

Special Issue Introduction

The Strengths of Our Methodological Divides: Five Navigators, Their Struggles and Successes

This special section of *Keio Communication Review* had its impetus at a plenary session at the 2004 annual meeting of the International Communication Association held in New Orleans, Louisiana, USA. Wolfgang Donsbach, then ICA president-elect and program chair for the 2004 meeting invited me to develop a plenary program that would highlight the strengths of the many methodological divisions that too often are seen as dividing communication researchers into diverse and often isolated camps -- quantitative versus qualitative, administrative versus critical, objectivist versus interpretive, culture vs culture, context vs context, and so on. Given a plenary as a venue and normative practices, we all thought immediately of identifying a single speaker but what we finally decided upon was quite different. A brief history of the process is useful in understanding the outcome as it is presented here in this special section.

Twenty years had passed since the landmark 1983 “Ferment in the Field” issue of the *Journal of Communication*, the first and still most notable compilation of the different and back then often hostile and contesting methodologies by whatever names they might be called -- perspectives, approaches, and methods -- that were increasingly claiming the study of communication as their own. Eighteen annual meetings of the ICA had intervened between 1985 when ICA’s annual meeting theme was “Beyond Polemics: Paradigm Dialogues” explicitly bringing together for comparison, contrast, and hoped for dialogue the many competing perspectives. Fifteen years had passed since the publication of the two volume set entitled “Rethinking communication” which was itself given impetus by the 1985 meeting (Dervin, Grossberg, O’Keefe & Wartella, 1989a,b). In 2004, residues of these past conflicts and divisions still marked the field (and still do today). Donsbach, wisely however, saw beyond these residues.

To plan the 2004 plenary I started by interviewing more than 35 communication scholars/researchers representing as many differences as I could including every continent of the globe as well as the main field divisions in methods, perspectives, and substantive foci. The results of the interviews surprised me. On the one hand, the animosities and competitions of the past were still present although as one interviewee commented “they tended to run silent and separately”. The field had grown markedly and differences that in 1985 could not have avoided each other now had developed entire departments nested around their chosen foci and approaches.

Further, the field had also grown in complexity. Whereas in 1985 virtually

everyone named the major polarities dividing the field as qualitative versus quantitative, administrative versus critical, and objectivist versus interpretive, in 2004 my 35 interviewees named an astonishing array of divides with an even more astonishing array of labels. Further, when asked who might be ideal plenary speakers to address the strengths of our divides, the 35 interviewees named more than 100 possibilities with only three nominees being named more than once. At the same time, it was clear that most of the younger researchers were entirely unaware of this contest-filled history and in many cases blissfully unaware of the alternative approaches that only 20 years earlier had collided and attempted to dialogue.

On the surface then it appeared as if the calls for dialogue had failed and what had resulted was the building of separate castles with very wide moats. Given the very nature of the academy and the ways in which discourse communities grow and rigidify around those pursuing common interests in common ways, this was a not surprising change. The surface of the change, however, belied a more complex underpinning.

Those I interviewed for the most part had developed a separate-but-equal stance toward the field and its many divides. The majority of those I interviewed were what I came to call “method purists” -- researchers who maintain an allegiance to a particular sub-set of methodological approaches in relative ignorance of alternatives. Here we have researchers and scholars practicing their specialties within their relatively closed discourse communities without knowledge of or disregard for those who might be studying the same phenomena in different ways using different vocabularies and assumptions in different discourse communities.

One example of the degree of isolation manifest among method purists came from a quick retort from one world-renown interpersonal scholar who when asked if he was aware of any interpersonal scholars who used critical approaches replied “That critical stuff, oh you mean what was in that special issue of the *Journal of Communication*.”

Surprisingly, the purists outnumbered what I came to call the “method segregationists” -- researchers who are at least marginally informed about alternatives and the underlying philosophic debates but who ardently believe that competing research methodologies are incommensurate. With this group, of course, ignorance sparks a stereotyping which is seen as unfair and hostile by those stereotyped, as, for example, when quantitative scholars reduce all qualitative research to merely “exploratory” or qualitative researchers conjure statistics without understanding what statistics can and cannot do as the enemy of holism and thus the enemy of any attempts to speak of humans humanly. Beyond ignorance and stereotyping, however, among the method segregationists we find a substantial number of communication researchers and scholars who genuinely believe that competing approaches are incommensurate and therefore must exist in isolation from each other. Any attempt at integration would by definition be folly.

It is fair to estimate that the method purists and method segregationists represent the majority of researchers in the communication field. This should not surprise us. The methodological choices for studying communication are many and finely nuanced. If we take the host of polarities which now drive these methodological choices and toss them into n-dimensional space, we can conclude, at least for now, that the resulting incomprehensible map is the methodological map for the study of communication and, to be fair, for the social sciences generally.

Surviving in the academy is not easy. One must find a center and a direction and a discourse community home and to do this amid the complexity of this n-dimensional space requires choices. It is small wonder then so few people have tried to traverse these divides by bringing multiple methodologies to bear on the practices of their research.

What is remarkable about the field of communication, however, is that we have more researchers attempting to traverse this “in-between” than other fields and that even amid method purists and segregationists the pulse of the in-between journey remains even if almost inarticulate a still present hope. I call the researchers who traverse the “in-between” method integrationists. What was most interesting about the interviews I conducted was that virtually every interviewee could name one person -- even if they saw that person as seriously deluded -- whom they saw as attempting to traverse the divides. And, virtually every department no matter how otherwise pure had one member of their faculty who held out the hope for method integration.

One conclusion, then, was that any attempt to address the strengths of our methodological divides had to come from integrationists who were themselves as dispersed and potentially isolated from each other as were the method purists and segregationists. A second conclusion was that whoever these persons might be, what would be instructive to learn from them would not be a traditional account of directions for doing research in an integrationist way. Rather, what would be instructive would be a more intellectual-autobiographical account of the journeys they have traveled. It was this conclusion that led to the calling the selected integrationists “navigators” and asking them to focus on their journeys and their successes and their struggles in finding strengths in the methodological divides of the communication field.

To identify the five navigators whose essays are presented here, a review was completed of articles in communication journals that focused on field divergences and convergences. The essayists were purposively selected to represent diverse foundations -- very different specialties, very different phenomena of interest, and very different cultural roots. What the five essayists have in common is that their career lines all have shown repeated forays into integrating perspectives, approaches, methods -- methodologies by whatever names they may be called. And, each has carved out something genuinely new and different by journeying in this in-between

way.

The individual essayists tell of their travels in some detail in the essays that follow. Suffice it to say that each engages for us the intersection of their personal journeys with their intellectual journeys, the implications and impacts of their choosing communication as an approach to studying the social and behavioral phenomena of their own interests, and the ways their attentions to multiple and sometimes competing inputs have informed their inventions,

The essayists describe their journeys in very different contexts -- thinking about communication theory (Craig); studying audiences and users communicatively (Dervin); reaching across paradigmatic divides to understand media and their effects (McQuail); developing new and culturally relevant approaches for measuring public opinion (Ito); and, studying journalism in a more fully communicative way (Zelizer);

What binds the five essays together despite these differences is their common attentions to solving research problems by risking travels down unfamiliar roads and their willingness as authors to open for us a small window into how they have navigated across sometimes seemingly impossible divides.

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DERVIN Brenda, GROSSBERG Larry, O'KEEFE Barbara, & WARTELLA Ellen (eds.). (1989a,b). *Rethinking Communication: Vol. 1: Issues; Vol. 2: Paradigm Exemplars*. Beverly Hills, CA: Sage.

A Path Through the Methodological Divides

by Robert T. CRAIG

To say no more than that our methodological divides in communication studies have enriched my work and made it more rigorous would understate the case. My struggles to find a path through the field's methodological divides have essentially defined my career, and the products, limited as they are, of those struggles have been among my principal contributions to scholarship in the field.

My serious involvement in communication studies began as an undergraduate student majoring in speech at the University of Wisconsin-Madison in the late 1960s. There I first encountered the great divide between the humanistic and scientific traditions of speech communication studies (Craig, 1990). The humanists defended an ancient and recently flourishing tradition of philosophical, historical-critical, and practical studies in rhetoric. The insurgent social scientists had, by contrast, the cachet of all that was new and modern, of "science," of "communication," of rebellion against tradition in a rebellious era (a time of political and cultural uprisings on university campuses). Among communication scientists, the whole discipline of rhetoric was often dismissed as an embarrassing anachronism soon to be superseded by modern social-psychological studies. Among humanistic scholars, the advancing social science of communication was often caricatured as intellectually shallow, crudely reductionistic — and yet deeply threatening to the status of humanistic studies. A budding rhetorical theorist who was equally attracted to the social sciences, I was torn between the two sides of this divide and fascinated by the great intellectual puzzle that it presented. The intellectual puzzle, as I later came to understand it, was fundamentally about theory and practice.

What became known in the twentieth century as rhetorical theory derived from a Western tradition extending back to the ancient Greek sophists, teachers-for-hire who espoused relativistic ideas and offered practical instruction in the art of persuasive public speaking (Kennedy, 1980, 1996). The philosopher Plato (1998) mockingly criticized sophistic rhetoric for its disregard for truth and lack of any systematic theoretical basis for its practice. Aristotle (1991), who wrote the first philosophical treatise on rhetoric, addressed these shortcomings, although not in ways that would have satisfied Plato or indeed have satisfied many subsequent philosophical and scientific thinkers down to the present.¹

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Aristotle justified rhetoric on practical grounds, arguing that the kinds of issues usually debated in public discourse are inherently matters of opinion about which the truth cannot be known with absolute certainty. Proponents of various conflicting views do their best to persuade the public, and there is reason to hope that truth will prevail against error if both are defended with equal rhetorical skill. Success in persuasion, as Aristotle pointed out, is partly a matter of luck, but some speakers are consistently more effective than others, so there must be some systematic basis for success that can be discovered, theoretically articulated, and taught as a practical art of rhetoric. However, there can be no scientific guarantee of persuasive success in the contingent world of practical affairs, any more than there can be scientific certainty about the truth on matters of opinion.

