DISPOSITIONAL REFLECTIONS

A Dissertation

by

BORIS H. J. M. BRUMMANS

Submitted to the Office of Graduate Studies of
Texas A&M University
in partial fulfillment of the requirements for the degree of

DOCTOR OF PHILOSOPHY

December 2004

Major Subject: Speech Communication

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ABSTRACT

Dispositional Reflections. (December 2004)

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In this dissertation, I explicate how scholars implicate themselves in the subfield of organizational communication studies by engaging in antinomic language-games which make the conduct of research (and textwork in particular) possible. My analysis suggests that the studied scholars enact these games to understand a more or less common object of knowledge, but also to constitute a more or less identifiable position in this given social space. Reflection on the ontological complicity between these position and subfield occurs uncommonly, however. I illustrate, in turn, that this lack of reflexivity hinders discussion about the way academic research practices induce breaks with the social realities which these scholars are trying to understand. In light of this argument, and based predominantly on a translation and extension of Pierre Bourdieu's ideas, this dissertation thus illustrates how the language-games of scholars in organizational communication studies sustain a limited practice of reflexivity and considers its effects on their production of knowledge.

ACKNOWLEDGMENTS

Finally, I express my gratitude to my lovely mum and dad, my wonderful sister, and all my dear family and friends around the world. Like all memorable texts, you continue to weave thoughts, ideas, and feelings into something inexpressible.

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CHAPTER I

INTRODUCTION AND IMPETUS

As the human sciences were invented around the eighteenth century, "man"

became an object of scientific investigation worth pursuing by means of special theories

and methodologies.[1] Along with the "birth" of the study of the "human subject"—a

subject for research or "research subject"—came a concern for the "academic subject"

and for the mutual relationship between these two agents. Although this relationship

has received much attention, I believe that its bearings on the practice of human science

research still provide opportunity for academic reflection, because scholars seem to

overlook the social conditions which make the constitution of and relation between

these two "subjectivities" possible. To better understand the object of the human

sciences ("behavior of human 'subjects'"), the parallel examination of the way scholars

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This dissertation follows the style and format of *Communication Theory*.

[1] If Michel Foucault (1994) is right in *The Order of Things*, the invention of the human sciences occurred through a shift in focus; a move towards the positively empirical which undermined the importance of language in the development of knowledge. In light of this argument, the development of ideas about "man," "the subject," "the self," or "the individual," needs to be seen in time and context. Humanity is a concept that shifts its center on a regular basis. "Man" is thus "a figure not yet two centuries old, a new wrinkle in our knowledge" (Foucault, 1994, p. xxiii). Recent happenings in the human sciences are marked by a return to language which also marks a change in the way scholars conceive of and study human "subjects." Correspondingly, notions of "subjectivity," "selfhood," "individuality," and "identity" have become much discussed objects of knowledge (e.g., see Althusser, 2000; Benveniste, 2000; Bourdieu, 2000b; Carr, 1999; Du Gay, 1996; Elias, 1991, 2000; Foucault, 1988, 1994; Hall, 1992, 1996; Mauss, 2000; Ricoeur, 1992; Rose, 1996; Sampson, 2003).

are implicated in their surrounding social contexts or "fields of study" thus seems

important. Pierre Bourdieu notes that

> the conditions of possibility of the scientific 'subject' and of the scientific object
> are one and the same. And to each advance in the knowledge of the social
> conditions of production of scientific 'subjects' corresponds an advance in the
> knowledge of the scientific object, and conversely. This can be seen most clearly
> when research takes as its object the scientific field itself, that is, the true *subject*
> of scientific knowledge. Far from undermining the foundations of social
> science, [the study] of the social determinants of [human science] practice is the
> only possible ground for a possible freedom from these determinations. And it
> is only on condition that he avail himself of the full use of this freedom by
> continually subjecting himself to this analysis that…[scholars] can produce a
> rigorous science of the social world which…offers [scholars] the means of a
> potentially liberating awakening of consciousness. (Bourdieu & Wacquant,
> 1992, pp. 214-215)

In light of Bourdieu's work, I conceive of the academic (or "scientific") field as

consisting of multiple "fields of study" which the reader might imagine as social spaces

which center on more or less commonly defined objects of knowledge (e.g.,

communication, psychology, or physics). In turn, each of these fields is divided into

more specialized "subfields"[2] (e.g., in the case of communication studies, studies in

organizational communication, interpersonal communication, or health

communication). By taking position within a field through their activities (teaching

students, writing publications, fulfilling administrative duties, etc.), scholars instate and

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[2] From here on, I will use the term "field" rather than the inelegant term "(sub)field" to
indicate "field" and "subfield." Correspondingly, I will only use the term "subfield"
when indicating a specific subfield like organizational communication studies.

reinstate a field's existence, but also their own existence.[3] This participation implies a commitment to and belief in a field's *doxa*, the fundamental presuppositions lie at the basis of its practices.

Bourdieu (1988, 2000a) suggests that the relatively special sociohistorical conditions which make the existence of a field possible often distract scholars from discussing how these conditions make their work, their perspectives, and their "identities" as scholars possible. The workings of a field frequently prevent reflection on the way it constitutes itself based on a particular *doxa*. This preclusion results, Bourdieu believes, from a field's struggle to legitimate its existence vis-à-vis other fields. That is, by fencing off discussion about the way a field as a totality practices research, the value and effects of these endeavors, and the "breaks" with the social realities studied that these practices induce,[4] a field prevents discussion about (let alone, alteration in) its workings.

As Thomas Kuhn (1970) suggests, often, a *doxic* alteration like this is considered costly, because it unsettles a field's routine practices. More costly, perhaps, is the fact that such a change forces scholars to reconsider what it means to be a scholar. To lower costs like these, a field often reinforces the "norm" that this kind of reflexivity hinders

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[3] Presumably, a scholar aligns himself or herself with a primary field of study and most likely with a primary subfield, but I acknowledge that scholars usually position themselves in multiple fields at the same time. A more expansive discussion of this matter follows in chapter II.

[4] For example, breaks are constituted by abstracting an object of knowledge based on a particular position in an academic field of study or assuming that those who are studied operate from the same logic as those who conduct a study. Bourdieu (2000a)

scholarship, because it slows down the production of knowledge and leads to fruitless relativism.

As an alternative, I argue that this kind of reflexivity may lead to better social conditions for producing knowledge. That is, this reflexivity might indeed unsettle the foundations of a field, but such confrontation does not need to imply that a field loses its legitimate position vis-à-vis other fields. Rather, a field can strengthen this position by better understanding its own values, presuppositions, and practices. Thus, instead of defending its position, a field can benefit from submitting its "selfish" commitments to "the rules of dialogic confrontation" (Bourdieu, 2000a, p. 119) and, thereby, learn to overcome the limitations of such commitments. In this study, I illustrate that discussion about a field's *doxa* can be started by analyzing how scholars operate from a set of presuppositions, how these presuppositions affect the production of knowledge, and how they reinforce an implicit "norm" regarding the practice of reflexivity.

As this study suggests, individual scholars personify a field's struggle for existence during their education, socialization, and overall experience of operating in a given social space. Hence, the field's struggle becomes an individual struggle for existence. Many scholars make great effort to maintain their position a field or subfield of study, because they are interested in its production of knowledge. However, the academic "game" has other stakes as well, because it enables "an existence" or "identity" which partly results from participating in this game. A field does not only make this existence possible by offering economic security ("economic capital"); the

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claims that failing to reflect on breaks like these leads to "scholastic fallacies."

game also involves a struggle for other "forms of capital," like educational and institutional credits ("cultural capital"); membership to particular groups or networks ("social capital"); and social worth ("symbolic capital"). Each of these forms, then, helps scholars to constitute a position in social space which more or less identifies "them." This "identification" is made possible, because these forms of capital are perceived to be relatively scarce. For example, there are only so many people who obtain a degree from Cambridge University (cultural capital); the Royal Statistical Society has a limited number of members (social capital); Karl Marx derives his reputation (symbolic capital) from the fact that his ideas are seen as important and relatively unique. These forms of capital thus derive their value from the fact that they are *perceived* to be more or less exclusive and valuable *in a particular social space.*

In this study, I investigate how scholars practice language to constitute their identifiable position in a field; what role forms of capital play in this practice; and how this practice affects the production of knowledge. Thereby, I show that the reflexivity illustrated through this kind of research may help scholars to recognize how their social determinants promote struggles for capital and how these struggles affect the way knowledge is created.

Several scholars suggest that reflexivity (defined in various ways) is important for improving the practice of human science[5] (see Alvesson & Deetz, 2000; Alvesson &

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[5] Reflexivity is certainly also important for the conduct of natural science. The division between human and natural science (or, more generally, "man" and "nature") is closely associated with the historical invention of "man" which I addressed at the beginning of this chapter. Foucault (1994) addresses the "institution" of this rupture with much wit

Sköldberg, 2000; Bourdieu, 1977, 1990a/b, 1988, 1998a, 2000a; Bourdieu, Chamboredon, & Passeron, 1991; Bourdieu & Wacquant, 1992; Hawes, 1978, 1994; Wacquant, 1993; Woolgar, 1988). Practicing reflexivity is sometimes equated with practicing individual introspection; applying the same instruments on oneself as the ones used to study others; thinking about the relationship between the human "subject" and the academic "subject;" thinking about the way one's research text constitutes a reality, but also is constituted by this reality; or combinations of these views (see Bourdieu & Wacquant, 1992). Most importantly, reflexivity often is thought to involve thinking about the "effects" of a researcher's "subjectivity" on his or her practice of research.

This study demonstrates how the practice of reflexivity implies thinking about the way a field's sociohistorical conditions implicate a multitude of "subjectivities" and the way the enactment of these subjectivities induces breaks with the social realities that are studied. As I will explain, this kind of reflexivity is enacted through language in a field and therefore implies more than the individual practice of introspection. In addition, I do not suggest that this kind of reflexivity should overtake a field's object of knowledge (e.g., the object of psychology, organizational communication, or anthropology); that is, I do not claim reflexivity in itself should become a field's principal object of inquiry. Instead, I maintain that integrating this kind of reflexivity into a field's research practices might help to improve how a field understands its object.

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and depth.

More particularly, my investigation centers on the way academic "language-games"[6] make the practice of research, positioning, and reflexivity possible in a field of study. Within a field, scholars construct epistemologies by socially defining objects of knowledge and generating concepts to guide their analysis. As Kuhn (1970) notes, these practices are often captured in reasonably accepted ways of thought and conduct in a field and thus determine the legitimate profession of a communal trade. Furthermore, Wittgenstein (2001) claims that language plays a principal role in this profession by arguing that scholars practice research through language-games. The everydayness of these games can be so engaging, Wittgenstein argues, that it becomes difficult (or undesirable) to reflect on the way one is committed to these games. That is, language-games work according to particular "rules," but these rules only can be experienced or "known" by participating in the games themselves. Hence,

> rule-following is a general practice established by agreement, custom, and training. Therefore, although rules indeed guide us and afford us with our measures of correctness, they are not independent of us and hence do not constitute a coercive standard imposed from outside our rule-following practices themselves…[Accordingly, the mastery of language implies] being

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[6] According to Ludwig Wittgenstein (2001), "the term 'language-*game*' is meant to bring into prominence the fact that the *speaking* of language is part of an activity, or of a life-form" (p. 10). Consequently, "language has no single essence, but is a vast collection of different practices each with its own logic. Meaning does not consist in the denoting relation between words and things or in a picturing relation between propositions and facts; rather, the meaning of an expression is its *use* in the multiplicity of practices which go to make up language. Moreover, language is not something complete and autonomous…for language is woven into all human activities and behavior, and accordingly our many different uses of it are given content and significance by our practical affairs, our work, our dealings with one another and with the world we inhabit—a language, in short, is part of the fabric of an inclusive 'form of life'" (Grayling, 1988, p. 67).

able to employ its expressions in the many different language-games to which they belong. (Grayling, 1988, pp. 80-81; p. 74)

Scholars easily become caught up in their habitual language practices, so that they cannot see beyond these practices,[7] because the fields in which they operate function as relatively autonomous social spaces which have developed particular ways of doing (seeing, speaking, writing, reading, etc.). Consequently, it is important for a field to "battle the bewitchment of [its] intelligence by means of [its] language" (p. 40).

In this study, I take a critical point of view to analyze how scholars' struggles for different forms of capital affect the language-games which are enacted to create knowledge in a field.[8] Even more exactly, this study deals with the way *textwork*-language-games constitute this production. John van Maanen (1996) invented the term "textwork" to signify the language practices that scholars enact to accomplish "textualization" (Clifford, 1988). These more or less regulated activities are academic practices *par excellence*, for they enable scholars to communicate their work. Additionally, textwork facilitates the constitution of a more or less identifiable position in a field which has personal, social, cultural, economic, and symbolic value.[9]

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[7] Scholars are thus like flies trapped in a bottle "when language *goes on holiday*" (Wittgenstein, 2001, p. 16).

[8] This study sharpens the link between Bourdieu's idea of games about acquiring capital and Wittgenstein's idea of language-games. Bourdieu frequently refers to Wittgenstein's (2001) *Philosophical Investigations*, especially in his book, *Pascalian Meditations* (Bourdieu, 2000a). Although, Bourdieu's conception of games goes beyond Wittgenstein's language-games, I foreground the latter in this study.

[9] Bourdieu (1990b) states that publication is, quite literally, the act of making oneself

Presently, textualization is practiced mostly by typing marks on the computer screen. These marks become recognizable and understandable as they form the words of a language. As sentences, they convey thoughts. So the unwritten is converted into the written in more or less intelligible ways (Clifford, 1988). Thereby, an author is able to "objectify" his or her thoughts; that is, to express them in such a way that they can be interpreted beyond his or her own direct experience and consciousness in the form of a "text" (or a "textualization"). In this case, texts refer to "definite forms of words, numbers or images that exist in materially replicable form" (Smith, 2001, p. 164; cited in Cooren, 2004, p. 389).[10]

In a field of study, a scholar's perspective or position imbues a text with an "identity." Subsequently, a text is subjected to the field's ongoing evaluation in light of other texts. The production of (textual) positions thus occurs vis-à-vis the *doxic* practices of the field. Accordingly, the reader can think of a field as a "linguistic market" on which texts are traded as products to gain capital (Bourdieu, 1991a). However, although this market metaphor is informative, it also misleads, because it suggests that *individual* scholars act in purely economical and rational or intentional ways to maximize gains and minimize losses. This is only partly true. In line with Wittgenstein's (2001) ideas, the rules of a field's language-games do not predate

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public, but also of "officializing," ratifying, or legitimizing oneself in a field: "To have a name or a profession which are authenticated and recognized means that you exist officially" (p. 82). Hence, textwork helps the making of a "name" for oneself in a field.

[10] Unlike François Cooren (2004), who suggests broadening the definition of the term "text" by including oral texts, this study accentuates the production of written texts.

players, nor are players always conscious of them. Rather, these rules are experienced

in the act of participation.

To denote the broad rubric of scholastic textualization practices, I adopt Van

Maanen's (1996) concept of "textwork" which plays a central role in the enactment of a

field's linguistic market. Van Maanen defines textwork as

> practices that produce an article or book…[like] note taking; reading; outlining;
> corresponding; engaging in shop talk with colleagues; making formal and
> informal presentations of work planned, in-progress, and accomplished;
> drafting (and, alas, redrafting) articles as research proposals, reviews,
> commentary, journal articles, chapters and books; and so on. (p. 377)

However, I broaden this definition by arguing that the production of texts also involves

reading (by the author or others). Additionally, I claim that textwork does not stop

once a text has been published. Instead, this practice continues through reading the text

and appropriating it to create new texts.[11]

Furthermore, although Van Maanen articulates practices for engaging in

textwork, his definition does not connect these activities explicitly to the general

conduct of (human science) research in a field. Conversely, I see the larger arena of the

practice of research through the lens of textwork.[12] This does not mean that all the

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[11] Correspondingly, "any text is an intertext; other texts are present in it, at varying
levels, in more or less recognisable forms: the texts of the previous and surrounding
culture. Any new text is a new tissue of past citations" (Barthes, 1981, p. 39; cited in
Cheney & Tompkins, 1988, p. 460).

[12] Thus, "textuality" is a "methodological field…[engendering] all steps in the research
process" (Cheney & Tompkins, 1988, p. 463, citing Barthes). Accordingly, my focus on
textwork implies a centralization of the academic production of text as an object of

practices associated with research necessarily are undertaken with text in mind, but that textwork and "other" research practices (conceptualizing a study, "collecting" and analyzing "data") are indivisible. In this way, I argue that scholars position themselves in a field and acquire capital through all these patterned and regularized activities which involve interactions between author, reader, text, and field.

Finally, this study highlights the way individual scholars in a particular field, as authors and readers, practice textwork vis-à-vis other scholars and the field's textwork conventions. In so doing, I de-emphasize the historical analysis of the way this field's object of knowledge has been constituted through the practice of language.

Evidently, I did not conceive this study in a historical vacuum—in fact, the social conditions which constitute the main academic social space in which I operate have affected my practices highly. For long, philosophers of science, sociologists of knowledge, and rhetoricians of science have been interested in the way (natural and human) science is practiced.[13] Within these fields, scholars have made considerable

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attention, although I also address the research practices with which this production fuses. It is thus impossible to speak of textwork per se. Then again, only using the term "research practice" would not do justice to the focus of this inquiry.

[13] In the twentieth century, scholars increasingly started to examine all sorts of research practices, ranging from philosophical contemplation to biological experimentation. For example, scholars like Karl Popper (1959), Peter Winch (1958), Thomas Kuhn (1970), Imre Lakatos (1978), Paul Feyerabend (1975), and Michel Foucault (1972, 1994) began to consider philosophical questions pertaining to scholarship ("science" in particular) through various philosophical methods. Moreover, scholars like Karl Mannheim (1960), Bruno Latour and Steve Woolgar (1979), Karin Knorr-Cetina and Michael Mulkay (1983), and Norbert Elias (1987) started to investigate the social practice of science in its institutionalized form through ethnography, interviewing, and participant observation, as well as philosophical reflection and historical analysis. Whereas the

advances in understanding scientific practices, but they have paid relatively little

attention to the way scholars (others, but also themselves) are implicated in research

through the position they enact and struggle to uphold in a field of study. For example,

sociologists of knowledge may look at the way biologists socially construct "facts"

without investigating how the history of the biology field gives rise to these scientists'

operations. At the same time, these sociologists might fail to study their own

relationships to a field which gave rise to their positions and dispositions. Accordingly,

research in the three mentioned fields would benefit from studies that investigate the

effects of this reflexivity (or of its lack) on the production of knowledge.

In the next sections, I discuss the limitations of research in these three fields in

more detail by considering how these fields have looked (or failed to look) at "field"

effects on the development of knowledge. These brief discussions are in no way meant

to be comprehensive, yet can be seen as introductions which illustrate the historical

precedents of this study. Moreover, as William Outhwaite (2000) suggests, the

differences between these fields are imprecise, implying variations in emphasis and

approach rather than substance. In my discussion, I therefore classify works under

these three fields while acknowledging the imperfections of my classification.

Anglo-American philosophers of science made the formal analysis of scientific
statements their object of attention, French epistemologists—influenced by 1960s
Althusserian Marxism as well as Kuhn's (1970) ideas about the historicity of
knowledge—took an approach that combined historical and philosophical analysis to

The Philosophy of Science

At the start of the twentieth century, the Vienna Circle played an important role in the development of ideas about the purpose and conduct of human science research (Bernstein, 1983). Its reasons for existence were motivated by debates about the differences between the natural and human sciences as well as the *doxa* that the scientific method would expose the truth of human behavior. It postulated a "logical positivist" view on the world, implying the rejection of metaphysics and a belief in the possibility of developing knowledge through empirically verifiable hypotheses.

This group of scholars, then, perhaps can be seen as an early group of philosophers of science—although they may not have identified themselves as such. Their work set off a range of philosophical debates about the nature of knowledge and the way to knowledge. For example, Popper (1959), one of the most influential critics of the Vienna Circle's hypothetico-deductive, empiricist model, claimed that hypotheses were falsifiable rather than verifiable. Possibly, Kuhn's (1970) emphasis on the effect of "communalism" on the production of knowledge offered the most convincing attack on Popper's thinking. Kuhn suggested that a scholarly community usually operates in a "normal science" mode. This period can be seen as a kind of routinized "puzzle-solving" that involves little questioning of the "paradigm" which comprises underlying theories and methodologies and guides the testing of hypotheses. However, normal science is interrupted from time to time, Kuhn claimed, when a paradigm becomes questionable, because it is no longer able to guide communal practice. Hence, such

the study of science (Outhwaite, 2000).

"scientific revolutions" are not the function of external reality presenting an "aha-experience" in a scholar's mind; rather, they depend on the internal social dynamics and preferences of an academic community.

In a sense, Kuhn introduced the idea of a field, even though he did not use this concept. Kuhn's work motivated scholars to conduct studies on the social production of knowledge. In particular (although not only), these studies were conducted by scholars in the sociology of knowledge.

The Sociology of Knowledge

Especially from the 1960s onwards, many studies were conducted that looked at the social production or social construction of knowledge (see Barnes, 1977, 1985; Bloor, 1976; Elias, 1987; Knorr-Cetina & Michael Mulkay, 1983; Latour, 1999; Latour & Woolgar, 1979; Lynch & Woolgar, 1990). Like studies in the philosophy of science, these investigations were certainly philosophical in character as well, but also employed social science methods (interviewing, participant observation, etc.) to investigate the way science was practiced in various fields.

Latour and Woolgar's (1979) work was especially notable in that it offered an ethnographic account of the way human culture in a biology laboratory influenced scientific practice and enabled the social construction of scientific facts. Moreover, the authors convincingly argued that the producers of these "facts" were as much concerned with personal "gain" (especially credibility, i.e., symbolic capital) as with uncovering scientific reality. The authors hence concluded that the

results of the *construction* of a fact is that it appears unconstructed by anyone; the result of rhetorical *persuasion* in the agnostic field is that participants are convinced that they have not been convinced; the result of *materialisation* is that people can swear that material considerations are only minor components of the 'thought process'; the result of the investment of credibility, is that participants can claim that economics and beliefs are in no way related to the solidity of science; as to the *circumstances*, they simply vanish from accounts. (p. 240)

The strength of this research was manifold and still has significant bearings. First, its merit lied in the fact that the authors, besides "objectifying" the production of knowledge, also reflected on their own objectification practices. Although the authors mainly reflected on the effect of their participant observation on the knowledge they socially constructed towards the end of their book, their writings illustrated the importance and effect of such reflexivity. In addition, their work began to question the relationship between scholars' struggle for capital in a subfield of study and their production of knowledge. That is, it showed that research participants acted politically by pursuing interests through their research in a field of contesting forces. Finally, Latour and Woolgar's research stressed the importance of language in the form of rhetorical persuasion and invention ("inscription") in the social production of knowledge.

Latour and Woolgar's analysis was limited in that it examined the work of one laboratory. Even though it considered the way participants interacted with and were implicated in the field of biology, the study de-centered the relationship between scholars' "identity" practices and their development of knowledge within this social space, as well as scholars' reflections on these practices.

For many scholars, Latour and Woolgar's study exemplified the highpoint of "social constructionist"[14] research (see Gergen & Gergen, 2003; Hacking, 1999). However, although it may have soft-pedaled the effect of a field on research participants or the researchers themselves, Latour and Woolgar's account was not devoid of consciousness about a field's social conditions (as well as "circumstances"). It acknowledged that scholars engage in social construction, but also appropriate values, beliefs, practices, and conventions, developed long before them. In turn, Latour and Woolgar did not argue that these values, beliefs, conventions, and practices were invented "on the spot;" rather, they were inculcated through scientific education and socialization. Nevertheless, by relying on Bourdieu's early ideas—which were not as well developed at that time—Latour and Woolgar's analysis did not take different

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[14] Social constructionism or "social phenomenology" "asserts that social reality is a 'contingent ongoing accomplishment' of competent social actors who continually construct their social world via 'the organized artful practices of everyday life' (Garfinkel, 1967, p. 11). Through the lens of this *social phenomenology*, society appears as the emergent product of the decisions, actions, and cognitions of conscious, alert individuals to whom the world is given as immediately familiar and meaningful. Its value lies in recognizing the part of that mundane knowledge, subjective meaning, and practical competency play in the continual production of society; it gives pride of place to agency and to the 'socially approved system of typifications and relevances' through which persons endow their 'life-world' with sense (Schutz, 1970)…[This approach is often presented in contrast with "social physics" research, which takes an] objectivist or 'structuralist' point of view…[by] undermining the 'illusion of the transparency of the social world.' Breaking with commonsense perceptions enables it to uncover the 'determinate relations' within which men and women necessarily enter to 'produce their social existence' (Marx). Thanks to tools of statistics, ethnographic description, or formal modeling, the external observer can decode the 'unwritten musical score according to which the actions of agents, each of whom believes she is improving her own melody, are organized' (Bourdieu, 1980b, p. 89) and ascertain the objective regularities they obey" (Bourdieu & Wacquant, 1992, pp. 8-9).

forms of capital into account, let alone many other concepts which Bourdieu developed

in the years after their investigation (I will discuss these ideas in chapter II).

Besides the sociology of knowledge and the philosophy of science, a third field,

which I have coined "the rhetoric and language of science," has concentrated on the

way knowledge is produced. This field pays special attention to the importance of

rhetorical persuasion and the overall practice of language.

The Rhetoric and Language of Science

Rhetoricians of science concentrate on the way scholars persuade audiences

through (written) texts (see Bazerman, 1988; Bazerman & Paradis, 1991; Farrell, 1999;

Gaonkar, 1999; Gross, 1990; Gross, Harmon, & Reidy, 2002; Locke, 1992; Kinsella, 1999;

Scott, 1999; Taylor, 1991; Weaver, 1985). Very similar to the aforementioned

philosophers and sociologists, they reflect on the relationship between language and

epistemology. In "On Viewing Rhetoric as Epistemic," for example, Robert Scott (1999)

maintained that rhetoric is not a method for conveying timeless truth, but as a never-

ending practice of generating time-limited truths. Likewise, Barry Brummett (1999)

pointed out that scientific observation implies rhetorical mediation. Through language,

human beings constitute what they observe and thus what they know. As a result the

production of knowledge is, at best, "intersubjective" rather than objective.

In general, scholars in this tradition investigate how human science scholars

collectively practice language to conduct research (Lucaites, Condit, & Caudill, 1999).

For example, Alan Gross (1990; see also Gross, Harmon, & Reidy's, 2002) described

exemplars of natural scientific discoveries as inventions brought about by means of rhetorical techniques, such as analogy, variations in writing style and textual arrangement, figurative language, and so forth, and thereby challenges the intellectual privilege of science (most often natural science in particular). Although these rhetoricians of science have drawn even closer attention to language practices, their works pay almost no attention to the effect of Bourdieu's notion of field, let alone his ideas about reflexivity.

Perhaps Foucault (1972, 1994) can be seen as one of the most important and influential scholars whose work focuses on the role of language in the production of scientific knowledge (Foucault is especially difficult to classify under the labels I have used). Foucault's studies examined the development of fields of study in the most direct way by suggesting that fields in the human sciences are historical artifacts. In other words, they are human inventions, institutionalized and sustained through language practices (see Dreyfus & Rabinow, 1983). Foucault's analyses questioned, then, whether it was at all possible to separate the human sciences from the conditions which gave rise to them and thus examined whether "objective" human science, autonomous from history and social processes, was at all possible. Hence, his analyses invoked questions like: "Could the social sciences recognize their dependence for their possibility upon a background of social practices, and then treat this background scientifically?…If one could have a theory of the background practices that make specific social sciences possible, could such a theory account for the social role played by such theories themselves?" (Dreyfus & Rabinow, 1983, p. 161).

Conclusions

Based on this brief outline, I conclude that philosophers of science, sociologists of knowledge, and rhetoricians of science (and scholars with a more general interest in the role of language in science) have paid insufficient attention to the effects of fields on the production of knowledge; the effects of "capitalist" struggles which are commonplace within these fields; and the effects of the reflexivity propagated by Bourdieu. Consequently, little is known about the way scholars implicate themselves in an academic field and the effects of this "ontological complicity" (Bourdieu & Wacquant, 1992, p. 20) on the social creation of knowledge.[15]

Moreover, I surmise that there is an increasing interest in studying the relationship between language and research. The parallel, heightened attention for textwork is especially noteworthy in this case. That is, in the 1980s, especially North-American scholars began to focus on issues that concern textwork (see Atkinson, 1990; Clifford, 1988; Clifford & Marcus, 1986; Geertz, 1980, 1988; Goodall, 1999, 2000; Marcus & Cushman, 1982; Rosaldo, 1989; Van Maanen, 1988, 1996). For example, some of these scholars compared research to a rhetorical performance which depends on literary conventions and experimentation, and saw textwork as the weaving or braiding of meanings—in line with the meaning of the Latin word *textus*. "Textuality" thus emerged as a "methodological field…[engendering] all steps in the research process" (Cheney & Tompkins, 1988, p. 463).

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[15] Similarly, Jacques Derrida (see Dick & Ziering Kofman, 2002) claims, the relationship between a philosophers' identity and his or her institutionalized practice of thought has

The "re"-emphasis on the practice of language is not surprising, considering the historical return to language in the human sciences observed by Foucault (1994). However, few scholars who operate under this broad umbrella have paid attention to the relationship between a field's language-games, scholars' positioning practices, and the development of knowledge through textwork. When comparing Foucault and Bourdieu, Foucault's writings focus on the way fields structure perception around objects of knowledge through "discursive practices." In so doing, knowledge is linked to power. Bourdieu's writings emphasize the historical games for legitimacy of academic fields of study by arguing that these fields are organized around "antinomies" (elemental and apparently irresolvable conflicts or contradictions between positions) which create controversies. In turn, the legitimate existence of academic fields often stems from the idea that these fields are needed to resolve these apparent conflicts. Bourdieu's work therefore aptly addresses the everyday challenges involved in the practice of research and its relationships to the competitions that result from engaging in these antinomic language-games. Accordingly, Bourdieu's ideas help to overcome the historical determinism which haunts much of Foucault's work by suggesting how scholars, as individual agents, constitute and are constituted by the fields in which they operate.

Overall, Bourdieu's work provides a set of useful concepts and arguments for addressing an important set of limitations encountered by philosophers of science, sociologists of knowledge and rhetoricians of science. These concepts and arguments,

received little attention.

then, enable the investigation of the practice of research in a field of study. To establish the basis for such an inquiry, I translate (and also extend) Bourdieu's work in the following chapter. Before doing this, I will provide the reader with an outline of the subsequent chapters.

Outline of Chapters

Chapter II starts with a translation of Bourdieu's main concepts and arguments. Thereafter, I show how Bourdieu's work can be extended through a better conceptualization of the way agents enact an identifiable position in a field. Subsequently, I apply this conceptualization to the practice of textwork in a subfield of study and formulate two principal research questions.

In chapter III, I introduce the subfield of study (organizational communication studies, which forms a subfield of the larger field of communication studies) in which I have investigated the phenomenon of positioning through antinomic textwork-language-games. I discuss some of the main antinomic language-games which have characterized the historical development of this subfield. After this, I reconstruct the production of three types of texts (interview transcripts, published publications, and journal scripts) which functioned as the "data" for this study, and I also explicate their analysis or translation. My analysis constituted a set of language-games which the participating scholars enacted while reflecting on their positioning practices in the subfield of organizational communication studies.

In chapter IV, I subsequently offer my expositions on these language-games and provide responses to the two research questions. The first part of my discussion focuses on the games which explain how scholars and the subfield constitute each other through the generic practice of language. The second set of games emphasizes the way scholars constitute an identifiable position in the subfield of organizational communication studies through textwork. I follow these discussions with a summary of my inferences. To conclude, I suggest that the most fundamental antinomic language-game which lies at the basis of most of the language-games discussed in chapter IV centers on the dialectic between the discovery and invention of knowledge.

In chapter V, I draw implications based on the conclusions from chapter IV. Additionally, I discuss some of the limitations of this study and, where possible, suggest avenues for future study.

CHAPTER II

POSITIONING IN A FIELD OF STUDY

In this chapter, I maintain that the scholarly struggle to constitute an identifiable position in a field is an integral part of academic research, because a field presents the arena for this kind of struggle and because scholars are linked to a field through its system of dispositions or *habitus*. A *habitus* can be seen as a system of classification which consist of "historical relations 'deposited' within individual bodies in the form of mental and bodily schemata that function as *symbolic* templates for the[ir] practical activities—conduct, thoughts, feelings, and judgments" (Bourdieu & Wacquant, 1992, p. 16). Put differently, a *habitus* is a set of dispositions which scholars embody through education, socialization, and the experience of operating in a field. In turn, these dispositions guide scholars' perceptions, appreciations, and overall actions.

The contest in a field of study involves constituting a position which makes a scholar more or less identifiable or distinguishable. The value of this position is thus determined in its comparison to other positions. According to this view, a field functions as a symbolic market on which positions ("goods") are traded (Bourdieu, 1991a). This study focuses on the way scholars in a subfield produce knowledge while engaging in this trade; that is, by offering their positions in the form of textual products (texts) on a field's linguistic market with the aim of acquiring economic, cultural, social, and symbolic capital. Hence, textwork enables scholars to establish valued "relationships" with their positions (with "themselves") and with the positions of

others. In turn, scholars become more or less committed to ("invested in") sustaining or improving this position (this "identity") vis-à-vis those of others.[1]

I build this argument by translating and extending Bourdieu's work. Bourdieu has constructed an extensive body of work which many scholars have discussed, debated, and appropriated (e.g., see Brubaker, 1993; Calhoun, 1993; Calhoun, LiPuma, & Postone, 1993; Dumont & Evens, 1999; Everett, 2002; King, 2000; Sweetman, 2003).[2] In the following discussion, I first discuss the general workings of a field (academic, political, artistic, etc.), because the notion of field is the central lens through which I investigate the production of knowledge. Important concepts pertaining to the idea of field are: agent, *habitus*, *doxa*, forms of capital, and *illusio*. Agents (or acting individuals) and a field (the enactment of relationships between historical positions) are related to each other in fundamental ways through a field's *habitus* (its system of dispositions established based on a particular *doxa* or set of presuppositions). In addition, the field changes (or remains the same) due to agent's investment in and commitment to (*illusio*) struggles for different forms of capital (cultural, economic, social, symbolic).

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[1] In accordance with these notions, this dissertation is a textualization through which "I," the author, position myself in a field of study.

[2] Bourdieu's work has received only modest attention in communication studies (Conrad, 2000). Currently, a few scholars are promoting the merit of his ideas in this field. For instance, a conference panel, titled "Communicating with Pierre Bourdieu: Discovering New Applications for Old Theories," was held at the 2004 meeting of the *International Communication Association*. In addition, James Aune (2000) recently presented a paper, titled "The Scholastic Fallacy, Habitus, and Symbolic Violence: How Bourdieu Might Help Us Study Hegemony," at the 2000 meeting of the *National Communication Association*.

After the translation of these principal concepts and arguments, I explicate how agents (scholars and non-scholars alike) generally position themselves in a field. Here, I rely on Bourdieu's conception of identity as a "biographical illusion," but also extend his writings, because his work explains insufficiently how agents' create positions that identify or distinguish them from others in a field. Next, I look at the way these concepts and arguments can be applied to an academic field of study. Of particular importance, in this case, are the ways scholarly agents enact antinomic language-games to produce knowledge, yet also to position themselves. In turn, I specify these games of language in light of scholarly textwork and formulate two research questions for this investigation.

