factors.

In the following section I will propose a number of measures that can be enacted by different stakeholders involved in global academic publishing in English to foster participation by NNES academics.

1.2 From process to product: Insights for stakeholders of international English-medium publishing by NNES researchers

Nowadays, there is an abundance of courses, materials, and enterprises offering training in “technical English” or “medical English.” As with other areas of ELT, however, what it means to be a proficient user of scientific English is elusive, as linguistic competence is specific to the modes and pragmatic situations in which users engage. Scientists looking to consume the research of others and produce work of their own can benefit from approaches rooted in the social and intellectual atmosphere of their discipline and the possibilities afforded by the discourse community for contributions from outside the Anglophone center. Based on this knowledge of the transactions that take place when an editor reads a submitted manuscript and sends that manuscript on to referees, learners may practice using the linguistic forms that will help them do this.

Local mentors and other senior academics can play an integral role in this process of socialization, educating less-experienced researchers on recent trends, influential figures, and conventional wisdom. Part and parcel of this education is the recognition of the advantages and disadvantages inherent to the discipline, such as its degree of internationalization and the existence of local, national, and international networks. Furthermore, accomplished researchers can often broker these researchers’ entry into these networks, which will likely yield substantial benefits over the researcher’s career. Another aspect that merits reflection is the availability of personal and institutional resources that the researcher has at their disposal and the ways in which these may place limits on the type of research work they may produce. In addition to funding, quality research may require time, human assistance, and other support that is only available in more privileged settings. Having learned of the opportunities and barriers that exist in the discipline and in their institution, researchers can then operate within their limits or even test these limits, though always in calculated fashion.

Once NNES researchers have explored the community they wish to enter and have a grasp of the difficulties they may face, exhaustive front-end planning may help scientists design better studies and thereby avoid disappointing editorial decisions. Researchers should critically examine the target journal where they wish to publish their manuscript, identifying patterns in the published studies across a number of variables such as the characteristics of the authors who have published there before and the preferred submission types. Based on this information, they can make informed decisions as to the appropriateness of their potential study for the journal, plan rhetorical strategies to highlight the strengths and newsworthiness of their manuscript, and also head off possible criticism aimed at its shortcomings. This way, authors can continuously monitor their future publication and, with the assistance of any language support available, begin drafting their manuscripts with this specific audience in mind.

Language brokers can effectively aid NNES authors by first discussing the authors' needs and expectations with them and then adapting their work accordingly. My study contains examples in which there was a clear mismatch between my understanding of my remit and the expectations and goals of the authors; had I been more aware of the discipline-specific characteristics of each RA and the extent to which the informants needed and wanted assistance, I could have worked more efficiently and increased the satisfaction of these author-clients. Arriving at a mutual understanding on these parameters is rarely straightforward, and points of disagreement would no doubt arise from the different perspectives that scientists and language professionals draw from. One likely source of disagreement is the depth with which the editor revises the text. As scientists are often unaware of the porousness between linguistic form and content, many would doubtless say they prefer a cursory revision of their work over a lengthier, more in-depth treatment by the editor. However, there could be greater mutual understanding if these conversations on roles and priorities were to take place in the context of more prolonged contact or, ideally, following a training course facilitated by the language broker. This way, the language broker could focus their time and effort on deeper, discursive issues when necessary, and where texts need only lighter revision, they could adjust their approach.

More effective ERPP instruction would further enable NNES researchers to write more discursively conventional texts, thus increasing the likelihood that these texts will be well-received. Training initiatives that draw from the ERPP and EAP literature can provide novice and more advanced writers with empirical evidence and expert perspectives on the rhetorical patterning customary in particular specialties and subspecialties, linking these concepts of rhetoric with insights on the interpersonal aspect of research writing, possible cultural differences between the rhetorical culture of their L1, and the lexicogrammatical features used to produce fit-for-purpose texts. Additionally, such courses would provide a context in which to stress the importance of occluded genres and other research genres as a means of framing and negotiating relations with members of the discourse community. Lastly, this instruction could include a series of case studies and best practices on effective relationships between authors and language brokers, thus providing researchers with insight on what they can expect in an author-editor relationship and how they can derive utmost benefit from professional language support.