This Aristotelian methodology, abstracted from its philosophical context and rendered in up-to-date English as I have done here, is so surprisingly compatible with Deweyan pragmatism as to warrant the suspicion that something important has been lost in translation. Nevertheless, efforts to amalgamate Aristotle with pragmatism have been attractive to scholars (myself included) who have struggled to bridge the divide between the humanistic tradition of rhetoric and modern social science. “Neo-Aristotelian” research that applied concepts from Aristotle’s rhetorical theory in scientifically oriented critical and experimental studies persisted into the 1970s but was finally rejected by humanists and scientists alike (Craig, 1990). A method that reduced rhetorical studies to a causal analysis of persuasive effects made thin gruel for humanists, who preferred to follow other streams of the rhetorical tradition and emerging interdisciplinary critical theory that led to profounder, subtler ways of interpreting and appreciating rhetorical texts. A key problem for social scientists was that rhetorical theory, no matter how you cut it, is not scientific theory. The rhetorical tradition has accumulated over its long history a hodge-podge of practical exemplars and techniques, critical commentaries, and philosophical arguments, but social scientific theory, as it was understood by the mid-twentieth century, requires something quite different: systematic models and causal explanations of phenomena that are empirically testable. If there are systematic principles to be found for practical success in persuasion and other kinds of communication processes, it follows from this view that rigorous empirical study and theory construction are the way to find them.

It was, then, a quest for rigor and enrichment that drew me from rhetorical studies at Wisconsin to the graduate program in Communication at Michigan State University, which was and is a leading center of communication science. I had not rejected rhetorical theory but wanted to gain a solid grounding in communication theory and empirical research before choosing a direction for my academic career. The philosophical background I had developed in rhetorical and political theory as an undergraduate was enriched in graduate school by studies in the philosophy of science, social science metatheory, system theory, social psychology, and advanced

quantitative methods. My dissertation research took me to the antipode, a place at the very opposite end of the theoretical world from my undergraduate studies in rhetoric. Essentially a hi-tech persuasion study (Craig, 1976), it was based on Galileo Theory (Woelfel & Fink, 1980), which, as I only later realized, is ironically the exact antithesis of neo-Aristotelianism.

Galileo Theory models cognitive and communication processes as the lawful motion of concepts in a multidimensional mathematical space. To apply the theory it is necessary to measure the differences among a set of concepts at multiple points in time by means of a metric multidimensional scaling procedure that can, in principle, achieve any desired level of precision. The multidimensional space can represent conceptual structures in human populations or in populations of messages. The ultimate goal in developing Galileo Theory is to discover mathematical equations that can explain the motion of concepts in multidimensional space with degrees of precision approaching that achieved by theories of physics. Not much progress toward these cognitive laws of motion seems to have occurred in the thirty-odd years since Galileo Theory was invented, although the multidimensional scaling technique is still in use (e.g., Doerfel & Barnett, 1999).

The measurement and data analysis procedures developed in conjunction with Galileo Theory work rather well, and the philosophical argument that undergirds the theory is equally sophisticated, however wrong (Craig, 1983). My dissertation study was largely unsuccessful (Craig, 1976), but it took a few years for me to think my way out of its rigorous logic. The path I followed took me from cognitive social psychology to theoretical debates in cognitive science and Artificial Intelligence (Craig, 1978, 1979), and finally back to a reinvented amalgamation of Aristotle and pragmatism, for which I coined the term, “practical discipline” (Craig, 1983, 1989). Continuing the journey under the banner of practical discipline, my methodological stance has become progressively richer and more rigorous as I have struggled on through the field’s methodological divides.

I initially became aware of what appeared to be a critical gap in the logic of Galileo Theory: that it has no rigorous link from cognitive processes to practical activities, no way of describing or explaining how messages are actually produced. Even classical rhetorical theory, with its catalogs of commonplaces and figures of speech, seemingly does a better job of this than Galileo Theory; it describes specific techniques by which ordinary practical activities of speechmaking can be accomplished. Artificial Intelligence (AI), defined as the science of designing computational algorithms that model complex cognitive processes, can be equally as rigorous as Galileo Theory in principle, but in an entirely different way. The most rigorous methodological criterion for AI is that theoretically based algorithms must model in precise detail the cognitive structures and processes that actually generate complex intelligent activities such as conversation in a natural language (Boden, 1987). Proponents of “strong AI” speculate that computers will eventually be

capable of human-like thought and understanding, even consciousness and emotion.² In principle, an AI-based communication theory could progressively merge theory and practice, reducing the fuzzy art of communication to an exact technical science that even a robot could be programmed to learn. Galileo Theory also entailed that the practice of communication could become an exact science, but not by creating precise models of ordinary communication practices as AI might do. Instead, Galileo Theory proposed to *replace* ordinary communication practices with a precise technology based on the principles and methods of Galileo Theory itself (Woelfel & Fink, 1980). In this perspective, there is no critical gap between Galileo Theory and practical activities because *Galileo Theory is embedded in practical activities that can constitute ways of communicating*.

This insight led me to a new understanding of the problem of theory and practice, couched in terms of socio-cultural institutions (Craig, 1983, 1996a, 1996b). All theories are embedded in practical activities. A theory is only “a theory” by virtue of being designated as such and having some role in the practices of some institution. Scientific theories are designated as such within the institution of science and have meaning and value by virtue of the role they play in scientific practices. To “apply” a theory means to abstract certain practices from one institutional context and import them to another context where their role may be quite different. Insofar as the role of theory in scientific practices is oriented to the causal explanation of empirical phenomena, the application of scientific theories in other institutional contexts may involve transferring certain scientific practices (e.g., of categorization, inference, experimental manipulation, measurement, etc.) to nonscientific contexts where they function as instrumental “techniques” that may clash with contextual values and meanings. This would certainly be true of Galileo Theory. Were it to play the sort of role in public culture that its authors envisioned, the imaginable consequences of this “Galilean rhetoric” would be bizarre and unattractive (Craig, 1983). Plato would turn over in his grave.

The institutional interpretation of the problem of theory and practice suggests a rigorous methodological principle: that theories must be adapted to the institutions in which they are embedded, which means they must be evaluated with reference to the specific practices, values, and meanings that have evolved historically within those institutions.³ In communication and other fields of social theory, this methodological principle implies a profound reflexivity of theory and practice. Theories in those fields are *about* social practices. Those theories may be embedded in scientific practices, but they also frequently have roles internal to the nonscientific social practices they are about. For example, theories of media, as available ways of thinking and talking about media, can be embedded in the practices of media sources, critics, and audiences as well as media scholars. Social theories, therefore, are not only embedded in practices, they are often about some of the very practices in which they are embedded, and have the potential to change those practices (Carey, 1989;

Giddens, 1984; Taylor, 1989).

In 1983, I thus arrived at the following problem: What would be the institutional structure of an academic discipline — not a pure science or an applied field but a true “practical discipline” with its own distinct subject matter, theories, and methods — that would address the right kinds of questions to evaluate theories that are reflexively about some of the very social practices in which they are embedded (Craig, 1983, pp. 411-412)? Six years later, my first sketch of a solution was published:

... practical discipline provides a distinctive and appropriate methodological rationale for communication [studies], one that is firmly rooted in the history of the field, that covers the full range of its activities, and that names the central purpose around which those activities, at their best, cohere. ... As a practical discipline, our essential purpose is to cultivate communicative *praxis*, or practical art, through critical study. All of our work does, or should, pursue that purpose. (Craig, 1989, p. 98)

To illustrate the idea of practical discipline, I relied on two exemplars, one the classical art of rhetoric, the other a pragmatist conception of methodology. (Yes, back to Aristotle and Dewey, but how much richer than before!) As elaborated through the exemplars, a practical discipline would:

... cultivate a dialectic between theory and practice ... ground specific practical techniques in more general principles ... offer idealized conceptions of practice that are nevertheless judged finally by their usefulness ... [be] concerned with the ends of practice as well as the technical means ... and ... cultivate an appreciation of the ultimate paradoxes ... that confront practice (Craig, 1989, p. 105).

In attempting to articulate a coherent position, I found it helpful to venture further through the methodological divides to confront a broad array of alternative positions in the field. The idea of practical discipline was enriched and made more rigorous by putting it into dialogue with empirical science, hermeneutics, critical theory, action science, and other views (Craig, 1989, pp. 105-116).

Subsequent writings have focused on particular aspects of practical discipline such as the constitutive role of theory (Craig, 1993), a methodology for constructing grounded practical theory (Craig & Tracy, 1995), the dialectics of theory and practice (Craig, 1996a, 1996b), how various traditions of communication theory can be engaged in dialogue on the practice of communication (Craig, 1999a), and, again, the convergence of the tradition of practical philosophy stemming from Aristotle with contemporary pragmatism — a convergence that I acknowledged has yet to struggle with some of the implications of critical and postmodern methodologies (Craig, 2001a).

The concept of metadiscourse (discourse about discourse, or metatalk) has become increasingly important since the mid-1990s as a way of understanding the

dialectics of theory and practice that opens the process to more rigorous empirical investigation (Craig, 1999b; Craig & Tracy, 2005). How exactly does theory become embedded in practices, how is it adapted to practices, and how does it change practices? Metadiscourse would seem to be a key medium in which theory and practice communicate. Metadiscourse is an intrinsic aspect of all of our communication practices (Craig, 1996a, 1999a, 1999b, 2005, 2006). We talk about our ongoing communication in order to manage and reflect on the process for various practical purposes. Theory becomes embedded in, is adapted to, and changes our communication practices as we import theoretical vocabularies into our practical metadiscourse. For example, concepts from therapy have become deeply embedded in ordinary discourse about communication and are shaping the practice of communication in what Cameron (1999) has described as the contemporary communication culture.

Far from inhibiting or frustrating my work, my struggles to find a path through the methodological divides in communication studies have been, and, I believe, will continue to be, an invaluable stimulus to creativity, and a goad to ever more encompassing rigor, that has more than enriched my work. Indeed, it has largely constituted my work.