The Workings of a Field

Field

When studying human beings, Bourdieu (see Bourdieu & Wacquant, 1992) argues against disrobing individuals of their sociality: Agents (those who act and thus produce effects of all kinds[3]) practice life in relation to each other and are inherently social. Accordingly, they operate by positioning themselves in fields (e.g., the field of social classes, politics, or art). Fields can be seen as social spaces constituted as

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[3] Through his concentration on practice, Bourdieu's work has been associated with the pragmatist work of John Dewey (1930a/b). Alluding to Dewey, for example, Bourdieu (2000a) argues against the division between theory and practice by claiming that "appropriate practice (speaking a language or riding a bicycle, for example) is knowledge" (p. 80).

networks or configurations "of social relations, structured systems of social positions

within which struggles or maneuvers take place over resources, stakes, and access

(Bourdieu, 1990a)" (Everett, 2002, p. 60). These

> positions are objectively defined, in their existence and in the determinations
> they impose upon their occupants, agents or institutions, by their present and
> potential situation (*situ*) in the structure of the distribution of species of power
> (or capital) whose possession commands access to specific profits that are at
> stake in the field, as well as by their objective relation to other positions
> (domination, subordination, homology, etc.). (Bourdieu & Wacquant, 1992, p.
> 97)

A field has several characteristics. As suggested, a field has an "objectivity"—an

existence beyond the immediate consciousness and will of a single agent—which is

constituted by the distribution of material "resources and means of appropriation of

socially scarce goods and values" (Bourdieu & Wacquant, 1992, p. 7).[4] For example, a

university's existence depends on its number of notable professors, its reputation vis-à-

vis other universities, the sophistication and quality of its infrastructure (libraries,

computer networks, etc.), and so forth.

The objectivity of a field is also constituted by the agents who operate in its

space. A field is characterized by a *habitus* or a historically developed system of

dispositions. Agents embody this system through the field's practices of education,

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[4] Here, "objectivity" refers to the idea that a field's structures have developed over time;
hence, agents are not constantly inventing these structures "on the spot," but they are
influenced by these structures in their actions. In turn, though, these actions also affect
(modify, sustain, etc.) these structures. Thus, agent and field exist in a relation of
"ontological complicity" (Bourdieu & Wacquant, 1992, p. 20).

socialization, and through the experience of operating in a field (I will discuss the idea

of *habitus* in more depth in a moment). Consequently, a field can be seen as a space

which agents define by their actions or forces, much like a magnetic field or prism

which "refracts external forces according to its internal structures" (p. 17). Effects

produced within a field are "neither the purely additive sum of anarchical actions, nor

the integrated outcome of a concerted plan;" rather, it "is the *structure* of the game, and

not the simple effect of mechanical *aggregation*, which is at the basis of the

transcendence…of the objective and collective effect of cumulated actions" (p. 17).[5]

Thus, agents have a practical sense for how to operate in a field without necessarily

acting in conscious or intentional ways. Moreover, a field produces social effects that

transcend the mere sum of agents' individual actions.

Second, a field is a space of contest—as an analogy, the reader might imagine a

literal field like a Roman amphitheater—because agents struggle to acquire different

forms of capital (as I will explicate further in a moment).[6] This struggle is enforced

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[5] Correspondingly, "[a]s a game structured in a loose and weakly formalized fashion, a field is not an *apparatus* obeying the quasi-mechanical logic of a *discipline* capable of converting all action into mere *execution* (Bourdieu, 1990b, p. 88)" (Bourdieu & Wacquant, 1992, p. 102).

[6] Bourdieu explains that the "social world can be conceived as a multidimensional space that can be constructed empirically by discovering the main factors of differentiation which account for the differences observed in a given social universe, or, in other words, by discovering the powers or *forms of capital* which are or can become efficient…in this particular universe, that is, in the struggle (or competition) for the appropriation of scarce goods of which this universe is the site. It follows that the structure of this space is given by the distribution of various forms of capital, that is, by the distribution of the properties which are active within the universe under study—those properties capable of conferring strength, power and consequently profit

symbolically through the constitution of linguistic distinctions between valued

positions (e.g., a faculty position at an Ivy League university might be considered more

valuable than a faculty position at an unknown community college). In this contest for

capital, the division between positions (and the structures of a field) are at stake. Thus,

agents can try to change the distribution and relative weight of different forms of

capital in order to improve the comparative value of their position in a field (Bourdieu

& Wacquant, 1992, p. 18).

Third, a field exists on different levels of aggregation (Bourdieu & Wacquant,

1992). For example, the field of academia comprises fields of study (e.g.,

communication studies or astronomy); subfields within fields of study (e.g.,

organizational communication studies, interpersonal communication studies, health

communication studies, rhetorical studies); and specific universities, each with their

own colleges and departments. The borders between these fields are relatively

indistinct, even though agents enforce, uphold, lower, or heighten boundaries between

particular social spaces through their practices.[7] Thus, such boundaries are constituted

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on their holder ([Bourdieu,] 1987f: pp. 3-4)" (Calhoun, 1993, p. 69).

[7] Especially in recent publications (see Bourdieu, 1998a, 1998b, 2001), Bourdieu has
written about the interaction between fields (academic and non-academic). Bourdieu
admits that "the interrelation of different fields is an extremely complex one…I
believe…that there are *no transhistoric laws of the relations between fields*, that we must
investigate each historic case separately…relations between fields…are not defined
once and for all, even in the most general tendencies of their evolution. The notion of a
field does not provide ready-made answers to all possible queries, in the manner of the
grand concepts of 'theoreticist theory' which claims to explain everything and in the
right order. Rather, its major virtue, at least in my eyes, is that it promotes a mode of
construction that has to be rethought anew every time. It forces us to *raise* questions:

in language, but they have practical consequences beyond the linguistic. In turn, forces

produced in a field affect other fields. However, fields differ in their degree of

autonomy from other fields. That is, the higher a field's autonomy, the more its beliefs,

values, practices, conventions, and stakes are distinct from other fields. Perhaps, the

best example of a relatively autonomous field, in this regard, is the field of academia

itself, because agents in this social space act more or less independently from those who

operate in the political, artistic, or economic field (Bourdieu, 2000a).

Moreover, due to the permeability of fields and their imbricated nature, agents

generally constitute different positions in different fields and subfields. For instance, an

agent might constitute a professor position in the academic field as well as a position in

the field of psychology and the subfield of organizational psychology.

Habitus **and** *Doxa*

To overcome dichotomous thinking (e.g., agent/field, subject/object, or

individual/social), Bourdieu employs the notion of *habitus* or system of dispositions

(tendencies, propensities, or inclinations). Any field is characterized by such a "system

of classification" which consists of "historical relations 'deposited' within individual

bodies in the form of mental and bodily schemata that function as *symbolic* templates for

the practical activities—conduct, thoughts, feelings, and judgments—of social agents"

(Bourdieu & Wacquant, 1992, p. 16). A *habitus* comprises "postulates and axioms, or

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about the limits of the universe under investigation, how it is 'articulated,' to what
degree, etc." (Bourdieu & Wacquant, 1992, pp. 109-110).

even more fundamentally, the binary oppositions, labels, and categories we use to

understand…the world [and which disposition and generate our actions]" (Everett,

2002, p. 66). In turn, these postulates, axioms, binary oppositions, labels, and categories

define ideas about "common sense" in a field, that is, its *doxa* or "set of inseparably

cognitive and evaluative presuppositions whose acceptance is implied in membership"

(Bourdieu, 2000a, p. 100).[8]

Hence, *doxa* constitutes *habitus* constitute each other through the actions of a

field's agents: Through education, socialization, and the experience of operating in a

field, agents embody a *habitus* (and thus also a *doxa*), which shapes their perception,

appreciation, and action; reflects a field's historically formed relationships between

positions; and reproduces itself through behavior. Stereotypically, for instance, an

aristocrat may "know" how to compose himself or herself at a tea-party according to

the etiquette learned through upbringing and boarding school; a soccer player may

have a "sense" for the game of soccer; and an anthropologist may "know" how to

interpret field notes based on academic education, socialization, and experience. Each

of these agents reinforces *habitus* of a field by acting according to a particular *doxa*.

Therefore, they have all developed a "feel for the game" (Bourdieu, 1990a, p. 66).

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[8] There are similarities between Kuhn's (1970) notion of paradigm and Bourdieu's
concept of *habitus*: both imply that a field of study is propelled by a more or less
common *doxa* or "disciplinary matrix" (Kuhn, 1970, p. 182); that is, a set of beliefs,
values, and techniques which is shared by a scientific community. Bourdieu's
conceptualization is more informative than Kuhn's, I believe, because the notion of
habitus helps to explain how scholars embody the practice of a field and it also situates
this practice in historical relationships between positions to overcome subject/object,
agency/structure, individual/collective antinomies.

The notion of *habitus* does not rob agents from their individuality or their

agency. Indeed, agents constitute their uniqueness by giving relatively distinct

expressions to a *habitus* and enacting different positions. It is thus possible to speak of

"someone's *habitus*" and agents are

> most able to operate effectively [in that they can "be themselves"], when there is
> a clear affinity between their dispositional conduct and their position within the
> field. Different forms of habitus have different 'values' in different fields, and
> individuals have strong attachments—or 'interests' in—particular positions
> within particular fields. Place someone in a different position within the field,
> or in a different field altogether, and they will behave differently—and will be
> more or less comfortable or ill at ease. (Sweetman, 2003, p. 533)

Correspondingly, there generally is a relative "homology" or likeness in structure due

to a common history between the space of positions (field), which agents enact, and the

space of dispositions, which provides opportunities for acting in particular ways. This

homology is illustrated by the elegance which David Beckham displays when he

outperforms opponents on the soccer field. However, at times agents might also be

"out on a limb" due to the discordance between their *habitus* and the field in which they

are acting; that is, between the position they enact and their dispositions (Bourdieu,

2000a, p. 157). For example, one can think of the displacement experienced by a lay

person who tries to make sense of presentations at an academic conference on

mathematics. The "unpreparedness," mismatch, discordance, or "misfiring" of an

agent's *habitus* therefore becomes most obvious when he or she encounters uncertain situations (e.g., unfamiliar situations, crises, etc.) (Bourdieu, 2000a).[9]

Countering the critique that the *habitus* functions as an unchangeable "deterministic schema," Bourdieu claims that the *habitus* really is a system of *dispositions*; that is, of propensities, virtualities, potentialities, and eventualities. This system only "reveals itself in reference to a definite situation" and it is "*in relation to certain structures that habitus produces...discourses and practices*" (Bourdieu & Wacquant, 1992, p. 135).[10] Consequently, the

> notion of habitus accounts for the fact that social agents are neither particles of matter determined by external causes, nor little monads guided solely by inner reasons, executing a sort of perfectly rational internal program of action. Social agents are *products of history*, of the history of the whole social field and of the accumulated experience of a path within [this and other fields]...To put it differently, social agents will *actively* determine, on the basis of these socially and historically constituted categories of perception and appreciation, the situation that determines them. (Bourdieu & Wacquant, 1992, p. 136)

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[9] Bourdieu (2000a) notes that the dialectic between positions and dispositions is most evident in "young" fields, where positions are still ill-defined. Because these volatile posts "leave their occupants the possibility of defining them by bringing in the embodied necessity which is constitutive of their habitus, their future depends on what is made of them by their occupants, or at least those of them who, in the struggles within the 'profession' and confrontations with neighbouring and rival professions, manage to impose the definition of the profession most favorable to what they are" (p. 158).

[10] Bourdieu (2000a) reasserts that the notion of *habitus* is often used in ways he did not intend; that is, "as a kind of *monolithic* principle…; as an *immutable principle*; as *inexorable* (conferring on the past the power to determine *all* future actions); and *exclusive* (leaving no room in any circumstances for conscious intention)" (p. 64).

Bourdieu's conception, influenced by Maurice Merleau-Ponty's work, indicates

a relation of "ontological complicity" (Bourdieu & Wacquant, 1992, p. 20) between

agent and field that is linked by *habitus*. This view dodges the dichotomies between

subject/object, agent/field, individual/social: Agents are in the world through their

body and the body embodies the world in agents (Bourdieu, 1990a; Bourdieu &

Wacquant, 1992). In addition, this complicity implies that the *habitus* does not function

as a straitjacket which predicts every thought and move. Instead, Bourdieu's idea of

habitus necessitates improvisation and invention vis-à-vis the *doxa* or "conventions" of a

field. Bourdieu's perspective therefore centers on practice in context without assuming

that agents act as willfully or rationally as Jürgen Habermas (1984) suggests.[11]

Bourdieu's stance suggests that individuals frequently act dispositionally or

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[11] Moreover, Bourdieu's view is relatively different from Anthony Giddens' (1984, 1991) position. Agents may continuously "[monitor] the ongoing flow of [their] social life," as Giddens (1984, p. 3) argues, but this kind of reflexivity is not the same as Bourdieu's idea of reflexivity, implying the monitoring through which agents try to understand the effects of their positioning practices in a given social space. As my study will illustrate, agents are not always able to "elaborate discursively upon [the reasons for their activities]" (p. 3), even if asked to do so. They may even find it difficult to elaborate on the practices in which they engage and to reflect on their reasons for engagement. Furthermore, according to Loïc Wacquant, "[i]t is erroneous to include Bourdieu among the proponents of 'structuration theory'…Structuration theory, as its progenitor emphasizes (Giddens, 1990a, p. 310), is centrally concerned with issues of social ontology and conceptualization; the impetus behind Bourdieu's theoretical moves has always been a desire to grapple with new empirical objects, and he has evidenced little interest in refining a conceptual scheme. Moreover, Bourdieu's theory of practice predates Giddens' (1979, 1984) theory of structuration by at least a decade, and is rooted in a different set of philosophical questions (though recently Giddens [1986a] has fastened on the opposition of objectivism and subjectivism that forms the epicenter of Bourdieu's project)" (see Bourdieu & Wacquant, 1992, p. 3).

"habitually" within fields that yield and valorize particular dispositions (Bourdieu &

Wacquant, 1992).

Finally, Bourdieu (1991a) argues that the system of dispositions which make the

practice of language possible (i.e., the "linguistic *habitus*," which is one particular aspect

of a field's overall *habitus*) plays an important role in a field's operations, because it

affects agents' constructions of meaning and sustains or modifies a field's linguistic

market.[12] Thus, an agent's linguistic *habitus* refers to his or her

> propensity to speak and to say determinate things (the expressive interest)
> and…capacity to speak, which involves both the linguistic capacity to generate
> an infinite number of grammatically correct discourses, and the social capacity
> to use this competence adequately in a determinate situation. (Bourdieu, 1991a,
> p. 37)

Also through its linguistic *habitus*, then, a field influences the way agents act

(i.e., speak, write, listen, read) by embodying a system of dispositions through more or

less (depending on the field's autonomy) institutionalized practices of education and

socialization. For instance, a beginning graduate student may need to learn a new

"language" to operate legitimately in a field of study, while a physics scholar might face

the same challenge when wanting to publish in the field of cultural studies.

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[12] Bourdieu does not assume that all is language: Although the significance of language
cannot be denied, "one cannot fully understand language without placing linguistic
practices within the full universe of compossible practices: eating and drinking habits,
cultural consumptions, taste in a matter of arts, sports, dress, furniture, politics, etc. For
it is the whole class habitus, that is the synchronic and diachronic position occupied in
the social structure, that expresses itself through the linguistic habitus which is but one
of its dimensions" (Bourdieu & Wacquant, 1992, p. 149).

Forms of Capital, *Illusio*, and Language-Games

An important merit of Bourdieu's work lies in its demonstration of how human

beings are involved or implicated in the workings of a field, as if they were "caught up"

in a game (Bourdieu, 1977, 1988, 1990a/b, 2000a). This involvement results from

agents' *illusio*[13] or investment in a field through their *habitus* as well as through their

enactment of a position which provides them with forms of capital. "Capital" refers to

the value of socially scarce goods and values which an agent accumulates. These

accumulations can be classified into different forms. "Economic capital" is constituted

by "monetary and material wealth, commodities, and physical resources (e.g., land)"

(Everett, 2002); "cultural (or 'informational') capital" consists of educational credentials

or qualifications, skills, expertise, and "taste;" "social capital" consists of the resources

that link an agent into a network of other agents and provide him or her with group

membership (often colloquially referred to as "having connections"); and "symbolic

capital" involves recognition, status, prestige, and personal authority.

All these forms of capital are highly correlated, although this correlation does

not imply an association that necessarily is causal. For example, someone with a

doctorate degree from Cambridge University (cultural capital) does not receive by

definition a high-income position in an organization (economic capital). Further, the

first forms of capital (economic, cultural, and social) each become a form of symbolic

capital when agents in a given social space recognize these forms of capital as legitimate

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[13] *Illusio*—from *ludus*, "game"—refers to the "fundamental belief in the interest of the
game [of a field] and the value of the stakes which is inherent in its membership"

forms. More precisely, actually, this recognition implies a *misrecognition* of the fact that

this valorization is an arbitrary affair which is highly dependent on the social

conditions of a field (Everett, 2002). Consequently, as people traverse fields, the value

of their capital changes (e.g., a doctorate in English literature from Oxford University

may be invaluable in a particular academic field of study, but worthless in the field of

fashion designers).

Furthermore, Bourdieu defines specialized kinds of capital under the four

mentioned forms. Especially important for this study is the concept of "linguistic

capital," which is a subset of cultural capital, and refers to "the ability to demonstrate

competence in the use of magisterial, scholarly, or bourgeois language, in one's ability

to decipher and manipulate the complex structures of that language" (Everett, 2002, p.

63). Thus, someone with a fair amount of linguistic capital has the ability to speak from

a particular position in a given social space and to be heard. Accordingly, someone's

linguistic capital is highly correlated with someone's social and symbolic capital; that is,

someone's ability to speak and be heard depends for a large extent on his or her

membership to particular groups and his or her credibility.

In sum, agents who are related to each other through a field's structure of

objective relations engage in everyday life by struggling to secure and increase their

forms of capital. As Bourdieu notes, acting in a field is therefore *like* playing a serious

"game:"

(Bourdieu, 2000a, p. 11).

> We can indeed, with caution, compare a field to a game (*jeu*) although, unlike
> the latter, a field is not the product of a deliberate act of creation, and it follows
> rules or, better, regularities, that are not explicit and codified. Thus we have
> *stakes* (*enjeux*) which are for the most part the product of the competition
> between players. We have an *investment in the game, illusio…*: players are taken
> in by the game, they oppose one another, sometimes with ferocity, only to the
> extent that they concur in their belief (*doxa*) in the game and its stakes; they
> grant these a recognition that escapes questioning. (Bourdieu & Wacquant,
> 1992, p. 98)

Agents do not engage in these games, because they have "signed a contract:" In the

playing of the game, agents can become increasingly convinced about its worth

(implying an *illusio*); that engaging in the game is "worth the candle." This *collusion*,

Bourdieu argues, is the very basis of a field's competition (Bourdieu & Wacquant, 1992,

p. 98).

In his conception of games, Bourdieu is highly influenced by Wittgenstein's

(2001) notion of language-games. Like Wittgenstein, Bourdieu argues that a field works

according to particular "rules" which are experienced by participating in the games

(games of language and others) of a field. Like the mastery of a language, the

"mastery" of a field's game depends on agents' dispositions (linguistic and other).

However, Bourdieu (1991a, 2000a) extends Wittgenstein's work by suggesting that a

field's linguistic *habitus* disposes agents to inflict "symbolic violence" on themselves

and others by constituting meanings that create hierarchical relations in the distribution

of forms of capital. This violence creates power differentials in a field and implies a

> coercion which is set up only through the consent that the dominated cannot fail
> to give to the dominator (an therefore to the domination) when their
> understanding of the situation and relation can only use instruments of
> knowledge that they have in common with the dominator, which, being merely

the incorporated form of the structure of relation of domination, make this relation appear as natural; or, in other words, when the schemes they implement in order to perceive and evaluate themselves or to perceive and evaluate the dominators (high/low, male/female, white/black, etc.) are the product of the incorporation of the (thus naturalized) classifications of which their social being is a product. (Bourdieu, 2000a, p. 170)[14]

Thus, Bourdieu argues that power is not simply "forced" or "reasoned," but part of a field's *habitus*. This power finds its expression in language, yet also censors linguistic expression. In so doing, the *habitus* controls how agents in a field make sense of the world by reinforcing classificatory systems that privilege some "things" (people, issues, etc.) while marginalizing others.

Because people are educated and socialized to invest in a field's game through its *habitus* and to believe in its legitimate existence through accepting the field's *doxa*, it is not easy for agents to pause and reflect on the game. That is, due to their "feel for the game" (Bourdieu, 1990a, p. 66) and commitment to its continuation, they do not only take the game for granted, but often also prevent scrutiny or alteration of a field's *doxa*. In this sense, "every established order tends to produce (to very different degrees and with very different means) the naturalization of its own arbitrariness" (Bourdieu, 1977, p. 164). In other words, many fields (political, social, etc.) suppress the questioning of the arbitrariness of their existence, so that agents cannot "[give] the game away"

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[14] This definition resembles Dennis Mumby's (2001) conception of "power" as the "production and reproduction of, resistance to, or transformation of relatively fixed (sedimented) structures of communication and meaning that support the interests (symbolic, political, economic) of some organizational members [or agents in a field] over others" (p. 587).

(Bourdieu, 2000a, p. 5). This is especially true for the field of academia, which has managed to "break away" from the other fields.

This autonomy is a blessing and a bother, as I will discuss shortly. Before discussing the workings of the academic field in more depth, however, I point to an important limitation in Bourdieu's writings which presents an opportunity for extending his work.

An Extension of Bourdieu's Work

Bourdieu's writings do not explicate in very much detail how agents position themselves in a field through particular practices. Positions are seen as symbolically constituted "locations" or "posts" in a given social space which have acquired a more or less objective character throughout the history of a field. For example, by acting as professors, siblings, critical theorists, or artistic painters, agents constitute different positions in various fields. Each of these positions is more or less homologous to a system of dispositions. For example, a professor position is linked to the academic *habitus* (and, more specifically, to the *habitus* of a particular field of study). Hence, an agent who enacts this position "knows," more or less, how to act *as* a professor. In turn, forms of capital are linked to this position, making its enactment worth pursuing.

Bourdieu insufficiently explicates and studies how agents engage in, experience, and reflect on these positioning practices. Hence, I conceptualize these practices as activities through which agents make themselves identifiable in a field. That is, agents relate their "selves" to various positions (and thus to other agents who do the same)

and express these relationships through language. Thereby, they identify these

positions (these "identities") and make them more or less distinguishable from others.

This distinction is reinforced by a field's *habitus* which supports the idea that this

distinguishing characterizes the game for capital. I focus in particular on the linguistic

practices implied in a field's language-games which enable this positioning, but do not

deny that someone's identity is also shaped by other practices (e.g., the way one

"carries" oneself) and relatively fixed characteristics (e.g., one's biological appearance).

With this emphasis on language and identity, I explicate the positioning practices of

agents in the next section. Soon thereafter, I will use this discussion to explicate how

scholars enact an identifiable position in an academic field of study.

Positioning Oneself in a Field

Following the trail of George Herbert Mead (1934), I argue that agents in a field

constitute identifiable selves by interacting "with themselves" and others in a field.[15]

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[15] Mead (1934) critiqued the Enlightenment notion of a transcendental, essential self
(i.e., someone's reflexive objectification of his or her presence in the world
[Perinbanayagam, 2000]) by arguing that a self is constituted through the symbolic
interaction between individual and society (Holstein & Gubrium, 2000; see also Hall,
1992). Mead argued that a self is not constituted as a fixed essence inside an agent, but
through the *activity* of constituting an identifiable self or "identity" in a particular
context. Thus, Mead propelled the idea of a social self and "located" the self at the
intersection between conversation as introspection and conversation with others. Only
through sufficient experience with such interaction, Mead suggested, will someone be
able to become conscious of his or her self: "Consciousness, in effect, is always
consciousness *of something*, including consciousness of oneself as meaningfully
something (some thing) or another" (Holstein & Gubrium, 2000, p. 29). Mead was one
of the earliest scholars who emphasized the importance of language practices for the
constitution of identity, arguing that language allowed people to sustain an "I," a

Language plays an important role in this practice, because it allows agents to constitute "personalities" in a particular biography (Bourdieu, 2000b). A "personality" refers to a "biological individuality socially instituted by nomination and bearer of properties and powers which assure him (in some cases) a *social surface*, namely, the capacity to exist as an agent in different fields" (p. 303). A proper name alone (e.g., "Chris Martin"), then, only designates the same biological individuality in different fields (as in: "Oh, there is the individual body referred to as 'Chris Martin' again"). That is, a name defines a biological individual as an "empirical" individual and provides almost no information about the person indicated:

> As a label capable of being arbitrarily applied to any object, it says that the object designated is different, without specifying *in what respect* it differs; as an instrument of *recognition*, and not of *cognition*, it singles out an *empirical individual*, generally apprehended as singular, that is to say, different, but without analysing the difference. (Bourdieu, 1988, p. 22)

Through his actions in different fields vis-à-vis others, though, Martin can constitute himself (and can be constituted by others) as an "epistemic individual." His

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conscious individual or "subject," and a "me," the object of this subject's consciousness (illustrated by a sentence like: "I think about myself"). Hence, the "I" and "me" were thought to function practically by mutually constituting each other. Furthermore, Mead complicated the idea of a singular self, seeing it as a composite of multiple selves, which "emanate" from acting in different contexts, even though they are "held together" by means of one proper name (as Bourdieu [2000b] contends). Mead therefore compared the practice of self to participating in a game. That is, the constitution of a self in light of others elicits a particular performance of self through which others come to know someone "as himself or herself." Hence, someone's constitution of multiple selves always is affected by the social values, beliefs, and so forth, of the people with which someone interacts.

actions connect the name "Chris Martin" to a web of images which an interpreter (Martin himself or someone else) can construct from a point of view within a field.[16] In this way, Martin identifies a position in a network or configuration of positioned others (e.g., his actions might identify the position of a pop singer, father, husband to Gwyneth Paltrow, Englishman, and Fair Trade activist). The interpreter constructs this character (this idea of who Martin is) based on his or her understanding of Martin's position in relation to others; his or her understanding of the networks between positions; and the interpreter's own social trajectory.

Similarly, in a field of study, the name "Pierre Bourdieu" is connected (for example through reading his texts) to an image which the reader constitutes from a particular point of view. Bourdieu's texts thus identify his position in a network of other scholars which the reader constructs based on his or her understanding of Bourdieu's relation to others; his or her understanding of this network; and his or her own development influenced by education, training, and so forth. Consequently, when the epistemic individual, Bourdieu, "speaks" in someone's text, one hears "Bourdieu's" voice, assembled, filtered, and constructed through the reader's perceptions, appreciations, and contemplations. This reading therefore depends in part on the reader's *habitus*.

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[16] I use the empirical/epistemic distinction as a heuristic to illustrate the idea that someone's individuality is not defined just empirically. In the reality of practice, this antinomy is, of course, non-existent.

The social practice of linguistically constituting a personality in a field enables agents to identify and be identified by others as selves—as "themselves"—and provides them with a relatively constant "identity" in light of the positions enacted in that field. These positions, then, allow agents to constitute linguistic relationships "with themselves"[17] (Rose, 1996). At the same time, however, these positions allow them to relate their selves to others who locate themselves in the same social space. Because identity "emanates" from agents' actions in a field, Bourdieu (2000b) argues that identities are a "biographical illusions" of language. That is, language can never fully designate the "composite and disparate rhapsody of biological and social properties undergoing constant flux" (p. 300), even though it allows someone to identify himself or herself (and be identified by others) as a relatively stable, unified person.[18] Hence, an agent uses one proper name to denote the plurality of acting agents in the language-games of various fields[19] and agents may constitute different relationships with themselves and others in different moments and different fields.

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[17] Expressions which illustrate this relating are the Greek maxim "know thyself," but also colloquialisms like "I am not myself today," "I want to be with myself," "I, myself, am an anthropologist," "I am not good at this or that," or "I talk to myself."

[18] Jorge Luis Borges (1999) makes a similar argument in his brilliant essay, "The Nothingness of Personality."

[19] Jean-François Lyotard (1984) adopts this view on the practice of identity by appropriating Wittgenstein's (2001) ideas.

Although Giddens (1991) and others suggest that the practice of enacting a self can be seen as a reflexive *project*[20] (or, as Goffman [1959] claims, a staged performance[21]), I maintain that the practice of constituting an identifiable position in a field is highly influenced by the field's *habitus*. Agents learn to constitute their identities in certain ways based on their appropriation of different fields' systems of dispositions during their personal trajectories in fields. This implies that the *doxa* of a field, reinforced through this field's language-games, influences the way someone relates to himself or herself and to others. For instance, someone may learn to see himself of herself as an econometrist by operating in the academic field of econometrics. Consequently, he or she might act in the way the field's *habitus* dispositions the actions of agents who take position in this field ("do as econometrists do"). It is certainly possible (although not necessary) that the homology between an agent's position and his or her dispositions is more optimal for those who have operated in a field for considerable time and with considerable intensity, creating an impression of "being in

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[20] According to Giddens (1991), identities are reflexive, social "projects" whose success—and power—depends on people's ability to uphold self-narratives and build consistent feelings of biographical continuity. A similar position is taken by Kenneth Gergen (1994; see also Gergen & Gergen, 1986).

[21] Although Mead acknowledged the contexts in which identifiable selves are constituted, Irving Goffman (1959, 1986) concentrated in more depth on the influence of social settings through his "dramaturgical" approach to the study of human behavior. Goffman developed the idea that people "stage" their identity by enacting "scripts" in the form of "roles" (resembling the positions or posts of which Bourdieu speaks). Most notable, here, was the idea that people vary these performances when they "enter" and "depart" the stage, suggesting for example that someone who attends a formal party would conduct himself or herself differently while at the party ("on-stage") than at home ("off-stage").

the right field" or "doing what I was supposed to do." Therefore, the constitution of a

biographical illusion in a field is not so much a project or carefully staged play, but

more so a dispositional practice entrenched in the history of a field.

Moreover, agents become attached to and invested in the enactment of their

identifiable positions in fields, because these positions provide them with relatively

scarce forms of capital. The relationships which agents develop with themselves and

others in light of their positions thus enable agents to uphold different kinds of "self-

worth;" for example, economic self-worth, depending on the degree to which an agent

is constituted as a person whose activities are valued financially (economic capital);

cultural self-worth, depending on the degree to which an agent is constituted as a

learned person, expert, or intellectual (cultural capital); social self-worth, depending on

the degree to which an agent is constituted as a member of important groups (social

capital); linguistic self-worth, depending on the degree to which an agent is constituted

as a person who can say important things in the correct way (linguistic capital); and

symbolic self-worth—encompassing the previous forms of capital—, depending on the

degree to which an agent is constituted as a credible player in a field. These forms of

capital derive their value from the fact that they are perceived to be scarce; for example,

only a relatively select number of people are able to obtain a degree from a reputable

university (cultural capital), are able to publish in the most prestigious academic

journals (linguistic capital), and so forth.

In turn, a field's language-games are propelled by trying to distinguish one's

biographical illusion in ways that enable the acquisition of these forms of capital, yet

without becoming so distinct that one is no longer identified with the field. This struggle is not simply individualistic, rationalistic, or utilitarian, because the field's *habitus* reinforces these language-games. Thus, because agents are implicated in their field in terms of who they are, their "personal" reasons to defend or rebel against (change, etc.) the field are more or less reinforced by the field itself.

After having discussed the practice of constituting an identifiable position in a field in general, I now turn to a specific discussion of the field of academia, because this field has distinct characteristics and forms the context for this study. This discussion sets the stage for explicating how scholars position themselves in a field of study through textwork.

The Antinomic Language-Games of Academia

In *Pascalian Meditations*, Bourdieu (2000a) reflects on *skholè*, "the free time" implied in the scholarly condition; that is, the institutionalized condition of being "freed from the urgencies of the world" (p. 1). Bourdieu indicates that the field of academia has acquired relative autonomy vis-à-vis other fields, such as the field of politics or economics, each entailing different language-games—and thus a different logic of practice, *doxa*, and *illusio*. The autonomy of academia is reflected in the "distanced" disposition of individual scholars, which places them in the difficult position. That is, "the *fundamental ambiguity* of the scholastic universes and of all their productions—universal acquisitions made accessible by an exclusive privilege—lies in

the fact that their apartness from the world of production is both a liberatory break and a disconnection, a potentially crippling separation" (Bourdieu, 2000a, p. 15).

Having historically broken with the economic, political, and material urgencies of the world, the academic field was able to develop and arbiter the "rules" of its language-games, leading scholars to struggle for cultural, social, linguistic, and symbolic capital in particular. In these games, the way a scholar's enacts an academic perspective on a particular object of knowledge identifies his or her position in a field. Accordingly, "perspectivity" offers a way to capital and constitutes academia's main "currency." In other words, in the academic universe, agents might "feel entitled to perceive the world as a representation, a spectacle, to survey it from above and from afar and organize it as a whole designed for knowledge alone" (Bourdieu, 2000a, p. 21). This "spectator's theory of knowledge" (p. 51, citing Dewey) increasingly has led scholars to disregard their distancing activities and their implicatedness in a field's sociohistorical conditions. Even more problematic is that many scholars have learned to impose their perspectivity onto others by projecting their academic dispositions into those investigated and by presuming that these "subjects" operate similarly and under similar conditions (p. 53). Bourdieu intends to expose this "scholastic fallacy" by propagating the practice of a particular kind of reflexivity.

In line with the scholarly "break," different scholarly fields of study (and subfields of study), ranging from physics to linguistics, have developed in the field of academia, each being "the institutionalization of a point of view in things and in habitus" (Bourdieu, 2000a, p. 99). Historically, each of these fields has been engaged in

struggles to legitimate their existence and vis-à-vis other fields of inquiry. This implies

that, depending on the length and intensity of these struggles, a field educates and

socializes agents to alter their dispositions towards the world and to accept the

plausibility of a particular *doxa*. A field's language-games constitute the cognitive and

evaluative presuppositions (implied in a *doxa*) as "antinomies;" that is, elemental and

apparently irresolvable conflicts or contradictions between positions. In turn, these

opposing positions characterize a field's *habitus* in the form of subject/object,

intention/cause, agency/structure, materiality/symbolic representation dichotomies

(Bourdieu & Wacquant, 1992), research approaches (e.g., social physics/social

phenomenology), schools of thought or "isms" (e.g., structuralism/ poststructuralism,

modernism/postmodernism), and so forth (Bourdieu, 2000a).[22]

Antinomic language-games define the spirit of any field of study (and the field

of academia at-large), because they invoke and legitimate the game of scholarship by

giving it its game-like character; by giving its players a "feel for the game" (Bourdieu,

1990a, p. 66); and by sustaining its momentum without questioning its necessity. Put

differently, the creation of "controversy" through opposition is functional in that it

[22] According to Wacquant, "[t]he 'process-reduction' characteristic of European
language (according to Benjamin Lee Whorf), and the reinforcement it receives from
positivist philosophies of science, explain why 'we always feel impelled to make quite
senseless conceptual distinctions, like 'the individual *and* society,' which makes it seem
that 'the individual' and 'society' were two separate things, like tables and chairs, or
pots and pans' (Elias, 1978a, p. 113; also 1987, part 1). The common root of Bourdieu's
and Elias's stress on ordinary language as an obstacle to [human science] thinking
appears to be Cassirer, especially his analysis of 'The Influence of Language upon the
Development of Scientific Thought' (Cassirer [, 1942])" (see Bourdieu & Wacquant,
1992, p. 15).

makes players believe in the merit of the game; controversy helps to distinguish

scholars from each other as well as from those outside the field; and controversy helps

scholars to build social support by using these oppositions as pedagogical anchor

points in order to explain the workings of the field to newcomers (Bourdieu &

Wacquant, 1992). Thus, these antinomies,

> paradoxically, unite those whom they divide, since agents have to share a
> common acceptance of them to be able to fight over them, or through them, and
> so to produce position-takings which are immediately *recognized* as pertinent
> and meaningful by the very agents whom they oppose and who are opposed to
> them…The consecrated oppositions eventually appear as inscribed in the nature
> of things, even when the slightest critical examination, especially when armed
> with knowledge of the field (constructed as such) very often forces one to
> discover that each of the opposing positions has no content beyond its
> relationship with the antagonistic position of which it is sometimes just the
> rationalized inversion. (Bourdieu, 2000a, pp. 100-101)

Hence, questioning the *doxa* in which one is educated and socialized involves

questioning one's own position in a field, dispositions, biographical illusion, and

investment in the game. As a result, scholars do not easily reflect on (let alone, critique)

their *illusio*, their "immediate adherence to the necessity of a field" (Bourdieu, 2000a, p.