Due to the great variability in gatekeeping practices seen here and the frequency with which NNES authors justifiably develop negative opinions of the peer-review process, the academic community as a whole would do a great service to authors of all kinds by further standardizing the expectations to be met by editors and peer reviewers. By putting in place an ethical code of conduct and establishing certain basic parameters for the genre of referee reports, submitting authors will be able to more clearly interpret the issues pointed out by gatekeepers and thus guide their decisions for further revision.

1.3 Limitations and further research

The limitations of my research mostly stem from the complexities and sensitivities of the frameworks I have used to design my study and the sacrifices I have made for the sake of viability. In using convenience sampling to recruit informants, I introduced a self-selection bias that prevents the extrapolation of my findings to other populations. A more comprehensive picture of the writing, revising, and publishing done in the institution would require a multi-step approach aimed at gathering a wider range of viewpoints, although an increased representativity

of the sample would create tension with the qualitative corpus-based study of specialized discourse that I have carried out here.

An additional limitation concerns the modest engagement I had with the authors. All author perspectives were obtained in a single semi-structured interview and, in some cases, follow-up correspondence with these individuals and other interactions I had with them as part of our routine professional lives. More prolonged exposure would have allowed me to confirm impressions and compare the informants' experiences with these ten RAs against other work they did subsequently, thereby placing this RA in a much richer context. Also, in asking informants to explain the motivations and circumstances of their past writing, I introduced a certain recall bias that likely influenced the responses given. Recollection of the experience of writing a text months after it was published only allowed them to tell part of the story. Lastly, a more detailed and systematic approach to transcription would have yielded more robust findings on issues emerging from the discourse. Given the brief duration of our interaction, transcription grounded in theory would have allowed for a fuller analysis of the interplay between emic and etic perspectives.

In spite of my training and efforts to code the authors' texts systematically and faithfully, the qualitative nature of my study and the subjectivity inherent in rhetorical move assignments constitutes another limitation. My assessment of the communicative purpose in each unit of discourse was based on my interpretation of the function of each unit and not on their lexicogrammatical features. As a result, there is likely a discrepancy between my understanding of the rhetoric of these RAs and the way other analysts may have described them. Future studies would benefit from coding by two analysts to improve the reliability of move assignments and from a pilot coding phase to anticipate difficulties during the study proper.

The findings and limitations of this doctoral thesis can be used to design promising lines of future research. Similar ethnographic studies may measure the differences between the first journal to which a text is submitted and the journal that ultimately accepts to publish the paper in terms of the rewards reaped by the author. By determining these differences both quantitatively (e.g.,

impact factor) and as perceived by the researcher, such a study could shed further light on the lengths that NNES authors must go to in order to publish their work and the possible decrease in the value of their publication as a result of rejection. Also, the practices that emerged throughout this study could be used to conduct focus-group sessions aimed at determining how prevalent these practices are and to discover previously undocumented strategies employed by NNES authors. Data from these sessions could then be tested further, possibly challenging previously held views on effective publishing or adding to the battery of tools that authors may put into practice.

1.4 Closing remarks

This study on research and researchers through the lens of NNES English-language publishing is a testament to the complexities and the flaws inherent to the social ecosystem of scientific pursuit and the particular challenges that these Spanish biomedical researchers face when navigating the waters of scientific knowledge-making. Far from a straightforward enterprise, the strengths and vulnerabilities that have come to light in these scientists' stories and writing underscore the highly personal and oftentimes circumstantial nature that defines twenty-first-century writing in English for the Academy. Awareness of the issues, agents, and interactions that coalesce in complex textual artifacts meshing innovative knowledge and basic human behavior can help to define the limits that NNES researchers face and suggest strategies to overcome these obstacles.

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