NOTES

1. Philosophers by and large have tended to agree with Plato that rhetorical success is empty if not reprehensible unless it is the product of a disinterested effort to discover what is true and right. To think otherwise is mere sophistry, they might say. On the history of conflict between rhetoric and philosophy, see Ijsseling (1976).
2. Although technical AI fields like robotics and natural language processing are apparently doing quite well, critics have decisively refuted the speculative claims of strong AI, which rely on a fundamental ontological error (Dreyfus, 1992; Searle, 1980; Weizenbaum, 1976; Winograd & Flores, 1986). Like Galileo Theory but on a vastly greater scale, AI has persisted as a technology while losing its philosophical punch.
3. This implies that standard criteria of scientific validity may not be sufficient for evaluating theories that are embedded in nonscientific practices (Craig, 1983, 1989, 1993, 2001b). It also implies that theories may be evaluated differently in different cultures. It does not, however, imply that good theories are completely consistent with traditional cultural practices. Theories are useful because they both challenge as well as confirm existing ideas and practices (Craig, 1999, p. 131).

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Navigating an Impossible Dream: A Synergy of Possibilities, a Convergence of Constraints

by Brenda DERVIN

It is a gross over-simplification but useful to suggest that metaphorically branches of scholarship are architecturally constructed of castles surrounded by moats. The castles are intended to be strong, The moats are wide, protecting as they do the common agreements which define academic discourse communities -- agreements on vocabularies, definitions, classifications, assumptions, primitive terms, normative standards and practices, and foundational definitions on what is acceptable to study and how (Layder, 1990). This does not imply that practitioners within a particular discourse community have complete consensus. But it does set the outer edges of acceptable disagreement. (Dervin, 2003; Dervin, Shields & Song, 2005)

Necessarily, the infrastructures that link the castles to each other -- the moats and rivers between -- are too often underdeveloped, and treacherous. Why this is so is, of course, the subject of many contentious debates rooted in different theories of how creative social agents (scholars) act in, make sense of, and travel the journeys through time-space that are their scholarly lives, navigating as they must the socially inscribed and buttressed edifices that empower but also constrain the very idea of scholarship as activity.

It is also an over-simplification but useful to suggest that most scholars stay safely inside their castles, inside the discourse communities to which they have been educated and in which they etch out their own places. This makes perfect sense, of course, for we all know it is a hard enough thing to etch a place inside a castle without also taking on the task of constructing a journey along the uncharted moats between castles.

Yet, curiously, the field(s) of communication studies was at least at its inception focused on traveling uncharted roads. When the various departments of communication began to coalesce they brought to them castle-builders who traveled to this new meeting ground from many different origins -- rhetoric, journalism, language studies, cultural studies, sociology, anthropology, systems engineering, international development, to name but a few. Each had visions of how castles should be built.

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Each inhabited its own language community, its own discourse. But each shared what in retrospect was an impossible dream -- allegiance to the idea that the “old” disciplines and fields were not adequately zoning in on communication as foundational phenomena. Sociology’s first interest was society; psychology’s the individual personality; anthropology, culture, and so on. Those who gravitated to the “new” communication departments dreamed of making communication the central phenomenon, not a phenomenon on the periphery.

Even as these communication departments were being built, communication itself was becoming at one and the same time a hot topic for study and everybody’s business. Global events were propelling forces -- the rise of media; the increasing human consciousness of the other; the evidence of the impact of communication strategies on world-changing events in Germany, in India, in the Soviet Union; the diminishment of authority as a universally acceptable organizing force for human affairs. One might say that “communication” as a phenomenon worthy of study was an idea whose time had come.

For those of us involved near the beginning -- as I was as a graduate student in one of the founding doctoral programs in communication in the U.S. -- it was an exciting time. Our mentors in the Michigan State University program came from many origins -- cybernetics, sociology, psychology, rhetoric, comparative studies. They all seemed to accept that they each had something to offer the common effort to study communication as a foundational concept. There was a vital intensity afoot. In these early years, my mentors mostly ignored their differences. This was possible at MSU because everyone had seemingly accepted an empirical scientific approach to unearthing causal laws of communication as their common axiological focus. The possibility that there might be those who did not accept the same premises was at best only whispered in the hallways.

Simultaneously, however, there were other communities of communication scholarship being built on different axiological premises and as events evolved and scholarly societies formed, it is fair to say that the up-to-then unstated differences in how different communication researchers and scholars thought it appropriate to study communication imploded. My own journey got entangled in and at the same time was served by this implosion.

Prior to going to graduate school, I was trained as a journalist and worked as a public information officer for a variety of non-profit organizations. Fancying myself a reasonably talented communicator, I intended to devote my career to creating informational messages which improved the lot of average citizens. My well-meaning, albeit naive intentions, collided frequently with the communication mandates of the organizations for which I worked. I saw the top-down transmission oriented mandates of public education organizations as elitist at best. In the 1960s, as a journalist, for example, I wanted to interview women regarding their views after the Papal encyclical on birth control and was told by my editor that women’s opinions

were not important. As a public educator, I was mandated to develop instructions to teach impoverished citizens how to budget their money wisely and as I faced an audience of poor urban city Black Americans I understand in one of those moments of flashing insight that a bunch of well-to-do educators who did not budget their money particularly wisely wanted, as if by magic, poor people to be more perfect human beings than they.

In short, I understand that neither the well-meaning editors nor the well-meaning educators understood how to communicate communicatively. Their communication programs were based on premises of persuasion not dialogue. This is why, I reasoned, they so often failed so miserably. I was convinced, albeit naively, that given sufficient power, persuasion campaigns could be used to produce all manner of deleterious outcomes and occasional beneficial ones but they could not effect long-range improvements of the human condition in the absence of dialogue. In short, I believed then, as I do now, that communication to be communicative must be designed communicatively. In short, communication to be communicative must build bridges between different interpretive/contextual worlds.

At this point, my evidence was primarily intuitive stubbornness. Raised for the early years of my life as an orphan, I had been the object of many well-meaning directives for the betterment of my person. Left to my own thinking devices, I had -- rightly I might add in retrospect -- concluded that these directives were 99.9% wrong. My entire life experience primed me to believe that we had to find ways to communicate communicatively. Now, 40 years later, my conclusion remains the same and is buttressed by mountains of evidence.

By communicating communicatively, I have never meant the explosion of spontaneous people-to-people communicating that, for example, the new technologies now make possible. These modes have strengths and weaknesses -- a subject for a different essay. Rather, my obsession has been with re-inventing communication communicatively -- designing practices and systems which are inherently communicative.

An outsider looking at the trajectory of my work will note that it appears that I have "studied" a vast array of substantive topics in a vast array of contextual arenas. My career trajectory looks like that of a dilettante. In actuality, however, I have studied only one thing -- how to design communication communicatively whether that communication be research (as in studying audiences and users), or practice (as in intersecting with users of information systems), or policy planning (as in constructing policy alternatives to serve citizens well), or campaign design (as in constructing messages to facilitate citizen self-improvement), or community-building (as in conducting research symposia). I have persisted in this central focus despite the fact that, to my great surprise, what most communication scholars want from communication is not dialogue but the enactment of change as it suits their visions of the world. In short, even we communication scholars have difficulty being

communicative.

But the very fact of my saying that should warn the reader that I am, of course, caught in my own worldview. It turns out, an understanding some 45 years in the making, that my vision of communication as dialogic possibility and the competing vision of communication as persuasive instrumentality have an inherently dialectical and paradoxical relationship with each other. If I command you to be communicative, I have, indeed, become persuasive. If you dare to listen, you have become dialogic.

In many ways this one understanding crystallizes for me my years as communication practitioner and researcher. I still am obsessed with re-inventing communication communicatively. I am still appalled at how little both experts and scholars seem to understand what that might mean. But I am humbled by my obsession knowing that it is a more complex and elusive goal than I could have ever imagined and knowing that that knowing is strength.

I share this special section of this journal with other communication scholars who have themselves crossed impossible divides. I am still perplexed by why some people dare to do so -- albeit all too few; and why most people do not. In my own case, I suspect that it was because I was driven by an obsession -- a belief that communication could work better, not perfectly, but better; and because that obsession was driven by life experience.

This obsession led me to act, in fact, in opposition to the best academic practices. I kept seeing convergences across fields and paradigms where others saw divergences. I taught myself to attend to these and read voraciously. In the process, of course, I built for myself a home in a shaky boat on the moat between the academic castles. I made being "in-between" my way of being. It is, in fact, a wonder that I survived although I was probably helped by the fact that I was one of the first reasonably well-known females with a PhD in communication in the U.S. And, I was helped by the fact that a field anchored in practice -- library and information science -- found great value in my work and funded, published, and cited it.

It was important that I was able to anchor myself in a field of practice because of all the gaps which I have attempted to cross the one between research/theory and practice has been most central. To redesign communication communicatively is to design practice whether the living practices of daily performance or the practices that are the interstices of organizations and institutions.

As I traveled back and forth between practice and research/theory, I kept running into gaps that few others seemed to be paying attention to. The biggest one for me was the inadequacy of current interviewing/surveying practices for understanding audiences and users. I took the requisite survey research courses and studied the data on interviewing practices and found them wanting. These approaches kept imposing system and expert construed world views on their respondents and thus missed the extraordinary gaps that exist between expert models and lived

experience. The argument, in reply, much repeated, was that people are too unique and we have no choice but to map people to systems. But I persisted in believing that we could design communication communicatively and both efficiently and effectively find ways to map systems to people.

I turned to qualitative research that pursued attentions to uniquenesses but, in turn, found them wanting because while their interviewing approaches were more open-ended they were usually capricious and they unwittingly imposed expert worldviews either in interviewing conduct or data analysis. I concluded that we simply had no theory for the practice of interviewing communicatively. I set out to develop one.