102).

The claim that scholars participate in a field's antinomic language-games to

acquire forms of capital through a relationship of ontological complicity has not been

investigated intensively. Consequently, I investigate this relationship through the

following research question[23]:

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[23] Although the organization of this text suggests otherwise, I mainly developed my

RQ1　How do scholars and a field of study constitute each other through the practice of language?

　　a　　What antinomic language-games characterize the enactment of this ontological complicity?

　　b　　How does the contest for capital play a role in the enactment of this ontological complicity?

　　c　　How does the enactment of this ontological complicity affect the production of knowledge in a field of study?

Besides investigating these questions, I examine how scholars in a particular subfield of study practice textwork, a particular kind of scholastic language practices enacted through language-games. A field's *habitus*, I argue, dispositionally links scholars' research practices (and textwork in particular) to their constitutions of an identifiable position in a field or subfield of study. Depending on the relative autonomy of the field, its *doxa* defines a set of linguistic conventions, which are more or less unique to the field and which define the "rules" for its language-games. Vis-à-vis these conventions, textwork enables scholars to produce textual "products" of relative value. This production, then, allows scholars to position themselves through their texts on a linguistic market, a "system of relations of force which determine the price of linguistic products" (Wacquant, 1989, p. 47).

In accordance with this market analogy, language is not merely an object of contemplation or a simple instrument; rather a field's language-games constitute the character and spirit of the field which only can be experienced by participating in these

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research questions while analyzing the "data" for this study (see chapter III). It was namely only during this process that James Aune pointed me to Bourdieu's work, which became the central lens for this investigation.

games.[24] Someone's text (and the voice implicated in his or her work) is evaluated vis-à-vis someone else's text (and voice) in the field, creating a linguistic contest between scholars to distinguish their positions from others to acquire forms of capital which are perceived to be relatively scarce by those who operate in the same field. Thus, the textwork-game implies that each scholar, regardless of their position in the hierarchical organization of a field, "competes" for limited textual space in journals or books that are more or less reputable. Through this competition, then, scholars gain symbolic capital. At the same time, increased symbolic capital correlates with the amount of social capital someone attains, because someone's reputation enables one to become part of scholarly networks in which colleagues provide each other with valuable publishing opportunities. Simultaneously, this increased symbolic and social capital (in addition to increased textwork experience) also influences a scholar's linguistic capital; that is, his or her "capacity to produce expressions *à propos*, *for* a particular market" (Bourdieu, 1991a, p. 18).

In sum, the possibilities of officially positioning oneself in a field of study through published texts are not endless. In fact, the value of "being published" (an interesting term, which suggests the link between a scholar's identity work and his or her textwork) results from the way this selectiveness is perceived and reinforced in a

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[24] For example, it is difficult for me to grasp (and to be persuaded by) the *illusio* that drives scholars in analytical philosophy, because I do not participate in this field's language-games.

field.[25] A field's *habitus* "monitors" a scholar, so that it is difficult for him or her to distinguish himself or herself too fundamentally from others. For example, scholars may find it challenging to express their views and be heard in a field, if they are too radical in their perspective taking. Similarly, they may experience difficulties competing in other fields where they do not "speak the language."

Consequently, Bourdieu (1991a) argues that scholars often practice a neutralized and euphemistic language to mitigate (and sometimes conceal) the extremity of their distinction and to establish a consensus with others through a "practical metalanguage" (p. 85). Through euphemization, then, scholars practice textwork by balancing their personal expressive interests against the linguistic conventions of the field which censor their expressions. For example, scholars may refrain from including "inappropriate" content (e.g., fictional data or not citing the "correct" canons); writing in an

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[25] Not everyone may experience the competition of a field's linguistic market in the same way. Some scholars, for example, might believe the market works unfairly, while others believe that in its fairness. Moreover, once scholars have acquired a certain amount of linguistic, social and symbolic capital, others (colleagues, department heads, deans) as well as scholars themselves expect a certain repetition of the successful activities through which these forms of capital were acquired. Derrida, for instance, has a very identifiable position (constructed through the interaction between Derrida and various fields and subfields of study) which implies that people expect his work to have a particular character, style, quality, and so forth. He established this position through his past work, yet he still is expected to uphold this position by producing work that meets the standards which enabled him to establish this position in the first place. In this sense, even Derrida engages in competition with others (although it is impossible to say with whom exactly) to sustain his position in different fields. Now, Derrida might not perceive this competition to be as stressful as a starting philosopher does, because Derrida has earned his stripes, so to speak. However, if Derrida would stop to publish the work for which he is known, it is likely that the communities which make the constitution of his position possible will express their discontent. Hence, Derrida's

"inappropriate" form (e.g., poetic, too personal) or structure (e.g., not according to a

particular format) to follow a field's conventions. Additionally, editors and reviewers

(who mostly work from a similar *doxa*) may reject or correct textual

"inappropriateness." Moreover, the censorship imposed by a field makes it virtually

impossible to distinguish the form of a text (how something is said) from its content

(what is said). That is, textwork conventions blend these two elements, leading to

expectations in a field that a particular content should be expressed in a particular way.

In turn, a scholar's perspective on a particular object of study and his or her form of

writing become elements which identify his or her position in a field in light of its

textwork conventions.

Besides the form and content of a text, the value of a text (and the censorship

imposed on it) also depends on an author's position in the field. For example, a

distinguished professor, who has acquired a considerable amount of symbolic, social,

and linguistic capital in a field, may have learned how to write "to" or "for" the field

and thus have the feeling that he or she can express himself or herself more "freely"

than an assistant professor, who is still in search of an identifiable voice. Nevertheless,

even a person with a relatively little capital may produce work which a field considers

to be valuable, innovative, and thought-provoking, in light of the corpus of literature

that constitutes and reconstitutes the field "body" of knowledge.

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identity as a scholar depends in part on (and emanates from) his relative success in
these competitions.

Although Bourdieu's writings provide a basis for studying how scholars position themselves in a field of study through textwork, Bourdieu does not explicate or study how these practices are enacted in a particular field of study. To conduct such a study, I conceptualize these practices with more specificity in the following section and, then, use this discussion to formulate my second research question.

Positioning Oneself in a Field of Study via Textwork

Laurel Richardson's (1994) writing suggests that textwork can be seen as a "method of inquiry" which enables scholars to better understand themselves, others, and their objects of knowledge. Accordingly, her work intimates that failing to problematize the role of authorship (and readership) hinders the development of knowledge in a field. Along similar lines, Van Maanen (1996) argues that it is worthwhile to increase consciousness about a field's "writing ideologies," which he defines as "beliefs that serve to justify or rationalize language use and textual practices in specific fields." "[W]hen widespread and unquestioned," he contends, "such beliefs institutionalize as a matter of local culture writing styles and sink below awareness [and become taken for granted]" (p. 376). When this happens, practice in a sense "overtakes" scholarship, leading to a routinization of the way a field operates and a reinforcement of more or less standardized textwork conventions. In turn, although textwork is often portrayed as an individual affair, Van Maanen, like others (Deetz,

1996; Putnam, 1996), maintains that it is difficult to separate individual from social practice when thinking about the practice of textualization in a field of study.[26]

Furthermore, Van Maanen hints that textwork enables individual scholars to textualize "voices" (or to "voice" texts) which identify their position in a field by textualizing a perspective, object of knowledge (content), research approach, writing style, and so forth. That is, the publishing of texts, voiced through all these elements, "officializes," ratifies, and identifies their positions (it sustains a biographical illusion by "making a name") and, in turn, enables the acquisition of capital (Bourdieu, 1990b). As Bud Goodall (1991; see also 1999, 2000) notes, to "give voice to a text is, in part, to give voice to the self who composes it" (p. 21). In other words, everything a scholar writes (or says) "carries a recognizable stamp" (Van Maanen, 1996, p. 378), even if he or she expresses this stamp through a depersonalized style that gives the impression of authorial "non-presence." Although a scholar's spoken voice is important for the production of knowledge and the struggle for capital (e.g., in the form of teaching, presenting at conferences, discussions with colleagues, etc.), I center this study on the constitution of a written/textual voice in academic publications, because this practice plays such an important role in a scholar's positioning (Bourdieu, 1990b). Hence, I see textwork as a form of social action which helps to further a field of study, but also an individual scholar (Bazerman, 1988).

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[26] As Van Maanen (1996) mentions, "we do not write alone. Our choice of words, sentences, topics, and styles betray their origins in theory groups, training and employing institutions, collegial networks, and reading circles" (p. 378).

Based on previous research on textwork (see Atkinson, 1990; Bazerman, 1988; Clifford, 1988; Clifford & Marcus, 1986; Geertz, 1980, 1988; Goodall, 1999, 2000; Marcus & Cushman, 1982; Richardson, 1994; Rosaldo, 1989; Van Maanen, 1988, 1996), I design a twofold classification of textualization practices through which scholars constitute an identifiable position (or voice) in a field. Voicing a text occurs through "conversing" with audiences and by interacting with a field's textwork conventions. Accordingly, to be a serious contender in a field's language-games, scholars must be able to persuade themselves and others (Gross, 1990). That is, scholars need to distinguish themselves in careful ways from positioned others by creating a voice which is seen as unique (in terms of perspective, content, and/or style) and which makes convincing and valued knowledge contributions. Scholars thus may become more successful textworkers and solidify their positions as their careers advance, because they have developed a "feel for the game" (Bourdieu, 1990a, p. 66) which gives them an advantage over others. A voice, then, is comparable to a "subject position" (Foucault, 1972), a point from which someone can and may speak in a field, and therefore depends on a scholar's linguistic capital.

In the following two discussions, I explain in more depth how scholars voice a text. In line with my twofold classification, I first address the role of conversation with audiences in the constitution of an identifiable, textual position; and, after that, I discuss the role of textwork conventions in this constitution.

Textwork Conversations

Although the proper name of a scholar who voices a text suggests a singular identity, his or her voice may vary within a text, from text to text, from field of study to field of study, and throughout his or her academic career. Just as someone's biographical illusion is constituted through the symbolic interaction between agent and field (see Bourdieu, 2000b; Mead, 1934), scholars voice their texts through "textwork conversations" with various audiences. Thus, scholars (as does all authors) create textual relationships with themselves and others through this interaction. According to Umberto Eco (1992, 1994a/b), a flesh-and-blood, "empirical" author engages in an imaginary conversation with a model or implied reader while writing. In turn, the empirical reader converses with a model author whom he or she imagines. However, especially in academia, these conversations are enacted between peers as well (e.g., through e-mail correspondence), so that the model reader/author becomes less imagined.

Thus, in textwork conversations, the "other" is constituted through the conflation of a "literal" other and "oneself as another" (Ricoeur, 1992). Through reading and writing, a scholar becomes "a character of and for himself" (Said, 1966, p. 12). As "an expression of consciousness, something that reflects" (Bakhtin, 1986, p. 113), a text enables a writer to constitute a relationship with himself or herself by appropriating the text and reflecting on a reflection (Bakhtin, 1986, p. 113). Through this "distanciation" (Ricoeur, 1991), that is, textwork enables scholars to express a perspective on an object of knowledge, on themselves, and on "others," through an

identifiable voice.[27]

In academia, the effect of textual distanciation tends to be bolstered (e.g., by streamlining a text, writing in a distanced, neutralized style so that there is little correspondence between a scholar's voice "outside" and "inside" a text, etc.) to conceal a scholar's involvement in the production of knowledge and to prevent reflection on the scholarly break induced by academic practice. A scholar's *habitus*, in turn, influences how he or she practices this distanciation. Also a field's textwork conventions therefore play an important role in the voicing of a text, as I will discuss next.

Textwork Conventions

The practice of voicing a text through conversational practices occurs in light of a field's historically developed textwork conventions, "established practice[s]—whether in technique, style, structure, or subject-matter—commonly adopted [in a field of study]" (Baldick, 2001, p. 50). These conventions provide "rules" for organizing a text (e.g., in the form of introduction, literature review, methods, results, and discussion; staying within a preset limit of pages; etc.) and developing its style (formal/informal; personal/impersonal, etc.) and content.[28] In turn, these elements (organization, style,

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[27] An author's textual voice may be partly an expression or translation of his or her actual voice, but (as Borges [1998] and Said [1966], among many others, suggest,) it is never possible to "capture" oneself in text entirely.

[28] In *The Concise Oxford Dictionary of Literary Terms*, Chris Baldick (2001) indicates that configurations of these elements are sometimes ambiguously referred to as "form." Form is "can refer to a *GENRE (e.g., 'the short story form'), or to an established pattern of poetic devices (as in the various *FIXED FORMS of European poetry), or, more

and content) are configured in a particular way in a text, help to identify a scholar's position in a field, and affect its reception and evaluation. Consequently, scholars acquire economic, cultural (in particular linguistic), social, and symbolic capital by producing their textual productions in light of the dialectical relationship between their expressive interests and the textwork conventions which are embodied through a field's *habitus*.

Scholars thus are educated to work in light of these conventions, implying the *particular* practice language. Based on these conventions, scholars "know" what to do when producing a text, how a text should look like, and how to read and recognize a text. Put differently, these conventions enable a field to act together and produce texts as a collectivity. However, like language-games' "rules," textwork conventions are not fixed (although they may be perceived as relatively objective). Rather, they are sustained or transformed in the practice of textwork. In turn, scholars often follow or apply these conventions without very much reflection and they might find it difficult to articulate their "use" to others (e.g., a scholar might read something and have a sense that "something is not quite right").

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abstractly, to the structure or unifying principle of a design in a given work. Since the rise of *ROMANTICISM, critics have often contrasted the principle of *ORGANIC FORM, which is said to evolve from within the developing work, with 'mechanic form,' which is imposed as a predetermined design. When speaking of a work's formal [bold deleted] properties, critics usually refer to its structural and patterning, or sometimes its style and manner in a wider sense, as distinct from its *CONTENT" (pp. 100-101). In light of Bourdieu's (1991a) work, the form/content, organic/mechanic form antinomies which Baldick introduces probably exist in dialectical relationships in the practice of textwork.

To close, I formulate my second research question in light of this discussion on textwork:

RQ2   How do scholars position themselves in a field of study through textwork?

    a   What antinomic language-games characterize this textual positioning?

    b   How does the contest for capital play a role in this textual positioning?

    c   How does this textual positioning affect the production of knowledge in a field of study?

After having outlined the basis of this study, I now summarize the ideas and arguments I have presented in this chapter.

Conclusions

In this chapter, I have conceptualized how a scholar's constitution of an identifiable position in a field of study is an integral part of scholarly research. I built this argument by translating and extending Bourdieu's work.

In my discussion, I discussed the general workings of a field and important related concepts, such as agent, *habitus*, *doxa*, forms of capital, and *illusio*. After the translation of these principal concepts, I explicated how agents (scholars and non-scholars alike) position themselves in a field. Here, I relied on Bourdieu's conception of identity as a "biographical illusion," but also extended his writings by relying on Mead's (1934) work, among others. Through this discussion, I conceptualized in more

explicit ways than Bourdieu how agents enact the ontological complicity between agent and field.

Next, I applied my discussion to the way scholars implicate themselves in an academic field of study. Of particular importance, in this case, is how scholars enact "antinomic language-games" to produce knowledge, but also to enhance the value of their position. Subsequently, I explicated these games of language in light of scholars' textwork. Based on my explications, I formulated two research questions for this investigation, the first one focusing on better understanding the general enactment of the ontological complicity between scholars and a field of study, and the second one focusing on the way scholars position themselves in a field of study through their textwork. In the next chapter, I will explicate the "data" construction and analysis conducted to investigate these research questions.

CHAPTER III

DATA CONSTRUCTION AND TRANSLATION

"Data"—from the Latin verb *dare*, implying "to give"—denote "givens." In academic fields, agents constitute data as information that is more or less given. However, scholarly agents also participate in the production of data through their research practices. As this study will show, textwork plays a substantial role in this process of discovering and inventing knowledge. Through textwork, scholars constitute knowledge by positioning themselves in a field and establishing valuable relationships with their other scholars, research participants, and themselves.

In this chapter, I discuss the textwork conducted for this study in light of the *habitus* of "organizational communication studies" (the subfield under investigation). As a whole, communication studies is a relatively young field of study in comparison to more longstanding fields in the human sciences like psychology, sociology, or anthropology. Organizational communication studies, in turn, is a young subfield within communication studies in comparison to subfields like studies in rhetoric or communication. For the most part, it originated in North America. Stated very broadly, the general object of knowledge of organizational communication studies concerns the relationship between communication and organization (e.g., institutionalization, bureaucratization, marketing, or organizational negotiation).

In the first part of this chapter, I discuss my reasons for examining this subfield of study. Then, I reflect in more detail on organizational communication studies as a

subfield which has only recently gained relative autonomy from related fields like organization studies, management studies, or industrial and organizational psychology. Hence, its *habitus* resembles other systems of dispositions besides the more broadly defined *habitus* of the human sciences. In this first part of the chapter, I show how the *habitus* of this subfield has been (and is) constituted through several antinomic language-games. These games have helped distinguish organizational communication studies from other fields and subfields, and they have played an important role in the development of the field.

Thereafter, I explicate the production of three types of text (interview transcripts, academic publications, and journal scripts) that form the "data" for this investigation. Finally, I discuss the analysis or translation of these texts and lead the reader into chapter IV.

The Subfield of Organizational Communication Studies

Principal Interests in Studying This Subfield

I intentionally have centered this investigation on the subfield of study whose *habitus* coincides with my own system of dispositions. Being positioned in the subfield under investigation, I have a familiarity with and an "access" to its beliefs, values, practices, stakes, canons, conventions, and influential players that I would not have in other fields of study. My "participant observation" thus makes a more in-depth analysis possible of this subfield's *habitus*.

I also have more "personal" stakes in contributing to the research and

development of organizational communication studies. I am invested in this subfield, because I value its object of knowledge and its efforts to understand this object; that is, I see worth in organizational communication research, believing that it offers important insights which might help agents improve their social conditions. In other words, this subfield's *illusio* motivates me to participate in its language-games. Accordingly, I conducted this study to improve the social conditions of the subfield which makes these insights possible. Additionally, I conducted this study to bolster my position in organizational communication studies. For example, this textwork gives me the opportunity to improve my cultural capital by obtaining a doctorate from a reputable university; to acquire economic capital by being given a university position as an assistant professor; it might improve my social capital, because it gives me the opportunity to become a member of particular groups of scholars; and it might enhance my symbolic capital by improving my overall credibility in the subfield of organizational communication studies and the overall field of communication studies. In general, this study might thus improve the relationship "I" constitute with "myself" and others within a more or less well-defined social space by identifying my position.

However, investigating one's own playground also might have its drawbacks. Acting as "inside outsider" and "outside insider" is difficult, because my familiarity with the subfield might be blinding. Besides, there is the risk of losing capital instead of gaining it. For instance, the subfield or larger field may criticize, not recognize, or disapprove of this study, leading to a devaluing of my position, of the texts produced from this research, and even of my future, unrelated texts—then again, such

"controversy" might also reinforce particular debates which buoy the game of scholarship. A more general shortcoming of studying only one subfield of study is that such an investigation makes it impossible to analyze how scholars' textwork and positioning practices in a subfield result from its interaction and interconnection with the *doxa* and *habitus* of a larger field of study (in this case, communication studies) and neighboring fields and subfields.

I conducted this research with these considerations, these "risks," and these limitations in mind. In so doing, I operated from the *doxa* that the reflexivity which this kind of research illustrates is important, because it might conduce conversation about the workings of a field (the field under investigation and other fields).

After having conveyed my principal interests in studying the subfield of organizational communication studies, I now construct its main antinomic language-games. Here, I rely on several historical overviews of the subfield.

The Subfield's Antinomic Language-Games

The development of organizational communication studies has been reconstructed several times (see Putnam & Cheney, 1985; Redding, 1979, 1985a/b; Redding & Tompkins, 1988; Tompkins & Wanca-Thibault, 2001). In addition, a number of scholars have delineated its more or less common object of knowledge in (text)books and journal articles (see Conrad & Poole, 2002; Cheney, Christensen, Zorn, & Ganesh, 2004; Eisenberg, 1996; Jablin & Putnam, 2001; Jablin, Putnam, Roberts, & Porter, 1987; Miller, 1996, 2003; Mumby & Stohl, 1996; Rogers, 1996; Weick, 1987). In the subsequent

construction, I constitute five central antinomic language-games which characterize the development of the subfield and its *habitus*. These games concern struggles about: the field's distinction/indistinction from neighboring fields; the distinction/indistinction between rhetorical and social scientific research; the distinction/indistinction between theory and practice; the relationship between neopositivist/interpretivist/critical orientations to the study of organizational communication; and sustaining the field's coherence while fostering its eclecticism.

      Distinction/Indistinction from Other Fields. In the 1980s, Charles Redding (1985a) remarked that

> [a]cademic course work and research programs dealing with organizational communication always have been, and still are, offered in a bewildering diversity of departments or other administrative units. These include such entities as business administration, psychology, sociology, speech, speech communication—and, finally, communication. Thus if we were to define 'field' in terms of a single departmental 'home,' we might be forced to conclude that no field of organizational communication exists. (p. 6)

In the twenty years following this statement, the subfield increasingly developed a relative autonomy from other fields and subfields in the human sciences, such as organizational psychology, organizational sociology, organization science, or management studies. As Bourdieu (2000a) suggests, academic fields of study try to constitute a legitimate autonomy by distinguishing themselves from other fields, so that they can exist more or less independently (in the same way as individual agents try to distinguish themselves from others within fields). However, throughout time, the subfield never seems to have lost complete sight of cousin fields like these.

Accordingly, these fields at times function as reference points. Hence, the first language-game I construct revolves around a struggle for distinction and indistinction from these related fields.

Redding's (1985a) text suggests that the subfield's participation in this antinomic language-game (which was at least partly self-induced) occurred by trying to define or name ("nominate") itself in relative distinction from other fields and subfields. The origins of organizational communication studies involved

> business English, business speech, including business and professional speaking, basic communication skills (including applications of general semantics), Dale Carnegie courses, industrial psychology (of the 1910-1940 era), traditional management theory, industrial journalism, [and] 'proto-human relations' (about 1900-1940). (Redding, 1985a, p. 17)

However, the name "organizational communication (studies)" first started to be used in the 1960s. Herbert Simon, for instance, emphasized that organizational messages needed to be intelligible and persuasive. In the 1950s, scholars showed an interest in persuasion as well, for example, in terms of convincing employees that their organization had a legitimate existence and was "doing the rights things" and "doing right" (Redding, 1985a, p. 19). Nevertheless, at that time, this upcoming subfield was not yet named "organizational communication."

Before the 1960s, names like "management communication," "industrial communication," and "business communication" circulated (some of which are still

employed today).[1] During the 1940s, communication scholars concentrated on specific

types of organizations, such as business, industry, the military, and the government.

Many of the studies from this time were concerned with wartime communication. That

is, because the military and industry contributed to the country's mobilization,

industrial communication gained importance. In turn, due to the alliance between

academia, the military, and industry, communication experts were employed to train

military and industrial personnel to support the war. During this period, Redding

(1985a) notes, communication was seen mainly as a skill (implying a fusion of "English"

and "speech"). Communication was associated with "business speech" and it was

considered a "by-product" of other topics that were investigated, such as employee

motivation or leadership.

Around the 1950s, though, scholars began to institute graduate programs in

"industrial communication," for example at Michigan State University and Purdue

University. However, in the 1960s, the name "business and industrial communication"

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[1] Particularly in the 1980s and 1990s, scholars engaged in language-games concerning the distinctions and indistinctions between organizational communication studies and fields of study with a more or less similar focus, such as "business communication studies," "management communication studies," or "corporate communication studies." In the mid 1990s, Katherine Miller (1996) edited an issue of *Management Communication Quarterly* dealing with these debates, but also other scholars besides those contributing in this issue wrote about them. For example, "business communication" is considered to emphasize written (and to some extent also spoken) communication for retail, sales, and promotion (Shelby, 1993); "management communication" concentrates on improving the communication of management through becoming a more strategic and skilled communicator (Feingold, 1987); and "corporate communication" emphasizes the improvement of the relationship between internal and external communication (Van Riel, 1992).

was changed to "organizational communication" in order to cover communication processes and practices in organizations in a more general sense than was implied in the previous name. However, this seemingly simple shift may also be seen as the subfield's move towards casting its net of influence wider and heightening its importance as a field of study among a multitude of cousins.

       Rhetoric/Social Science. In light of the developments discussed so far, the dialectical relationship between rhetoric and the social sciences played an important historical role in the development of the *habitus* of organizational communication studies. For long, social scientists who studied organizing processes and practices saw communication as a relatively inconsequential variable. Consequently, the humanistic study of communication[2]—"speech" or "rhetoric"—was best suited to provide the space for studying organizing within the field of communication studies (Redding, 1985a).

       Like many fields in the human sciences, then, also organizational communication studies has struggled throughout time with the controversy caused by seeing fundamental distinctions between scientific and humanistic approaches to studying its object of knowledge. In turn, many organizational communication scholars presently may be considered "social scientists," but scholars in this subfield continue to be influenced by the study of organizational "rhetoric"—even if currently labeled "discourse" or "language"). Correspondingly, the relationship between language and

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[2] Conversely, rhetoricians would perhaps see communication studies as the social science study of rhetoric.

organizing is a central object of study (again) at the moment (see Fairhurst & Putnam, 2004; Putnam & Fairhurst, 2001).

Theory/Practice. Moreover, historically, the subfield has been engaged in a language-game about "theory" and "practice." Redding and Phillip Tompkins (1988) state that the "dominant impulse behind the study of organizational communication has always been pragmatic—attempting to discover how individuals or organizations, or both, can be made to function more effectively" (p. 11). The practical side of communication almost has been permanently a focus of the field. For instance, in the early days of the subfield, many scholars saw communication as a skill that would benefit organizational members. Accordingly, "human relations" scholars saw communication as a tool for improving organizational productivity through the improvement of workplace relationships. However, this practical emphasis also attracted managers and popular writers to the topic of organizational communication and thus created a tension between the academic and non-academic textualization of the field's object of knowledge (Redding, 1985a).

In turn, Redding (1979; see also Redding & Tompkins, 1988) questioned at the end of the 1970s whether organizational communication studies could be seen as a "proper" subfield, because it had not developed acceptable scientific theories yet (p. 309). At this time, therefore, the subfield sought to define its legitimate existence by questioning its theory-developing capacities and potentialities vis-à-vis neighboring fields like organizational psychology, organizational sociology, organization science, or management studies. This antinomic language-game contributed to the formation of

the subfield's *habitus* by highlighting the lack of theory as a problem in comparison to other fields and subfields of study. That is, through this game, scholars were able to demarcate and distinguish themselves from these other academic fields as well as those who were operating in the field of popular writing.

Neopositivism/Interpretivism/Critical Theory. In addition to the games about theory and practice, the subfield enacted language-games concerning the "incommensurate" relationships between neopositivist (or "functionalist") and interpretive (or "naturalistic") approaches to the study of organizational communication. These games were important, because they helped to form the subfield's "interdisciplinary" character (Redding 1985b); that is, its embrace of a plurality of metatheoretical and methodological positions regarding a variety of organizational communication topics or content areas (ranging from organizational leadership to organizational networks). Of particular importance (although indubitably not of sole importance), here, were the conferences on *Interpretive Approaches to Organizational Communication* which were held in the 1980s in Alta, Utah. Through these meetings, scholars produced principal texts that sustained and furthered this debate in particular (see Putnam & Pacanowsky, 1983; Tompkins & McPhee, 1985; Tompkins & Wanca-Thibault, 2001).[3]

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[3] Considering the interconnectedness of fields of study, it is not very surprising that the attention for interpretive approaches to the study of organizational communication coincided with the "interpretive turn" in anthropology and sociology.

The subfield changed considerably by promoting the importance of interpretive research (Tompkins & Wanca-Thibault, 2001). This entailed, for instance, that organizational communication was viewed no longer only "mechanistically." In different words, organizational communication was seen no longer as a strategically controlled transmission process in which material messages travel from one place to another through channels; nor was the subfield's object of knowledge only studied through positivist methods in search of causal relationships between variables (Krone, Jablin, & Putnam, 1987).[4] Instead of treating communication as something that occurred within organizational "containers," the interpretive view implied that individuals socially constructed organizational realities (Krone, Jablin, & Putnam, 1987). In turn, some scholars suggested studying these processes through interpretive methods that analyzed how individuals constitute these realities of shared meaning.

Additionally, some scholars started to enact critical theorist positions. That is, these scholars raised consciousness about managerial ideologies and domination (coined "managerialism") and focused on employee emancipation through organizational empowerment (Redding & Tompkins, 1988).

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[4] Although no longer called "mechanistic" and having undergone considerable development, the neopositivist view still plays an important role in organizational communication studies (Miller, 2000). For example, some scholars take this position when studying organizations as networks between nodes and as complex systems (see Monge & Contractor, 2001, 2003). Scholars taking this stance often (but not always) consider the enactment of organizational forms to occur in pattern-like ways. Hence, they believe that these processes are more or less predictable.

Unity/Diversity. The final language-game in which the subfield has been involved perhaps most directly influences the field's current practice of research, as was illustrated by a recent book which Steven Corman and Scott Poole (2000) edited. This game concerns the subfield's struggle to remain a more or less coherent field in light of its fostered eclecticism between metatheoretical and methodological positions as well as organizational communication topics.[5] In Corman's and Poole's book, scholars engage in this language-game by exploring whether "common ground" exists between neopositivist, interpretive, and critical perspectives on organizational communication. Mumby (2000) argues, for instance, that the subfield has a tendency to fragment by constituting itself through antinomies:

> [T]he argument goes that as a field (not just organizational communication, but the field as a whole) we seem to have reached a particularly agonistic point in our development where we have a Babel of voices, none of which seem to be doing a particularly good job of establishing dialogue and making connections with each other…too often…this so-called multivocality is reduced to a simple binary opposition. This opposition takes various forms: subjectivism versus objectivism (Burrell & Morgan, 1979); social constructionism versus positivism (Stewart, 1991); interpretivism versus functionalism (Putnam, 1983); modernism versus postmodernism (Habermas, 1981), and so forth. (pp. 74-75)

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[5] Eric Eisenberg (1984) coined the term "unified diversity" (p. 23) when he argued that ambiguity could be viewed as a strategic practice. In accordance with this idea, organizational communication studies can be seen as an eclectic field which is characterized by a more or less ambiguous object of knowledge (or perhaps several objects) and held together by a common name. This ambiguous commonality "foster[s] the existence of multiple viewpoints [through the creative use of symbols, as well as] agreement on abstractions without limiting specific interpretations" (Eisenberg, 1984, p. 231).

Mumby suggests that it is important to look for the overlapping intersections between the historical discourses ("blurred discourses of knowledge") which underlie these controversies in order to overcome these antinomies. Similar to Charles Conrad (2000), I believe that the search for intersections may temper the spirit of these language-games. However, this conversation still leaves the roots of these controversies in tact by failing to reflect on the antinomic relationships between the positions which scholars enact by operating from a particular *habitus* (which reflects the subfield's history). In turn, it is important to contemplate, as James Taylor (2000) also claims,[6] to what extent scholars' *illusio* (i.e., their investment in a position and in the overall antinomic language-games of the subfield) prevents them from reflecting on these games and their effects on the production of knowledge.

In closing, I conclude that organizational communication studies (like any field or subfield of study) has been shaped through a number of antinomic language-games in the last sixty years. These games concerned struggles about: the field's distinction/indistinction from related fields; the distinction/indistinction between rhetorical and social scientific research; the distinction/indistinction between theory and practice; the relationship between neopositivist/interpretivist/critical orientations to the study of organizational communication; and sustaining the field's coherence and fostering its eclecticism. As this study will illustrate, these games have had (and have)

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[6] Taylor (2000) writes: "Is part of the problem…that we have been caught behind the lines, defending *positions* [italics added] that no longer have much strategic value…? Has the quarreling…degenerated into futile name calling, at exactly the moment when the names [neopositivism, interpretivism, critical theory] no longer mean what they

important bearings on scholars' practice of textwork and their constitution of an identifiable position.

Next, I explicate the construction of "data" for this study. In the first section, I discuss the production of three kinds of texts for analysis or translation (usually referred to as "data collection"). Thereafter, I reflect on the practice of interpreting or translating these texts.

The Production of Texts for Translation

Three types of texts ("data") were produced for this study: interview transcripts, academic publications, and journal scripts. The interviews constituted (dis)positional reflections by engaging in actual conversations—rather than the imaginary author-reader conversations conceived by Eco (1992, 1994a/b)—with scholars who have played and play an important role in the development of organizational communication studies. In addition, reflecting on and analyzing these scholars' publications helped to focus my investigation. Finally, I wrote in a journal to consider my own position and dispositions.

The next section presents the practices undertaken to achieve these textualizations. As far as interviewing is concerned, I first describe the production of transcripts, followed by a more critical discussion on the practice of interviewing. Thereafter, I reflect on the collection of participants' academic publications and, subsequently, on my journal writings.

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once did?" (p. 191).

Interview Transcripts

      The Production of Transcripts. I conducted two loosely scripted conversations of approximately one hour each with eleven full professors and one associate professor to produce twenty-four interview transcripts. Each participant was selected, because he or she lived in North America; was (and is) actively involved in organizational communication research; had extensive experience in the practice of textwork in the subfield under investigation; and had received his or her doctorate within approximately the same timeframe (mid to late 1970s or the 1980s).[7] As discussed, this was an important time the subfield, because it underwent significant changes. Thus, due to their continued influence on the subfield's development and their extensive experience of operating within its domain, these participants are very critical players. Correspondingly, each of these agents has acquired a substantial amount of cultural (especially linguistic), social, and symbolic capital in the subfield itself, the larger field of communication studies, and (in most cases) other fields or subfields of study. Finally, most, but not all, of the twelve participants were employed by "research I" universities.

      I used the *National Communication Association* and *International Communication Association* directories for the organizational communication division to develop a list of

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[7] Although all of these scholars frequently engage in the practice of reviewing and/or editing manuscripts for publication, I did not emphasize these editorial practices in the interviews. Rather, I examined the practices these scholars enacted to publish their own works. I do not deny that editorial experiences highly influence the practice of writing and reading for publication, yet sharpened my object of inquiry by setting parameters.

potential participants. My sampling frame consisted of twenty-eight full professors. To

generate a sample of twelve participants, I e-mailed a message requesting participation

to three participants at a time. Of the seventeen scholars who received this message,

five kindly declined participation. I sampled purposively to construct the group of

participants as a variety of positions (constituted by different content areas,

metatheoretical perspectives, and methodological approaches). I also tried to include a

proportionate number of men and women (eight men and four women participated).

One of the participants was a professor in my own department at Texas A&M

University, yet this person did not function as a member of my doctoral committee.

Furthermore, I developed a script for each interview.[8] I conducted pilot

interviews with two professors from my own department to rehearse the interviewing

and to test the questions.[9] The first interview focused on constructing overall

reflections on the ontological complicity between the scholar and the subfield, and on

the practice of constituting an identifiable position through textwork in organizational

communication studies (see appendix A). The script consisted of two main parts, but

the conversations often developed nonlinearly. Generally, all questions were

addressed.[10] In the first part of the interview, I asked a participant to reflect on his or

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[8] Texas A&M University's Institutional Review Board approved the research proposal
which included these scripts. Accordingly, I asked each participant for his or her
consent before each interview.

[9] I did not include these interviews' transcripts as data for this study.

[10] As the conversations progressed and I learned to better delineate my object of
knowledge, I added, changed, or deleted particular questions.

her academic trajectory. Thus, in this part of the interview, the participant

reconstructed his or her biography in light of the developments of his or her academic

career. In the second part, I asked the participant to reflect in general on the

relationship between his or her positioning practices and his or her textwork. At the

end of this conversation, I requested the participant to select two published texts

(journal articles, book chapters, or complete books) for the follow-up interview, one

dating from early in his or her early career and a recent one.[11]

The second interview focused on the relationship between positioning or

identity practices and textwork through a historical comparison of actual texts (see

appendix B). This interview consisted of several parts (here, too, each conversation did

not follow the same sequence, but all questions were addressed). In the first part, a

participant reflected on the general research practices conducted to produce the two

texts. Then, I asked the participant to reflect on the relationship between each

publication and what he or she is (and was) trying to accomplish with his or her work

in order to understand the participant's constitution of textual voice. Additionally, a

participant reflected on the conversational aspects of this constitution. Finally, we also

spoke about specific writing practices enacted in each publication. A comparison of

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[11] I gave participants the freedom to select single-authored and/or co-authored texts. Although textwork is always, in a sense, a "co-authored" affair, I do not deny that there is a difference in experience between "single"-authoring and co-authoring a text. Accordingly, I included this distinction as an interview topic for the second interview (see appendix B).

these publications made it possible to constitute ideas about the influence of experience; that is, of the development of a participant's *habitus*.

Moreover, the participants were aware that I could not guarantee their anonymity due to reliance on their published materials. While constructing this text, however, I concluded that it would be best to protect participants' privacy completely. That is, it seemed better to let participants themselves determine what they wanted to express about themselves. For this reason, I did not directly quote or cite participants' publications, but paraphrased passages or ideas from their works based on my readings of these texts to provide illustrations.

We mostly spoke over the telephone, although six conversations were held during the 2003 *International Communication Association* conference in San Diego, California.[12] I documented each conversation through audiotapes and field notes, and then used this documentation for transcription. A trained typist transcribed seventeen tapes while I transcribed the remaining seven. This generated approximately twenty pages of single-spaced text per conversation, creating about five hundred pages in total.

After having described the conduct and transcription of these interviews, I now take a more critical stance on the practice of constructing these "data" by relying on a recent article from Mats Alvesson (2003).

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[12] Evidently, variations in communication medium influenced the reflections we constituted. When telephoning, participants and I were able to hide nonverbal cues from each other. Alternatively, when talking face-to-face at the conference, we were interrupted sometimes by colleagues entering or exiting elevators, waiters serving fish and chips, and so forth.

Deliberations on Interviewing. Alvesson (2003) suggests that interviewing can be viewed from at least three possible, ideal-typical, angles: neopositivist, romanticist, and localist. A neopositivist sees interviewing in its most "realist" sense as the retrieval of data from interviewees and is "eager to establish a context-free truth" (p. 15). A romanticist intends to establish rapport with interviewees and focuses on "interactivity with and closeness to interviewees—seen as participants" (p. 16). A romanticist, then, believes in the coconstruction of data. Finally, a localist sees interviewing as a completely participatory affair in which both interviewer and interviewee construct meaning as they position themselves in a particular context. In other words, in interviews, "people are not reporting external events but producing situated accounts, drawing upon cultural resources in order to produce morally adequate accounts" (p. 17).

I adopted the localist perspective on interviewing in this study, because I believe that this position best captures how meaning is constructed conversationally in a particular interview setting. Additionally, this position best enables me as a researcher to practice the kind of reflexivity I aim to illustrate through this investigation. To make sense of an interview encounter, I thus interpreted the transcript of a conversation in light of the larger contexts in which a conversation was held, for example, in light of the structural factors of the subfield of organizational communication studies and more momentary factors (e.g., the time of day, place, etc.). In accordance with the ideas that the interview context matters, both in structural and more momentary ways, Alvesson formulates several challenges which a scholar encounters when interviewing. I

describe next how I dealt (or did not deal) with most of these challenges in the following paragraphs.

The first challenge posed by Alvesson is "the social problem of coping with [to large extent unfamiliar] interpersonal relation and complex interaction in a nonroutine situation" (p. 18). The participants and I constituted reflections on the language practices through which participants position themselves in organizational communication studies. These nonroutine conversations about relatively routine practices might thus have resulted in reflections that had only some commonality with participants' ordinary reflexivity practices. As textualizations of conversation in practice ("discourse"), these reflections may mislead the researcher (or the reader of a research text) in another way. Through distanciation (Ricoeur, 1991), textualization "cements" the interview discourse. Henceforth, textualization leads to a kind of "eternization" (Bourdieu, 1990a, p. 83), implying that textualization sets discourse "aside" from the practical momentariness of the interview situation.

Moreover, Alvesson notes that part of the process of interviewing involves "finding out what it is all about (beyond the level of the espoused)" (p. 18). In this study, this finding out occurred throughout the research process: during the development of the research proposal; the interviewing; and the writing of this dissertation. To constitute reflections on my position and dispositions, I mainly relied on journal writing (as I will discuss in a moment).

Furthermore, Alvesson writes that interviewer and interviewee rely on "cultural scripts" when communicating in an interview situation, that is, "available vocabularies,

metaphors, genres, and conventions for talking about issues" (p. 20). These scripts are, I believe, part of both agents' linguistic systems of dispositions, that is, their propensities to speak in certain ways. When interviewing, I hence brought "my" script, while the participants brought theirs. Especially the first conversation was useful for improving communication and understanding about each other's scripts (see appendix A). In addition, my familiarity with their published texts was beneficial.

Additionally, the participants and I undeniably engaged in some form of "impression management." As Alvesson suggests, an interview is a performance. Our performative situation was defined in part by the differential (hierarchical) positions that the participants and I occupied in the same social space. Both participants and I, then, were "*politically aware and politically motivated*" (Alvesson, 2003, p. 22). Accordingly, we may have felt the need to present ourselves and others in a particular light. Hence, it was very difficult to verify the truthfulness of our communication or the deeper reasons behind our expressions.

Finally, I tried to deal with the difficulties of linguistically "representing" (or "translating") the interviews by conducting two separate interviews with each interviewee. This made it possible to return to certain topics after the first conversation and it helped me as well in my construction of consistencies and inconsistencies between these two encounters. Additionally, I as much as possible constituted participants' voices in their "original" language in chapter IV.

Academic Publications

To complement the interview transcripts, I read several of each scholar's publications in addition to the two texts which he or she had selected for the second interview. I used the *ComAbstracts* database (maintained by *The Communication Institute for Online Scholarship*, *CIOS*) which indexes each participant's major journal publications. Further, I used the *LibCat* search engine to locate books in the Texas A&M University library catalogue.

My readings of these publications served as background readings which informed and guided the interviews, particularly in terms of fine-tuning questions for the second interview. Accordingly, this study does not provide an archaeology of the textual constitution of key ideas or concepts within organizational communication studies or an investigation of the historical development of this subfield's research practices in light of key texts (similar to Bazerman's [1988] work).

Journal Scripts

To complement the two previously mentioned textualizations, I reflected for three years on my interests in this research and my position in organizational communication studies by regular writing in a journal. Through this practice, I developed insights into the trajectory of my own position and dispositions.

Accordingly, I weaved some of these writings into the text of this dissertation to support, illustrate, and reflect on some of my arguments.[13]

Now that I have discussed the practice of constructing my "data," I will reflect on the practice of analyzing or translating these data in the subsequent section. In so doing, I explain how the information I used to write chapter IV was constructed from a particular point of view.

The Practice of Translation

Interpretation is "indefinite" (Eco, 1992, p. 32) if not punctuated. In addition, this "self-aware act of deciphering" (Bourdieu, 2000a, p. 53) is an act of translation (of turning one language into another). Thus, when interpreting, scholars often are inclined to translate the language of their research "subjects" into an academic language by projecting themselves into the minds of the people studied. Accordingly, sometimes translation is treated as a "re-creation" while overlooking (or ignoring) the difference in perspective which such recreating involves.

Understandably, the practice of translation is closely related to all other research practices ("conceptualization," "data collection," etc.) and constitutes an essential part of the practice of textwork. In turn, translation is not an activity which occurs

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[13] According to Wacquant (see Bourdieu & Wacquant, 1992, p. 41), Bourdieu is skeptical about the practice of journal writing for the purposes of research, if it does not contribute to reflecting on the ways in which the researcher's position is implicated in a field of study which centers its activities on investigating a particular object of knowledge.

separately from any of these activities or separately in terms of time (e.g., "after the data have been collected").[14] Like any of these other practices, that is, translation is an integral part of the construction of "data." When translating, I engage in Eco's (1992, 1994a/b) imaginary conversation with a reader (a conflation of myself and someone else). Hence, also the writing of this text involved an ongoing conversation which extended beyond the interviews I had conducted. Nevertheless, the literal other disappeared and was transformed into a textual character (an "epistemic individual," as Bourdieu [1988] would say). At the same time, then, this "other" person functioned as the model/implied co-author of this text and to some extent also its model/implied reader. Chapter IV's text thus can be seen as a continuation of the interview conversations in that it conveys reflections which were sparked by the interviews.

In light of these remarks, it is impossible to determine the beginning or ending of the practice of translation, as it is impossible to do so for textwork in general.[15] In spite of this notion, it is important to discuss some of the activities undertaken to

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[14] Then again, the chapter organization of this dissertation suggests such a demarcation. Perhaps a chapterless dissertation would have been better, in this regard. However, my conventional structuring of this text undeniably enhances the clarity of this text, because it meets most readers' expectations.

[15] I make the same argument in "Reconstructing Opa: Last Meditations of a Meteorologist" (Brummans, 2003). Additionally, I write in my journal on January 4, 2003, that the "drama of scholarly textwork performance never stops. Even a published text performs. Publications are not only the products or remnants of textwork performances. They are products-in-use which perform to various audiences when they are appropriated (read, addressed, referred to, etc.). Hence, by analogy to Goffman's (1959, 1986) work, a published text performs, in a sense, "onstage," while the text production occurs "backstage."

develop my sense of translation, because this offers another reflection on my own

habitus.

In this study, I adopted a broad analytical perspective which Linda Putnam and

Gail Fairhurst (2001) call "literary and rhetorical language analysis." While taking this

perspective, I centered on the way language enables the enactment of an identifiable

position in a subfield of study through textwork. Putnam and Fairhurst note that

rhetoric does not only involve persuasion;

> it is also a means of human understanding and a process [and practice] of
> constructing social reality (Watson, 1994). This perspective stands in opposition
> to the view that rhetoric is embellishment distinct from some other social reality
> (Bowles & Coates, 1993; Keenoy, 1990; Vaughn, 1994). (p. 106)

Nevertheless, this study did not involve the minute analysis of literary tropes

(metaphor, metonymy, synecdoche, and irony) which characterizes this approach

according to Putnam and Fairhurst (2001). That is, I did not read the three types of texts

("data") in search for such tropes (even though I sometimes pointed to them in chapter

IV to make a point). Rather, I developed themes based on an iterative process of

reading and writing; first, with only the interview questions (see appendix A and B) in

mind and, then, based on the ideas discussed in chapter II, in particular the two

research questions developed in that chapter. Additionally, I engaged in the practice of

translation when asking participants to make sense of their own behavior or to reflect

on an idea, presupposition, theoretical premise, belief, or value. For example, I asked

participants how Eco's (1992, 1994a/b) ideas on writing and reading could be applied

to their practice of textwork. After conducting several interviews, I also asked

participants to reflect on ideas that other participants put forward—without revealing their identity.

To interpret the interview transcripts, I entered each transcript into a software program called *QSR NUD*IST VIVO* (hereafter referred to as *NVIVO*). This program has been designed to organize texts into themes and enables users to highlight and code portions of the text while passing through a particular document. These highlighted and coded passages enable the development of themes; that is, discursive reoccurrences, coherent topics, or trains of thought and conversation.

I used this program as an instrument for organizing codes of text, even though the organization itself depended on my *habitus*; the ideas and arguments discussed in chapter II; participants' publications; the journal; and so forth. Thus, I mainly used *NVIVO* to sort through the transcripts and develop an initial, provisional sense of possible themes. To do this, I first divided each conversation into "units" of information that could be interpreted "autonomously;" separate from the complete text of the interview transcript. This intervention showed, as Bourdieu has pointed out many times, that the constitution of knowledge depends on several breaks; for example, the break between actual practice and talking about practice in a non-routine situation (as Alvesson [2003] suggests); the break between language-in-action-and-context (discourse), text, and broken-up-coded-text. In turn, I reconstituted the coded

fragments as a textual whole in chapter IV to suggest a coherence which denies the breaks I induced to arrive at this reconstituted whole.[16]

After the "unitization" of the transcripts, I began to classify each "unit" according to its content-similarity or theme by applying implicit rules. Appendix C shows the list of the thirty-three initial labels which I developed to name these preliminary themes. After repeatedly reading the three textualizations with these preliminary themes in mind, I collapsed and altered this list to develop a provisional typology of major themes and subthemes (see appendix D). As appendix D shows, this typology consisted of four main themes: "Knowledge Production as Discovery and Invention;" "Positions in the Subfield of Organizational Communication Studies;" "Reflections on a Scholarly Life;" and "The Practice of Textwork."

While developing this typology, I also began to locate and read specific academic texts that could help to make sense of these themes. In so doing, I decided to delimit my study by concentrating on the relationship between positioning in a subfield of study through the practice of language (and textwork in particular). Accordingly, I moved several of the subthemes implicated in "Reflections on a Scholarly Life" (see Appendix D) to the background. My main reason for doing this was that other scholars have already reflected extensively on living the scholarly life. Examples are: Peter Frost's and Susan Taylor's (1996) *Rhythms of Academic Life*; an issue of *Communication*

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[16] I sometimes constructed the interview transcripts passages in chapter IV as a dialogue in order to somewhat counter this illusion. Accordingly, I provided parts of the conversational context (e.g., the question I posed, reactions, etc.) rather than only some textual snippets.

Theory edited by Patricia Geist (1999) on being (dis)enchanted with academia; and, more fictional (but nonetheless striking): David Lodge's (1984) *Small World*, Richard Russo's (1997) *Straight Man*, or Jane Smiley's (1995) *Moo*. I sporadically incorporated some relevant ideas from this theme in chapter IV.

Bourdieu's work provided the best lens for this investigation, because it addressed most of the themes classified in appendix D. In addition, Wittgenstein's ideas about language-games provided a useful complement to Bourdieu's work, because of this study's emphasis on the scholastic practice of language in a subfield of study (correspondingly, I coined the term "antinomic language-game" to fuse Bourdieu's and Wittgenstein's conception of games). I used Bourdieu's central concepts (translated and extended in chapter II: field, *habitus* and *doxa*, forms of capital, *illusio*, and antinomic games) and some of Wittgenstein's concepts (explained in chapter I: language-games and "rule"-following) to transform my initial typology into a definitive typology. This typology contained three main themes (each implying one or more antinomic language-games): "The Ontological Complicity between Scholars and the Subfield," "Positioning in the Subfield via Textwork," and "The Discovery/Invention of Knowledge" (see appendix E for an outline of the complete typology).

This final typology did not cover exactly all the information from the themes in the initial typology (appendix D). Moreover, because the antinomic language-games interact closely with each other, I frequently failed to "color between the lines," so to speak. That is, due to their mutual inexclusiveness, it often was not possible to discuss

one game without discussing another. Discussions therefore involved a continuous "foregrounding" and "backgrounding" of these games.

As the reader will notice in chapter IV, in response to the first research question, the first theme focuses on the dialectical relationship between participants and the subfield of organizational communication studies. It more or less covers the theme of "Positions in the Field of Organizational Communication Studies" from the initial typology (the reader might compare appendix D with appendix E). The second theme, responding to the second research question, centers more precisely on participants' textualization practices enacted to constitute an identifiable position in the field. Here, I used information from the "Positions in the Field of Organizational Communication Studies" theme and the "Practice of Textwork" theme. Finally, the third theme, "The Discovery/Invention of Knowledge," can be seen as a theme which overarches the previous two themes and comprised information from the theme with the same name of the initial typology. Hence, I conclude chapter IV with an exposition on this broader theme.

After having discussed the practice of "data" construction and analysis, I now conclude by summarizing this chapter.

Conclusions

In this chapter, I discussed the textwork conducted for this study in light of the *habitus* of organizational communication studies. In the first part of the chapter, I explained my reasons for examining this subfield of study. Then, I reflected in depth

on the *habitus* of organizational communication studies by constructing the history of this subfield in the form of five antinomic language-games. These games concerned struggles about: the field's distinction/indistinction from related fields and subfields; the distinction/indistinction between rhetorical and social scientific research; the distinction/indistinction between theory and practice; the relationship between neopositivist/interpretivist/critical orientations to the study of organizational communication; and sustaining the field's coherence while fostering its eclecticism. I argued that these games have helped to distinguish organizational communication studies from other fields and subfields, and they have played an important role in the development of this subfield's system of dispositions.

After this, I explicated the production of three types of text (interview transcripts, academic publications, and journal scripts), which formed the "data" for this inquiry. Finally, I discussed my translation of these texts from a particular point of view which I enacted by relying on reading and extending Bourdieu's and Wittgenstein's work.

In the next chapter, I present and reflect on the outcomes of the data construction and translation practices which I have discussed in this chapter.

CHAPTER IV

EXPOSITIONS ON POSITIONING IN A SUBFIELD OF STUDY

In this chapter, I present my expositions on the two themes which I developed by analyzing the interview transcripts, publications, and journal scripts (see chapter III). In light of the first research question, I reflect on the way participants[1] and the subfield of organizational communication studies constitute each other through the general practice of language. More precisely, this discussion focuses on the antinomic language-games through which a participant and the subfield constitute a relationship of ontological complicity; the role of capital in this constitution; and the effects of this ontological complicity on the production of knowledge.

Subsequently, I reflect on the way a participant positions himself or herself in the subfield of organizational communication studies through textwork (research question two). This discussion addresses what antinomic language-games characterize this positioning; what forms of capital play a role in this positioning; and how this positioning affects the production of knowledge.

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[1] Frequently, I have used expressions such as "some participants" or "several individuals." This makes it impossible for the reader to know to how many participants exactly the text is referring. That is, often, it was impossible to say precisely how many participants' discourses supported an argument. In turn, I sometimes tried to provide the reader with a "rough estimate," using words like "most" or "a few." I realize that differences in the "amount of evidence" I provide have different persuasive effects on the reader (this also depends, of course, on the reader's *habitus*).

Thereafter, I summarize the inferences I have made in these two sections. Based on this summary, I conclude that the antinomic language-game about the discovery/invention of knowledge underpins most of the games I address in this chapter. I therefore discuss this language-game in greater depth in the final section.

The Ontological Complicity between Scholars and the Subfield

My first discussion deals with the investigation of Bourdieu's conception of the ontological complicity between agent and subfield, enacted through an agent's embodied system of dispositions or *habitus* (in line with the first research question formulated in chapter II). Three kinds of antinomic language-games, each involving particular kinds of struggles for capital, explain the relationship between scholar and the subfield, and its effect on the development of knowledge. Each of these games centers on a particular antinomy: I/*doxa*; I/we; and we/they, respectively.

The first game focuses on the way an individual scholar ("I") more or less embodies the subfield's *doxa*, its fundamental presuppositions through the *habitus* and thus operates according to the subfield's "conscience" (or "superego") by operating from an "individual" conscience. This game is played with the acquisition of social and symbolic capital as its main stakes, although the game frequently prevents reflection on this contest by appealing to stakes that extend beyond capital gain.

The second game, revolving around "I" and "we," suggests that scholars speak of "we" ("the field," "the community") when referring to themselves ("I"). By using this metaphor, I suggest that participants establish a valuable (and more or less valued)

relationship with themselves by implicating themselves in the subfield through their language. This is counter-intuitive to common belief that research is a relatively solitary practice and suggests that social and symbolic capital is an important factor in the production of knowledge.

Third, the final game suggests that scholars constitute an antinomy between "we" (participants' primary subfield) and "they" or "others" (i.e., other fields and subfields of study; "not we/us"). Although it is fairly well known that the specialization or "autonomization" of fields leads to competition between fields, this study indicates that scholars' antinomic language-games contributes to the debates about the distinction/indistinction between social spaces and affects the practice of research. Here, the contest for social and symbolic capital seems evident, too, because participants strife to be well embedded and reputable in their own subfield. However, being linked into networks of other fields and subfields of study is also seen as an asset, as long as a scholar does not lose his or her ties with the primary subfield.

Each of these three games affects the subfield's development of knowledge. That is, each of these games conceals some of the underlying motivations for why scholars participate in the enactment of this subfield (besides the fact that they are interested in better understanding a more or less common object of knowledge). Hence, I suggest, these games make it difficult for the field to practice Bourdieu's kind of reflexivity.

In the following sections, I discuss and illustrate the three games. First things first, I start with the I/*doxa* antinomy.

I/*Doxa*

Participants' discourses suggest that they tend to diminish the importance of their own positioning or identity practices through euphemization in speech and text.[2] They thereby balance their personal expressive interests against the *doxa* of the subfield[3] which censors the foregrounding of self (which can be seen as part of the enactment of scholars' expressive interests). *Doxa* functions, in a sense, as the field's "superego" (which may be seen as a collective biographical illusion). In other words, it functions as a conscience which is embodied through the *habitus* and which affects scholars' expressions of their "personal" biographical illusions (i.e., their "egos" or "I's"). Parallel to this, I note that participants' spoken (and written) language seldom suggests *hubris*. They express themselves with a sense of modesty instead of the self-importance

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[2] I notice this tendency in my own language as well and frequently feel compelled to write with the provisionality that I have learned to associate with operating in the field of academia and the subfield of organizational communication studies more particularly. My linguistic *habitus*, then, dispositions me to write by inserting terms like "in a sense," "perhaps," "from this perspective," "sometimes," "it seems," "in part," and so forth, to euphemize my expressions and identify my position in ways that are more or less appropriate to the way academics express themselves. Moreover, while writing, I continuously notice my tendency to constrain "myself," that is, my expressive interest, by working vis-à-vis the field's textwork conventions which increasingly are becoming part of my own dispositions.

[3] One can speak of the *doxa* and *habitus* of the subfield of organizational communication studies, but I want to remind the reader that fields and subfields naturally are closely interconnected and imbricated. Consequently, the *doxa* and *habitus* of a subfield like organizational communication studies is closely linked (and more or less homologous) to the *doxa* and *habitus* of communication studies and, even more abstractly, of the overall field of academia. In chapter III, I already pointed out that this investigation is limited to one subfield. Hence, I am unable to draw precise inferences about the effects of the interconnectedness between the subfield under investigation and other fields and subfields.

that is stereotypically associated with academics. Frequently, participants seem to be

aware of this stereotype. Participant 2 notes, for instance, that "academics are way too

invested [indicative of *illusio*] in what they do…way too ego-driven" (I.2A, 746-750).[4]

Correspondingly, he constitutes his own *illusio* by stating that he "does not take himself

too seriously" and does not "hold too much score by the reputation he has as a

scholar," which may exemplify the idea of euphemization and the operation of the

academic *doxa*:

> *Participant 2*: [O]ne of the things that, you know, I always like to do is just…is
> not take myself very seriously, right? [I try] to sort of…in a way…kind of not,
> not believe or not hold too much score by, you know, the reputation that one
> has as a scholar. That's not to say that I don't take scholarship seriously, but
> that I sort of have, you know, and I have recognized that what we do is sort of
> an infinitesimal drop in the bucket with scholarship as a whole. And so, you
> know, I mean, what I do as a scholar is just…it's an important part of my life,
> but it's just, it's one small aspect and I just think it's really very important not to
> get caught up in the scholarly life too much, because then I do think you sort of
> lose perspective…I mean, if you can make a little sort of a difference, that's
> great, but I mean ultimately, you know, what I do is not world changing, you
> know. Maybe in some tiny way it might well be, but I think it's just really

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[4] From here onwards, I indicate passages taken from the interview transcripts by the
following code: (I[nterview].[participant # + first [A] or second [B] interview],
[transcript line #]). Each participant is designated by an individual number (e.g.,
"participant 2") to create a sense of anonymity—creating a fictional proper name
seemed inappropriate, because naming someone is quite a "personal" affair
(particularly also in light of Bourdieu's [2000b] discussion). Each participant, then, is
constituted as a "personality" (in Bourdieu's [2000b] sense]), created through the
selection of textual passages from interview transcripts. Through the interaction
between my textwork (writing in a certain style, reading the transcripts in a certain
way, selecting transcript passages, conversing with my idea of a participant's
biographical illusion, etc.) and the reader's (someone other than me) translation, a
participant's identity is constituted as a more or less identifiable position in this text vis-
à-vis other participants (other "personalities"). Thus, each participant constitutes an
"epistemic individual" (Bourdieu, 1988).

important to adopt sort of a, you know, a healthily ironic orientation. (I.2A, 750-771)

This passage is interesting, because participant 2's words (e.g., "I do not take myself too seriously") illustrate how he linguistically constitutes his position in the larger field of academia (and organizational communication studies in particular) by enacting a particular relationship with himself. This enactment takes place in light of a relationship with a superego which, I believe, partly reflects the larger *doxa* of the field of academia and the subfield of study in which participant 2 operates. It might be commonsensical to expect this mutual complicity, but it might be less expected that this practice of language resonates with a scholar's contest for social and especially symbolic capital. That is, by euphemizing his discourse, participant 2 (like many of the participants) is able to identify and strengthen his position in a way that is legitimate and respectable. It allows him, in a sense, to put *himself* (his identity) on the field's linguistic market and it provides him with the ability and opportunity to speak (to have a voice) which improves his linguistic capital. I observe this relating to oneself in other instances as well.

For example, upon reflecting on an important shift in the way participant 1 approaches research, she says that she gave herself "the *permission* to do something other than [the approach for which she had become known]" (I.1B, 696-697). Also here, the participant constitutes a relationship with herself, but suggests that her superego *allows* her self to alter the way she practices research. In light of this example, then, the subfield's *habitus* seems to function as the arbiter of individual scholarly actions by

giving scholars the impression that their actions are "the way they're supposed to be," "natural," "inevitable," and driven by "fire in the belly" (I.1A, 76) or "passion" (I.9A, 845) rather than mere strategic. In fact, participant 2 believes that being strategic is unwise:

> *Participant 2*: I actually think being strategic is a bad decision. It always comes back and bites you in the ass [laughing]. I mean, I think you just have to do…what's interesting for you and do it to the best of your abilities. You know, obviously, I mean, you can be strategic with a capital "S" and strategic with…you can be strategic in terms of like: 'Okay, you know, I probably should submit a paper to this conference,' but, you know, in terms of the bigger issues like deciding what you should research, you know, what your areas of interest should be, I mean I think it's a bad idea. (I.2A, 927-948)

The impression that a scholar's academic trajectory occurs "naturally" or "serendipitously,"[5] which, in a way, is implied in the "non-urgency" of the field of *skholè*, reinforces the idea that a scholar works for "the common good" and conceals his or her "capitalist" tendencies. Participant 2's discourse suggests no great concern for economic, cultural, social, or symbolic capital. In spite of this, he does speak of not wanting to become "caught up in the life of scholarship too much," implying an awareness of these capitalist struggles.[6]

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[5] Participants 2, 9, 10, and 12 each suggest that certain parts of their career developed serendipitously.

[6] Of course, the discourse which participant 2 presents in this interview situation might also simply be an indication of impression management. My study suggests that this impression management also occurs outside interview settings; that is, as part of academic practice (e.g., through textwork), implying that scholars monitor their expressive interest (the way they are perceived) vis-à-vis the conventions of the subfield and the evaluations of colleagues.

Overall, academic language-games like these often disguise scholars' *illusio*, their investment in the game of scholarship. In fact, the academic *doxa* seems to state that it is best to downplay this competition and its influence on the production of knowledge in order to create the impression that scholarship is autonomous from other fields, such as the fields of politics and economics. In so doing, a field legitimizes its place in society by making itself seem less focused on furthering itself by using scarce resources which all people in a society need and, often, help to produce.

Correspondingly, these scholarly language-games often prevent reflection on the *doxa* of academia. That is, I notice that participants vary in the extent to which they reflect on and recognize the idea that their work positions them in fields and subfields. Some participants, for example, are skeptically about placing one's own subfield under the microscope:

> *Participant 7*: Hey, you know, I do have one question for you. It's about your project. You know, you could be criticized for spending too much time focusing inside the discipline. How would you respond to that? (I.7B, 727-729)

Participant 3, in turn, believes that over-engaging in reflexivity may interfere with academic work:

> *Participant 3*: I get a headache doing that stuff. That level of self-reflexivity, you know, I get a headache, eventually. And the way I deal with it is eventually say: 'I'm going to stop. I'll go and get my work done.' (I.3A, 311-316)
>
> *Boris*: So reflexiveness can go too far?
> *Participant 3*: Yeah. Do work! Now! Every now and then do it, but don't make reflexivity your life. You'll drive yourself nuts and everybody around you. (I.3A, 706-709)

Statements like these illustrate that the academic *doxa* and *illusio* have become

appropriated in such ways that the superego of participants does not permit reflection

on the language-games they enact. Interesting, here, is that the subfield enacts

language-game "rules" which prevent the questioning of these rules themselves. In so

doing, I believe that the subfield prevents the game from being "given away." Thus,

the subfield's *doxa* inhibits its own alteration (sustenance, progression, degeneration).

Nevertheless, some participants seem to "push the envelope," so to speak, by

reflecting more readily on the subfield's enactment of these language-games. This

suggests that some participants relate their selves in a somewhat different way to the

subfield's *doxa*. Correspondingly, their *habitus* disposes them in such ways that

questioning their own system of dispositions and their investment in taking position in

the subfield seems an appropriate and important part of academic practice. Participant

2 says, for instance:

> *Participant 2*: [W]hat is interesting about this study is that, it's really an interesting example of, you know, the ways in which…and I think one of the most important things we do as researchers is to sort of, is to study the taken-for-granted, right? It's kind of study the commonsense practices that people generally aren't reflectively engaged in. And, you know, the idea of sort of turning the lens on ourselves, and looking at, you know, the process of research, scholarly practice as, you know, largely, you know, taken-for-granted process, I think is really useful. And I was thinking…thinking back on our first interview, how in some ways quite difficult some of the questions were to answer. And I think that had much less to do with the questions, but more to do with just, you know, a general kind of, perhaps, lack of reflexivity that all of us have in terms of…we all come out of particular, you know, scholarly communities and we all in a fairly early point in our training try to internalize the principles of our academic community, and I think probably…I am sort of almost twenty years now beyond my Ph.D. training, you forget how, sort of how…how…revelatory that stuff was initially when you were learning it. It just sort of becomes a way of feeling…of talking, and way of thinking and experiencing and practicing.

And so to sort of sit down and consciously reflect is a really interesting and beneficial and difficult thing to do. I mean it's…yeah it's just…I really don't mean, and I could tell when I was answering some of your questions…I certainly wasn't making stuff up as I went along, but it was like, I was trying to sort of really walk carefully through the issues that were coming to mind and sort of trying to make connections that I hadn't perhaps thought about for a good while. I mean it was certainly sort of an interesting exercise in self-reflexivity. (I.2B, 10-34)

Participant 2 is relatively conscious about his *habitus* (illustrated by words like: "[we] internalize the principles of our academic community; it just sort of becomes a way of feeling, of talking, and way of thinking and experiencing and practicing"), although it is impossible to find out how this consciousness operates in the actual practice of his research. He also thinks about the possible effects of his work on society, although he realizes that constant thought about the utility of his work could easily lead to a kind of inertia (perhaps this still reflects traces of the *doxa* which states that a scholar should not over-engage in reflection on *doxa*):

> *Participant 2*: I think any time, any time you put pen to paper, I mean there's always, I mean, you have to recognize, you know, the responsibility that involves, right? But, again, I mean, that doesn't take place in a vacuum…I mean, that sense of responsibility is, you know, takes place in the context of a larger group of scholars who serve as the, you know, the initial community to which you're speaking. (I.2B, 440-446)

Participant 2 acknowledges that his sense of responsibility depends in part on the relationship he constitutes with the subfield (on the relationship between ego and superego). Due to this consciousness, he reflects on *doxa* rather than merely being subsumed by it.

To summarize the previous discussion, I have suggested that the subfield of organizational communication studies constitutes itself through relatively opposing forces between those who sustain the subfield's *doxa* and those who question it. Through this antinomic language-game, participants relate themselves (their "I") to the *doxa* of the subfield in more or less distinct ways and they more or less constitute the field's presuppositions as their own. This game affects the development of knowledge in the subfield because it constitutes a controversy about the extent to which the subfield (and thus individual scholars) should reflect on its own *doxa*. For example, participant 12 associates this kind of reflexivity with the practices of a philosopher rather than with a field researcher:

> *Participant 12*: [Y]ou know, I suspect that I would be more self-reflexive, you know, if I had chosen philosophy as a topic, but I'm a field researcher primarily…and, you know, I'm very much about trying to understand *other* people's lives [laughing]…You know, I live in my life. I don't need to spend huge amounts of time trying to understand it. (I.12B, 640-650)

In other words, participant 12 associates her biographical illusion with a position in the subfield of organizational communication studies ("a field researcher"), but this association is accomplished in part by contrasting this position with a position in a different field (philosophy). Further, it is interesting that participant 12 tries to investigate *other* people's lives. This suggests that she does not think much about the social conditions which make her identity as a scholar in a subfield possible, let alone about the notion that the *distinction* between scholarly "subject" and research "subject" is what largely enables her scholarly "subjectivity."

Language-games like these, then, reinforce a *doxa* which gives reflexivity a special connotation *vis-à-vis* the practice of research by implying that there is a dichotomy between "research" and "reflection on research." This antinomy makes reflection on the social conditions which make research (and researchers) possible seem unnecessary, impractical, or self-indulgent. Because scholars embody this *doxa* in their *habitus*, they thus develop a "norm" or "standard" for the practice of reflexivity which keeps reflection on *doxa* at bay. Accordingly, because this kind of reflexivity is not valued by the subfield, a scholar cannot gain much linguistic and symbolic capital by practicing this kind of reflexivity in speech or text. Hence, by limiting the expression and thought of individual scholars, the subfield inhibits conversation about the way the subfield constitutes particular conditions which promote certain kinds of scholarly dispositions and positions. As the next passage indicates, for example, it is relatively difficult to engage the subfield of organizational communication in a systematic discussion about the historically enacted antinomy between theoretical and practical work:

> *Participant 11*: I do know some people who have left org. comm., have ceased to be active at org. comm. business meetings, at conferences, and so forth, precisely because they're disappointed in that regard, that they want more direct contact with social problems, as opposed to a more distant theorizing and reflection about that. I mean, it's ironic; at times, for example, that lots of people will write about praxis [laughing], but yet there's not much engagement for people's lived experiences. So, I mean, and of course there's a role for theorizing about praxis…but that would be where I would want to see things go further. (I.11A, 345-353)

In what follows, I continue my discussion about the way the subfield fences off reflexivity by turning to the second language-game which closely relates to this first language-game. The second game focuses on the way participants implicate themselves in the subfield by constituting "I" in light of "we."