The understanding of the interviewing gap led to what I call my 35 year detour. I wanted to re-design communication and had to start by re-designing that subset of communication that is studying the needs and views of audiences and users so that communication systems and practices could be designed to be responsive. For 35 years I have worked on what is now called the Sense-Making Methodology, a communication based approach to research design, practice, and analysis for studying audiences and users. While I call this work a detour, it is in actuality more an exemplar of the larger goal. Research is communication and hence to re-invent research practice is to re-invent an example of communication practice.

Because I was obsessed and had cast myself into the moat between castles, I paid too little attention to the castles I was peering into as I sailed by. In retrospect I understand that it mattered little what the chronology of my exposure to different academic discourse communities was because I grabbed from each what I found of value and continued on my way. It is fair to say that it wasn't until I had been an academic for 25 years that I deliberately set out to find and conquer a new discourse community. Up to then, accidents happened and brought with them serendipitous exposures. At Michigan State's doctoral program it was, for example, primarily non-US males who welcomed me -- a rare female doctoral student -- as friend and on the way introduced me to continental scholarship and the whispered name "Marx." Searching for a first academic job, it was Syracuse University's School of Information Studies willing to take a chance on a newly minted female PhD. Then an administrative upheaval there forced me to search again and I ended up at the University of Washington where I found my most important mentor -- Richard F. Carter (2003), the man whom I call "the genius I needed", a scholar perhaps more obsessed with the idea of re-inventing communication than I. Being one of the first female PhDs in communication, I was in the right place at the right time to be elected the first female president of the International Communication Association. This catapulted me into a world where all approaches to communication scholarship met, and, alas, often -- back then in 1985 -- collided.

The accidents continued and each new exposure brought me to new insights into what it would mean to communicate communicatively. In the process, it is fair

to say that I traversed most of the divides of the communication field -- quantitative and qualitative, objectivist and interpretive, administrative and critical, universal and contextual. I also visited with some depth of exposure virtually all the fields from which our founders emigrated -- sociology, psychology, anthropology, cybernetics, journalism, language studies, film studies, rhetoric, and so on. Along the way, I made some good friends via their writings -- Bateson, Beltran, Bourdieu, Bronowski, Bruner, Douglas, Foucault, Freire, Gadamer, Galtung, Goffman, Giddens, Habermas, Hayles, O'Neill, Rorty. It is possible to trace the impacts of every one of them in my development of Sense-Making. They now travel with me everywhere I go. They are a diverse lot. We'd probably have trouble getting them to be civil to one another at a meeting. Now, 35 years later, I still see myself as standing in-between.

My Sense-Making Methodology (Dervin & Foreman-Wernet, 2003), for example, posits that to understand users we must see them as changing as they move through time-space, sometimes struggling to align themselves with community and system and sometimes struggling to separate themselves. It posits that struggles with self identity and collective identity are inherent struggles in the human conditions and potentially pertinent to every moment of intersection between a person (as audience member, as user) and a system. It posits that we must ask informants how they see these struggles in their own terms and that this is in fact a more efficient way to understand another's views or needs than to require that they map themselves onto our imposed expert maps. It posits that we can build, using the new technologies, interfaces for bridging the gaps between artificial expert worlds and the material/interpretive worlds of the everyday.

Of course, because of my obsession with reinventing communication for practice, I have extended the inventions I have designed to a variety of practical contexts. A few examples include journalistic design, the library reference interview, the conduct of a graduate seminar, the structure of an academic dialogue, and the review of a plethora of academic theories about media effects (Dervin & Foreman-Wernet, 2003; Dervin, Foreman-Wernet, Jansen, Schaefer & Shields, 2001; Dervin, Shields & Song, 2005). On a larger scale, I have extended the same dialogic principles into the design of the 1985 "paradigm dialogues" meeting of the International Communication Association (Dervin, Grossberg, O'Keefe & Wartella, 1989a,b) and to design of the 2004 plenary (Dervin & Song, 2004) which served as the impetus for this special issue of *Keio Communication Review*.

I continue to define myself as "in-between" because focusing as I have on communicatings as the "verbings" by which people make and unmake their movements through time-space, I at one and the same time include, for example, both the interests of sociology and those of psychology in my purview. Naturally, in assessments of my work, those who ascribe to sociological positions see me as too psychological, and those who ascribe to psychological positions see me as too

sociological. I feel I am in good company in this regard because Anthony Giddens, long considered the doyen of British sociologists, has been object of the same criticism (Best & Kellner, 1991).

Yet, it is important that I conclude by returning to the humility with which I feel I must now address my goal to re-invent communication communicatively. On the surface it may appear as if I want everyone to be out here in the moat, in the in-between with me. It may appear as if I define this moat as where open-minded and true communicative goals exist. But what I have come to understand about communicating is that those who reside with seeming safety inside castles are using communication in their struggles to stay inside, to stay in line; while those of us seeming to be risking danger in the moats are doing the same thing -- using communication in our struggles to stay outside, to fall out of line. We all travel in between.

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An Empirical Approach to Political Communication and a Critical Approach to International Communication

by ITO Youichi*

The first flash of memory that I have of my life is of seeing my father in a Japanese Navy Hospital in 1944. I was two-and-half year's old. My father's warship had been attacked by an American submarine and had subsequently sunk in the southern Pacific Ocean. After drifting for half-a-day in the middle of the ocean, my father was miraculously rescued by a Japanese ship. The second flash of memory that I have of my early life is that of something that occurred about half-a-year later. Some thirty Douglas B-29 bombers were roaring in the sky above us. I saw this scene from inside the air-raid shelter in our garden.

Probably because of these experiences, when I was a small boy I read many books and saw many movies dealing with the Japanese-American War. As I grew up, my interest turned to the political, military, and diplomatic *causes* of the Pacific War as well as the Second World War in general.

One of the important points I learned was that there was no dictator in Japan who was the equivalent of Hitler in Germany or Mussolini in Italy. Some Chinese and Americans have accused Tojo Hideki and Emperor Hirohito of being dictatorial. However, Mr. Tojo became the Japanese Prime Minister only six weeks before the attack on Pearl Harbor. When the island of Saipan was invaded by the American forces in August 1944, one year before the Japanese surrender, Mr. Tojo apologized to the National Diet (the Japanese parliament), resigned as prime minister, and completely retired from politics. Another well-known reason for Mr. Tojo's resignation was that he could not get along with Mr. Kishi Shinsuke, who was the Minister of Commerce and Industry at that time and who became the prime minister after the war (1957-1960). The pre-war Japanese Constitution did not give power to the prime minister to fire his cabinet members. Actually, after the war, Mr. Tojo said that weak prime ministerial powers or weak leadership was the main cause of political confusion and the Japanese tragedy that resulted from it. Mr. Tojo was hanged in 1948 as the head of the Japanese government at the time of the attack on Pearl Harbor.

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** AUTHOR'S NOTE: The Japanese, Chinese, and Korean names in this article, including the author's, are given in their traditional order, the family name first followed by the given name.

Emperor Hirohito may be responsible for not being able to stop the Japanese aggressions in China or the attack on Pearl Harbor. The reason for this was that the Emperor was not given much political power under the Japanese Constitution at that time either. Compared with the German Kaiser, the Russian Czar, or the Chinese Emperor, the power of the Japanese Emperor was much more limited. Partly because of a unique Japanese history in which the Emperor (the head of state) and the Shogun (the head of the military) had shared supreme power since the late 12th century, there existed many contradictions and ambiguities in the prewar Japanese Constitution promulgated in 1889, which later brought about the difficulties of controlling the military.

Another important reason was Emperor Hirohito's personality. He was basically a marine biologist and was not much interested in the military or politics. Actually, he published three academic books in his lifetime. All these books deal with shellfish, seaweeds, and marine bacteria. Even the Soviet Union and the Japanese Communist Party, who wanted to abolish the imperial system in Japan soon after the war, could not present any evidence that Emperor Hirohito promoted or expressed any pro-war jingoism in the 1930s and 40s. Then who or what was responsible for Japanese history after the Manchurian Incident of 1931 until the end of the Pacific War in 1945? This question was left unanswered for me until quite recently.

After I graduated from Keio University, I entered the Japan Broadcasting Corporation (NHK). Two years later, I got a Fulbright scholarship and went to Boston University to study communications. In a class on international communication we read *The Silent Language* and the *Hidden Dimension* by Edward Hall (Hall, 1959; Hall, 1966). Edward Hall was, and I understand, still is, highly respected as a cultural anthropologist. However, I was shocked that there were so many misunderstandings in his books regarding Japanese customs, way of life, and way of thinking. These misunderstandings later on influenced my ideas regarding academic methodology.

Although I returned to the NHK after one-year's study at Boston University, I quit NHK after a year and entered the Graduate School of Keio University. When I became an assistant professor, my students and I read the Japanese translations of Edward Hall's two books mentioned above as well as his *Beyond Culture* (Hall, 1976). I was intrigued by what the Japanese translators of *The Hidden Dimension* and those of *Beyond Culture* (altogether four, but all different) have to say in their postscripts to the extent that although Japanese readers might feel uncomfortable or embarrassed by some of the descriptions and observations of the original author, this book was nonetheless worth translating into the Japanese language. These comments, I thought, explained the shock that I felt when I read Edward Hall's books as a graduate student.

At that time, a similar phenomenon took place in Japan, only in the opposite direction. A famous Japanese social critic, Yamamoto Shichiei, presented himself

as a Jew living in Japan named Isaiah Ben Dasan, and published a book entitled *The Japanese and the Jew* (in Japanese) (Ben Dasan, 1971). The book not only sold more than one million copies but was granted a very prestigious book award (the Oya Soichi Award). As the book sold so well, the Japanese publisher decided to translate it into English and sell it overseas. I was intrigued to learn (many years after it was translated) that the American translator (although I do not know whether or not he was Jewish) felt so uncomfortable and embarrassed by many parts of the original Japanese version that he omitted them. As a result, the English version became much shorter than the original Japanese version. The Japanese readers' understanding, including mine, was that Yamamoto criticized the Japanese culture by contrasting it to the Jewish culture, which, according to Yamamoto, is "just the opposite" of the Japanese culture. I have no idea what bothered the American translator, but there must have been misunderstandings and exaggerations that he found hard to swallow --- something similar perhaps to what the Japanese translators pointed out regarding Edward Hall's *The Hidden Dimension* and *Beyond Culture*. Interestingly, no Japanese book reviews cautioned that the content of the best-selling and award-receiving book, *The Japanese and the Jew*, might make American or Jewish readers uncomfortable or feel offended. The Japanese, including myself, simply did not notice.

These experiences in the 1970s brought me to the following conclusions: (1) It is extremely difficult to discuss foreign cultures, and especially difficult to interpret human behavior in foreign cultures. (2) Those who discuss foreign cultures cannot escape from the mind-set that their own culture is normal and foreign cultures are by and large abnormal, weird, or inscrutable. (3) Research based on episodes, facts, or even statistical data is not reliable as long as they are arbitrarily selected by the researcher.

In the late 1970s and early 1980s much discussion was undertaken regarding "Western cultural imperialism," "Orientalism," and the like. At that time, I was asked by many Western as well as non-Western scholars and researchers at international conferences if the Japanese as non-Westerners suffer from American or Western cultural imperialism. My answer was that the Japanese certainly recognized and felt the "cultural imbalance" or even "threat" in the middle of the 19th century (or at the early Meiji period) and soon after the Second World War (especially during the American occupation period) but it was no longer a problem in the 1970s and 80s. It was not just my personal opinion. There was no Japanese expert or journalist at that time who argued that Western or American "cultural imperialism" was a serious problem for Japan. My non-Japanese friends asked me why I felt that way and reminded me of the unbalanced news flows between the United States and Japan, the existence in Japan of Tokyo Disney Land, McDonalds, Kentucky Fried Chicken, and so on. Although I did not have answers readily to hand at that time, it became gradually possible for me to answer these questions.

As for news flows, it is true that the news flows between the United States and Japan were unbalanced. Japanese mass media's coverage of the United States at that time was about ten times more than the American mass media's coverage of Japan. However, many empirical surveys repeatedly revealed that Japan was better covered than Germany, Italy or Israel although less covered than the United Kingdom, France, or the (former-) Soviet Union. Armstrong (1982) content analyzed American newspapers and compared the space given to the changes of Japanese prime ministers and those of German chancellors. The result was that the American newspapers tended to give more space to the change of Japanese prime ministers than to German chancellors. Should the Japanese have complained that Japan should be covered to the same extent as the United Kingdom, France, or the Soviet Union? Furthermore, news flows between major Western European countries such as England, France, and Germany and Japan were well balanced, and the rest of the world, except the Soviet Union and China, covered Japan better than the Japanese mass media covered them. Then, it is only natural that the Japanese did not think that the international news flows pattern as a whole was a problem for them.

What about the existence of Tokyo Disney Land, McDonalds, Kentucky Fried Chickens, and so on? Here again, the question was not really valid. As in the case of news flows, the comparison was made only between the United States and Japan. If we look at the flows of popular cultural products on the global level, Japan exported more than she imported. This pattern was true even between Western Europe and Japan, except for the United Kingdom in some years. At that time major Western European countries, including France and Italy, changed their national broadcasting policy and dramatically increased the number of channels and length of broadcasting time. As a result, new commercial TV stations were flooded with American dramas and Japanese animations. After the animations, "karaoke" penetrated into even Scandinavian countries and small country towns in the United States. I discuss these phenomena in my paper entitled "Trend Winds Change: Japan's Shift from an Information Importer to an Information Exporter, 1965-1985" published in *Communication Yearbook/13* (Ito, 1990).

Some Western observers argued that the appearance of so many Caucasian models in Japanese TV commercials and magazine advertisements had to do with "Western cultural imperialism" or even the Japanese national "inferiority complex". To me, however, the "inferiority complex discourse" sounded like an example of psychological projection. In other words, those who want to enjoy superiority want others to feel inferior to them. Do frequent appearances of children or dogs in TV commercials or advertisements, for example, mean that viewers suffer from inferiority complexes regarding children or dogs? Furthermore, not only white but also black talent, including celebrities such as Carl Lewis, Mohamed Ali, Florence Joiner, and Naomi Campbell, often appeared in Japanese TV commercials. Although the air-time frequency of black talent was definitely less than white talent, they were

still conspicuous considering the extremely small percentage of black residents in Japan. Some Westerners, who had lived in Japan for many years, complained that the way Caucasian models in Japanese TV commercials and advertising were used was blatant stereotyping and sometimes even insulting. They often appeared as cowboys, bunny girls, or in the nude. In a recent shampoo commercial, the straight black hair of a Chinese starlet (Zhang Ziyi) is admired and envied by curly-blond-hair models. Reflecting the “*kanryu* or Korean style boom” in recent years, many Korean actors and actress are used in TV commercials and advertisements.

Ignoring all these complexities, some Western scholars and researchers are still indulging in the old “Western domination model”, which I nowadays call “Western narcissism”. For example, according to Eric Kramer, an Oklahoma University professor:

In this article we argue that increasingly a single aesthetic body-image is emerging globally and that it is Caucasoid.....Due to global media domination, Western tastes are becoming world tastes. But this sets up an untenable situation whereby, the more a non-Caucasian internalizes this globalizing aesthetic, the more they attempt to “adapt” and fit the ideal mold, the more they are likely to come to see themselves as hopelessly inadequate, if not ugly (Kramer, 2000, p. 83).

However, the reality is that most of the “Caucasian body-images” that we are receiving every day over the internet are pornographic. I searched for and collected English-language articles similar to Kramer’s and criticized them in my article entitled “Globalization and Western Narcissism” (Ito, 2003), which, I believe, belongs in the category of “critical study” on international communication.

Robin Gill, who speaks and writes the Japanese language and lives in Japan as a free-lance writer, published a Japanese language book entitled *Anti-Nihonjinron [Anti-Japanism] Book*, in which he criticizes the Japanese prejudice against Western cultures that he called “Occidentalism” (Gill, 1985). He also wrote a long column entitled “Occidentalism: Preoccupation with the West” in *Asahi Shimbun* (Gill, 1990). Major examples of Japanese “Occidentalism” are as follows: Westerners are selfish or too ego-centric and too calculating, and never apologize because they are afraid of being sued. However, according to Gill, unfriendly or unfavorable prejudices are easier for Westerners to handle because they can refute them. Problems arise when the instances in which the Japanese envy Westerners are based on prejudices or preoccupations that have some seemingly good reasons. For example, the Japanese believe that white and black people have stronger sexual desires and potency because they are meat eaters. On the other hand, the Japanese believe that they themselves are more “botanic” because they live on rice, fish, vegetables and bean products.

According to Gill (1990), the Japanese believe that the Westerners’ brain works like an “on/off switch”, enabling them to adapt to new situations very quickly. Even

Doi Takeo, an internationally famous psychiatrist wrote as follows:

... the Westerner's expression of thanks is generally speaking, brief and to the point, with no unpleasant aftermath. If he says "thank you," that "finishes" it; there is none of the Japanese's lingering sense that ---- as the word *sumanai* literally signifies --- things "are not finished" (Doi, 1973, p. 90).

In other parts of his book Doi makes the same argument regarding apology because the Japanese use "*sumanai*" or "*sumimasen*" when they apologize as well. When the Japanese apologize by saying "*sumimasen*" it means that the feeling of apology is not yet finished and will last a long time. However, the Japanese believe that once Westerners say "Thank you" or "I am sorry", that's it. They soon forget what happened and their attention shifts to other matters. The Japanese envy this "efficiency".

The Japanese who are going to the United States are advised as follows: "In America, when you want to say "maybe", say "no". When you want to say "no", say "no way", and when you want to say "no way", shout "f**k you!" Although this is taken as a half-joke, this kind of image held by the Japanese affects Japanese translations of English expressions. Gill (1990) points out that the Japanese translations of English language documents or literature are always made to be simpler, clearer, and "more logical". In order to meet readers' expectations, Japanese translators tend to translate "B rather than A" in the English original as "B not A". Reserved expressions such as "seems to be", "might be", "would be" tend to be translated into Japanese as simple "is" or "are". These translations further reinforce the Japanese image of Americans as straightforward, simple, and "logical" in the strictest sense. In the recent American movie entitled "Lost in Translation", an American business executive dispatched to Tokyo makes a speech in English before Japanese employees. A Japanese interpreter standing next to him is supposed to interpret his speech paragraph by paragraph. The American businessman becomes perplexed because all the paragraphs that take two or three minutes for him to say are translated by the Japanese interpreter as five to ten second phrases. Although that movie scene is only a caricature, it reflects the stereotyped image of Americans who are believed to be straightforward and say nothing subtle, delicate, unclear, or complicated.

These experiences reminded me of the misgivings that I had about Edward Hall when I was a graduate student. For any scholar or researcher in the world their own culture is normal and foreign cultures are somewhat abnormal, weird, or inscrutable. Similarly, for those who have strong beliefs or ideologies, theirs is normal and all others are abnormal. Therefore, the research method by which the researcher arbitrarily or non-systematically selects episodes, facts, or statistical data in order to "prove" some theory or hypothesis is quite problematic. This method, which I consider a kind of "critical method", is valid when its user's intention is to criticize and not prove something because revealing and discussing the intentionally ignored

episodes, facts, or statistical data can make effective criticism. I believe, however, those who intend to *prove* something should use some systematic method where the results are unpredictable or uncontrollable.