I/We[7]

Participants' discourses suggest that they try to enact a position which is identifiably distinct from other positions, but never so distinct that they lose social and symbolic capital and are no longer implicated in (or identified with), in important ways, the subfield's network of positions. In light of the idea that social and symbolic capital is an important stake in the scholarly game, I observe at times how effortlessly some participants turned to "we" or a generalized other (e.g., "scholars," "the field," etc.) when responding to questions that were directed to them personally.[8] The tendency to state personal responses in the form of collective language suggests that participants identify their position vis-à-vis others in the subfield, as so many scholars have argued

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[7] Bourdieu (1998a) speaks of the dialectical relationship between "I" and "us" (p. 142).

[8] To substantiate my view, I provide the following illustrations: "a general kind of, perhaps, lack of reflexivity that all of us have" (I.2B, 22-23); "For most of us, the reason we chose this as a career, is we wanted to avoid those kinds of things [i.e., having to work towards quantifiable outcomes like managers and administrators]" (I.3B, 22-23); "I do think that we've been working to change that [censoring textwork through upholding particular textual conventions]" (I.5B, 64-65); "[In] the 70s[, we] were [in] a time when we were fighting in the field about what theory building should look" (I.10B, 125-126). Moreover, towards the end of the 24 interviews, I wrote in my journal that "[s]ome participants' discourse—textual and spoken—seems destitute of me, myself, and I" (J.08.09.03). From here onwards, I indicate passages taken from my

(see Bourdieu, 2000b; Elias, 1991, 2000; Mead, 1934; Ricoeur, 1992). Indeed, it indicates that an individual is only an individual in the company of others. Through the constitution of a biographical illusion in language, which is based on a particular *habitus*, participants seem to construe a sense of collectiveness, a connection or belongingness to organizational communication studies, communication studies, and the field of academia at-large. Hence, they identify themselves (and are identified) as "organizational communication scholars."

Yet, these pronouns do not signify this tendency alone. As the illustrations in this section will suggest, participants' general discourses indicate an inclination or disposition to see (or not see) "I" in "we." This inclination influences to what degree participants privilege (or marginalize) their own position in the subfield as well as those of others. Through language, that is, participants symbolically privilege or marginalize their position and the positions of others on a spectrum that ranges from being relatively central to being relatively marginal in the subfield. In other words, in this language-game, they constitute relations with themselves and others by perceiving their own position vis-à-vis those of others in hierarchical ways. The relationship between "I" and "we," in turn, becomes a metaphor for the way scholars implicate their selves (themselves) in the subfield.

Counter to the common presupposition that a promoted professor no longer needs to worry about his or her position in a field, I note that most participants still engage in "capitalist" struggles and, accordingly, are still invested in sustaining or

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journal by means of the following code: (J[ournal].[month].[day].[year]).

improving their position. Participant 7, for instance, constitutes his identity in

organizational communication studies as "an oddity:"

> *Participant 7*: Well, [in] this discipline, yeah, I suppose I'm something of an
> oddity…Yeah, that's the way I would put it. First of all, I'm interested in
> science. Science isn't very popular in this discipline, right now. There are still
> some people who do it, but fewer and fewer all the time it seems, and they come
> under a lot of criticism.
> *Boris*: Why do you think that is?
> *Participant 7*: Because of the politics of social research, basically.
> *Boris*: Ha…is that something you can elaborate on…?
> *Participant 7*: Well, sure, I mean, at the same time I was, you know, in the late
> 80s, a lot of critical theory was starting to come into the discipline from Europe,
> basically, and, you know, I always thought that was very interesting stuff.   You
> know, basically, I'm a political liberal and so, there's not much about the sort of
> basic ideas behind that that I disagree with. But, since then, I think it's become, I
> don't know, maybe not so inclusive, or it seems to exclude perspectives that I'm
> interested in, I suppose, and deny them and even, in some cases, call them 'evil'
> and 'corrupt' and so forth.
> *Boris*: [Yes, there has been] an interpretive turn...and so you think that that sort
> of put you in the margins as…a scientist?
> *Participant 7*: Oh sure, yeah, yeah. Yeah, definitely. (I.7A, 129-165)
>
> *Participant 7*: [I]nterpretivism…was not taught to me as some sort of an
> opposing position. You know, it was, you know, another sort of useful way of
> looking at things. Ever since then, it seems to have sort of turned into a group
> that seems intent on battling the people who, you know, favor science…And so,
> yeah, I mean, the whole thing is sort of distressing, I think. (I.7B, 270-278)

These passages suggest that a scholar's perspective towards a more or less

ambiguously defined object of knowledge sometimes can become "obsolete" in light of

a field's developments. Such changes can thus affect an agent's position (e.g., the

biographical illusion of being "a scientist") by placing him or her "offside"—as they say

in soccer; that is, in a zone of the field where someone is not supposed to be.

Participant 7 constructs his identifiable position in this zone, because he believes that

the subfield increasingly devalues the position of a social scientist (which, he believes, many people see as "evil" or "corrupt"). In light of the dialectic between a scholar's expressive interest and the subfield's censorship, participant 7 thinks that his linguistic capital has decreased (implying that it is more difficult for him to be "heard"). Concurrently, his expressive interest has become dominated by scholars who have changed "the politics of social research" (i.e., the subfield's *doxa* and, in turn, its *habitus*) by marginalizing his position.[9]

Furthermore, this passage points out that the fact that a scholar has become a professor (and thus has acquired a relatively high amount of economic, linguistic, social, and symbolic capital) does not necessarily imply that he or she is less concerned about sustaining his or her position in the field. Thus, enacting a worthwhile position implies more than simply "playing it safe" (e.g., by obtaining tenure or being promoted). The game is ongoing and often uncompromising, implying the need for continuous investment in one's position and keeping up with the relatively unpredictable shifts of a field. Research is therefore not only a matter of making a difference in terms of creating knowledge; it is also a matter of upholding a voice which is "fashionable" enough in the eyes of a particular surrounding social space.

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[9] Ironic, in this case, is that the historical move towards complementing the neopositivist approach to organizational communication studies with an interpretive and critical approach, promoted in the 1980s, now increasingly has come to define the "name of the game" and has decentralized the position taken by certain "traditionalists."

In hindsight, several participants believe that making *a* difference by being able to participate legitimately in the subfield's language-games was particularly important for them as starting scholars. For instance, participant 4 notes that "at that age, you want to be able to establish, you know, how or what it is that you are focused on, how it is distinct from what others have focused on" (I.4A, 60-62). In a similar vein, participant 2 considers that his early voice reflected a relatively naïve, idealist *credo*, especially when thinking about one of his publications:

> *Participant 2*: Here [in the publication from the beginning of his academic career], I am much more interested in taking a particular framework and applying it to a particular phenomenon…and so my focus is, I think, much more outward, is much more sort of, maybe perhaps narrowly-circumscribed in terms of attempting to explain a particular phenomenon, you know, and sort, and make a contribution to the literature, right?…And, you know, to a certain degree, it reflects, you know, relatively speaking, sort of a lack of sophistication…It's like I'm sort of: 'Okay, I've done all this training and I've got this sort of knowledge that I want to impart to the world,' [laughing] right?
> *Boris*: [laughing]
> *Participant 2*: Critical studies, "ra ra ra," right [laughing]? You know, let's critique functionalism, let's critique interpretivism, right, and so it's like: 'I've got this message for the world,' you know [laughing]? (I.2B, 113-134)

This passage suggests that a beginning scholar is, in a sense, searching for his own voice through imitation; that is, by searching for the "we" in "I" and vice versa. As far as textwork is concerned, participant 11 says, for example, that he "simply imitated, to some extent, other academic styles [i.e., other voices] and then tried to adapt those to [his] own purposes" (I.11A, 528-529). In turn, he says: "I have, I think, freed myself up

from some of the more burdensome aspects of academic styles. At least I'm trying and

I still want to keep moving that way" (I.11A, 541-543).[10]

Interesting is that the "progress" associated with an academic career is seen in

the light of *freeing* oneself from certain academic conventions (implying a "loosening

up" of the connection between I and *doxa*). Participant 11 has freed himself from a

position that no longer suits his "updated" biographical illusion. This "empowerment"

intimates a *change* in power as well: the acquisition of a significant amount of social and

symbolic capital over the years makes someone feel "freer" than before. A change in

the freedom with which someone can produce an identifiable voice also implies a

change in linguistic capital which makes such liberation of "self" possible.

Correspondingly, participant 11's words perhaps do not only signal an instantiation of

self-empowerment in the commonsensical use of the word: Participant 11's expressive

interest (as any participant's expressive interest) is partly determined by the censorship

of the subfield which he has more or less embodied during his academic trajectory.

Although participant 11's discourse suggests that he speaks from a position which

involves increased freedom of expression (i.e., less need to rely on someone else's voice

through imitation), I therefore question to what extent his new position is not partly the

result of a continued incorporation ("imitation") of the subfield's system of

dispositions. Thus, the "freeing" of self may not be entirely a freeing "of" or "from" self

nor may it be a freeing "from" "we." Instead, it seems to involve an improvement in

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[10] Similarly, participant 9 states that his "soul revolted" (I.9A, 591) when he was taught
how to practice stereotypical, traditional scientific language in the production of

the way "I" is able to *influence* "we" by more successfully defining himself *as* "we"

through the acquisition of linguistic, social, and symbolic capital.

Because the contest for capital s inherent in the language-games of academic

fields (and fields in general) and because scholars are implicated in these games

through positions which grant them different senses of self-worth, participants never

really seem to be free from this "capitalism," regardless of the amount of capital they

acquire. In accordance with this observation, I notice that several participants express

their increasing fatigue or discontent with this struggle, involving the continuous

gauging of "I" in light of "we." Participant 5 says, for example:

> *Participant 5*: [W]hen I find myself getting into this mode of talk [a mode in
> which one compares one's accomplishments with others] I realize—this is a
> realization I've made recently—is I find myself talking about how stressed out I
> am and how much work I need to do. And I'm tired of that kind of talk. I'm
> tired of it with my family and I'm tired of it with my colleagues. (I.5A, 398-401)

Although several participants are tired of their investment in this struggle,

symbolic capital remains a major driving force on the scholarly market. That is, to stay

in the game, they (like everyone else) must partake in it. Individual scholars, in turn,

cannot easily change this game without convincing a significant part of the entire

subfield that such a change would be worthwhile. For instance, participant 1 may no

longer be as much concerned with the game of "requisite numbers" (I.1A, 395), that is,

with having to publish yet another text for the sake of staying in the game. She

nonetheless still realizes (like most participants) that she "live[s] in a political

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academic texts.

environment[, where] you have to get tenure, you have to get promoted, so you do

what you have to do" (I.1B, 439-443). Now that she has gained a considerable amount

of symbolic capital, "how much contribution [her published work] leaves" (I.1A, 420)

has become a driving force for her. This suggests that the stake of the game has shifted

for her from wanting to make *a* difference to wanting to make a *durable* difference. In

this instance, I believe that the need to leave a heritage reflects the need to leave a

lasting impression which identifies participant 1's position in the subfield. In so doing,

she perhaps hopes not to be forgotten as a person. A scholar's biographical illusion (his

or her "being someone") thus seems to be closely entwined with his or her position in a

social space which grants him or her a community in which he or she is valued.[11]

Hence, the need to be someone is dependent on the need for others and contributing to

a field offers ways to contribute to one's own position (and conversely).

Overall, I conclude that the I/we language-game provides scholar with an

important starting point or "context" through which they define their relationships

with themselves and others. This language-game involves a constant positioning of "I"

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[11] Participant 4 corroborates this observation by saying: "*Participant 4*: I really feel called to encourage people to consider to expand the possibilities that they see in their life and to make connections between their emotional life and their personal life and the work that they do and, I mean, you could [say] there is a moral calling, you could also see it as very selfish, because I'm also saying [that] if I could get people to do more of that, it will make my work life more interesting, because those are the kinds of people that I want to be in community with and I think it's had some impact. I mean, I'm not the person who started all this. There's so many people within the field…who have been really doing work that has a real heart to it for a long time. And I just wanted to be a part of that community, to be asking important questions about, you know, what makes life worth living and those kinds of things" (I.4B, 472-486).

vis-à-vis "we." In turn, this game affects the way knowledge is produced in the subfield by privileging or marginalizing a *particular* "I;" that is, an "I" which expresses himself or herself within boundaries which the subfield considers appropriate and which "fits" the way "we" sees and knows "we." This gauging practice thus enables a participant to acquire linguistic, social, and symbolic capital. Hence, the subfield's system of dispositions holds itself in place by censoring its own constitutions of identities ("I's"). However, as I will illustrate in the next section, this language-game is also influenced by the language-game which revolves around the antinomy between "we" and "they."

We/They

While the I/we antinomy affects the ontological complicity between scholar and his or her primary subfield of study, a scholar often is more or less implicated in other fields and subfields of study, too. In the case of organizational communication studies, the nearest fields and subfields are social spaces like organization studies, management studies, or industrial and organizational psychology, but also subfields within communication studies, such as or health communication studies or small group communication studies.[12] Accordingly, some participants constitute their position

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[12] Moreover, participants 4, 6, and 11 reflect on the humanities/social sciences antinomy. Participant 4, for example, believes that this distinction is dysfunctional and would rather see them as "blurred genres" (Geertz, 1980), even though he acknowledges that this view has not led to an "ability to do this cross talk" (I. 4B, 613). Participant 6 believes that "we have got to retain our foothold in the social sciences. If we don't do that, then we lose all credibility with all kinds of communities and we

within organizational communication studies vis-à-vis their position in other fields like these.

Sometimes, this constitution involves distinguishing one's position from the positions scholars enact in other fields. For example, a participant might perceive his or her position in organizational communication studies to be better than the position enacted by a scholar in management studies, because this position implies a better understanding of the way people organize through symbolic processes and practices. However, this "personal" game for distinction is not merely personal, because this game finds its parallel in the historical language-games which of the subfield of organizational communication studies (as I discussed in chapter III). Accordingly, this historical struggle for distinction/indistinction from other fields and subfields seems to have become part of participants' systems of dispositions. Consequently, as participant 10's discourse illustrates, participants' enactment of the I/we language-game seems to be affected by their enactment of a we/they language-game:

> *Participant 10*: I reflect on even earlier work that I did on the attitude-behavior relationship. I was very creative in that…my dissertation…it turned out to be some award-winning work, both at the dissertation-level…and I think felt very comfortable and free to do some model-building and…but I remember being chastened by something and that was that I had introduced a concept that had a clear meaning to me and I had labeled it "locus of control," but that of course is a psychological construct that I didn't want anything to do with…
> *Boris*: [laughing]
> *Participant 10*: …but…and as a matter of fact, psychologists beat me up regularly for that…I think their construct signifies something that had a very different meaning…and so I think, if anything, that point early in my career, it taught me to stay faithful to the language use of whatever community I had

cannot affect policy changes" (I.6B, 709-711).

entered…because those things had real meaning…and…I think I've always been that way…I'd really got beat up pretty good as an assistant professor…including things…getting reviews sent back: 'What the hell were you thinking using this construct? Yeah, this is already in use'…This is almost like it has a trademark on it…and they were probably right, you know?…I may have taken it too far. (I.10B, 275-309)

This excerpt illustrates that another field's *doxa*, incorporated in this field's linguistic *habitus*, can symbolically marginalize those who do not "speak the language." Hence, participant 10 seems to have struggled with positioning his voice in a particular field and has learned to "stay *faithful* to the language of a particular community" (indicating a fidelity towards the *doxa* of the field in which one operates). Participant 1's discourse, though, suggests that she is less willing to change her position to legitimately operate in another field (in this case, management studies), because she believes that management scholars are "parochial:"

> *Participant 1*: It's just that their [scholars in the field of management studies] orientations are so different, the languages are different, and so here you really, you almost…uh…I don't know how to put it exactly, but…it's very much…uh…a simple, clear baby-steps that you have to take with these people and overexplain every damn thing, okay? Because they just…but they think communication is soft and squishy and something they don't wanna deal with, so you have to make that palatable. (I.1B, 281-291)

> *Participant 1*: Uh…I will also get mad and choose not to publish…like I'm increasingly doing less with the management audiences…you know…because a part of me just gets mad at them and gets mad at their parochialism. (I.1B, 607-613)

Striking about these passages is the obvious constitution of the field of management studies as an "other" field ("other" in the sense of being other than I/me and "other" than us/we). In this particular instance, this otherness is associated with parochialism

(which might simply imply a difference in *habitus*). As far as textwork is concerned, the constitution of the we/they antinomy affects how a text is constructed by focusing on making organizational communication concepts, theories, and methods "palatable." Correspondingly, participant 1 constitutes her model reader in management studies as a pupil rather than a peer.

Participant 1's insertion of the word "palatable" is intriguing, because it assumes that the production of a text is not solely determined by the content of research. That is, participant 1's passage suggests that scholars position their texts (and their textual voices) through textual forms which are acceptable to the "taste"[13] of a field. In turn, this indicates that publishing involves the marketing of textual products for a more or less well known social space. In accordance with this argument, participant 1 indicates having more trust in editors and reviewers from her primary subfield (organizational communication studies) than editors and reviewers from management studies:

> *Participant 1*: I know and trust the editors [of a particular journal in the field of organizational communication]. And they're not gonna let me get away with anything. They're gonna tell it to me straight and, but when they tell me something…because I know where they're coming from, I know their background, I know who they are, it makes a difference, you know, it has more weight with me than somebody from a management discipline, who, I think is…certain individuals would be somewhat parochial and, you know, have their own axe to grind and less likely to want to respond. (I.1B, 861-871)

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[13] In his book *Distinction*, in particular, Bourdieu (1984) explicates the notion of taste (cultural capital) in light of a field's *habitus*.

Surprising is that participant 1 does not assume that organizational

communication scholars also "have their own axe to grind." This passage suggests,

therefore, that a field of study constitutes a playground on which scholars compete for

capital, but that this playground also offers a "safe haven" with room for trust,

collegiality, and honesty. The familiarity of a field's *habitus* thus provides a scholar a

sense of identification and belongingness—a familiarity which only can be experienced

when operating with a similar *illusio* in the field (e.g., when writing for a field's

journals, attending its conferences, etc.).[14]

Moreover, participant 1 believes that "there is less of a need to prove…[herself]

…to…[the management studies'] audience" (I.1B, 613-614), intimating that she used to

feel this need more in the past and implying that her biographical illusion at first

depended on proving that her work was valuable for this field. In light of this remark,

participant 1 states at the end of our second conversation that, after having participated

in this study, she now better understands her academic trajectory:

> *Boris*: [With s]o many things that you are saying, um, legitimization of the
> discipline…of yourself as a scholar…seems…important…
> *Participant 1*: Yes, yes, absolutely, and actually I hadn't fully drawn this as a sort
> of theme throughout my academic life [prior to the interviews], but I have felt

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[14] In accordance with the notion that a field of study constitutes a social space in which scholars constitute an identifiable position based on their dispositions, I recount a witty dream which I dreamt on November 5, 2002. That year's annual meeting of the "*National Communication Association* was [held] in Plato's cave and I was to serve drinks to professors" (J.11.05.02). This dream could be analyzed as follows: I constitute the field of communication studies as encompassing my "I" like a cave. Reality is outside the cave, whereas the field (inside) is engaged in language-games which involve the interpretation of this reality's reflections. Within this cave, I am a servant who is trying to gain different forms of capital in order to become like "the others."

this underdog mentality. For a long time, probably because I…I…I started out with so many colleagues in management who devalued communication so much and then to hear people talk about, you know, just sort of the general barbs about being a comm. major, uh, along the way, with our students…I have felt passionate about that legitimization. (I.1B, 981-1040)

This reflection implies that participant 1 operates from a biographical illusion of being "the underdog." She constitutes this position for herself and the subfield of organizational communication studies ("I" and "we") vis-à-vis other fields and subfields of study, but vis-à-vis the social field in which (undergraduate) students do not readily see the importance of communication research. More interesting, perhaps, is that participant 1's feelings of being an underdog partly find their origin in the historical developments of organizational communication studies; for example, in the struggle to distinguish the subfield's efforts from popular writing or the research produced by a field like management studies. I further support my observation through a passage taken from my second interview with participant 10:

> *Boris*: Mm…so in that regard seeing yourself as a knowledge creator…you're doing that far more for sort of…the world…or for the community…or anything like that…you're really interested in creating knowledge that is helpful to other people?
> *Participant 10*: Precisely…that's exactly right…and it's not just helpful, but there's a second order effect here, which is trying to help build the credibility of the community…because, you know, I have no doubt that we as a discipline have really improved in our status over the last ten to twenty years, since I was…well, thirty years…since I was a grad. student. Communication was not completely known and understood…it was seen as only having its roots in speech. There were isolated good researchers in the eyes of people in other disciplines, but I think we have slowly and systematically created a meaningful corpus of work that is recognized by peers across disciplines. We positioned some of our own work there, but it's also an area where they have come to our work to look for findings and knowledge…and it gives me a real thrill to know that some of what I've done has not just been of help to us, but of help to

them…and in the process of doing that…in fact, if there's any ego here, I think the ego has been the sense of trying to be a team member who builds a team identification and perhaps at times team victories…and if I leave the field fifteen years from now and it is stronger and continues to be stronger and accepted in much the way more traditional fields are, you know, then that will be very satisfying to me…That's why I think, if I think strategically about audiences, too, I think strategically about when I'm gonna put work in another discipline and what voice I wanna have and how much I have to accommodate to their language…and to their structures and how much I wanna bring communication…and sometimes I'm very strategic in the kinds of references I make in the bibliography. (I.10B, 913-957)

Participant 10's words ("credibility" in particular) suggest that there is a relatively direct link between the symbolic capital of the subfield of organizational communication studies and his personal symbolic capital. In fact, it seems that several of the scholars with whom I have spoken take the status of the subfield to heart. Thus, the subfield indeed seems to be embodied in their *habitus*, making it seem as if their personal actions, motivations, perceptions, and so on, are (like) those of the subfield (as I also mentioned in the I/we section). In turn, participant 10 suggests that his "team participation" has not only contributed to furthering organizational communication studies, but also related fields ("it gives me a real thrill to know that some of what I've done has not just been of help to us, but of help to them") which he seems to use as points of comparison. Hence, "if he leaves the field in fifteen years, he hopes the field is stronger and accepted in much the way more traditional fields are strong and accepted."

Based on statements like these, I believe that the connection between a scholar's *habitus* and the subfield becomes stronger (although perhaps less noticeable) as he or she becomes more entrenched in the stakes of a given social space. At the same time,

this connection also seems to be strengthened between systems of dispositions across

different fields and subfields.[15]

Nonetheless, not all participants constitute as great a concern for distinguishing

organizational communication studies from other fields. For instance, participant 8

states:

> *Participant 8*: I think some, and occasionally worry, about the role of
> communication as a discipline in these context[s]. I worry about it, or at least
> think about it, because I didn't come from communication. A lot of what I do is
> also done very well by folks in other disciplines: information science,
> management, social psychology, psychology in general, sociology, in some
> cases, economics, in some cases. In some cases, there's a lot of overlap between
> what we do in our field and what they do. In some cases, maybe the overlap
> between what I do and what my friends in management school or in psychology
> field is higher than the overlap between what I do and some of my friends in
> communication do. Which doesn't bother me. (I.8A, 216-226)

I explain participant 8's response by reflecting on his *habitus* and the content of his

research. He "did not come from communication studies." Hence, the way he practices

research is, as he suspects himself, "closer to what a more traditional [social] scientist

[does]" (I.8B, 594). In addition, participant 8's *habitus* (more than some of the other

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[15] Correspondingly also I am educated and socialized to distinguish the object of
(organizational) communication studies from other fields' objects of knowledge. This
education and socialization succeeds all the better, I believe, because these practices link
me (my "I") to the subfield which defines this object. Put differently, the I/we
antinomy creates the impression that the relationship which I constitute with myself
depends on the relationship which "I" constitutes with "we," yet also on the
relationship which "I" constitutes with "they" (via "we"). Hence, as participant 10's
discourse indicates, as a scholar's relation with the subfield becomes stronger, the
subfield's victory increasingly becomes my victory. In this respect, for example,
participant 10's academic trajectory more or less parallels the trajectory of the subfield
over the last 30 years.

participants) resonates with the system of dispositions of scholars in certain other fields.
It is perhaps due to this similarity that participant 8 perceives less contest with scholars
in other fields. That is, by practicing a "practical metalanguage" (Bourdieu, 1991a, p.
85) and supported by a relatively high degree of social capital in other fields,
participant 8 is able to more easily able to traverse different academic social spaces and
practice research. Thus, his positions in different fields are constituted by practicing
research in ways that are relatively traditional and thus more accepted in these different
social spaces. This kind of agility provides him, in turn, with symbolic, linguistic, and
social capital within and across these fields.

In conclusion, it seems that "other" fields and subfields of study play an
important role in the language-games through which participants produce knowledge
and, at the same time, constitute their positions in the subfield of organizational
communication studies. Some scholars seem more versatile than others in terms of
constituting a position in these other fields. In this regard, success seems to depend in
part on the degree of similarity between a scholars' *habitus* and the *habitus* of scholars in
these other fields.

With regard to organizational communication studies, the historical language-
game of distinction and indistinction from neighboring fields (see chapter III) has
influenced the we/they antinomy in particular and still seems to have significant
bearings on scholars in current times. While some of the participants (e.g., participant
7) who perceive themselves as "traditional social scientists" see the move of the subfield
towards more interpretive and critical approaches as a threat to their positions, others

who perceive themselves in similar ways (e.g., participant 8) seem able to bolster their

symbolic, social, and linguistic capital in organizational communication by successfully

and simultaneously positioning themselves in other fields. Accordingly, peers in

organizational communication studies see the relatively high amounts of capital which

these scholars have acquired in other fields as valuable to their own field. This is so,

because such kind of success helps to enhance the importance of the subfield in relation

to these other fields and reinforces the legitimacy of its existence. It is perhaps true,

then, that those who are best able to master the language-game of their own field and

the language-games of various other fields of study are most able to acquire linguistic,

social, and symbolic capital across fields. In this case, their expressive interest more or

less "matches" the conventions of other fields and, consequently, they perceive less

censorship than those with different kinds of expressive interests.

I conclude that the we/they games frequently lead to controversies between

fields and subfields which are more or less fruitful (or perhaps more or less fruitless),

because in these debates scholars often try to defend their own turf. This observation is

not new, but this study suggests that these struggles are not only competitions about

knowledge; rather, they involve struggles for capital which are, at the same time,

"personal" and "social." As Bourdieu (2000a) states, to improve the way in which

knowledge is produced in a field, it is therefore important that scholars subordinate

their "'selfish' interests to the rules of dialogic confrontation,"[16] both within and across

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[16] Bourdieu (2000a) explicates this point by writing that "[e]ach individual conquest of
reflexivity (such as the discovery of the scholastic illusion) is destined by the logic of

fields and subfields of study.

In the next section, I reflect in more depth on the way the dialectic between a scholar's expressive interest and the subfield's censorship is enacted by examining scholars' reflections on their textwork. I have organized this section around two sets of highly imbricated antinomic language-games, one pertaining to the way participants enact a position in the subfield of organizational communication studies by voicing a text, the other pertaining to the way participants enact a position in the subfield by voicing the production of knowledge.

The Constitution of a Position in the Subfield via Textwork

After having discussed the ontological complicity between participants and the subfield of organizational communication studies, I now turn to a discussion of a set of antinomic language-games which concern the way participants enact an identifiable position in this subfield (and related social spaces) through their textwork. In so doing, I continue to contemplate the influence of "capitalist" struggles on this process and their effects on the production of knowledge (in line with the second research question

competition to become a weapon in the scientific struggle and so to become a necessity for all those engaged in it. No one can forge weapons to be used against his opponents without having those weapons immediately used against him by them or by others. It is from this social logic, and not some illusory, sanctimonious deontology, that one can expect progress towards greater reflexivity, imposed by the effects of mutual objectification and not simple and more or less narcissistic turning of subjectivities upon themselves. The scientific statement of the logic of the functioning of the scientific field can also help, by making it more conscious and more systematic, to arm the mutual surveillance within the field and so to strengthen its efficacy (which does not rule out cynical uses of the knowledge thereby offered)" (p. 119).

formulated in chapter II).

I have organized this section around two sets of closely intertwined antinomic language-games. The first set consists of two language-games which pertain to the way participants enact a position in organizational communication studies by voicing a text in particular ways. The first game centers on the degree of correspondence between a participant's voice outside and inside a text, while the second game centers on the way participants converse with themselves and "others" when textworking. The second set consists of two language-games which pertain to the way participants enact a position in the subfield by voicing their production of knowledge in particular ways. Here, the first game revolves around the relationship between the textual production of knowledge and the textual product of knowledge which is consumed on the subfield's linguistic market. The second game deals with the way participants voice the dialectical relationship between single and co-authorship.

Voicing a Text

The two antinomic language-games I discuss in this section concern the ways in which participants voice a text and, thereby, constitute more or less identifiable positions in the subfield of organizational communication studies. The first game concerns the way participants deal with the correspondence between their voice outside and inside a text, while the second game deals with the way participants voice a text through textwork conversation with themselves and "others."

Voice Outside/Inside Text. Participants enact their position in the subfield by enacting a more or less identifiable position in text. This occurs, in the first place, by constituting a textual voice which more or less corresponds with the producer's "actual" voice "outside" the text. Some participants reflect fairly intensely on the similarity or dissimilarity between these two voices, while others adopt a relatively "standard" voice without very much reflection. Reflecting on this difference is perhaps fairly accepted. Stereotypically, everyone knows that scholars' writings often sound intellectual and distanced, because they are written in the third person or the sovereign "we," contain plenty of field-specific jargon, and so on. However, I argue here that the reason for creating (or not creating) this correspondence depends in part on whether participants believe this correspondence will bolster their position in the subfield. Thus, as "textualizing agents," they work from a set of presuppositions (*doxa*) about this correspondence. In turn, they balance their expressive interest in light of these presuppositions to acquire capital.

Contrary to what I presumed before speaking with scholars about the mentioned correspondence, the discourses of several participants suggest that they have not reflected very much on this correspondence before. For instance, participant 3 mentions:

> *Participant 3*: [I]f you've got people who are doing, you know, auto-ethnography, it had better be the same [i.e., there better ought to be a correspondence]. And it maybe that we have different expectations for the different kinds of…so, if you're reading neopositivist work, you know, the person isn't gonna come through...and so on, and so on. But I think in the middle where you're not quite sure, I've never really played around with that in my own head.

> *Boris*: You just sort of adopted this style without thinking about it too much?
> *Participant 3*: Yeah, what seemed to work and then try to do what the editors told me I had to and that kind of thing. (I.3A, 671-684)[17]

Participant 3 expects that a scholar's *habitus* influences his or her perspective on this issue. Interestingly, participant 3's own words suggest that this is also the case for himself, because he tends to constitute his textual voice without much contemplation; that is, by "doing what seems to work" and by following what editors [with a more or less similar] *habitus* "tell him to do" (which indicates a fairly unproblematic acceptance of the subfield's censorship). Hence, "obedience" pays, even though it reinforces the unreflexive reinforcement of taken-for-granted textualization practices. It may not be surprising, then, that participant 3's "conventional" practice of voicing a text also corresponds with relatively little reflection on the way he enacts a position in the subfield in general. Unlike participant 3, participant 2 reflects more readily on the enactment of his voice:

> *Participant 2*: Yeah, and I've sort of been confronted with it…I mean I can remember I had a prospective grad. student…come to campus, and I met with her in my office. And she'd read my [title] article in a class and she said…she

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[17] Some other reactions were: "*Participant 1*: Oh, boy, uh, [lengthy pause], um, you know, I don't think so much about the person I'm creating in writing…except insofar as I want to be thorough, I want to deliver punch, I want to, um, I wanna not be wrong [laughing]…You know, I'm really out on a limb" (I.1A, 634-644). "*Boris*: Do you think that your voice in a text reflects who you are as a person? *Participant 7*: Gosh, I guess, I would like to think so…I don't know if I'm the best person to say, though, because I can't read my text from the outside, so it's sort of always in my head and so I hope I would construct it to be in line with how I am as a person" (I.7A, 460-469). "*Participant 10*: Huh, that's a very interesting question. There, I think I probably have a less developed schema and awareness" (I.10A, 467-468).

goes: 'Oh, my goodness, I…it's really weird, you don't at all match up with the image…'
Boris: Yeah…[laughing]
Participant 2: She thought I was like in my 50s…[laughing]…and this was like maybe 10 years or more ago, right [laughing]? And, yeah, I mean, I think the scholarly persona that comes across…the textual persona is probably quite different from…I mean, I don't at all see myself as a serious person. (I.2A, 725-740)[18]

As this excerpt indicates, due to the reliance on a field's textwork conventions, a textual voice tends to be more "conventional," "coherent," "serious," "distinguished," than the textualizing agent's "actual" voice. Interesting is that participant 2 is aware of this textual effect and recognizes that the way he textualizes his voice (as well as the organization, style, and content of a text) is to some extent a misconstruction ("I don't at all see myself as a serious person"). He nonetheless enacts this misconstruction, implying that he more or less "consciously" misrepresents or misrecognizes himself in

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[18] Participant 4 has thought about the correspondence between his textual voice outside and inside a text as well. He states: "*Participant 4*: I've thought about that a lot my whole life. And I think that the…I don't want to be critical of people who compartmentalize their work as a separate thing. I've had a variety of sort of critical moments in my life when I've thought about this. One was I remember when I was in college, having an argument with this young woman about…she was gonna become a doctor, but she was deeply Christian. And we talked about how one could be extremely a devout believer and also be a scientist. And she wanted to say that these things could be separate and they could inform each other, but one wouldn't necessarily contradict the other…I've always tried to be more transparent about that connection. And, I guess, I've always figured that the best way to be a strong force as an academic is to be open and vulnerable about those kinds of relationships and connections between one's own sort of concerns, passions, and desires and the work that you do. And I think, the other thing is, I've always chosen my areas of study based upon the things that have really mattered to me personally. So, yeah, I would hope that that would be the case. So, I would hope that my voice in text is very much continuous with my voice outside of text…So, yeah I think there is a continuity there" (I.4A, 443-467).

academic texts in order to follow particular textwork conventions. Nevertheless, I

believe that simply following conventions is not the only reason for participant 2's

practice, for it also makes him seem "scholarly" ("intellectual," "knowing," etc.). Thus,

these conventions generally sustain a scholar's guise of intellectualism or scholasticism.

In turn, through this practice, a scholar reinforces the legitimacy of his or her position in

academic fields and subfields and sustains his linguistic and symbolic capital.

Although several participants are more or less reflexive about the

correspondence between their voice inside/outside a text, most participants' discourses

suggest that their decision to construct this correspondence is more directly based on a

sense of contest. Participant 5, for instance, enacts this correspondence in light of the

textwork-game:

> *Boris*: Have you become freer in your writing once you have become more
> tenured?
> *Participant 5*: Yes and no. I mean, it depends on where I want to be. Like I still
> want to publish in [*Communication*] *Monographs*, right? And that means that I
> might not have the freedom…So, I think I will always write like this [following
> relatively traditional textual conventions] to a certain degree, for certain outlets
> where I know I'm gonna reach more people. But that I'll always try to fool
> around with it in a way that I can kind of push those boundaries out a little bit
> further. And then, if someone already respects my work, then they'll say: 'Well,
> I should look at this. This is [name of the participant]. I know her work, but see
> what she's doing now,' you know? And maybe accept it, because of the
> foundation that I've established through more traditional structures. (I.5B, 322-
> 337)

These words suggest that participant 5 practices textwork vis-à-vis particular

conventions, turning her voice and the textual knowledge she creates into a rhetorical

instrument to gain social, linguistic, and symbolic capital. The mentioned

correspondence, then, does not need to be close per definition. Rather, this passage

implies that a scholar creates this level of correspondence depending on what he or she

wants to accomplish in a field or, as participant 5 says, "where [she] want[s] to be." In

turn, the passage indicates participant 5's *illusio*, her investment in the textwork-game

with its own set of stakes (i.e., "to push particular boundaries out a little bit further").