In 1987, I happened to come across the memoirs of Mr. Hata Seiryu, who was the Editor-in-Chief of the Asahi Shimbun, the most prestigious national newspaper in prewar Japan. He also worked as a leading journalist before World War II. Reflecting on his entire journalistic career and especially the hectic 15 years from 1930 through 1945, he wrote just before his retirement in 1987 the following :

Newspapers at that time did not necessarily try to flatter those in power. Rather, they wrote to please readers. I may sound evasive, but there certainly existed some kind of mechanism that aggravated the situation through subtle interactions (between newspapers and the public). Readers were hungry for articles reporting the exploits of the victorious Imperial Forces. Newspapers indulged themselves in a competition to appear more patriotic and to see who could print the most articles urging and exalting victory. Newspaper companies cooperated through the dispatch of entertainers, calls for patriotic songs, campaigns for contributions to build more airplanes, and in various other ways. The heavy responsibility that the newspapers bear is second only to that of the government. However, I disagree with the claim that “the general masses were victims”. Newspapers form public opinion, but public opinion also influences newspapers. The general masses are not like horses that can be tamed and trained to do their master’s bidding (“Senso”, 1987).

Were there any wars in history that were caused by public opinion rather than dictators? Historians say yes, and that the oldest example is the Peloponnesian War between Greece and Sicily that erupted in 415 BC. Thucydides, the great Greek historian who was embedded with the Greek forces, describes how the Greek decision to send an expeditionary force to attack Sicily was influenced by jingoistic public opinion at that time. However, as that is ancient history let us not discuss it here. (See Ito, 2002 for further details.) The Spanish-American War of 1898 is a better example.

The *Encyclopedia Americana* describes the Spanish-American War as follows: Spain “was in no condition militarily or economically to fight the United States”, and consequently the Spanish government “employed every means to prevent the outbreak of war”. The Spanish government informed the American ambassador in Madrid “that the Spanish government was making all the concessions that public opinion would tolerate”. However, President McKinley, on his side, was “under tremendous pressure from public opinion to embark on a war to liberate the Cubans from “Spanish tyranny” and avenge the Maine (that sank in Havana harbor from an explosion causing the death of 260 members of the crew).” (*Encyclopedia Americana*, 25, 360w). And, of course, American newspapers at that time helped whip up pro-war sentiments.

There are hundreds of books written on the Spanish-American War and Japanese history from the Manchurian Incident until the end of the Pacific War. However, their methods are all historical or journalistic. How can we, as social scientists, contribute to the study of the mechanisms of that interaction between public opinion and the mass media that may cause a major war?

There exist no opinion-poll data on the Spanish-American War or the Japanese wars in the 1930s and 40s. However, it seems to me that if newspapers are supposed to reflect public opinion, it should be possible to deduce public opinion from the contents of newspapers. Actually, many scholars have done this in the past but only in descriptive ways. My idea is to extract public opinions from old newspapers not only as text data but also as statistical data.

Newspapers report a variety of opinions representing organizations and individuals. My students and I classified every opinion in the newspapers into the following categories: the government, the ruling party, opposition parties, the mass media, the business community, labor unions, specialists, intellectual leaders, ordinary people, foreign governments, and so on. Then, coders judged to what extent each of these opinions was favorable or unfavorable to the issue or subject of our concern. By adding up the points, we estimated to what extent each section was favorable or unfavorable to the issue at hand.

Compared with the government and the mass media, it is difficult to establish and rationalize the category of “public opinions”. However, we have statistical data for components of public opinions such as ordinary people, opposition parties, intellectual leaders, and so on. By combining these components, it should be possible to establish the category of public opinions and measure the percentages of favorable or unfavorable opinions regarding the issue of our concern. Even if different experts cannot agree on the definition of public opinion, the longitudinal statistical data of public opinions should be valuable as long as they depend on a consistent and unified definition.

So far, I have completed more than five research projects based on this premise. (See Ito, 2002 for papers published in English). In order to classify each opinion in the newspaper articles into appropriate categories, we need to have strict, detailed, and concrete definitions and understanding of the government, the mass media, and public opinion. This is one of the contributions that this method makes to the conceptualization and theorization of the interactions among the government, the mass media, and public opinion.

Last summer my father died at the age of 91. A few months later, I received a long letter from his younger brother who is now 86 years old. He wrote to me that he greatly missed my father and regretted that he did not have a chance to discuss seriously the meaning of the wars for which they had to sacrifice their most precious years. As a matter of fact, they had another younger brother but he lost his life on a battlefield in China. Therefore, my father’s younger brother earnestly wrote to me

that he would like to learn what my father said about the war during his lifetime because he would like to learn the meaning of the war as much as possible while he still had his full faculties.

The social sciences, including their methodologies, are supposed to be neutral, objective, and rigorous. However, they do not exist in a vacuum. In order for social scientists to produce unique and meaningful works, they need special motivations. These motivations may be traumatic experiences or events such as those currently going on in the Middle East, some kind of mental complex, or national fiasco, as in the case of Japan from 1931 through 1945. As for my method of research, I will use the “critical method” when I wish to criticize some theory or study and use the empirical or orthodox scientific method when I wish to prove something. Finally, one of the practical lessons that I learned regarding international communication education since my first encounter with Edward Hall’s books is that it is better not to stress the differences too much because, after all, people tend to think that their own culture is the normal standard. The differences become exaggerated as people talk about them. If, for example, people are repeatedly told that Americans are individualistic and the Japanese are collectivistic, negative stereotyping ensues on both sides, and in several years, the following may happen: Americans will come to believe that the Japanese are all authoritarian and the Japanese will come to believe that Americans are all selfish.

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Reflections on the Field, its Relevance for the Public Interest and its Methodological Divisions, based on Personal Experience

by Denis MCQUAIL

If I begin at the beginning in 1958 -- the year of first graduation -- I was quite innocent of inter- or intra- disciplinary clashes over methodology or of any subject under the name of Communications. In fact I graduated in history, which appeared to me to be characterised by clashes of personality and style, not methodology. Essentially one chose either to recount “the facts”, or the facts as they fitted some narrative of national or human development.

I eagerly made the transfer to the social sciences, attracted by three things: the promise of generalization; the potential of explaining and predicting with some measure of certainty; and a sense of the relevance of contemporary social research to real world events and problems. My initiation into disciplinary conflicts was largely confined to Merton’s warning that schools of thought diverged according to whether one gives priority either to certainty or to significance in knowledge (“We don’t know if what we have found is significant, but at least its true” versus “it may not be true but at least its significant”).

My idea of the public interest, which I hoped to serve in the course of earning a living, was shaped by personal background (itself influenced by the optimistic climate of post-war reconstruction and reformism in Europe) plus youthful faith in the potential for social engineering for a better society. The potential of social science then seemed considerable. My adoption of television as a field of research happened accidentally (another Mertonian principle exemplified – that of serendipity). I had some doubts that it could count as being in the public interest, because, from that vantage point, it seemed insufficiently serious or problematic. Forty years later I do not have the same doubts.

I served a short but intensive apprenticeship under a man of eclectic talents, Joseph Trenaman, who was an autodidact with no first degree and thus no disciplinary bias. He had a strong belief, derived in part from his career in the BBC, in the potential of mass media to be an effective popular educator. He was also a devotee

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of statistical methods, especially using quasi-experimental designs, attitude scaling and factor analysis. His attachment to such methods was modified by the view that they should only be applied after acquiring a good understanding of any topic of public attitude and of the discourse and universe of idioms current in the relevant public.

Trenaman was also a strong believer that scientific research should be guided by social and moral purpose. I did my best to follow my mentor's footsteps for the short period his tutorship allowed to me. Our research into the social impact of the new medium of television, especially its role in politics, was informed by expectations of public benefit as much as by fears of harm. My own Ph. D. dissertation research into the television audience aimed to identify and understand social obstacles to the enjoyment of cultural benefits from television.

My first experience of research in communication was exciting, satisfying and educative, although I was also affected by the wider sense of disappointment at that time at discovering so little seeming influence from television, so many "no significant differences". Bernard Berelson had recently and wrongly declared the field of communication largely defunct. I was nevertheless somewhat shocked when our published research report on the political influence of television was dismissed by one reviewer as an example of "abstracted empiricism", in C.W. Mills' then fashionable phrase.

My personal feelings aside, I had some sympathy with this view and resolved to keep my eye open for such ideological icebergs, without giving up the general line of inquiry into television audiences and effects using standard survey techniques. Then and subsequently, there did not seem any very reliable alternative to the established methods of empirical research if certain key questions were to be answered. Even so, I was beginning to learn that methodological choices can also be philosophical and political choices. Methodologies are also cultural practices and carry more baggage than appears at first sight.

I did have some private doubts about the adequacy of our methods, however apparently sophisticated, to reveal much about fundamental processes at work in communication. I was struck by the unlikelihood that the complexities of opinion, attitude, belief and behaviour could be captured by numbers, except in very crude ways. It also seemed to me illogical to expect quantity of "exposure" in itself to have a linear relationship with the qualities that are represented by ideas, beliefs, feelings, tastes, etc. or with significant personal actions and choices. It was at least clear that important issues of public of communication could not be tackled without reference to the intellectual disputes about quantity and quality that raged during the 1960's. They were also at the centre of political and ideological disputes in the public sphere, especially in respect of the part played by the media as instruments of power for governments and the established order generally.

Intellectual life was both enlivened and made problematic by the political and

ideological storms of the period from about 1965 to 1975. This was also a time when the notion of a public interest as something to be determined by public authorities was irretrievably fragmented.

By this time I had transferred from being a full time media researcher to become a teacher of sociology, including research methodology and methods. I was also working with Jay Blumler on the mediating role of audience expectations and experience (a focus that came to be known as “uses and gratifications”) in the process of influence of political communication and on the larger question of patterns of choice. Our public interest goal was to shed light on the possibility for reconciling underlying audience needs with the imperatives of media communicators and industries, with some notion of informing public policy. We also hoped, in some way, to be able to speak “for the audience” through research.

Our methods still involved a combination of open-ended inquiry in detail if not great depth and the conversion of knowledge gained into conceptual instruments (the gratifications verbalised) for application in surveys. We were dealing essentially in qualities, but trying to quantify them for operational reasons and for achieving dependable descriptions and empirical generalisations. In retrospect, I believe that we had some success methodologically, although I am less sure there was a visible public interest benefit.