The words, "it depends on where I want to be," are interesting, because they imply that

participant 5 constitutes a close relationship between her biographical illusion, her

position in the subfield, and her practice of textwork. Furthermore, she believes that

her freedom depends on the amount of symbolic, social, and linguistic capital she has

gained over the years. At times, this may entail that she has to write according to a *doxa*

which disallows the close correspondence between her voice inside and outside of a

text which characterizes her central biographical stance in academia.[19]

At the beginning of this investigation, I assumed that the correspondence which

I am discussing here would be important for participants, because they believe that

their textual voice offers a fair translation of who they are; that is, their "actual,"

"authentic" identity. Based on the way participants enact the voice outside/inside text

language-game, though, it seems that the degree of this correspondence is constructed

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[19] Participant 5 constitutes this position through words like the following: "*Participant 5*: I'm more thinking of: What can I do in my process of researching and writing that best represents the experience that they [the research participants *and* the researcher] know to be true, you know? So, I guess, I'm really an interpretive, ethnographic researcher at heart and always have been in the sense of wanting to know what their experience is like" (I.5A, 682-686). "*Participant 5*: Probably the most important is that we create a cadre of scholars that do meaningful work. When they read work that pulls them close they write in ways that pull people close" (I.5B, 204-206).

in light of the contest for capital. In this effort, some participants identify their position

by diverting attention away from the way they position themselves in the subfield

through their textualization of a voice (i.e., by adopting a more or less standard

"academic" voice). Conversely, others attract attention to the way they enact their

position by reflecting on their voicing of a text in a text or by writing with a voice that

seems unusually "unacademic." Participant 4, for example, allegedly enacts a position

which is relatively deviant from other positions in the subfield. As participant 10

suggests, a voice *like* participant 4's voice (a "deviator" from the game, yet within the

game) is needed, however, to propel the game forward and to constitute a productive

tension between "mainstream" and "non-mainstream" work:

> *Participant 10*: I think [participant 4's name]'s ideas just constantly push the
> envelope…and he *positions* himself to do that…I think there almost has to
> be…let's put it this way…I think for somebody like an [participant 4's name] to
> have some of his effect, there has to be a mainstream that he responds to, okay?
> And without that, without that dialectic, it's almost impossible…I mean, his
> piece has to look unique in the context of a lot of pieces that don't look unique,
> okay? (I.10B, 680-688)

Striking, here, is that participant 10 speaks of "*an* [participant 4's name]" (as in

"*a* Bourdieu") rather than simply "[participant 4's name]." This implies that participant

4's position is *a* position in a field which has gained its potency and uniqueness due to

the effect of the subfield (the "field effect"); that is, due to the fact that this position is

sufficiently idiosyncratic from other positions in the same social space.

In general, most participants seem to take a moderate position in between the

extremes of constituting a completely standardized voice and constituting a voice

which corresponds as closely as possible with their "actual" voice. For example, I would not position any of the previously discussed participants on either side of this continuum. When reflecting on the subfield's overall *doxa* regarding the discussed correspondence, most participants suggest that their texts should not center too extensively on the author per se; rather, there should be a "balance" between someone's own voice and the voices of others (e.g., scholars or research participants). In this vein, participant 11 says:

> *Participant 11*: I increasingly write from a position of my own values and my own commitments and concerns about certain issues and that sort of thing, but I also feel responsibility, at the same time, to maintain a degree of objectivity or a degree of distance…I don't want to immerse myself in any particular work so completely that the work ends up being about me. (I.11A, 625-632)

The sense of responsibility which participant 11 displays can be taken as an indication of *doxa* embodied (and thus of the enactment of the I/*doxa* language-game), because he sees it as his personal responsibility to assure that a text does not center on his voice alone.

Perhaps the most important conclusion I want to draw from the discussion on voicing a text, then, is that participants intend to resist giving readers the impression that their efforts are entirely arbitrary and depend on their imagination or opinion without any connection to truth or reality. Accordingly, it seems that most participants want to constitute their texts as existing at a certain distance from (or "beyond") themselves; that is, beyond their position in a subfield of study and perhaps even beyond the confines of academia. Thus, by enacting language-games which enhance

the debate about the correspondence between voice outside and inside text, participants are able to guard off discussions about the way an author's constitution of a textual voice relates to his or her construction of textual knowledge. These games make it therefore possible to divert attention away from the fact that the scholarly production of knowledge implies a view which emanates from operating in a field or subfield of study. This game, then, prevents scholars from thinking and writing about the ways in which their textwork induces breaks with the realities they are trying to study, making their objectifications seem more objective (or more subjective) than they are.

The next game I discuss continues along the same lines of this argument and deals with the way participants voice a text through textwork conversations with themselves and "others." This game also enables participants to implicate their position in the subfield.

Textwork Conversations with Self/Other. When reflecting on the conversational practice of writing and reading, which involves empirical/model authors and empirical/model readers (as Eco [1992, 1994a/b] describes), I notice that the conceptual division between author, reader, and text, tends to "blur together" (11B, 571). The author/reader distinction seems as difficult to uphold as is the distinction between self and other. Through textwork conversations, participants implicate themselves (their positions) in the field and secure their (and the field's) survival. Hence, these conversations allow them to connect "I" to "we" (scholar to field) by creating texts that, through intertextuality, become part of the subfield's corpus of literature (its "body" of knowledge).

Participants' discourses frequently suggest that they write "for" themselves.

First of all, this indicates that textwork is a practice of self-*discovery* (as Richardson

[1994] suggests). Part of my conversation with participant 3 supports this idea:

> *Boris*: Do you have a very clear idea or have you had that all the times
> throughout your career, of a particular purpose you have with the work that
> you're doing?
> *Participant 3*: Well, there's a selfish one.
> *Boris*: Yeah?
> *Participant 3*: Yeah. I finally figured out, you know, that most of the questions
> that I was curious about, nobody was gonna answer for me. I was gonna go
> have to find the answers myself. And, I guess, that's selfish. It's not selfish in a
> pecuniary gain kind of sense, but it was that these are issues that I can get
> insights from other people. But that I'm gonna have to search for my own
> answers. So, maybe that's the motivating…
> *Boris*: And then they happen to be, oh, it almost sounds like they happen to be
> interesting to other people then as well.
> *Participant 3*: Sometimes, yeah [laughing]; some more than others. (I.3A, 165-
> 181)

Although participant 3 often speaks about the importance of learning from colleagues

by conversing with them, this passage indicates the significance of textworking

"with/for oneself," because it enables the constitution of relationships with oneself.

Hence, the textualizing agent constitutes himself or herself, at once, as empirical/model

author and reader (as also Mikhail Bakhtin [1986] and Paul Ricoeur [1991, 1992]

maintain). Similarly, participant 4 believes that he increasingly writes for himself:

> *Participant 4*: [T]he older I get and the more mature I get as a writer the more I'm
> writing for myself, because I don't feel like I have to necessarily please an
> audience in a way. They're a model audience for me in the sense that I really
> want to write something that is going to be exciting to them, not so much in the
> sense that I want to be sure to meet their expectations in some way. Do you see
> what I mean? Does that make sense?
> *Boris*: Yes.

Participant 4: It's not a judgmental kind of thing or an anticipation of judgment. It's more like I really want, I feel like this is a community that—this is a funny thing to say—but I feel like the community depends on me to play a kind of role [laughing] and I want to come through with that. I don't want to let them down. I don't want to put out something that's gonna be mediocre or uninspiring. I think when I give a conference paper, people look in their program and they say: 'Oh, [name of participant]'s gonna be giving that paper. We should go to that, that's probably gonna be kind of interesting.' And I want to do that. I don't want to, they don't necessarily look for me to give them a good statistical analysis anymore, but maybe something provocative they haven't thought about it. I'm happy with that role. (I.4A, 387-406)

I notice two things here. To start, participant 4 is relatively reflexive about the role of his voice in the subfield of organizational communication studies. This is no instance of high-mindedness, because his observations seem accurate (the reader might recollect participant 10's earlier comment in the previous section on the importance of participant 4's position in the subfield). Second, participant 4's model reader or audience seems to have become embodied and forms an integral part of the way he practices textwork. Simultaneously, participant 4's voice or position has become implicated in the workings of the subfield in that colleagues need this voice to convince themselves and others they operate in a subfield which produces, as participant 10 states, "mainstream" and "non-mainstream" research. Accordingly, participant 4's writing "for" self more or less equals writing "for" others (and vice versa). This example illustrates, in turn, that participant 4's position has become a valuable (and relatively scarce) commodity which colleagues cherish, because it animates the subfield's ("we") antinomic language-games.

Secondly, the practice of conversing with oneself through writing and reading also is a practice of self-*persuasion* (in line with Gross' [1990] dictum that "[r]hetorically,

the creation of knowledge is a task beginning with self-persuasion and ending with the

persuasion of others" [p. 3]). However, contrary to what Gross' words intimate, I argue

that this practice has no a clear starting or ending point: textwork conversations are

ongoing. In these conversations, self and other are altered in unnoticeable ways, so that

the conceptual distinction between self and other becomes virtually indistinguishable.

As my conversation with participant 9 indicates, this alteration makes textual

distanciation (Ricoeur, 1991) possible and enables a scholar to reflect on himself or

herself in relation to the knowledge he or she creates:

> *Participant 9*: [Ideas evolve in the dialogue]…I think when you write you successively take the role of writer and reader…of talker and listener. In other words, you write for a while and then…I print it up and then sit down and read it and say: 'I don't like that.' And so, I'm responding as a reader…and since I'm now a veteran in reading projects…[laughing]…
> *Boris*: [laughing]
> *Participant 9*: …various kinds of theses and dissertations and articles and so on; when I read what I've written, I basically go into my critical mode and then say: 'Oh, that has to be rewritten.' And so I rewrite it…I think it's a dialogue, in which you search for form and the your critical sense takes over and you say: 'No, that's not right,' and you go back and do it again.
> *Boris*: Hmm…so there's sort of on an introspective level a dialogue going on…
> *Participant 9*: Yes…
> *Boris*: …and then there is…
> *Participant 9*: It's not even introspective; once you've printed it out, it's no longer introspective…your reading something…you may have written it, but it…you now read it from a very different perspective…
> *Boris*: Uh, I understand what you mean. And then, of course, you engage into another sort of dialogue with reviewers and such…
> *Participant 9*: That's right. (I.9B, 616-643)

Participant 9 assures that the conversational practice which involves "himself as

another" (see Ricoeur, 1992) is not a form of introspection, because the textual voice

becomes an "entity" in its own right; as participant 9 says, "[I]t's 'it'" [I.9B, 472]). This

resonates with Ricoeur's (1991) idea of distanciation in that a text contributes to the practice of textwork conversational by giving an author the opportunity to step "outside" himself or herself and to become a character in his or her own biography.[20] In addition, participant 9's words suggest that his *habitus* (the embodiment of a textwork *doxa*) "tells" him what is "right" and "wrong" in terms of content and form (as Bourdieu [1991] suggests). Correspondingly, participant 9 judges his textwork based on the system of dispositions which he developed over the years while operating in various fields of study.

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[20] My conversation with participant 2 also supports the idea that publishing a text helps someone to constitute "an identity" (to be "someone") in a given social space. Thus, the magic of print indeed seems to officialize or ratify someone's position in a field, as Bourdieu (1990b) says. Participant 2 mentions, in this regard: "*Participant 2*: I don't think that for me the magic of getting something published has ever gone away; it's always a thrill to see something in print…and actually for me the most thrilling part is when you get the gallies…all of a sudden you see it as it's gonna look in printed form. My favorite part…to do the corrections on the gallies…that's sort of fun 'cause it's at a very last stage in the process and, yeah, it's like magic, it's like: 'Wow!' And in some ways for me it's like this sort of reaffirmation of: 'Wow, yeah, I'm a scholar; this is kinda cool!…[laughing]…What, little old me?'…and it's almost like: 'Wow'…but what is also, what is interesting, too, is like when I read those gallies, right? And I'm like: 'Huh?! I don't remember how I got here with this but, you know, this is pretty good, you know [laughing]?!' And, sometimes, I'll come to a particular point and I'll go: 'Hmm…I don't remember writing that.' So then it's like: 'Hey now…so how am I gonna get to the end from where I am now [laughing]?!' Because I don't sort of completely remember the argument I made…Yeah, so in some way, even like seeing the published article is sort of another sort step in the writing process…because it's like you finally…you've seen the argument in final form and you sort of reread it and so….it really true in the case of that point…you become another reader…so I'm sort of…obviously from a very different perspective, I am also like reading this…making sense of my own argument and, again, it's that whole thing, you know, of convincing yourself…you're your primary audience member…and it's absolutely the case…It's like: 'Am I gonna be convinced by the argument I'm making here, right?' And I can't do anything about it now, because it's published right? And then I'm like: [sighs] 'It works.' And then people take potshots at it" (I.2B, 678-725).

Participant 2's discourse also supports the idea that one writes to persuade

oneself by treating oneself as another:

> *Participant 2*: Yeah, I mean, maybe I mentioned this in some other form in our
> previous interview, but that, you know, I don't work with outlines, right? I
> work with very sort of broad ideas and I just start writing and see where it takes
> me and so, you know, in terms of sort of constructing the argument, I think part
> of what I'm doing is like: 'Well, okay, this sort of makes sense; now where can I
> go with this?' It's sort of a…it's very much a sort of a…you know, process of
> self-persuasion, I think. I mean, ultimately, you are always your most important
> audience, right? I think you have to be persuaded by your own argument and
> build that argument through the process of self-persuasion. I mean, I think, in
> some ways, that's the only way that you can write, I mean, how else can you
> write? I mean, in some terms it's kind of a truism, but I think it's also, you
> know, a really important reminder about, you know, what's involved in
> constructing scholarship, in constructing, you know, argument. Yeah, I would
> say: 'Definitely, I engage in self-persuasion…primarily.' (I.2B, 317-345)

This passage suggests that identity or positioning practices and textualization

practices are interdependent in such a way that, through and in the production of text, a

textual position is constituted which "suits" the rhetorical argument which a scholar

creates. Nevertheless, the persuasive effect of a text certainly is not only accomplished

by "convincingly talking to oneself;" as participant 11's words suggest, it also involves

persuasion of "others" and is constituted in the practice of textwork:

> *Boris*: I have used Goffman's [1959, 1986] ideas of dramaturgy…and I sort of
> framed the writing process as a staging of your self, as an expression of your
> self. And I'm curious…what your reflections are on that. If that process
> actually…[does] that metaphor [represent] the way…you work?
> *Participant 11*: I would say that metaphor doesn't fit my experience all that
> well…When I'm really into a project and really into writing, I don't have that
> much of a consciousness of myself as here is the persona I'm projecting or here
> is the stage on which I'm acting or that sort of thing. I'm quite familiar with
> Goffman and, yeah, that's not so much, I don't want to say that's irrelevant, but
> that certainly would not be dominating my thinking. And so, actually, these

days, it's more of a model of a conversation, metaphor of a conversation and I'm sitting there thinking there is somebody at the other end of this book or article. How do I engage them? What do I want to say? How do I want to handle that interpretational space that we were talking about and that kind of thing? And I would say that's most true when I'm really into it, I mean, into the flow[21] of the experience. The more I'm into the flow of it, the less conscious I am, I would say, of myself, but maybe I'm being unrealistic there, but that's how it feels to me. (I.11B, 522-542)

This segment indicates that a scholar's textual positioning emanates from the flow of textworking (similar to the Wittgenstein's argument that language-games are experienced in the practice of language). Hence, it seems that, being fully submerged into the practice of textwork conversation, a scholar's *habitus* works most optimally and it seems therefore relatively unnatural to reflect on the positioning practices which are part of such conversation.

Furthermore, once a text has been produced, it contributes to the positioning of scholars when it is read and appropriated by readers in the field, implying a continuation of Eco's imaginary textwork conversation between model/empirical author and reader. As participant 11 says, "writing tends to…[fix] ideas and identities in time" (I.11B, 363-364). Depending on the field's appraisal, this fixation can be both beneficial and detrimental to a position. Thus, the objectification achieved through textwork allows a scholar to archive and weave ideas, insights, and constructions together. Thereby, his or her position can become "part [his or her own] literature"

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[21] Participant 2 speaks about this flow experience as well when reflecting on the experience of writing his dissertation. He says: "*Participant 2*: I think you know, in some respect, I had sort of one of those, you know, flow experiences that, what's his name, Csikszentmihalyi Mihaly [see Csikszentmihalyi's (1990) *Flow*] talks about, you

(I.9B, 458) and, through intertextuality, part of a field's body of literature. Accordingly, textwork solidifies a position in a field for which "I" can be held more or less accountable. Textwork thus implicates a scholar in a field by connecting "I" to "we" through textual products which a field's linguistic market needs to exist.

In light of these observations, I infer that textwork-games involve struggles for capital, although scholars do not necessarily always engage in these struggles in conscious or tactical ways. Furthering one's position ("oneself") through textwork is not always a matter of the intentional and conscious application of a *technique*—in other words, a scholar seems to be only partly in control of the constitution of his position. Rather, one's position is constituted in the social enactment of the textwork-games—that is, through the interactions between author, reader, text, and textwork conventions within a social space. Positioning is thus part of the "craft" which scholars embody by being educated and socialized into a field's *doxa*.

In correspondence with the idea that textwork is practiced dispositionally, some participants (e.g., participant 8) reflect relatively little on the ways in which their textual production of knowledge involves self and other, because their *habitus* censors such questioning and even threatens their position. In turn, these participants do not emphasize the conversation with self and other when reflecting on their textwork, but foreground "the ideas" or "the knowledge" which they try to "pass onto" different audiences. Here, audiences function as "templates" and are conflated with the

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know [laughing]" (I.2A, 233-234).

publication venue which a scholar targets. In this respect, participant 12 states, for

instance:

> *Participant 12*: I wanna clarify something that I said earlier so that it doesn't
> sound like I'm being contradictory. When I said before that I don't spend a
> whole lot of time thinking about, you know, the audience and doing things like
> putting together a literature review…that's with the assumption that, you know,
> given outlets have, you know, a fairly well understood audience. You know,
> once you understand who the audience is, then, you know, you're writing
> within a genre…and you don't need to spend a whole lot of time thinking about
> it. (I.12B, 487-504)

In line with this passage, participant 12 mentions that she has "very different textual

selves, because [she does] a fairly wide variety of work" (I.12A, 459-460). In this case,

then, a scholar's textual voice seems to be more purely function as just a rhetorical

instrument which a scholar adjusts to achieve the publishing of a text.[22]

In conclusion, the language-games revolving around the conversation with

self/other antinomy suggest that participants' textwork is aligned with their struggles

for capital. Through textwork conversations, participants are able to make themselves

(i.e., their positions) integral and more or less irreplaceable parts of the subfield's

linguistic market. While textworking, they converse with themselves as others. This

"other" is a conflation of the author's sense of self (of his or her position), readers who

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[22] Participant 6 makes a similar point: "*Boris*: Do you have a particular audience in mind
in that way that you sort of dialogue with, when you write? *Participant 6*: [S]ometimes,
yeah. Sometimes, you play around with an imaginary audience, but most of the times,
I'm writing because of the ideas and the data, and then after awhile I'm starting to
think, okay, where might there be a potential outlet for this? So, then, I start thinking
about what part of the academic community I'm trying to reach" (I.6A, 558-568).

comprise the field and are more or less implicitly imagined by the author, and the impersonation of the field's textwork conventions ("audience templates"). Hence, during these conversations, the author speaks with, discovers, and persuades himself or herself and others, and, in turn, develops a text which gives him or her a more or less identifiable or distinguishable position in the field's linguistic market.

Developing knowledge through textwork is thus not simply a matter of following a format or merely channeling ideas. Instead, it is an intimate process of putting and keeping oneself on the map by balancing one's expressive interests in light of the field's censorship. This positioning affects the production of knowledge in that scholars do not only engage in this production for the sake of learning more about the studied human "subjects" or their carefully crafted object of knowledge (crafted from a point of view taken within an *academic* field of study). Rather, they learn that it is important to develop knowledge that offers them a stronghold on the field's market. This disposition may lead scholars to textualize their object of knowledge in ways that is not always conducive for better understanding an object (e.g., by over/under-interpreting "data," projecting themselves into the research "subjects" without acknowledging this projecting, failing to acknowledge the privileged position in the academic field from which they study human "subjects" in other fields who do not operate from such advantaged position[23]). Instead, it is conducive for the way they are

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[23] Participant 3 acknowledges this "scholastic fallacy" by stating: "*Participant 3*: I think there's an irony...for critical theorists that doesn't exist somewhere else for folks who don't deal with those kinds of issues. One of the things that I think that has interested me about the genealogy of critical organizational theorists—critical org. comm.

perceived and evaluated by others who help to make the practice of research in a given space possible.

In the previous two sections, I discussed the way participants voice a text and, thereby, often fend off reflection on the way their voicing of a text influences the development of knowledge as well as on the way their struggle to implicate oneself in the subfield affects this development. The next set of antinomic language-games deals in more depth with the way participants voice the production of knowledge in particular ways to enact a position in the subfield.

Voicing the Production of Knowledge

This second set of antinomic language-games consists of two games. The first game centers on the relationship between the textual production of knowledge and the textual product of knowledge (i.e., the text as it is consumed). The second game deals

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theorists—is, you know, we've gotten to the point where there are fewer and fewer people doing that who've actually worked for a living. And if they have worked for a living, it's in a managerial kind of perspective. So, we get folks writing about, you know, shop-floor humor who don't work on the shop floor" (I.3B, 139-146). "*Participant 3*: I think…you know, a large proportion of, you know, academic research is just boredom relief, because, you know, I've tried to say: 'Okay, how does this in any way, shape, or form translate to the lives of real people?' And, you know, most of the time I go: 'It can't. So why is it I'm reading this stuff?' I go: 'Oh yeah, I'm an editor, I have to' [laughing], but I think that's the immediate thing…is, you know, is there, you know, a practical implication for this? And then the flipside of that is often when there is a practical implication, the implication is, well, you know, managers should fundamentally change their orientation toward the world and the organizations that they dominate. And my response is: 'Why would they want to do that [laughing]?'" (I.3B, 263-275).

with the way participants voice the dialectical relationship between single and co-authorship.

      Knowledge Product/Production. Overall, participants soft-pedal the importance of incorporating reflections on the production of knowledge in their texts, while favoring communication about the outcomes of this production ("results," "findings," etc.). In so doing, they fence off reflection on the way their textwork (their *modus operandi*) induces breaks with the realities of practice which scholars try to investigate. This fencing off, in turn, makes it seem unnecessary or undesirable to question the privileged position in academic space from which participants, as scholarly "subjects," examine human "subjects" in very different kinds of social spaces.[24]

      Interestingly, the overall academic *doxa* states that a scholar should not hoodwink the reader of an academic text.[25] Still, most of the participants in the study

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[24] As Bourdieu (2000a) explains, an academic text commonly obscures the practices (textualization and other) through which it was produced "and above all the *modus operandi* of which it is a product" (p. 53).

[25] Perhaps the best illustration of this convention is offered by the "Sokal affair." In 1996, Alan Sokal, a physics scholar, published "Transgressing the Boundaries: Towards a Transformative Hermeneutics of Quantum Gravity" in *Social Text* (a cultural studies' journal). Sokal "demonstrated" how physics could benefit from taking a cultural studies' perspective on quantum mechanics. In so doing, Sokal supposedly wrote with the intention of resetting the longtime dogma of the Enlightenment hegemony that had dominated the natural sciences and "the Western intellectual outlook [at-large]" (Sokal, 1996, p. 217). After its publication, however, Sokal admitted that his article was a case of make-believe; a hoax written to show how easily a rhetorically cunning scholar could get away with writing a nonsense text which flattered a the editorial ideology of a journal. Evidently, this illustration provides a parodic example of how easily a scholar can mislead particular readers. Nevertheless, the impact of this hoax was significant,

admit that, as participant 8 says, "the way research gets written, is not the way it's

done" (I.8B, 488-490).[26] Thus, there seems to be more or less agreement in the subfield

of organizational communication studies that the reader does not need to see all that

goes on behind the scenes. Participant 1, for example, talks about the iterative "back-

and-forth" practice of her research which is not disclosed to the reader:

> *Boris*: And how do you, then, go from that discovery moment sort of, when you
> find you're getting some real interesting data to theorizing, for example,
> conceptualizing in your mind, and then, um…?
> *Participant 1*: Oh, yeah, that's all very painful [laughing].
> *Boris*: Is it [laughing]?
> *Participant 1*: Oh, yeah [laughing], well, it is, because it's a lot of fits and starts,
> you know, stops and starts, I guess…Well, maybe it wouldn't be that way for
> other folks, you know. I mean, maybe they're just hit by a lightning bold…you
> know, they're just writing and writing and writing but, for me that really begins
> an iterative process of having this gut-level feeling that there's something there
> and, then, that sends you to the literature, and that sends you to your
> colleagues, that sends you to your data, and you just kind of, for a period of
> months, if not years, go back and forth, back and forth, back and forth,[27] and
> really attempting to sort of flesh out what's there, and what are the key aspects
> and sometimes my background is completely, um, completely prepares me for
> that and sometimes I'm just completely ill prepared for that and I have

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setting off a host of articles in which scholars discussed the social construction of
knowledge, the ethics of textualizing research, and the need for editorial expertise.

[26] I also speak with participant 4 about this issue: "*Boris*: It's so interesting to see how
then the final product, when it's in front of me here, how, you know, the underlying
process or the developmental process seems so much more non-linear than the text
actually presents right now. *Participant 4*: Mm, I think that's true. I think you really,
and I think there are a lot of things, semiotic things that add to that, you know, the
typesetting and the whole framing of it, makes it look like it sort of was born
whole…out of a brilliant person's head" (I.4B, 95-106).

[27] Similarly, participant 2 says: "[Y]eah, you've got this completely kind of circular, not
really circular, but in a hermeneutic sense, sort of a circular process that goes on that
isn't at all reflected in the final version of the paper" (I.2B, 629-631).

wonderful colleagues who, uh, you know, who agree to read my work and make great suggestions. (I.1A, 252-271)

Most participants, then, follow the *doxa* that academic textualizations should not reflect on and chronicle the knowledge production process in such detail that it becomes the center of the text (similar to the idea that the text should not center solely on the author). Participant 4 says, in this regard:

> *Participant 4*: My problem with that [reflecting on and chronicling the research and textualization process in minute detail] or my issue with that is that the struggle is not sometimes as interesting to the reader. I mean, it's interesting, but it's a very different thing in a way, and so while I applaud epilogues and attempts to deconstruct and demystify these things, I'm not so sure that if that becomes the focus of the writing that we've gotten something better, because I'm still very committed to the practical and substantive impact of the work, you know, on an audience, apart from how this trials and tribulations of me getting to the point where I felt I had something I could say. (I.4A, 146-161)

I believe that the presuppositions that textual reflections on the practice of production do not contribute to "the practical and substantive impact of the work" also need to be seen in light of the *doxa* which dissuades over-indulgence in this kind of reflection in published texts, deeming it unnecessary or inappropriate. Participant 8 takes a similar view:

> *Participant 8*: I…recognize that the way research gets written is not the way it's done. When I'm reporting research it is reported as: Here is a set of questions or hypotheses; here's how the data was collected; here are the criteria; and so on and so forth; and here is what we've found; and here is what it means for future research. And, um, I recognize that that's not the way in which research is actually conducted. Research is much messier, much more nonlinear, and then the question comes up: Why is this then that you are reporting it in this fashion? And my response is that I'm reporting it in this fashion because it's like…it's…it makes it easier for someone to take away from the research the core ideas that

need to be taken away. Like saying, um, and I just think of a very poor analogy, but I'll go ahead and make it and that is, um, when you think of food preparation…the things in the kitchen are much more messy and nonlinear and the ways in which the food is prepared but, finally, when the food is served on your table, it's served with a certain elegance and in a certain layout…and, um, the person who wants to consume it is more interested in seeing that elegance and valuing it for that rather than knowing the nitty-gritty of the different processes that went into making. Sausage making, I guess, is another example. People say you don't wanna see how sausages are made…and I think that the advantage is that…I understand that all research is messy, and I don't deny that, um, I don't think that my interest as a consumer of that research is served by knowing that, um, by chronicling the messiness as it happens, but rather in a way that allows me to focus on the ideas that came out. I want to know if people messed up on an idea, that I wanna know about; a bad research practice, that I wanna know about, because there were certain weaknesses in the study. (I.8B, 489-522)

Statements like these make reflection on the production of knowledge seem undesirable. However, more undesirable, perhaps, is to destabilize one's position in a field by reflecting on this production and on the effects of the breaks which the scholarly *modus operandi* induces. Hence, the history one has established in a field or subfield of study can make this kind of reflexivity difficult. For example, if participant 8 were to publish academic studies on organizational communication topics which also questioned the effects of his positioning as a scholar in an academic field, he would risk losing capital, because his work could be considered inconsistent with his previous body of literature. In addition, because participant 8 is more or less persuaded about a particular *doxa* and because he has learned to operate from a particular *habitus*, I think it would be relatively unlikely that such a change would "present itself" as a necessary and worthwhile change to him.

Interesting, furthermore, is that participant 8 makes the distinction between form and content which Bourdieu (1991a) denies. Participant 8 focuses on "the ideas that come out" of an investigation and believes that the way these ideas are communicated is best achieved by using a standard form of expression. Perhaps also here, participant 8's adheres to this practice and to the differentiation between form and content, because he has been education to operate based on a particular *habitus*, which entails that a scholar needs to censor his or her own textwork in light of particular linguistic conventions. Based on a *habitus*, then, some agents (e.g., someone like participant 8 who adopts a neopositivist position label) may privilege content or over form (or vice versa or neither) and thereby constitute a particular identity in social space.

Moreover, participant 8's view suggests that readers receive an academic text (especially, an empirical study) in fairly uniform ways, because they are familiar with its standard format (participant 12 gives a similar response). This view assumes that audiences are more or less similarly dispositioned towards this form without questioning the reading practices in the production of knowledge. In light of this issue, I ask to what extent a reader should be placed in a position where he or she has little choice but to suspend disbelief and trust an author's textualization?[28]

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[28] Along the same lines, I wrote in my journal: "Why would the reader trust the author, suspend disbelief, and follow ropes wherever they lead like Theseus trusted Ariadne?" (J.06.02.03). In reference to the sausage analogy which participant 8 makes, for instance, I note that I have gladly stopped eating sausages after learning how they are produced and what ingredients they contain.

When reflecting on the responsibilities which participants feel towards their readers (and thus also, in a sense, towards themselves), several participants speak of wanting to be "faithful" or "fair" in their translations of the works of scholars or the "words" of the human "subjects" they are trying to study. Nevertheless, participant 3's discourse suggests that a scholar oftentimes has (or takes) quite some "poetic license" (see Baldick, 2001, p. 197) when communicating the ideas of others to others:

> *Boris*: How do you deal in your work with the idea of interpretation? Is that something that, for example, of course, research builds upon people's ideas and how comfortable are you interpreting other people's works through reading, for example, and, I mean, I've been reading…hermeneutics and it's a very critical process, because how do you know that you are reading these…well, you can't even use words such as "truthful," "accurate," anything like that. How do you deal with that sort of a tension? How do you know you're capturing the argument that Foucault is making in a sort of way that…?
> *Participant 3*: Well, I worry about that, because I want to be fair. In terms of: Do I ever really feel that I need to have fidelity with…? Not at all. I think ideas are to stimulate other ideas. If ideas that I had to get stimulated are different, that's good. That's what the process is about. So, I think it's very important to be clear and fair about where the other person's ideas end and yours take off…and thus they become responsible for my errors. But I've never worried about that kind of thing.
> *Boris*: So, you sort of trust your own reading of other people's work?
> *Participant 3*: Or I tended to focus on people who it's impossible to tell what they meant. So, you read, you know, European philosopher[s], you know, and I read people like Burke who, you know: What did he say? That kind of thing. So, it's really…It's very hard for people to say definitively that this is what he meant in this section. That'll give you a lot of license. What did Foucault mean? (I.3A, 547-572)

This excerpt suggests that participant 3 knows that his texts weave his own ideas and his approximations of the ideas of others together in a way that is convincing to a reader. However, because an academic text is written to persuade (and to support a position in a field), the text may also create the impression that its author was more

confident in terms of his or her translations than he or she actually was. For example,
in this text, I often write words like "Bourdieu argues" or "Alvesson believes." As said,
I thereby constitute these scholars as epistemic individuals; that is, sentences like these
correspond with my readings of their work, yet also constitute a particular "version" of
these scholars by defining their biographical illusion in a particular way. Thus, I can
never be certain where the other person's ideas "end" and where my own ideas "take
off." Stated differently, in my textwork, other and self become entangled so that it is
difficult to see where I influence and am influenced. Accordingly, participant 2
constitutes the practice of interpretation as a constant reflexive struggle which involves
the continuous questioning of his translational endeavors and the continuous
conversing with self and other in light of his field and subfield:

> *Participant 2*: [A]m I still being faithful to the idea that I'm working with here?
> And I'm sort of constantly kind of sort of struggling with the extent to which I
> am sort being faithful to…and I mean, even when you're working
> with—obviously, most of my work is library research, so it's like—I mean, any
> time you're, you take up somebody's ideas, you're always being selective, right?
> So, which parts are useful, which parts aren't and in that selection process…I
> mean, you're sort of constructing a particular argument. It's like: 'Am I still
> being you know faithful to these ideas? It's not that I'm doing enough violence
> to it that it's no longer…[laughing]...you know the idea that I'm talking about?'
> (I.2A, 295-316)

This passage implies that the practice of textwork involves the constant
delineation for the reader (self and other) of what I would call "interpretational space."
This entails that "I," as a scholar, continuously seeks to set parameters for a reader by
translating my ideas and those of others in particular ways. Sometimes, participants
seem to be comfortable with demarcating a wide space, as participant 3 suggests.

However, in reflection on one of his earlier texts, participant 10 wonders if his co-

authors and he may not have defined the interpretational space "too narrowly:"

> *Boris*: [T]his theory is introduced to me [in a particular publication that the
> participant has co-produced] in a particular way and I was wondering in that
> regard if you had any ideas about how you deal with sort of interpretational
> space…with that I mean…Do you sort of set very consciously certain
> boundaries for the reader in which he or she can move around with their
> interpretation? And do you limit that space to more or lesser extent?
> *Participant 10*: I think we were probably seeking to limit it pretty considerably. I
> think we were seeking to limit it in terms of…uh…in several ways. First,
> because [the scholar who proposed the theory] is so…can be so complex…that
> was perhaps one…And, in fact, parenthetically, I note that when [another
> scholar in organizational communication studies] offered [an interpretation of
> the same theory] in another journal, not long after that…I was kind of surprised
> that…the different reading she offered of [the same theory's originator]…and it
> made me wonder, at the time, and to use your term, if we had perhaps limited
> the space, the interpretive space, too much!…You know, it was my reflection on
> her reading…and [I] thought: Man!…maybe we had too narrow a reading and,
> additionally, maybe we create too narrow a reading for other people. (I.10B,
> 149-169)

Striking here is that participant 10 realized the dimensions of the

interpretational space which his co-authors and he had created when he himself became

a reader in another organizational communication scholar's interpretational space. In

this regard, both this other scholar and participant 10 tried to demarcate the same

conceptual field, which the theory's originator delineated first. This suggests that,

often, the distance which a scholar has from his own work only becomes noticeable for

an other (in the literal sense), illustrating that a scholar indeed can be "caught-up" in his

or her own views and bewitched by his or her own language (see Wittgenstein, 2001, p.