This particular communication research enterprise was also assaulted in the early 1970’s, at least in our corner of the world, from the direction of critical sociology and we (in a collective sense) were accused of psychologism, individualism, behaviorism, functionalism and service to the mammon of media industry. There was another objection to what we were trying to do that I found more convincing: that we proceeded with little attention to the intrinsic character of the cultural object and the cultural experience. We were simply “not seeing the movies” and not even trying to.

In self-defence at this time, I prepared with Michael Gurevitch an argument for considering our and similar work as distinct from functionalism and in keeping with phenomenological sociology. But in England the critical schools of political economy and semiology were in the ascendant and the task of studying audiences was allocated to critically motivated reception analysis that sought meanings of audience experience in everyday life and sub-cultural experience. In terms of Merton’s choice mentioned above, the slogan might read “We are not looking for certainty or significance but for what is culturally true”. An endless accumulation of truths was not to my mind a very satisfying goal.

A temporary escape was offered in 1975 by an assignment to work for the Royal Commission on the Press inquiry into the standards of British newspapers, then as now widely thought to be abysmal. My task was to conduct a content analysis designed to yield some evidence on the burning issues of bias and diversity. It was an almost purely quantitative inquiry on the Berelsonian model, as determined by the

wishes of the commissioners and their civil service advisers. At least it was by definition “in the public interest”, and I could imagine that the results yielded some positive results for the democratic system, as part of a process of public accountability of media and giving pointers to policy.

The work was again formative for me personally, despite the barren nature of the methods dictated by circumstance. I discovered the limitations of content analysis, but also the indispensability of having defensible evidence of a quantitative and “factual” kind in any significant public debate on politically sensitive issues (almost always the case where media are involved). It also opened up intriguing questions about the relation between normative (or just qualitative) concepts relating to press standards and empirical evidence. I was also made aware of the strength of the existing tradition, largely in the United States and Scandinavia, of research that was both critical and empirical in relation to media content and performance, thinking of the work of Lemert, Stone, Rosengren and numerous others.

My transition to Amsterdam in 1977 as this work ended was fairly traumatic, although it provided another escape from sterile disputes at home, which had not yet affected the continent in the same manner and degree. The move was to provide new and important lessons relevant to our theme. One concerned the cultural relativity of my own understanding of what was going on out there and by extension the cultural relativity of our field. Rightly or wrongly I felt hindered by barriers of language and culture from doing either survey research or content analysis of any ambition or significance, for years at least. I had also lost a connection with the familiar contours of my home public sphere and hampered in identifying a public interest. The latter proved more easy to repair than the other deficits

My response to changed circumstances was to refocus on the kind of work that might escape the problems signalled. This involved choosing more “culture-free” aspects of the field, namely theory, comparative research, the impact of new technology and media policy. Of course nothing is entirely culture-free, but my choices were at least in line with the aspiration, never entirely given up, to contribute to something like a “science of communication” that has some general application.

I turned especially to the task opened up by my Press Commission experience, namely that of developing a framework of concepts for research into press performance evaluation (McQuail, 1992). The key terms were much the same in the Netherlands as in England, as in the USA, especially diversity, equality, fairness, truth and independence. The intellectual aim was to objectify such ideas in the form of concepts that could be matched by observable data about media content, conduct and structure. The search for operationalisability was also a search for meaning as much as a search for quantities or “facts”. I wanted also to help develop and strengthen normative theory of press and media. My contribution to “better media” would be a clearer understanding of what this means. Without being conscious of it, I was following another dictum of Robert Merton that empirical research goes far beyond

the role of testing theory: it helps also to formulate and clarify theory. The public interest aim was to facilitate evaluative inquiry and inform public debate and possible policy-making with reference to media accountability (McQuail, 2003).

My end brings me back to the beginning, with the challenge of relating quality to quantity, meaning and measurement, in a satisfactory and fruitful way, where “quality” can be understood as human difference, cultural variety or “ideas” in many forms. The challenge that seems posed more strongly in the field of communication than other social or human sciences. I reach some summary conclusions on the topics for this special section.

1. Methodological disputes are both fruitful and barren. The alternative methodologies and observational methods at our disposal can be considered as alternative languages or forms of discourse offering different but not necessarily incompatible versions of the “truth”. Exposure to alternatives is beneficial. Exclusive, or prioritising of, claims will produce conflict without light.
2. My own experience has highlighted the somewhat arbitrary part played by the climate of the time in terms of more or less fashionable theory and also of political events and ideology. We have to respond to unpredictable events and circumstances. Our practice of science is much shaped by cultural variation and the material facts behind that. This can have positive and negative consequences.
3. There are limits to the range of communication phenomena that any one science can satisfactorily deal with because of the immense diversity, creativity, unpredictability and randomness of much of actual human communication.
4. We not only have to struggle to reconcile quality and quantity. We have to find a place for imagination and creativity for which there are no rules. We also have to recognise the part played by serendipity, another Mertonian notion, and of course, luck (good or bad).
5. There are large areas of our field that do not fit within a common disciplinary framework.
6. There is no certainty about what does or does not constitute the public interest. In my own account I have identified at least four possible versions: working as a servant of the public; pursuing truth disinterestedly on the utilitarian assumption that it will somehow add to the sum of human welfare; following some personal ideal of the good society or good conduct; taking a critical view of communication phenomena according to a chosen value position. We make our choice and whatever it is, it does provide a goal. With a destination we have some chance of finding our way, even if it is never guaranteed. Without a destination, at least we’ll never lose our way, but are unlikely to reach anywhere we want to be.

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One Journey Through, Across and Around Communication

by Barbie ZELIZER

Whenever the topic of methodological and disciplinary divides is broached, one presumes a need to place or situate oneself on one side or another, regardless of which terrain is being sectioned. Placement involves selection, identifying with a presumed perspective and marking boundaries. It requires decision-making about identity that divides the rest of the world into insiders and outsiders and is accompanied by labels that appear natural and self-evident -- between quantitative and qualitative researchers, empirical and interpretive scholars, or behaviorist and cultural enclaves.

At the time of its invocation, claiming placement in the academy makes sense for a variety of reasons and strategic purposes. It helps communicate to others who one is and with whom one can generate conversation most easily. But in the long run, proclamations about placement can become predictable, static and even counter-productive. They take on a fatigued and overly familiar cast that can undermine the larger field that they originally seek to define. Thus, the notion of thinking about one's placement in the field as a journey rather than a sequestered and time-honored position is valuable. It offers a way to think alternatively about scholarly identity and how one fits into the broad range of a discipline in a manner that sits well not only for individual scholars but for the field of communication as a whole.

This paper traces my personal journey through, across and around the field of communication. Recognizing that each of us reflects the strategic decisions that we have taken in our personal and professional lives, I argue here that it is critical to value the broad, contradictory and often idiosyncratic nature of our identity as scholars and our consequent placement alongside other scholars as a way to maintain the vitality of our field. Moreover, I argue that it is on the margins of the areas of inquiry we inhabit that we can most effectively keep the field of communication vital and responsive to the concerns of the public sphere that put us here from the onset.

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Placement as Journey

The word “journey” is popularly referenced as the act of traveling from one place to another or the time and distance involved in doing so. Suggesting an action taken in search of a defined aim or objective, realized incrementally, journeys connote a sequencing of action over time and space, a sense of direction, and often a consistency of purpose.

Thinking about one’s place in the field as a journey draws on three related observations, which have both literal and figurative dimensions:

- | One’s placement is not static, but changes.

- | One’s placement today is impacted not only by where one has been before but by the people one meets and the issues one encounters. It is also impacted by the people one does not meet and the issues that are not encountered. In other words, who one meets on one’s journey directly shapes the places one inhabits in the field. Placement is, therefore, both constructed and contingent.

- | Who one is today is not who one will be tomorrow. Alongside any recognition of placement needs to be a similar recognition of the certainty of change and the need to accommodate unknown trajectories as one sets and resets boundaries.

These notions, while commonsensical to many, have not been intrinsic to how academics “place” themselves in the academy. Rather, we tend to fall back upon a degree of nearsightedness, by which one’s place in the field becomes somewhat sanctified. It is presumed to be static, to emerge from a pristine process of knowledge acquisition, and to bear a somewhat lofty, steady and often uncritical guardianship over the field’s future. By contrast, when seen as journeys, academic trajectories can be thought of as less certain entities. They are more porous and more accommodating to one of the overarching tensions involved in being an academic – how to manage the uneasy co-presence between internal consistency and the inevitability of discrepancy and change. In a field like communication, which is widely impacted by changes in technology, in institutional settings and in the public’s perceptions of and relations with the media, this is no small feat.

Journeys, however, do not take shape in a random fashion. They draw upon perceived expectations. In Biblical usage, the word “journey” referred to the permissible distance one was allowed to travel on the Sabbath: According to Jewish tradition, one was permitted to travel the equivalent of 2,000 paces from the city walls without violating Jewish law (Exodus 16.29). Such an example has much to say about the fact that journeys proceed within marked though often unarticulated boundaries, according to certain rules, and around presumed violations. They are thus only recognized as journeys when they work within prescribed expectations.

Such has long been the strength and weakness of communication. The field’s strengths have derived from the fact that certain journeys have been vastly successful

and thus widely repeated. Such journeys have charted the territory in ways that have allowed us to etch out a coherent field alongside the more longstanding areas of inquiry, like sociology and psychology. But the field's weaknesses have emerged too: certain journeys have become so naturalized that they are now recognized as "places," while other journeys, less central to the field's sense of self, have been turned into "places" that remain marginal to the field's center. There is, then, an unarticulated consensus of what matters – and what does not – in terms of the field's definition, that draws from the notion of placement and codifies journeys as places often to the field's detriment.

How all of this has played out in my own journey is the topic of this essay. I address it by first offering a partial personal biography, drawing from the assumption that how one frames questions derives directly from who a given scholar is. I then explain how that biography links up with my sense of what the larger issues in the field perhaps need to be. The basic point of doing so is to demonstrate that it is primarily on the margins of our consensus with others that we can think most productively about the value of what we hold as constant. Disciplines become somewhat tiresome, predictable and wither when there ceases to be active discussion of the givens that constituted them to begin with. This is particularly the case with communication, a case made more crucial by its role in serving the public interest. In other words, withering is not something that communication scholars can afford to do.