40).

Moreover, the criteria for defining the "faithfulness" of a translation of

someone's ideas are often part of a scholar's system of dispositions and of the *doxa* of

the field in which one operates. Correspondingly, such criteria frequently are hidden

from reflection as well. Participant 2 notes, for example:

> *Participant 2*: Yeah, I'm not sure I could give you a set of criteria [for what it
> means to be faithful to someone's ideas in terms of interpretation]. I mean, I
> think it's, partly because…oftentimes, you know, you're sort of juggling with…I
> mean, you're juxtaposing different ideas, and so it's this sort of constant sort of
> hermeneutic process of: 'Okay…I mean, I'm interpreting this work particularly,
> but then, at the same time, I'm juxtaposing it in relationship to other work and is
> there connection between them of the connection I'm trying to work out? You
> know, is that a sort of viable way to talk about these ideas? And at the same
> time: 'Am I sort of being faithful to both these ideas?' And I think it's…I think
> it's, it's a couple of…I think it's partly intuitive, it's partly…[sighs]…you know
> it's…I'm saying: 'Something that is interesting and useful here'…I mean, I think
> that has to be a test as well. (I.2A, 323-337)

This passage indicates that the criteria used to determine the faithfulness of

participant 2's translational efforts are at least partly embodied in his *habitus* (they are

"partly intuitive"). In this respect, the conversation with self which forms part of the

practice of textwork implies an interaction with his own system of dispositions.

However, the last sentence indicates that the symbolic value of a translation is

important, too. Hence, determining whether his translation will be considered

"interesting and useful" by the field also forms an important factor in this practice.

Even in the act of translating, then, participants sometimes are engaged implicitly or

explicitly in the struggle for symbolic capital.

In spite of all these authorial responsibilities, though, the reader of an academic

text has certain duties as well. For example, I could rely solely on another scholar's

reading of a Foucault's work without reading Foucault's original work. In other words,

I could count this other scholar's account *as* Foucault's account. Patently, this would be a foolish thing to do, even though this practice is commonplace. That is, scholars are sometimes unwilling or unable to translate certain works themselves and thus rely on others to do the work for them. Hence, although translational variations constitute a fruitful polemic in the production of knowledge, they are sometimes also simply indications of lax and lackadaisical scholarship. More problematic with regard to the latter, however, is that the uncritical adoption of a translation often leads to an undue reification of ideas and concepts as *the* legitimate reading of their meaning. Accordingly, participant 2 states:

> *Participant 2*: [O]n the one hand I'm like: 'Wow! It's great that people, you know, see my work as, you know, a useful interpretation of, you know, a particular scholar, whether it's Foucault or whoever.' But at the same time, I guess, I do get bothered by the fact that people don't…like [read the original themselves]…I mean, my work should not be taken at face value, right? I mean, just like I was saying earlier about…your work is always selective, right? You always interpret work in particular ways. And so, yeah, it sort of bothers me that people wouldn't go back to…I mean, if you're gonna write about Foucault, you should be reading Foucault, however you interpret it, but at least you can sort of, you know, hang your hat on your own particular reading of that, and then sort of position that in terms of the way the field. (I.2A, 397-414)

In sum, as with the previous language-games, also through language-games concerning the dialectic between knowledge product and production, participants identify their position in the subfield of organizational communication studies and other fields or subfields of study. In this regard, it seems that participants (or perhaps better said, the subfield's *doxa*) shield off reflections on the way their textwork (their *modus operandi*) affects their production of knowledge. At times, this might lead to

providing readers with too little insight into the kitchen, so to speak. However, readers

might also too readily consume that which is presented on their plate. Careful

reflection and conversation about these practices is thus necessary to avoid creating

"false" realities. Here, I do not mean to say that perfect translation of ideas and

arguments is at all possible. Rather, I suggest that the "capitalist" struggles in a field

may seduce scholars at times to use this impossibility to their advantage at the cost of

the knowledge which the field seeks to produce.

Very closely related to the knowledge product/production antinomy is the next

antinomic language-game. This game pertains to the way a single agent (or a few

single agents) makes use of his or her collegial network to produce textual products

(and, thereby, to strengthen his or her position in the field). Thus, an individual with

relatively high amounts of social capital (as well as linguistic and symbolic capital) is in

a more privileged position to participate in a field's textwork-game than someone who

has relatively low amounts of these forms of capital. However, generally, a field does

not take this privileging into account and rewards the sole author(s) who are linked to

the product rather than the network used to achieve this production.

Single-/Co-Authorship. Most (if not all) participants acknowledge that

textwork is a collaborate affair. Consequently, any text can be seen as an intertext (as

Cheney & Tompkins [1988] contend); that is, a co-authored product which is produced

through actual conversations with colleagues and more imaginary ones with model

readers (as Eco, [1992, 1994a/b] says). In line with this notion, most participants

confirm that they rely on other scholars to read their work, provide suggestions, and so

on. Nevertheless, their position in the subfield is an individual one and the subfield

accredits symbolic capital to individual authors. This occurs, for example, in the form

of printing the individual author(s) at the beginning of a page, yet the names of

colleagues who "helped out" on the "sidelines" of the paper (e.g., bottom of the first

page, at the end of the paper). Nonetheless, these unofficial contributors operate within

the official chalk lines of the same social space.

Part of my conversation with participant 9 illustrates the difficulties involved in

trying to uphold the single/co-author distinction in textwork:

> *Participant 9*: I think that he's [Kuhn (1970) in his book *The Structure of Scientific Revolutions*] right, in the sense that there's an environment and you enter into a dialogue, in which there are ideas which you are collectively developing, but you can't say they're your ideas, because even though each time you write, you write seriously as if they were your ideas, but you're really only borrowing them; you know, they can't stay yours…because the minute you've expressed them, they enter back into the dialogue…
> *Boris*: Hmm…What is interesting, then, is that the academic system…does very much focus on the single individual who sort of writes and gets particular credit for something, but in a sense credit should be given to a lot of people, then, at the same time, I reckon.
> *Participant 9*: Yeah, but that's the way of the world…If Schumacher wins a race, all the people who made it possible for Schumacher to win the race don't get any professional credit. (I.9B, 407-426)

Interestingly, participant 9 acknowledges the irony of the single/co-author antinomy,

yet justifies its enactment by saying that this is "the way of the world." In so doing, he

confirms Bourdieu's (1991a) argument that the academic textwork-game revolves

around the acquisition of capital, although participant 9 does not know how to change

this game or how to escape it.

Only infrequently, it seems, does an individual, a field of study, the field of academia, or any field for that matter, pause to contemplate the fact that his, her, or its "individuality" is the result of a multitude of relationships. Hence, I consider that the single/co-author reinforces a form of symbolic privileging. That is, I think that the individual agent is privileged (given symbolic, social, and linguistic capital), even though he or she does not produce individual products. The agent's position in a field, which is for a large part constituted through these products, depends for a great deal, for example, on his or her cultural and social capital. For instance, scholars with degrees from reputable schools may have a higher chance to work with scholars who know the "ropes" of the textwork-game in a particular field. In addition, scholars are often able to publish because they are part of a relatively tight network of colleagues who provide each other with publishing opportunities. This seems to occur especially once some has gained significant symbolic, social, and linguistic capital in a given social space. For example, participant 7 says, in this regard:

> *Participant 7*: [M]erely getting it [a text] out there [through publishing], you know, that's gratifying, I suppose, because it's sort of a professional affirmation that you've been allocated a scarce resource to put your ideas out there. But that's not the same as having them, practically speaking, extend beyond you very much. Sad, it seems to me, and the older I get, the more it seems this way that that's more of a matter of sort of…a sort of personal relationship to people. (I.7B, 477-483)

As this excerpt illustrates, it is not necessarily only individual talent which helps an agent to sustain or improve his or her position in a field; an agent's ability to acquire social capital is important as well.

I question to what extent the supposed "individualism"/"collectivism" dichotomy which the single-/co-author language-game induces benefits the production of knowledge in the subfield under study. Due to the contest for capital, certain scholars' voices might not be heard (e.g., because they have not gained sufficient amounts of social capital), while others' voices are heard constantly (and perhaps not always in critical ways). In other words, if the production of knowledge becomes in part a matter of having a good reputation and of knowing the right people, I wonder whether these practices do not take away from a field's criticality.[29]

After having discussed the general enactment of the ontological complicity between the participants and the subfield of organizational communication studies and the way participants position themselves in this subfield through their textwork, I now summarize the inferences drawn in the two previous sections. In so doing, I revisit the two research questions which have guided this investigation.

Summary of Inferences

Scholar, Language, and Subfield

My first research question (reformulated to fit this study) stated:

RQ1 How do scholars and the subfield of organizational communication studies constitute each other through the practice of language?

   a What antinomic language-games characterize the enactment of this ontological complicity?

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[29] That is, do practices like these not hinder a field in terms of submitting its "selfish interests to the rules of dialogic confrontation" (Bourdieu, 2000a, p. 119])?

b How does the contest for capital play a role in the enactment of this ontological complicity?

c How does the enactment of this ontological complicity affect the production of knowledge in the subfield of organizational communication studies?

My analysis suggested that participants and the subfield constitute each other by enacting interconnected language-games which center on three antinomies: I/*doxa*; I/we; and we/they. Each of these games involves particular kinds of struggles for capital and affects the development in particular ways.

In the first place, participants use euphemistic language to relate themselves to the subfield. I saw this euphemization as a sign of the embodiment of the *doxa* of the academic field and, more specifically, the subfield of organizational communication studies. At the same time, participants seem to constitute a relation with themselves (their position signified through "I") and with the subfield's *doxa* through language. Thereby, they gain linguistic, social, and symbolic capital in this social space in the form of expressing themselves in legitimate and appropriate ways. I noted that the euphemization which the subfield's *doxa* reinforces hides the fact that scholars struggle for these forms of capital, making it seem that scholars mainly work for the sake of knowledge. At the same time, it prevents reflection on the field's *doxa* by making this kind of reflection seem unnecessary, undesirable, or self-indulgent. Nonetheless, some participants more readily reflected on their relationship with the subfield's presuppositions. In turn, the debate about whether and to what extent one should reflect on the subfield's *doxa* influences the development of knowledge. That is, a *doxa* which restricts the practice of reflexivity hinders its own alteration (improvement or

degeneration) by making itself more or less invisible and "sacred" (i.e., not to be questioned).

Second, and in close parallel with the I/*doxa* game, participants' language practices suggested that they enact a position (an "I") in the subfield which is distinguishable, yet never so distinct that "I" cannot be identified with the subfield ("we"). This I/we dialectic plays an important role in constituting the ontological complicity between scholar and field. The language-game which revolves around this dialectic involves a constant positioning of "I" vis-à-vis "we," frequently in the form of hierarchical relationships. Hence, I claimed that this antinomy allows scholars to symbolically privilege or marginalize themselves and others by positioning themselves and others in more or less central (or peripheral) ways in the same social space. My analysis indicated that scholars, in turn, cannot "play it safe" in the game for capital. The game is ongoing and continues to involve scholars (even though they might constitute different stakes along their academic trajectory). Moreover, I intimated that the I/we language-game affects the way knowledge is produced in the subfield by privileging or marginalizing a particular "I;" that is, an "I" which expresses himself or herself within boundaries which the subfield considers appropriate. Hence, scholars learn to take their positions vis-à-vis others in the field into account when developing knowledge, not only because knowledge production is an inherently collaborative undertaking, but also because individual scholars do not want to be dissociated from the field through the work they do. Thus, social and symbolic capital are the main stakes in this game.

Third, the I/*doxa* and I/we language-games are partly influenced (and reinforced) by another language-game which revolves around the dialect between "we" and "they." "Other" fields and subfields of study play an important role in the language-games through which participants constitute their positions in a particular field. Some scholars seem more successful in constituting their position in these other fields. This success seems to depend in part on the degree of similarity between a scholars' *habitus* and the *habitus* of scholars in other fields, which seems to be related to someone's ability to acquire linguistic, social, and symbolic capital in these "foreign" and to some extent "exotic" social spaces. In turn, success partly depends on being able to speak "the language" of another field and on a scholar's willingness to invest in learning this "foreign language." I concluded that the we/they struggles lead to controversies between fields that might benefit, but certainly also impede, the development of knowledge. On the one hand, the subfield encourages its agents to go out and explore other fields (and frequently rewards them for doing so). On the other hand, its *doxa* also ensures that the agents' focus remains their own subfield by setting as a principle that scholars must keep publishing in their primary field, illustrating once more the mutual connections between "I" and *doxa*, and "I" and "we," through which scholar and field bind each other.

Scholar, Textwork, and Subfield

My second research question (reformulated to fit this study) stated:

RQ2   How do scholars position themselves in the subfield of organizational communication studies through textwork?

    a   What antinomic language-games characterize this textual positioning?

    b   How does the contest for capital play a role in this textual positioning?

    c   How does is this textual positioning affect the production of knowledge in the subfield of organizational communication studies?

Here, my analysis indicated that participants position themselves in the subfield of organizational communication studies (as well as other field of studies) through two sets of antinomic language-games. The first set of games focuses on the way participants voice a text and revolve around the dialectic between voice outside and inside text, as well as textwork conversations with self and "others."

First of all, participants constitute a position in the subfield through textwork by creating a textual voice which more or less corresponds with their "actual" voice. I concluded that the degree of correspondence between these two voices conveys a scholar's position in the subfield and provides him or her with linguistic and symbolic capital. That is, in line with the I/*doxa* antinomy, participants constitute this correspondence in relative accordance with their incorporation of the subfield's *doxa*. Thus, they censor their expressive interest to acquire these forms of capital. Several participants downplay (or might have never considered) this correspondence, suggesting that their *doxa* limits consideration of the effects of developing a voice on their production of knowledge. Others are more reflexive about this issue, defining their relationship with *doxa* in less constrained ways. Overall, however, most

participants believe that their texts should not center too intensively on their scholarly position. Interesting, here, is that the *doxa* promotes the struggle for position, yet at the same time hides this struggle by preventing reflection on this contest. As with the I/*doxa* game discussed earlier, the voice inside/outside textwork-game therefore seems to counter-balance the idea that the creation of texts (and the knowledge that is produced through them) depends on the mere imagination of a scholar. In turn, participants' discourses suggest that their textual knowledge does not solely depend on the positions they enact in a field of study. By enacting a textwork-game like this, which enhances the debate about the correspondence between voice outside/inside text, participants are thus able to guard off reflections about the way an author's constitution of textual voice is involved in the construction of textual knowledge. Accordingly, they make it possible to distract attention from the fact that their work implies a view which emanates from operating in a field of study. This game, then, inhibits thinking and writing about the ways in which textwork induces breaks with the realities scholars are trying to study (I shall elaborate on these breaks in the final chapter).

Closely related to this first language-game is the second language-game which revolves around the way participants constitute a voice through textwork conversations with themselves and "others" (this game strongly resonates with the I/we language-game discussed previously). Through textwork conversations, participants are able to make themselves (i.e., their positions) integral and more or less irreplaceable parts of the subfield's linguistic market, because "I" becomes connected to "we." During these

conversations, that is, the author speaks with, discovers, and persuades himself or herself and others (a conflation of "we" and *doxa*). Subsequently, he or she develops a text which gives him or her a more or less distinguishable position on the subfield's linguistic market, providing him or her with social, linguistic and symbolic capital. Hence, the production of knowledge is not only about learning more about investigated human "subjects," but also about sustaining oneself as an important player in a field. This disposition may result in textualizations that are not always best for understanding an object of knowledge, but for the way the scholarly author who constitutes this object is perceived and evaluated.

The second set of antinomic language-games through which scholars position themselves in the field involves games which center on the voicing the production of knowledge. Two games are important here, one emphasizing the dialectic between knowledge product and knowledge production, the other focusing on the dialectic between single-authorship and co-authorship.

First of all, in light of the language-game which centers on the dialectic between knowledge product and production, most participants are not inclined to share the production of knowledge with a reader very intensively. In a sense, this suggests that the game has a built-in mechanism through which it prevents itself from being "given away." Participants' systems of dispositions reinforce the idea that this sharing is more or less unnecessary, undesirable, and so forth. Participants, in turn, are encouraged to follow this *doxa*, because the field rewards those who take and adhere to this practice. In so doing, it hinders reflection on and conversation about the way textualization

practices affect the production of knowledge. At times, scholars may thus ask readers to suspend their disbelief where readers have good refrain from such suspension. Nonetheless, readers might also have become accustomed to readily suspending such disbelief. I argued that therefore thorough and systematic reflection and conversation about these practices is needed to prevent "false" realities from being constituted.

Second, with regard to the single-/co-author language-game, I concluded that the subfield promotes "individualist" positioning practices by improperly acknowledging the fact that scholars rely on their network (social capital) to gain access to and power on a linguistic market. The subfield does not take this dependence into account, though, and frequently suggests that this kind of competition is "part of the game," "natural," or "inevitable." While the production of knowledge is an inherently collaborative ("socialist") affair, the subfield thus promotes a sense of "capitalism" which places pressure on those who are producing texts and which encourages academic opportunism. In this game, the knowledge one develops is not only important, but also the way one is able to play the textwork-game by being able to link oneself (one's position) into principal networks which provide its members with important opportunities (linguistic capital) for entering and succeeding on the linguistic market.

Based on this summary of inferences, I conclude that one antinomic language-game underpins most of the language-games I have discussed in this chapter. This game revolves around the dialectic between the discovery and invention of knowledge, as I will illustrate in the next section.

The Discovery/Invention of Knowledge

Knowledge is often thought to be "discovered;" that is, "channeled from up above," "stumbled upon," "found," "uncovered," or "unearthed." At the same time, knowledge is thought to be "invented," suggesting that it is "produced," "imagined," "created creatively ("for the first time"). The language-game which revolves around this antinomy, and which the participants in this study enacted in their discourses, is perhaps one of the oldest and most durable academic games played.[30] Currently, it might be difficult to sustain the extreme position which completely rejects the idea that knowledge is invented in some way or another (or discovered, for that matter). But, as I will illustrate, this does not mean that the tension between these two viewpoints has disappeared. I suggest that this tension is induced in part by scholars' systems of dispositions, their *illusio*, and their struggles to identify positions which are valued. Hence, the relationship between a scholar's "epistemology" (his or her conception of the nature and grounds of knowledge) is linked inseparably to his or her "ontology" (or conception of the nature and grounds of existence or being) and, not to ignore, his or her "axiology" (or conception of the nature and ground of value). I furthermore believe that a scholar's *doxa* captures these "ologies."

In the first place, I notice how many participants, regardless of their *habitus*, seem to hold onto the idea that their work is more than mere opinion or a convincing

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[30] Thought about this antinomy goes back to at least as far as Plato's work (1998a/b, 2000). The debate to which this antinomy gives rise resonates to some extent with Bourdieu's classic debate between social physics and social phenomenology (see chapter II).

argument (especially the I/*doxa*, voice outside/inside text, and knowledge

product/production language-games reinforce this debate). Thus, through their

discourses, participants imply that there is more to their work than sophisticated

wordsmithery. Many participants believe that they are studying something which

partly extends beyond themselves; that is, beyond their position in the field of academia

and the subfield of organizational communication studies. For example, participants'

language suggests that they see their work as a matter of trying to solve a murder

mystery, arranging the pieces of a jigsaw puzzle, or painting a painting that comes as

close as possible to particular "form."[31] Accordingly, they frequently employ words

which imply the idea of discovery.[32] At the same time, however, no participant

completely denies the idea that he or she partly invents and constructs an object of

inquiry in collaboration with others. In this respect, everyone acknowledges that

knowledge is not produced "in a solitary way" (I.4A, 325) (this debate is especially

supported through the I/we, textwork conversations with self/other, and single-/co-

authorship language-games).[33]

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[31] Participant 9 notes: "*Participant 9*: You know, that's how…a painter with paint…you have something in front of you and a lot of paint and what do you do? You've got an idea, you try it out, but I think most paintings you look closely are in fact covered over by [laughing]…covered over by an earlier version that they weren't happy with" (I.9B, 774-781).

[32] Few participants have different motivations: "*Participant 4*: I'm drawn to a sense of possibility and a sense of connection more than I am drawn to a sense of specificity and understanding. And so the goal of my work is to continue to open up things rather than to try to order things or get them into kind of an arrangement" (I.4A, 203-206).

[33] "*Participant 2*: '[A scholar is not] a mere cipher for ideas…I think that's a very

The antinomic relationship between discovery and invention partially seems to result from the subfield's historical imposition of this antinomy in parallel to a similar imposition in neighboring fields and subfields (see chapter III). In fact, the *habitus* of the field of academia at-large seems to reinforce this antinomy. However, this antinomic language-game impacts participants in different ways, instilling an awareness in all of them that their stance towards this dialectic is a position vis-à-vis positioned others in the field. For instance, a scholar who constitutes his or her biographical illusion by "embrac[ing]…the…postpositivist moniker" (I.10A, 298) is often well aware of the fact that his or her position is defined vis-à-vis others positions in the field. For example, participant 8 frequently makes a statement like: "I expect that I'm actually at variance with some of the other responses you've got, but I guess that's why you are talking to me" (I.8B, 542-543). Thus, as Bourdieu (2000a) suggests, participants constitute the subfield as a game of differing perspectives implied in different positions and dispositions. I support this observation through several examples.

First of all, as participant 1's discourse suggests, the fact that a scholar foregrounds the practice of discovering knowledge when reflecting on his or her research practices does not indicate necessarily that this person takes a "social physics" (see chapter II) position. Participant 1 states: "[M]y empiricist [neopositivist or social physics] work was more confirmation…my interpretive work is more discovery" (I.1A,

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problematic way to think about the production of scholarship…I mean, we're all here sort of intimately involved in various sorts ways in the construction of knowledge

211-212). Her research often begins with a "gut-level feeling that there's something

there" (I.1A, 263-264). This starts a long and intense "process that is really ongoing, but

you sort of punctuate periodically to stop and write and to put that in some kind of

form" (I.1A, 498-500). Interesting, here, is that participant 1's words suggest that she

turns "something" into "data" though textualization and, then, "mine[s] this data"

(1.1B, 731). This implies that a textualization contains something that initially was "felt

was there." However, this "something" still needs to be "excavated" in order to

uncover valued knowledge. In ways similar to participant 1, participant 6 suggests that

her investigations often start with something that "compels" her:

> *Participant 6*: My research comes about, because I am compelled to answer
> questions. Questions that are occurring in my life. Questions that I'm reading
> about and thinking something is missing. And so that's what drives me. To
> publish an article really isn't, I mean, yeah, okay, it's another thing that you've
> written and it counts towards something in terms of merit, etcetera, but for the
> most part why, I mean, at some point that doesn't make any difference. I mean,
> you earn enough money to live by. There's gotta be another reason why you're
> doing this. And for me it's the subject matter is so compelling. (I.6A, 503-509)

The idea that "something is missing" or that a subject matter provides the

impetus for a scholar's actions indicates that there is something beyond (yet also

within) participant 6 which "drives" her. This, I believe, illustrates how a scholar's

habitus (field embodied) interacts with a field of study (objective historical relationships

between positions) in which he or she operates, leading to a relative homology between

his or her position and dispositions. As Bourdieu notes, a *habitus* "reveals itself...only

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claims...and I think...we can't...absent ourselves from that process" (I.2B, 360-363).

in reference to a definite situation. It is only *in relation to* certain structures that habitus

produces...discourses and practices" (Bourdieu & Wacquant, 1992, p. 135). In other

words, participant 6's *habitus* reveals itself "to" her when she enacts her position in a

given social space. At the same time, though, someone's system of dispositions is

inventive:

> '[T]he *habitus*, like every 'art of inventing,' is what makes it possible to produce
> an infinite number of practices that are *relatively unpredictable*' even if they are
> also 'limited in their diversity' ([Bourdieu,] 1990a, p. 55, my emphasis). Such
> limits stem from both the checks upon action imposed by one's prior experience,
> and from the structure of the field as it is encountered. (Sweetman, 2003, p. 535)

Hence, participant 6's "knack" for discovering something that is "out of the

ordinary," "beyond commonsense," or "in need of answering" results from her *habitus*.

She has been trained to "sense" or "see" objects of inquiry in a way that is partly

conform the subfield. Thus, the word "compels" linguistically creates the impression

that something compels her, but this compelling occurs when her *habitus* reveals itself

in an encounter with a definite situation. In case of research, such an encounter is

constituted when an investigator engages with research participants, ideas, theory,

data, or a text, within the context of a field or subfield of study. Participants' discourses

illustrate these encounters; for example, participants may wonder what research

participants are "trying to tell [them]" (I.5A, 690); look for ideas that are "out there

waiting to be discovered" (I.8B, 574); "sit down with an idea" (I.2A, 574); "play with

ideas" (I.9A, 108); "look for a theory" (I.10A, 508); or "see categories [in data]" (I.12B,

113). Additionally, a text may "kind of [write] itself" (I.2A, 218-219); or a scholar may

engage "with the text" (I.11B, 567).

I believe that instances like these are linguistic indications of participants'

implicit acknowledgment or recognition that their system of dispositions encounters

situations in which their *habitus* reveals itself to them in practice. Accordingly, this

revelation gives them a feeling of "it all makes sense" (I.6B, 616) or "[t][hat's inevitable!;

[t]hat's exactly the way it had to be; it couldn't have been said any other way, because

that's the way to put those elements together" (I.9A, 744-746). However, without

language, this recognition could not be achieved, because language enables scholars to

objectify these moments of "subjective" recognition (in line with Ricoeur's [1991]

conception of distanciation). Participant 9 notes, in this respect:

> *Participant 9*: [U]ntil you possess the resource of language, you have no way of,
> in fact, distanciating yourself from yourself and, therefore, you can't enter into
> this dynamic in which you try something and you don't like it and then so
> you've learned a little bit about how to do it differently and then you do it
> differently and you don't like that and so there's a dynamic that takes over. It
> has to do with the stepping in and out of subjectivity into objectivity. And I
> think that's what science is about! As far as I'm concerned…that's the…the rest
> of it is window dressing…I mean, that's what a scientist is…is that ability to be
> both, experiencer and observer. (I.9B, 797-813)

Thus, in the practice of being subject and object, scholars recognize something

they have been trained to recognize (e.g., a "gap" in literature or theory, something

which has not yet been studied empirically, etc.). In turn, their *habitus* constitutes their

object of study in a particular way. For example, organizational communication

scholars may "see" compelling objects of knowledge pertaining to organizational

communication (e.g., "networks," "leadership," "culture"), because their system of

dispositions provides them with a particular intellectual gaze on the social world. As

Bourdieu (1984) argues, the participants thus have developed a "taste" which is

distinctive of their subfield. Physics scholars, for instance, may not "see" the objects

which organizational communication scholars "see." To illustration of this argument, I

present another passage from my conversation with participant 9:

> *Participant 9*: [At a certain point]…I can begin to see some shape in the ideas,
> that the ideas begin to make some kind of sense and seem to connect up with
> what we've been learning.
> *Boris*: And, so this idea that these ideas are starting to make sense and you even
> say that they are starting to take shape…um…do you have an idea that these
> ideas are already out there or are you really inventing these ideas?
> *Participant 9*: Oh, I think they're invented…they're invented within
> constraints…[long pause and sighing]. They, they…you work with what you've
> got! And you don't have an unlimited choice, you work with ideas, things that
> you've read, you work with things you've been told, you work with the
> language, you know a certain language, language allows you to do some things
> and not do others, so it's not an unconstrained process of invention. You're
> always working within a set of constraints. But within that set of constraints, it's
> invention, yes. (I.9A, 478-500)

The constraints to which participant 9 alludes can be interpreted as the historical

structures of the social spaces in which he operates as a scholar. More surprising,

though, is that participant 9 (who identifies with Plato's stance[34]) believes that ideas are

(in the most absolute sense) "objective." Nevertheless, it is in textwork (and the

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[34] "*Participant 9*: I guess I'm enough of a Aristotelian, or perhaps Platonian, I guess
probably even so, to think that there are forms—um, "Platonist" is the correct word—to
believe there are forms and that you search for forms and they're not, you know,
they're not packaged for you, you have to work through the material to get to the form
that it should be in" (I.9A, 711-722).

practice of language more generally), conducted vis-à-vis the "constraints" of a field's

linguistic market, that these ideas are "invented" with the intention of approximating

("discovering") their perfect form. Participant 9 describes this process in the following

way:

> *Participant 9*: [Long pause]…Well, you work it out. You discover the form as
> you go…you're looking for form and you have material that you put together,
> but the material in a way is just a means to an end…the end is the form…and so
> it's…Somehow in the back of my mind I'd still like to write the perfect
> essay…And I don't, but I keep trying to write the perfect…find the right
> expression, so that when you read it, it seems natural and inevitable. It's about
> form. (I.9A, 696-704)

> *Participant 9*: Well, the forms are not out there; they're in here; I don't know
> where the forms are [laughing]…They have to be found; they have to be
> discovered…and sometimes you get close and you say: 'Gosh, that was closer.'
> But it's very…you know, you don't get it quite right. (I.9A, 758-764)

In light of the idea that forms exist to which scholars aspire, participant 6

mentions that her recent publication (involving an interpretive, thematic analysis of

interview transcripts) "all of a sudden" began to make sense at a point in the "data

analysis" process:

> *Participant 6*: [Y]ou don't know when you're doing this kind of work, exactly
> what you're gonna find and, then, the joy in all of it is that, all of a sudden, it all
> clicks. Once we have these themes and we knew what the structure was, it was
> just like: 'Oh, it all makes sense.' It all came together. And I think anybody…if
> somebody read the data they'd see all these different stories and things like that.
> And they might pick up on some of these pieces immediately, because it was
> pretty easy to pick up on some of it. (I.6B, 612-619)

I believe that the "clicking" which participant 6 describes is an indication of the

moment of revelation or recognition which I just discussed. Interesting is that

participant 6 believes that others would experience a similar kind of recognition,

because the analysis was a matter of simply "picking 'it' up."[35] I question, though,

whether readers with very different sets of dispositions (e.g., scholars in different fields

of study or villagers in the south of Holland) also would have been able to simply pick

up these themes or the underlying conceptual framework which participant 6

developed.

Several other participants' discourses illustrate the moments of recognition they

have experienced in their textwork. For instance, participant 2 recalls the following

textwork episode:

> *Participant 2*: I remember [name of participant's advisor] and I had this conversation in late fall of 1983, and I went home over Christmas and I took a bunch of readings with me, and I sat down and basically the chapters of the dissertation sort of fell into place. And I came back in the spring, wrote a proposal fairly quickly, defended the proposal I think in early February of 84, and then defended the dissertation on the 1st of August 1984. I mean the dissertation kind of wrote itself, I mean, in terms of writing, it was probably the best writing experience I've actually ever had, I mean to this day, I mean it just kind of unfolded before me…all of the pieces of the jigsaw puzzle were there. I mean, I wasn't quite sure how they fitted together, but then once I had this fairly clear image of how the pieces fitted together and how the chapters of the dissertation would be laid out, then, you know, it just became a matter of just sort of writing it up. I mean, that is not to say that it wasn't, you know, a struggle. But, [for] the most part, I think, I mean, each chapter went through like two drafts and that was about it, you know? (I.2A, 210-232)

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[35] Sometimes, this moment is constituted as a moment in which someone is "hit by a lightning bold" (I.1A, 262), an "aha-experience," or even a spiritual experience. Many scholars speak of such moments. In the documentary *Derrida* (Dick & Ziering Kofman, 2002), for example, Derrida notices that his book *Of Grammatology* (1976) suddenly came together one summer.

Through words like "the dissertation sort of fell into place," or "it just kind of unfolded

for me," participant 2 suggests that his *habitus* revealed itself to him. Furthermore

interesting is that participant 2 believes his works springs from a resonance with certain

ideas, ways of thinking, ways of practicing textwork, and so forth:

> *Participant 2*: Yeah, I think maybe [the development of knowledge is a matter of operating on the basis of a belief], I mean, I'm not sure if I would put it as strong as a belief, maybe, maybe resonance. I think there's a way in which particular ideas and particular ways of thinking and understanding sort of resonate with you more. And I really don't think that it's a question of: 'Okay, which way of thinking corresponds or exactly would observe reality?' It's a question of: 'Which way of thinking just makes sense to you and sort of resonates with you in a particularly profound and maybe even moving way?' You know, that it actually sort of it connects with you on an emotional level…you know, and maybe that is faith, at some time. I mean, I don't do quantitative research, because it just never resonated with me…it just doesn't, you know, rock my boat, that kind of thing…you know. So, in that sense, yeah, I'm talking about faith of some kind. (I.2B, 347-387)

The experience of resonance described here illustrates that participant 2's system of

dispositions plays an important role in giving him a "personal," "emotional,"

"aesthetic" sense for determining how to practice research.[36] This sense is the

embodiment of the subfield's *doxa* ("belief," "faith") (I connected to *doxa*). It also

indicates that a scholar is implicated much more fully in his or her work ("with body

and mind," as Bourdieu suggests), than terms like "epistemology," "ontology,"

"axiology," or "metatheoretical position" imply. That is, these latter terms give the

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[36] Likewise, participant 4 says: "*Participant 4*: I think it's less an epistemology [participant 4's metatheoretical stance in the field of academia] than it is either…I would call it either an ontology or an aesthetic…And I'm increasingly seeing it as an aesthetic, which is that I have a *taste* [italics added] for a particular kind of inquiry that

impression that (academic) knowledge is developed in a very mindful, conscious way,

yet the participants' discourses indicate that their practice of producing knowledge

more or less involves their complete sense of being. In this regard, the use of separate

terms like epistemology, ontology, and axiology, might thus be questioned, because the

distinctness in meaning which these terms denote is relatively far removed from the

actual experience and practice of research.

In correspondence with the idea that the production of knowledge occurs in part

dispositionally, scholars do not readily acknowledge or consider the *doxa* which guides

their actions; and even if *doxa* becomes an object of reflection, scholars often want to

believe that their work extends beyond the system of presuppositions from which they

operate. Participant 7 mentions, for example:

> *Participant 7*: Well, I'm involved in the social construction of an account for things that are happening out there, I suppose. And I think my account could be right or wrong, and so, then, to sort of test, this is where the sort of empirical work comes in, to sort of test the reasonableness of the account, I guess. You know, I feel it's important to go back and compare it to the world…and, of course, you're gonna say: 'So then I have to persuade the audience that the test I've constructed really does reflect the real world.' And that's true, of course, but this is the crux of the problem…but, I mean, then, you know, there are obdurate, physical facts, you know. There are things that happen that nobody really disagrees about...and I don't think these are just a matter of some sort of arbitrary cycle construction. You can't construct a way a car that's about to hit you, for example, or a mob that's about to kill you, or something like that. True, it's a socially constructed account, but I think at some point it hits the real world. I mean, that's all I can say. It's just a feeling that I have. I suppose that's what it boils down to. And that's the faith…where the faith comes from. (I.7A, 397-438)

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has a particular kind of goals" (I.4A, 279-285).

Here, participant 7 connects the "faith" he has in his *habitus* ("it's just a feeling I have") and in the *doxa* of neopositivist human science research to the more or less unquestioned faith in life and death. Through this language-game, participant 7 creates the impression that reflecting on his *doxa* is a trivial, philosophical affair. By enacting this language-game, that is, participant 7 does not need to alter the way he has learned to do research and exist as a scholar. Such alteration would involve relatively high costs (e.g., time, effort, loss of social, linguistic, symbolic capital, etc.), in particular for a professor who has made a name in the subfield by enacting a neopositivist position.

Noteworthy, furthermore, is that participant 7's language suggests that he has debated this issue before ("of course, you're gonna say"), indicating that he has learned to argue in favor of his position and is more or less familiar with being attacked. It thus seems that he has learned to identify himself in opposition with others in the subfield by holding onto a particular *doxa*.

The importance of *doxa* also reintroduces the role of self-persuasion and persuasion of others (Gross, 1990) in the practice of textwork. This is so, because presuppositions have to be learned and incorporated and this process involves being taught how to persuade oneself and others about the legitimacy and value of these presuppositions. Upon reflection, scholars may try to circumvent the notion that they once were persuaded to accept a particular *doxa* and that this *doxa* now drives their academic undertakings. However, as I have suggested, it is unattainable to uphold strict borders between the discovery of something that is inevitable and the invention of a persuasive account. As the next two passages from my second interview with

participant 10 indicate, confrontation with this antinomy may therefore bring a

scholar's biographical illusion in conflict:

> *Participant 10*: I recall some organizational scholars following the mid 80s and the interpretive turn actually focusing so much on narrative that it led to a process of storytelling that at a certain point lost touch with the data and had as a greater impulse the need to get people thinking and stimulated about and idea, so that they could run with it and I couldn't view that as a form of scholarship, I suppose, just as I can view that as something that can be done in teaching, but it never felt comfortable to me as a social scientist, because there again…and we go back to my earlier interviewee with you…I always felt a need to stay as true as possible to the data as I understood the data…and so even my case studies were as much as possible an effort to stay true to the data. (I.10B, 396-406)

> *Boris*: [Y]ou still think that, within research, within a particular study… [does textwork involve]…a form of persuasion…or is it more the idea that….that the ideas by themselves make sense without you having to persuade the reader?
> *Participant 10*: [Long pause]…Well…that's an interesting juxtaposition, isn't it? Yeah, that's nice…that's nicely done…I guess, here I would differentiate between the work I do that's theory-building and model development in which I see it as more persuasive because you're marshalling a lit. [literature] review, you don't have much data, you're creating a model and your developing propositions. I see that as very persuasive. When it gets to the point of testing aspects of that model and you're doing, you know, what I would call more normal science work…then, I don't know…again I tend to think that the data tend to speak for themselves, but although I know that's not true because you're writing it up to magnify your interpretation of data, but there's less reason for dispute and therefore there's less concern with persuasion.
> *Boris*: Mm…but that's on a scale then? I mean less reason for dispute rather than, you know, absolute reason for no dispute [laughing]?
> *Participant 10*: Uh, yes, yes, yeah, there's less reason for dispute, because no, no, I certainly would not come from that camp of strict positivists who would say, you know: 'There's no other interpretation.' (I.10B, 998-1032)

As these passages illustrate, participant 10 identifies himself as "a social

scientist" who intends to "stay true to the data," implying little need for the practice of

persuasion—because the data are persuasive in themselves. Nevertheless, participant

10 cannot deny the tension implicated in enacting this position. Consequently, participant 10's questioning of his textwork in light of the knowledge discovery/invention antinomy implies a questioning of his position in the subfield (and the biographical illusion that is associated with this position). During this moment of reflection, participant 10 faces the antinomic character of the knowledge discovery/invention language-game which I have discussed in this section. In so doing, he encounters a seemingly paradoxical question: How can "a social scientist who stays true to the data" maintain that his work involves the linguistic construction of these data?

To conclude, based on the illustrations I have constructed in this section, I surmise that participants try to argue (often in implicit ways) against the arbitrariness of their position. This occurs by almost automatically ("dispositionally") alluding to some form of discovery of knowledge when talking about their production of knowledge, even while doing so, they realize that it is impossible to escape the idea of invention. It might be that exposure of the arbitrariness of their position is fenced off through the enactment discovery/invention language-game, because such exposure forms a threat to a scholar's privileged position. Through this language-game, then, participants seem to valorize the significance of their work and keep up the appearance that their work involves unraveling complexities which cannot be grasped through simple "common sense." Rather, understanding these complexities requires an "academic sense" or, more precisely, a "sense" that is associated with operating in their subfield of study. "Sense" is thus a privileged commodity attained through the

acquisition of cultural, social, and symbolic capital. Hence, the ability to "see" is

implied in being a member with certain kinds of expertise, "taste," and credibility, of

the same social space. Consequently, this language-game prevents scholars from seeing

how the social conditions in which they operate affect their activities as scholars; their

commitments to the constitution of these conditions; and how operating from systems

of dispositions which are linked to these social conditions might involve constituting

breaks with the realities they try to investigate. It seems, therefore, that these

"discoveries" (i.e., recognitions that knowledge is produced when one's *habitus*, the

embodiment of these conditions, encounters definite situations, and implications for

knowledge drawn from these recognitions) are perhaps just as important as the

"discoveries of knowledge" more commonly associated with solving murder mysteries

and jigsaw puzzles.

CHAPTER V

CONCLUSIONS AND MIRRORS

In this final chapter, I step beyond the conclusions made in chapter IV by discussing the broader implications of this study. Subsequently, I reflect on some of the limitations of this investigation and suggest ideas for future research.[1]

Implications

Through this study, I have illustrated the importance of reflecting on the social conditions of a subfield of study which make the practice of producing knowledge possible. Language plays an important role in enabling the practice of this reflexivity.[2] Language can help scholars understand how they construct an object of knowledge (organizational communication, psychoanalytic psychology, or the sociology of knowledge) in relation to the social conditions in which they operate. Thus, as

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[1] I realize that this text might be appropriated in unforeseeable ways. However, it seems worthwhile for "I," the author, to propel this text into interpretational space with some direction.

[2] In this study, I have focused on participants' language practices without implying that language is all there is to social life. My analysis of language needs to be seen in relation to the objective structures of the fields and subfields in which participants operate and in relation to "nonlinguistic" practices (e.g., browsing library stacks, touching keyboard, staring at an empty page, etc.). I am not arguing for another antinomy here. The social practice of language is essential for the constitution of meaning, but it fuses with other practices, making the linguistic/nonlinguistic divide untenable. Hence, I agree with Bourdieu (see Bourdieu & Wacquant, 1992) that it is important to keep the confluence between the linguistic and the non-linguistic in mind.

Wittgenstein (2001) notes, language is able to constitute a "form of life" (or "life-form")

(p. 10) within a given social space. By reflecting in language on language, scholars in a

field or subfield of study might therefore continuously try to induce consciousness of

the effects of their investments (*illusio*) in language-games which are part and parcel of

their dispositions (*habitus*) and of the social conditions (relationships between their

position and those of others) which make these games possible. Hence, through this

reflexivity, they might be able to improve the way in which they understand an object

of knowledge, because they are more critical about the breaks with investigated realities

which their investments in an academic field induce.[3]

In line with Bourdieu's ideas, this study suggests that the homology between a

participant's dispositions and his or her position in the subfield of study is relatively

high. During the routine practice of research, this homology gives scholars the feeling

that everything is "in its right place." However, this study suggests that when scholars

are asked to reflect on their "second nature" or "practical sense," some participants'

habitus is conflicted, because it is confronted with the antinomic language-games which

constitute their "sense for the game." In such case, the *habitus* "misfires," so to speak,

because it is not sufficiently "capable" or "prepared" to cope with the contradictory

oppositions it produces. Such an instance of reflexivity, then, marks the *return* from

holiday of language rather than the going on holiday of language of which Wittgenstein

(2001) speaks. In so doing, this event creates opportunity for considering how a scholar

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[3] As Ernst Cassirer (1942) argues, language may in this regard "be compared with the spear of Amfortas in the legend of the Holy Grail. The wounds that language inflicts

might be enwrapped in an academic game which induces breaks with the realities he or she tries to investigate. In other words, an event like this helps a scholar realize that he or she constitutes *a* view on *a* reality from *a* position in *a* field of study's language-games, involving *a* historically developed set of presuppositions, practices, and stakes.

Unawareness of (or unwillingness to recognize) these breaks tends to create potentially fallacious knowledge about particular objects. That is, by not reflecting on one's position in the *academic* field or subfield of study from which one constructs knowledge about those who most often operate in non-academic fields (and thus do not share the same *doxa* and *habitus*), one risks projecting dispositions into the other which are not his or her own. In so doing, this "scholastic epistemocentrism" (Bourdieu, 2000a, p. 50) creates knowledge that is mostly *academic* (in the literal sense of the word), that is, based on the *doxa* of a given field or subfield of study. Interestingly, the discourses of only a few participants in this study suggested a critical awareness of the way their scholarly "subjectivity" affects the construction of knowledge. Frequently, participants seemed to persuade themselves that their reconstruction of the practical "logic" of research "subjects" was "accurate," "inevitable," or "self-evident." Participant 6's statement about the way she analyzed the data for one of her studies offers a fitting illustration of such "self"-persuasion:

> *Participant 6*: I think anybody, if somebody read the data, they'd see all these different stories and things like that. And they might pick up on some of these pieces immediately, because it was pretty easy to pick up on some of it. (I.6B, 620-622)

upon human thought can not be healed except by language itself" (p. 327).

In chapter IV, I maintained that the "inevitability" of one's "findings" results from a scholar's implicit acknowledgment or recognition that his or her system of dispositions encounters a situation in which his or her *habitus* reveals itself to him or her in practice. It is important to reflect on these moments of recognition and on the modes of operating (*modus operandi*) which underlie the construction of knowledge in a field of study, because such reflexivity helps to guard scholars against, as it were, "themselves." That is, this reflexivity guards scholars against their own blindfolds; their own eagerness to make an argument that is innovative, valued, etc.; their investment in a contest for capital through the conduct of research; and their attachment to a position whose constitution took much time and energy.

A common way of guarding against imposing academic "logic" on those studied is referred to as "member checking" (Lincoln & Guba, 1985, p. 236). This practice suggests that it is useful for a researcher to talk with research participants in order to check whether his or her academic data translations resonate with the translations of the people who are investigated. Following this logic, participant 1, for instance, mentioned that she spoke extensively with key "informants" for one of her studies on organizational change:

> *Participant 1*: [I]f anything what was happening that I wasn't quite sure that I'm really handling it this way or that way…I mean I really used them heavily and that's why I make mention of their…that they even read the chronology. I asked them to read the chronology, because I really felt like I was telling their story and, if I was telling their story, it wasn't just my story, it was their story and that there had to be some degree of buy-in by them that this is how they were seeing things. It was their accounting for their own behavior for that reason…so I did that a lot. There was innumerable phone calls and site visits and, you know,

lunches and drop-ins and all of that to do that, because I really wanted to get it right from their perspective. (I.1B, 753-763)

Here, participant 1 engaged in self-persuasion as well by presuming that her informants would be able to reflect in accurate ways on their own dispositions by providing them with a pair of *academic* spectacles. In this instance, participant 1 is uncritical of the ways in which her own dispositions are linked to the subfield of organizational communication studies and of the ways in which transposing these dispositions onto agents who operate in a different field might be problematic. Thus, a researcher might ask research participants to view themselves from an *academic* point of view when discussing his or her translation of their actions, without acknowledging the conflation of different kinds of "subjectivities" which this request invokes. Accordingly, a researcher might persuade himself or herself, research participants, and the community who reads his or her work, that "this is the way people really act, for they confirm it themselves!"

Participant 2 provides another example of the way in which scholars' inattention for the "bewitchment" of their own language-games can affect the production of knowledge about the social world:

> *Participant 2*: [W]hen we originally conceived of [a new concept], we saw [our analysis of this concept] as this sort of, kind of, you know, perhaps fun, interesting…[type of analysis] analysis. [W]e came up with these sort of…these conditions [which could be indicative of the concept]…sort of features of it. And now other authors have taken up [this concept], you know….all of a sudden, [this concept] has been ontologized, it is an organizational condition, right?! It's like: "Okay, can we find [this concept] or not [laughing]?! How does [this concept] work?! Well, we never intended it like that, right?! I mean, it was meant to be just sort of an interesting, fun, you know…[type of analysis]

analysis, perhaps you would say, a different way of thinking about the relation between [this concept] and [another concept] in organizations. So, I mean, literally, I mean, we invented the term "[label used to denote this concept]." But the idea that there's actually a condition of [the concept] was never further from our heads, you know? (I.2B, 407-425)

This passage illustrates that an academic concept (an abstraction created to understand an object) easily can be reified through research. Hence, sometimes the very act of conducting research (and textwork in particular) on an object of knowledge *instates* (*creates*, *"realizes"*) this object and brings it into being. It is therefore important for scholars in a field to reflect on the extent to which the realities they investigate have been created or constituted through *their* own acts of research. In other words, besides studying an object of knowledge, it is important to consider how the social conditions which make this research possible in the first place affect the understanding of the object under investigation.

Thus, by changing the "rules" of the language-games (and *textwork*-language-games more particularly) in a field of study, so that scholars are held responsible for explicating how their *modus operandi* induce breaks like these, a field as a whole might improve its battle against "the bewitchment of [its] intelligence by means of [its] language" (Wittgenstein, 2001, p. 40). By subjecting its subordinating its "'selfish' interests to the rules of dialogic confrontation" (Bourdieu, 2000a, p. 119), that is, a field might be able to offer itself the means for creating the consciousness which is necessary for bettering its production of knowledge. Correspondingly, a change in the research practices of a field only can be sustained if the community as a whole *appreciates* this kind of reflexivity; that is, by positively evaluating the integration of this reflexivity into

a field's texts and conversations. In other words, through education and socialization practices, a field needs to develop a system of dispositions (*habitus*) which respects this kind of reflexivity and propagates the value of collectively confronting scholars' "personal" interests in text and conversation. Hence, by using language to improve language, reflexivity is amphoteric in that it wounds as well as heals the basis of scholarship.[4]

After drawing these implications, I complete this dissertation by pointing towards the limitations of this investigation. Where possible, I use these limitations to fabricate springboards for future research.

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[4] Let me repeat Bourdieu's (2000a) words here to substantiate this argument: "Each individual conquest of reflexivity (such as the discovery of the scholastic illusion) is destined by the logic of competition to become a weapon in the scientific struggle and so to become a necessity for all those engaged in it. No one can forge weapons to be used against his opponents without having those weapons immediately used against him by them or by others. It is from this social logic, and not some illusory, sanctimonious deontology, that one can expect progress towards greater reflexivity, imposed by the effects of mutual objectification and not simple and more or less narcissistic turning of subjectivities upon themselves. The scientific statement of the logic of the functioning of the scientific field can also help, by making it more conscious and more systematic, to arm the mutual surveillance within the field and so to strengthen its efficacy (which does not rule out cynical uses of the knowledge thereby offered)" (p. 119).

Limitations and Suggestions for Future Study

The Imprecision of Participants' Objects of Study

The first limitation I discuss is perhaps not a limitation in the strictest sense of the word. However, I think it is important to briefly reflect on the relatively narrow focus of this investigation.

This investigation's emphasis on the way scholars enact an identifiable position in a given social space through language has led to an imbalanced discussion of the way participants constitute specific objects of inquiry in specific studies in light of the broad object of organizational communication. I presume that the constitution and investigation of an object of inquiry plays in itself an important role in the enactment of a position in a field. However, I decentered this relationship in this study, because investigating this relationship for each of the twelve participants would have been too major an enterprise for the scope of this dissertation.[5] A more encompassing investigation within a particular field or subfield of study would be worthwhile, nonetheless, because it would contribute to an even better understanding of the way scholars position themselves in a field and thereby lead to a more insightful understanding of the ontological complicity between scholars and their social spaces.

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[5] The scale of an undertaking like this can be illustrated by comparing such an analysis with Bourdieu's (1991b) book *The Political Ontology of Martin Heidegger* in which he

The Puzzles of Bourdieu's Work

During the course of this investigation, I have begun to question several aspects of Bourdieu's ideas. These questions result from the interaction between Bourdieu's texts (and those of others), my own textwork, and the text which constitutes this dissertation. Because I have not been able to resolve these issues so far, I present them here with the intent of encouraging future research.

Effects of Fields on Fields. First of all, my reflections have emphasized participants' positioning through language practices like textwork in the context of the main subfield of study in which they operate (organizational communication studies). In so doing, I have examined only infrequently the effects on these practices of other fields of study, the larger field of academia, the political field, and so on. This limitation offers several avenues for continuing this kind of research, as I will suggest in the next paragraphs.

Bourdieu defines a field as social microcosm or social space constituted as a network or configuration "of social relations, structured systems of social positions within which struggles or maneuvers take place over resources, stakes, and access (Bourdieu, 1990a)" (Everett, 2002, p. 60). Maybe the most commonsensical notion of a field is a physical field; that is, a physical place or precinct with more or less clearly defined boundaries. However, with a field of study, the academic field, the social field of classes, or the political field, it is difficult to determine these borders, even though they are enforced, upheld, lowered, and so forth, in practice.

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studied the political ontology of only one scholar.

Especially in recent publications, Bourdieu has written about the interaction between fields (academic and non-academic) and their imbricated nature, yet these interactions still are not understood very well. Indubitably, this interaction has major effects on the positioning and research practices I have examined. For instance, the way a participant constitutes his or her position may depend on his or her functioning as an agent in organizational communication studies, but certainly also (and at the same time) in other fields of study, such as his or her university, the social field (in which he or she might be a parent, partner, relative, and friend), fields constituted by various organizations (voluntary, religious, sports), and so forth. Consequently, each agent constitutes a position in fields with different systems of dispositions, differences in *illusio* and *doxa*, and differences in the worth of his or her forms of capital. This creates complicated (and at times conflictual) interactions which impinge on a scholar's identity work in multifarious ways.

Clearly, these interactions affect the practice of textwork and research in general, yet, to my knowledge, no research has been conducted on this issue. For example, universities may place different demands and emphases on scholars in terms of what to focus on in their work. A college dean may, for instance, stress the importance of obtaining research grants funded by prestigious foundations like the *National Science Foundation* or prefer book instead of journal publications, while the subfield of organizational communication studies may reward scholars in particular for their journal publications. The *National Science Foundation*, as a field in itself, may in turn privilege certain types of and approaches to research.

When confronted with differential demands like these, scholars may be conflicted in terms of which position in which field to "prioritize." Participant 4's discourse illustrates such a conflict:

> *Participant 4*: I see myself as a teacher…of adults in various settings. And so there's not that big a difference between a graduate seminar that I might teach here on a Tuesday and then a seminar I might do with Time Warner Communication on dialogue on a Wednesday. It's really not that different. And it's often the case that I get more out of the work that I do outside of the university. So, I see it as pretty fluid or pretty continuous, you know? There have been many times in my life when I've wondered about that, you know, because lots of people really do want to make it *an opposition*…Yeah, and, I mean, there are institutional ways in which it can look that way. I think it grows out of the typical sort of worst-case scenario that everybody then wants to plan around, you know, like the professor who basically, you know, ignores his research and teaching to do consulting, but that's never been what I've done. I mean, I think I've always tried to let all the work inform the other work…but I've taken a tremendous amount of criticisms over it, because sometimes academics are worse about it than non-academics. But oftentimes, like any boundary spanner, you know, when you're in one place they're saying: 'Why aren't you in the other place?'" (I.4A, 215-238)[6]

Participant 4's "personal" struggles are in part the embodiment of the struggles of the subfield of organizational communication studies, which always seems to have struggled with antinomies like theory/practice and academic/non-academic work. In light of the supposed difference between academic (e.g., professor, scholar, theoretician) and non-academic identities (e.g., consultant, entrepreneur, politician, musician), the *habitus* of a scholarly agent can thus be conflicted in that it has to account for this

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[6] Likewise, participant 10 stated that, although the subfield of organizational communication studies currently is more accepting of operating on the intersection between academic and non-academic fields, there was a time when he "became schizophrenic about [the tension involved with doing both academic research on

distinction on a personal level. Nevertheless, this struggle is not his or her own (even

though he or she is often held accountable on an individual level); that is, the *field's*

historical antinomies oppress a scholar in this instance, making it difficult to practice

research, teaching, and so forth, because he or she has to legitimize his or her position

in light of dichotomies like these. For example, a scholar might need to defend his or

her position, because this position is considered "too theoretical," "not theoretical

enough," "not practical enough," or "too applied." Thus, theory and practice or

academic and non-academic work often are seen as existing on a continuum, implying

that one can be more or less of one or the other. However, this view cloaks the

underlying antinomies in light of which individual scholars constitute their

biographical illusion in a field and it continuously invokes undue struggles for

positional distinction. Conversely, some might also use these antinomies to their

personal "advantage," as participant 4's words indicate ("the professor who basically

ignores his or her research and teaching to do consulting"). This also hinders the

practice of scholarship, although in a directly opposite way to those who are oppressed

because they actually do want to do good research, teaching, and service.

   A study like this highlights these issues and perhaps introduces (or re-

introduces) these matters into the discourses of a social space. Nevertheless, the

incentive to speak about these matters must be constituted in some way by the game

itself. That is, the game must reward scholars by means of capital in return for their

practice of this kind of reflexivity. Hence, as Bourdieu argues, reflexivity must become

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organizations and organizational consulting]" (I.10A, 361).

part of a field of study's *habitus* in order to have an impact on the way a field works. Because a field of study is linked to a web of other fields of study within the field of academia, though, this solves only part of the problem. Consequently, although it is a lofty goal to state, a true transformation only could be realized by making this kind of reflexivity part of the *habitus* of the academic field at-large.

Effects of *Habitus* on *Habitus*. Closely following the previous discussion, I have not been able to explain the interaction between the systems of dispositions of the various fields and subfields in which an agent acts. The word "*habitus*" has a rather monolithic connotation, yet Bourdieu assures that a system of dispositions functions in dynamic ways. At first, I considered that systems of dispositions, like fields themselves, operate on different levels of aggregation; that is, as a set of Matryoshka nesting dolls which nicely fit into each other. This view now seems simplistic, for it suggests that different systems of dispositions interact with each other in relatively harmonious ways. As my study suggests, this interaction can be conflictual at times.[7] In fact, I anticipate that conflict is needed to cause a *habitus* to change. Bourdieu's spends relatively little words on the way a *habitus* changes and more study on this subject would be valuable.

Further, in line with the previous discussion, it is unclear how interactions between sets of dispositions of multiple fields affect the practices of a single agent. Can one really speak of "*his* or *her habitus*," in this regard, or should one always specify a

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[7] Could it be that multiple systems of dispositions operate in the same field at the same time? Or would such differentiation automatically imply the constitution of subfields?

statement like this by saying: "his or her *habitus* in field X"? Similarly, how does the

conflicting of different systems of dispositions affect the practice of an agent? How is

he or she able to act at all in such case rather than being startled by inertia or

indecision? Questions like these require additional study as well.

Last, it is unclear how an agent learns to embody a system of dispositions

through education, socialization, and the experience of operating in a field. I expect

that this inculcation involves complex social practices. I have talked unassumingly

about such practices without basing my arguments on investigation. It is interesting to

contemplate how someone learns to see an object of knowledge; that is, how the

spectacles provided by a field of study become a view. This process surely involves

persuasion of self and others about the plausibility of a *doxa* which might seem foreign

at first.[8] Nevertheless, as this study illustrates, this *doxa* can become quite natural,

logical, and incontrovertible, over time. Comparatively, Latour and Woolgar (1979)

relate academic communities to exotic tribes. These tribes employ various rituals of

initiation and indoctrination to ensure the sustenance of their legitimate collective

existence.

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[8] As Bourdieu suggests, if a scholar operates in a field while he or she is *conscious* of the fact that he or she is operating in a field (i.e., if he or she practices reflexivity), "the notion of field functions as a conceptual shorthand of a mode of construction of the object that will command, or orient, all the practical choices of research. It functions as a *pense-bête*, a memory-jogger: it tells me that I must, at every stage, make sure that the object I have given myself is not enmeshed in a network of relations that assign it its most distinctive properties" (Bourdieu & Wacquant, 1992, p. 228). In other words, through reflection, "I," the researcher, must continuously examine the way my view

It is important to examine persuasion practices like these more carefully in various fields of study to understand the practice of this *doxic* communication within a field of study, because such reflexivity will help to understand how the academic "subject" is constituted and reconstituted in light of research "subjects." Subsequently, this might lead to a better understanding of how advances in knowledge in a field correspond to the social conditions which give rise to the production of this knowledge by fostering scholarly positions and dispositions.

Effects of Practices on Practices. Finally, let me say a few words about the interaction between the practices investigated here and other practices in light of Bourdieu's emphasis on the practical connectedness of social life. Although I have argued that positioning practices and textwork are integral parts of the overall practice of research, I did not mean to imply that the practice of textwork is all there is to research. As this study suggests, research involves a plethora of intersecting embodied practices, varying from walking through the library to stumbling upon a colleague who happened to have read a book which provides the "missing link" between two seemingly unconnected arguments. Besides, many other types of activities, like teaching, mentoring and collaborating with students, giving workshops or presentations to non-academicians, talking to one's children, playing music, reading non-academic literature, and so forth, have an affect on the practice of research.

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and practice of research is constituted by the historical relations I have embodied in the form of my system of dispositions.

Most participants seem to refrain from compartmentalizing their lives in terms of their practices (academic and non-academic)—as participant 9 puts it, "it's all just work" (I.9A, 165-166).[9] In light of this, it is never possible, really, to study *a* practice in its solitude, because other practices constantly intervene. Nonetheless, in future studies, it seems wise to give participants more liberty to explore the interaction between the practices they believe have important ramifications for their practice of research.

Questions of Time and Context

To conclude, I ask how the inferences drawn from this study may pertain to other scholars in similar or dissimilar contexts. For instance, how would graduate students, assistant professors, or associate professors in organizational communication studies reflect on their practice of reflexivity, enacting an identifiable position in a field or subfield of study, and textwork? It seems likely that experienced scholars operate from a more deeply ingrained set of dispositions than new assistant professors or

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[9] Participant 4 is more reflexive than most of the other participants about the intersections between the activities which constitute his "life." He says: "*Participant 4*: I remember when I was in college, having an argument with this young woman about…she was gonna become a doctor, but she was deeply Christian. And we talked about how one could be extremely a devout believer and also be a scientist. And she wanted to say that these things could be separate and they could inform each other, but one wouldn't necessarily contradict the other…I've always tried to be more transparent about that connection. And, I guess, I've always figured that the best way to be a strong force as an academic is to be open and vulnerable about those kinds of relationships and connections between one's own sort of concerns, passions, and desires and the work that you do" (I.4A, 447-461).

doctoral students. It also seems plausible that some of these advanced professors have a better sense of the game than inexperienced ones, even though, being so engrained, they might find it difficult to articulate or reflect upon.

Throughout this inquiry, I have reflected on the effects of tenure, promotion, experience, and acquired capital, on practice suggesting that the perception of the game might change as a scholar's trajectory unfolds. In so doing, though, I have based my inferences on information drawn from conversations with a distinct set of scholars whose trajectory more or less parallels the trajectory of the subfield[10] and who all actively pursue research. Although most of these scholars seem to have a good "feel for the game" (Bourdieu, 1990a, p. 66), they often seem to be quite entrenched in it as well, implying less distance than I had expected. I therefore anticipate that scholars in different stages of their career, but also scholars whose careers emphasize research to a lesser degree, will reflect on the practices investigated here in other ways. Examining such variations is useful, because it enables further reflection on the constitution of *illusio* and *habitus* over time and under different circumstances.

Moreover, I wonder to what extent my inferences are field-specific and subfield-specific. Would my reflections have differed, had I interviewed all the twenty-eight participants who comprise the sampling frame of this study? More generally, will the reflections of scholars in other subfields of communication studies (health communication studies, interpersonal communication studies, etc.), human science

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[10] This is certainly not true for all the twelve participants. That is, some participants entered the subfield relatively late in their academic career.

fields like management studies, psychology, or sociology, or natural science fields like

physics, chemistry, or biology, be considerably dissimilar? I presume that the effects of

the historical developments of fields imply important differences in these fields'

systems of dispositions. However, the effect of a common history may also imply that

there will be some commonalities between, for instance, subfields within a field of

study or between fields within the fields of the human or natural sciences. In fact, in

the most general sense, even the human/natural science distinction seems imprecise,

because the field of *skholè* comprises both these social spaces. However, at present,

statements like these are relatively speculative and they would benefit from additional

investigation.

It seems appropriate to ask these questions of "transferability" (Lincoln & Guba,

1985, p. 124) because they probe to what degree a researcher would draw similar

inferences if he or she were to conduct a similar study in a different time and context.

Responses to such questions only can be drawn, however, through supplementary

studies. Yet, besides questions of transferability, I wonder whether the reader, through

reading this text, has been able to reflect on the way he or she enacts a position in a field

or subfield (of study), and how this enactment affects his or her production of

knowledge (academic or non-academic). This is the actual "transferability" I hope to

have induced through this text. Correspondingly, I hope that reading this text has

shown convincingly how the practice of reflexivity can improve the production of

knowledge in a given social space by heightening consciousness about the social

conditions which make this production, but certainly also its producers, possible.

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APPENDIX A

SCRIPT FOR INTERVIEW I

Thank you most kindly for taking the time to talk with me today. As I stated in my introductory e-mail message, for my doctoral dissertation study I would like to learn more about how organizational communication professors see themselves and what role their identity plays in what John van Maanen calls "textwork." Your participation in this research is important, because you frequently engage in these practices and I would greatly appreciate your input and help. In this first conversation, I would like to get a sense of who you are as a scholar and what you do. In the follow-up conversation, we will take a closer look at your academic practices. First, let us speak about how you generally see yourself as a scholar:

1   Where did you go to graduate school? Where have you gone from there until now?

2   What and who have or had important influences on you in your career, both academically and non-academically?

3   For what reasons have you chosen an academic career? For what reasons is organizational communication important to you? What is it that you hope to accomplish through your work?

4   How would you currently describe yourself as a scholar, for example in terms of your metatheoretical stance or the main topics your research focuses on? What might be reasons for why this description has or has not changed throughout your career?

5   How do you think others in the subfield of organizational communication studies see you as a scholar? Why do you think others have come to see you this way? How do you think you can influence their views? What might be reasons for why such influence is important or unimportant to you?

Now, I would like to get an idea of what you do as a scholar, in particular research-wise:

1   When looking at your day, week, or semester, how would you say you organize your research work?
2   What practices does your research work consist of? In your answer please describe mental practices that enable you to conceptualize and design a study, practices that enable you to implement a study, and writing and reading practices.
3   What choices or decision points are important when it comes to publishing your research?
4   For what reasons may your research practices have changed throughout your career? What practices work or do not work for you?
5   How do your individual research practices differ from your collaborative research? What are important differences when comparing the production of a single-authored to a co-authored publication?

In the follow-up conversation, I would like discuss more specifically what we have talked about today by analyzing two publications, one best reflecting what you tried to accomplish in organizational communication studies at the beginning of your academic career as an assistant professor or the publication with which you have had the most impact as a young scholar, and one that best reflect what you currently are trying to accomplish with your research or the publication with which you recently have had the most impact on the subfield of organizational communication studies. The publications may be journal articles, book chapters, or complete books:

1   What two publications would you like to choose? (If needed, please take a few days to think about this and send me your choices by e-mail.)
2   Is there anything you would like to add that we might have overseen in this conversation about how you work and think as a scholar in the subfield of organizational communication studies?

Thank you most kindly for talking with me today. Without your participation, this study could not be a success. I appreciate your input and help, and look forward to our second conversation.

APPENDIX B

SCRIPT FOR INTERVIEW II

Thank you most kindly for taking the time to talk with me today. This is the second interview with you for my doctoral investigation. In this conversation, I would like to learn more specifically how your practice of producing academic publications relates to the way you see yourself. Let us therefore look at the two articles you selected and begin with discussing your research practices:

1. What sort of practices do you recall undertaking to produce the earlier publication? For example, you could think of mental practices that guided you in the conceptualization of this piece, reading, talking with others, presenting it at a conference, having others read rough drafts, practices of conducting the study, and so forth.
2. What about the second, more recent publication?
3. For what reasons might your practices (not) have changed over time?

Let us now turn more specifically to talking about the relationship between the publication and what you are or were trying to accomplish with your work:

1. What is (or was) the major message or idea you wanted to convey with the early publication? What about the second one? What could be reasons for (no) change?
2. How does the first publication reflect what you are (or were) trying to accomplish in the subfield of organizational communication studies? What about the second publication? What could be reasons for (no) change?

Further, I would like to talk more about some matters pertaining to audience:

1. For what reasons did you choose this journal (or publisher) for the first publication? And what about the second publication? For what reasons might certain journals or publishers better suit what you are (or were) trying to accomplish with your work?

2. In what ways do you conceive of audience? How would you describe the audience or audiences of the first piece that you had in mind while writing? How do (or did) you want this audience to see you? What writing strategies did you use to reach this audience? What about the second publication? What are significant changes or stabilities you see when comparing these two publications?

3. What were your reasons for describing other people, for example research participants or other scholars you cite, in the way you did? How may your way of representing people (not) have changed over time?

To continue, I would like to get a better understanding of some specific writing strategies you used in this publication:

1. How would you describe the voice that you have used to present the first study? What about the second publication? How would you describe the perspective you take in this publication? What about the second publication?

2. To what extent does your voice indicate your own involvement as a researcher in these studies? How does the voice of these publications reflect what you are (or were) trying to accomplish with your work as a scholar in the subfield of organizational communication studies?

3. What reasons did you have for organizing each publication as you did?

4. As what kind of "genre" would you classify the first publication? What reasons did you have for writing it by following the conventions defined by this genre? What about the second publication?

To conclude, let us talk more about similarities and differences between the way you were and worked at the beginning of your career and the way you are and work at the moment:

1. Overall, how has the way you see yourself as a scholar, writer, and thinker changed or not changed over time when comparing your

publications in general? What would be reasons for this change or stability?

2   Is there anything you would like to add that we might have overseen in the two conversations we have had?

Thank you most kindly for talking with me today. Without your participation, this study could not be a success. I appreciate your input and help.

APPENDIX C

LIST OF INITIAL THEMES

Anomalies in Practice
Career Practices
Editorial Practices
Generic Textwork in Practice
Genres in Practice
Indefinite Interpretation
Individual and Collaborative Textwork
Influences of Time and Generation
Ironies
Knowledge Discovery and Invention
Mentoring Practices
Methodological Matters
Moralities and Ethics
Outlet Decision Practices
Persuasive Productions of Knowledge
Practices of Power
Publication Structures
Read and Written Identities
Reflections on Reflexivity
Reflections on the Subfield of Study
Reifications
Representations
Resistances
Self Positioned in Light of Others
Self Positioned in Light of Self
Separations of Spheres
Serendipities
Stabilities and Alterations in Practice
Structure/Agency
Subfield Effects
Teaching Practices
The Practice of Audiencing
Voicing Text/Textualizing Voices

APPENDIX D

INITIAL TYPOLOGY OF THEMES

Knowledge Production as Discovery and Invention
Positions in the Subfield of Organizational Communication Studies
      Legitimating the Subfield Itself
      Pigeonholing and Being Pigeonholed
      Positioning Self in the Subfield
Reflections on a Scholarly Life
      Extensions of Oneself beyond Scholarship
      Overextensions
      Practical Ethics
      Reflections on Reflexivity
      Serendipities
The Practice of Textwork
      Eco's Imaginary Conversation
      Textwork Conventions and Form
      General Reflections on Practice
      Interpretational Practices
      Textual Multivocality

APPENDIX E

DEFINITIVE TYPOLOGY OF THEMES

The Ontological Complicity between Scholars and the Subfield
      I/*Doxa*
      I/We
      We/They
The Constitution of a Position in the Subfield via Textwork
      Voicing a Text
            Voice Outside/Inside Text
            Textwork Conversations with Self/Other
      Voicing the Production of Knowledge
            Knowledge Product/Production
            Single-/Co-Authorship
The Discovery/Invention of Knowledge

VITA

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