Journalism as a Road Toward Communication

I started my professional life somewhat out of the fold – as a journalist. And it was as a journalist that many of the concerns germinated that have occupied me as an academic ever since. Questions about whether journalists had the right to provide authoritative stories about the world were primary in my days as a wire service reporter, when I saw numerous instances of that authority being mishandled, misshapend and ultimately misreported.

To this day, I remember reporting on a certain Palestinian demonstration on the outskirts of Bethlehem, which made it onto the front pages of the U.S. press. The story that was published barely resembled what I had witnessed, and to make matters worse, the featured version bore the names of reporters who had been nowhere near the scenes on which they reported. I remember feeling both bewildered and somewhat cheated, in that I knew that few others had the on-site knowledge to critique what was conveyed as the story of that demonstration. Moreover, I wondered how many other news stories were put forth with a similar set of disjunctions.

From such a background I gravitated toward the academy, where I thought I could pursue the questions that were bothering me as a reporter. Pragmatic questions about how journalists had the power to report the world in the way they did propelled

me toward an academic engagement with journalistic authority, seen through the prism of communication.

On my journey, I went in unexpected directions. I dabbled in the academy both abroad and in the United States, receiving a wide-based exposure to European and U.S. derived approaches to communication. In Jerusalem, I worked with Elihu Katz and Daniel Dayan at the Hebrew University, where I was able to broach empirical sociology and semiology in a way that sensitized me to the fact that different kinds of scholarship had different strengths and weaknesses and that no complete answer could ever be provided by one type of inquiry. By the time that I came to the States to study for my doctorate at the University of Pennsylvania, I was acting on that observation, taking classes not only in communication but also in folklore, anthropology, sociology, literary criticism and linguistics. Studying with a wide array of notable scholars – Larry Gross, Roger Abrahams, Charles Bosk, Dell Hymes, Bill Labov – I learned that journalism needed to be accounted for as a phenomenon that imploded disciplinary nearsightedness, and the value of my training suggested that such was the case regardless of what one studied.

This meant that by the time I was ready to declare myself a full fledged academic, I was an interdisciplinary rag doll. My multiple perspectives – borne out by a PhD in communication with an emphasis on performance studies in folklore -- made sense to me but not necessarily to others in the academy. I had a degree in communication and I studied journalism -- both points which would seem to have a clear resonance to others in the field -- but not in ways that many others recognized. This was because I was invested in tracking journalism through linguistic, cultural, visual and interpretive prisms that were not part of the most frequented frame for thinking about journalism as part of communication.

Thus, I faced a definitional problem from the very beginning. In my case, I was wedged in between two definitive populations – journalists, on the one hand, who were not very interested in anything an academic had to say (even if she had formerly been among them) and academics, on the other, who in both the broad and narrowed analysis of journalism did not readily use the methodological or epistemological tools that I had come to favor (see, for instance, Zelizer 1993a and Zelizer 1998). Specifically, here I refer to what might be called the default setting of journalism scholarship, shaped by two fields – sociology and political science (for more on this, see Zelizer 2004).

Sociology set the stage for thinking about journalism, tracking the structures, organizations, and institutions that guided journalists' work as well as the relationships and work routines involved in gathering and presenting news. Extending largely from the newsroom ethnographies of the seventies (Tuchman 1978; Gans 1979), this work was responsible for developing a focus on the structures, functions and effects through which journalists worked (ie., Tunstall 1971, Curran and Gurevitch 1991). Elsewhere, political science tended to think about journalism through an emphasis

on its public impact – that is, seeing journalism through its effect into the political process (ie., Entman 1989, Patterson 1993). Promoting a largely normative interest in journalism that derived from longstanding expectations about journalism acting in primarily capitalist democracies as government’s fourth estate, this inquiry assumed an interdependency between politics and journalism and queried how journalism “ought” to better serve its publics under optimum conditions.

Neither lens reflected what I was most interested in studying. I wanted to force a pause into the process of academic inquiry, to look at how journalism made sense to journalists and how they imported their collective knowledge into the material we call news. Journalism, to me, was both a craft, a way of thinking and a lived practice, and I wanted to find a way to accommodate such nuances in my scholarship. Though these issues drew the greatest degree of interest from journalists, they did not exactly want to give me a say in speaking about their world. And while certain academics were looking at things in a way that resonated with me (especially Carey 1989, Schudson 1995), they still remained few and far between. To make matters worse, though I was not trained as a historian, I gravitated toward thinking about journalism in earlier times and saw journalistic practice as connected with collective memory, and so my insistence on temporal nuances made me even harder to place. And through it all, issues of public interest – of thinking about journalism and how and why it went wrong and right and what this meant for the body politic – remained at the heart of my concern.

Now, years later, I can reflect on all of this with a more generous degree of equanimity than perhaps I felt at the time. But I do know that it produced an extraordinary degree of navel gazing over the past 15 years or so. I have always found myself engaged in navigating around the issue of identity, and that navigation has permeated my job searches, my topic permutations, even the courses I teach. My first job was in a dept of rhetoric and communication, where, because I studied media, I was codified as a cultural studies scholar. Later when I had a stint elsewhere as a visiting historian, I had to give up my contemporary interests in journalism to focus on the past. And most recently, I served as a fellow at an institution defined by its contemporary interest in the news, which meant that I needed to drop my historical interests for the interim.

This happens to most scholars, in that we reinvent ourselves to fit the context at hand – the dissertation committee, the tenure and promotions committee, the editorial board of a journal, the foundation or government agency’s request for proposals. We are surrounded in the academy by a number of interpretive communities, each of which establishes and maintains itself on the basis of shared interpretive strategies and tacit knowledge about what matters as evidence and why (Zelizer 1993b). These interpretive strategies are neither constant or natural, but they are continually being negotiated by other people with like interests – sometimes in hierarchical or politicized ways. How they settle and resettle questions of value is central to

understanding the collective that emerges around them. Thus, not only does communication itself constitute an interpretive community of sorts, but so do the other disciplines which inhabit its borders. The subfields of communication function in much the same way. All we need consider is the proliferation of ICA's divisions and interest groups as evidence of the ways in which we have systematically learned to demarcate our boundaries as a field, to mark with whom we are willing to converse and under which circumstances.

All of this suggests that the forces that help maintain a social group's solidarity matter. Such a notion has been suggested by a wide variety of scholars, including Emile Durkheim (1965[1915]), Thomas Kuhn (1964), Michel Foucault (1972), Nelson Goodman (1978), and Mary Douglas (1986). What each of them demonstrates is that social questions matter in framing scholarship as much as intellectual ones. The more we surround ourselves with people who think like us, the less we need to challenge the strategies by which we set ourselves and our inquiry in place. Conversely, the more we encounter people who think differently than us, the more we need to revitalize the givens that have allowed us to grow comfortable and more entrenched in our received view of how we think the world works.

On Communication as an Academic Intersection

In thinking about the field of communication, these ideas bear particular relevance. Its interdisciplinary nature, the changing tides of circumstances with which it regularly must deal, and the heavily traveled roads on which it treads with other fields all suggest that communication functions as an academic intersection for many journeying across the academy. It offers a pause that can and should continue to lead in many directions. This means that keeping it porous and keeping ourselves talking about what communication might be remains not only of value but of necessity.

We need to keep asking a slew of questions that have to do not with what we know but how we come to know it and why. This includes questions like what makes us conceptualize communication in one way or another – a particularly strident doctoral advisor or some seemingly intrinsic relation with our own lives? How do we account for what we think we see, and which tools do we use to explain it? To whom do we hope to speak and under which institutional constraints? How do many of us navigate the terrain we share with others with whom we do not necessarily agree? How often do we attempt to negotiate consensus (even partial) across different ways of knowing?

As guardians of the character and future of the field, we need to keep asking these questions. Our goal should be how to keep vibrant the kind of generosity of spirit by which our own field first came into being. The numerous sociologists, political scientists and social theorists who watched our field emerge in the beginning

recognized (happily or not) that communication had enough of a core that justified it claiming its own terrain.

But there is need to recognize that the terrain is everchanging. The same generosity of spirit that allowed us to come into being needs to be kept alive us afloat. We need to be continually cognizant of and respectful towards different methodologies and epistemological viewpoints. If we do not, we shall wither.

There are many ways to accomplish such an aim – conference sessions on issues that by definition cut across methodologies, journals that track issues in ways that use more than one epistemological viewpoint, even university curricula that force an address to the varying areas of our discipline. Even when thinking about the public interest, where much of the ongoing academic intervention comes from funded research, there is need to open ourselves up beyond the obvious questions. Political activism is one way to address public interest, which has not occupied center ground in our field because it presumes a slightly different epistemological answer to the question of what communication is for and a different methodological preference for how to make that happen. As someone who falls into that part of the academy that doesn't ask questions in a way that tends to get easily funded, my intervention in the public interest has moved in directions other than funded grants: I write columns, produce essays for the media, consult on endeavors having to do with journalism in trade organizations and trade journals, and advise on professional and pedagogic curricula. There is need to recognize that all of these activities – not just some – keep the conversation going, though they take different pathways in doing so.

Being in the academy is thus in part shaped by our being in the world. It is up to each of us to figure out how to keep communication attentive to all voices and areas of the field, not just those that provide recognizable kinds of data or familiar methods for examining new data. Our negotiations over these issues will always be ongoing because the parameters of our field are always changing. Our journey thus needs to continue for as long as we call ourselves academics, shaped by a recognition that the value of navigating methodological divides is not only to address our own tensions about where we fit in the academy, how we got there, with whom we need negotiate whether we stay or move on. Rather, we need to remember that the value of navigating methodological divides is that it keeps the field vibrant, relevant and provocative. It keeps our field's eyes open to the world. Our mission in addressing the public interest means that we can do no less.

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